THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL

REVIEW

OR

QUARTERLY JOURNAL

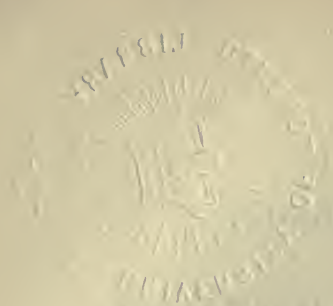
OF

PRACTICAL MEDICINE AND SURGERY.

VOL. LII.

JULY—OCTOBER, 1873.

LONDON:
J. & A. CHURCHILL, NEW BURLINGTON STREET.
MDCCCLXXIII.



CONTENTS OF NO. CIII.

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1873.

Analytical and Critical Reviews.

PAGE

REV. I.—1. Air and Rain: the Beginnings of a Chemical Climatology. By ROBERT ANGUS SMITH, Ph.D., F.R.S., F.G.S. London, 1872 . 1

2. On Chemical Climatology. By R. ANGUS SMITH, F.R.S. 'Journal of the Scottish Meteorological Society,' January, 1870, No. xxv . ib.

REV. II.—1. Théorie des Bruits Physiologiques de la Respiration. Par le Dr. L. BERGEON. Paris, 1869. Pp. 20 . . . 13

2. Des Causes et du Méchanisme du Bruit de Soufflet. Par le Dr. BERGEON. Paris, 1868. Pp. 103 . . . . ib.

3. Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques. Art. "Auscultation." Par ALFRED LUTON. Paris, 1866. Pp. 122 . ib

4. Étude sur la Respiration; Recherches Physiologiques sur le Méchanisme des Bruits respiratoires. Par M. le Dr. BONDET, Médecin de l'Hôtel Dieu de Lyon. 'Gazette Hebdomadaire,' 1863, p. 798, et seq. ib.

5. Études Pratiques sur les Murmures Vasculaires, ou Bruits de Souffle et sur leur valeur Séméiologique. Par M. A. CHAUVEAU, Sécrétaire adjoint de la Société Impériale de Lyon. 'Gazette Médicale de Paris,' 1858, p. 247, et seq. . . . . ib.

REV. III.—A System of Dental Surgery. By JOHN TOMES, F.R.S., &c., and CHARLES S. TOMES, M.A., &c. &c. Second edition, revised and enlarged, with 268 engravings. London, 1873 . . . 32

REV. IV.—Cooper's Dictionary of Practical Surgery and Encyclopædia of Surgical Science. New edition, brought down to the present time. By SAMUEL A. LANE, Consulting Surgeon to St. Mary's and to the Lock Hospitals; formerly Lecturer on Surgery at St. Mary's Hospital; Member of the Court of Examiners of the Royal College of Surgeons. Assisted by various eminent surgeons. In two Volumes, Vol. I, 1861; Vol. II, 1872. London 39

PAGE

Rev. V.—On Food: its Varieties, Chemical Composition, Nutritive Value, Comparative Digestibility, Physiological Functions and Uses, Preparation, Culinary Treatment, Preservation, Adulteration, &c. Being the Substance of Four Cantor Lectures by H. Letheby, M.B., M.A., Ph.D., &c. 57

Rev. VI.—The Principles and Practice of Veterinary Surgery. By W. Williams, M.R.C.V.S., F.R.S.E., &c. Edinburgh and London, 1872. Pp. 685 71

Rev. VII.—1. Traité de Chirurgie d'Armée, par L. Legouest. Paris . 92
2. Le Chirurgie Militaire, par Leon le Fort. Paris . . ib.
3. La Mortalité dans l'Armée, par Le Dr. J. C. Chenu. Paris . ib.
4. Code des Officiers de Santé, par P. A. Didiot. Paris . . ib.

Rev. VIII.—1. Illustrations of the Influence of the Mind upon the Body in Health and Disease. By Daniel Hack Tuke, M.D., M.R.C.P. 1872 110
2. The Power of the Soul over the Body, considered in relation to Health and Morals. By George Moore, M.D. 1846 . . . ib.
3. Theory of the Influence exerted by the Mind over the Body in the Production and Removal of Morbid and Anomalous Conditions of the Animal Economy. By John Glen, M.A. 1855 . . . ib.

Rev. IX.—1. On the Pathology of the Morbid State commonly called Chronic Bright's Disease, with Contracted Kidney ("Arterio-Capillary Fibrosis"). By Sir W. Gull, Bart., M.D., D.C.L., F.R.S., and Henry G. Sutton, M.B., F.R.C.P. 'Med.-Chir. Transactions,' vol. lv, pp. 273—326 132
2. On the Pathology of Chronic Bright's Disease with Contracted Kidney; with especial reference to the Theory of "Arterio-Capillary Fibrosis." By George Johnson, M.D., F.R.S. 'Proceedings of the Royal Medical and Chirurgical Society,' vol. vii, No. III, pp. 101 to 105 . . . . . ib.

Rev. X.—Neuralgia and Kindred Diseases of the Nervous System; their Nature, Causes, and Treatment; also a Series of Cases, preceded by an Analytical Exposition of them, exemplifying the Principles and Practice of Neuro-Dynamic Medicine. By John Chapman, M.D., M.R.C.P., M.R.C.S. London, 1873. Pp. 512 . . . 138

Bibliographical Record.

Art. I.—Recent Medical Periodicals 148

Art. II.—On Mankind, their Origin and Destiny. By an M.A. of Balliol College, Oxford. London, 1872. 155

PAGE

ART. III.—The Microscope and Microscopical Technology. A Text Book for Physicians and Students. By Dr. HEINRICH FREY, Professor of Medicine in Zurich, Switzerland. Translated from the German and Edited by GEORGE CUTTER, M.D. New York, 1872. Pp. 658 . 156

ART. IV.—Ozone and Antozone—their History and Nature. When, where, why, how is Ozone observed in the Atmosphere? By CORNELIUS B. FOX, M.D. Edin., M.R.C.P. Lond., Fellow Brit. Meteor. Soc., Fellow Obstet. Soc., Member Scottish Meteor. Soc., &c. London, 1873. Pp. 329 158

ART. V.—Handbook of Medical Information and Advice: containing a brief Account of the Nature and Treatment of Common Diseases, &c. By a Physician. London, 1872. Pp. 352 160

ART. VI.—The English Factory Legislation. By ERNST EDLER VON PLENER. Translated from the German by F. L. WEINMANN. With an Introduction by A. J. MUNDELLA, M.P. London, 1873. Pp. 175 162

ART. VII.—Descriptive Catalogue of the Teratological Series in the Museum of the Royal College of Surgeons of England. By B. THOMPSON LOWNE, M.R.C.S.E., &c. London, 1872. Pp. 110 . . 163

ART. VIII.—The Journal of Anatomy and Physiology. Nos. X and XI, May and November, 1872 164

ART. IX.—A Handbook of Therapeutics. By SYDNEY RINGER, M.D. Third Edition. 1873. Pp. 576 165

ART. X.—The Diseases of the Stomach; being the Third Edition of the 'Diagnosis and Treatment of the Varieties of Dyspepsia.' Revised and enlarged. By WILSON FOX, M.D., F.R.C.P., &c. London, 1872. Pp. 236 166

ART. XI.—General Surgical Pathology and Therapeutics. By THEODORE BILLROTH, Professor of Surgery in Vienna, &c. Translated from the Fourth German Edition by CHARLES HACKLEY, A.M., M.D. New York, 1871 167

ART. XII.—Sydenham Society's Publications 169

ART. XIII.—On Cerebria, and other Diseases of the Brain. By CHARLES ELAM, M.D. London, 1872 . . . . . ib.

ART. XIV.—Proceedings of the Dublin Obstetrical Society of Session 1871-72. Dublin. Pp. 164 171

ART. XV.—The 'Epistles' and 'Art of Poetry' of Horace, translated into English Metre. By ANDREW WOOD, M.D. Edinburgh, 1872. Pp. 140 172

Original Communications.

PAGE

ART. I.—Memoir on Leprosy in Syria. By JOHN WORTABET, M.D., Beyrout 173

ART. II.—On the Morbid Histology of the Brain and Spinal Cord, as observed in the Insane. By J. BATTY TUKE, M.D., F.R.C.P.E., Visiting Physician, Saughton Hall Institution for the Insane; late Medical Superintendent of the Fife and Kinross District Asylum . 199

ART. III.—Original Notes of Dr. W. Hunter's Lectures on Anatomy, Physiology and Surgery 210

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

Report on Scandinavian Medicine. By J. W. MOORE, M.D., M.Ch., Dubl., &c. 216

Report on Pathology and Principles and Practice of Medicine. By FRANCIS C. WEBB, M.D., F.L.S., Member of the Royal College of Physicians, Physician to the Great Northern Hospital 243

Report on Toxicology, Forensic Medicine and Hygiene. By BENJAMIN W. RICHARDSON, M.D., F.R.S. 254

BOOKS RECEIVED FOR REVIEW 266

CONTENTS OF NO. CIV.

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1873.

Analytical and Critical Reviews.

PAGE

REV. I.—1. Die Seuchen, ihre Ursachen, Gesetze und Bekämpfung. Von Dr. Fr. Oesterlen. Tübingen, 1873 269

2. Epidemiology; or the Remote Cause of Epidemic Disease in the Animal and Vegetable Creation. By John Parkin, M.D., F.R.C.S. London, 1873 . . . . . ib.

3. What is Malaria? By C. F. Oldham, M.R.C.P.E., Assistant-Surgeon, H.M. Indian Forces. London, 1871 . . . . ib.

4. On Relapsing, or Famine Fever. By R. T. Lyons, Assistant-Surgeon, Bengal Army. London, 1872 . . . . ib.

5. Fever and Cholera from a New Point of View. By Alexander Smith, M.D. Edin., Staff Surgeon-Major, British Forces in India, 1873. ib.

6. Disease Germs, their Supposed Nature. By L. S. Beale, M.B., F.R.S. London, 1870 . . . . . ib.

7. Disease Germs, their Real Nature. By L. S. Beale, M.B., F.R.S. London, 1870 . . . . . ib.

8. Remarks on the Prevalence and Distribution of Fever in Dublin. By Thos. W. Grimshaw, M.D., Physician to Cork Street Fever Hospital, &c. Dublin, 1872 . . . . . ib.

9. Ueber die Diagnose des Flecktyphus. Von C. Wunderlich. Leipsig, 1871 . . . . . ib

10. On the Period of Incubation of Typhus, Relapsing Fever, and Enteric Fever. By Charles Murchison, M.D., F.R.S. 'St. Thomas's Hospital Reports,' vol. ii . . . . . ib.

11. Fourth Annual Report of the State Board of Health of Massachusetts, January, 1873 . . . . . ib.

12. Ueber die Ætiologie des Typhus. Vorträge gehalten in den Sitzungen des ärztlichen Vereins zu München. Von Buhl, Friedrich, v. Pettenkofer, &c. München, 1872 . . . . ib.

REV. II.—An Exposition of the Fallacies in the Hypothesis of Mr. Darwin. By C. R. Bree, M.D., F.Z.S. Pp. 418 . . . 305

PAGE

Rev. III.—De l'Electrisation Localisée. Par le Dr. Duchenne (de Boulogne). Paris, 1872 319

Rev. IV.—1. Lehrbuch der Ohrenheilkunde; mit besondere Rucksicht auf Anatomie und Physiologie. Von Dr. Joseph Gruber . . 339

Handbook of Diseases of the Ear; with especial reference to Anatomy and Physiology. By Dr. Joseph Gruber, Aural Surgeon and Lecturer at the General Hospital, Vienna. Wien, 1870 . . . ib.

2. A Clinical Manual of Diseases of the Ear. By Lawrence Turnbull, M.D., Physician to Department of Diseases of Eye and Ear of Howard Hospital, Philadelphia. 1872 . . . . ib

3. Lectures on Diseases and Injuries of the Ear. By B. W. Dalby, M.B. Cantab. . . . . ib.

The 'Archiv fur Ohrenheilkunde,' edited by Professors Von Troeltsch, Politzer, and Schwartze; the 'Monatsschrift für Ohrenheilkunde,' edited by Voltolini, Gruber, Ruedinger, and Weber; and the 'Archives of Ophthalmology and Otology,' edited by Drs. Moos and Knapp . ib.

Rev. V.—The Principles of Animal Mechanics. By the Rev. S. Haughton, F.R.S., &c. London, 1873 354

Rev. VI.—1. Statistical Report of the Health of the Navy for the Year 1870. London, 1872 365

2. Army Medical Department Report for the Year 1870. London, 1872 ib.

3. Eighth Annual Report of the Sanitary Commissioner with the Government of India for 1871. Calcutta, 1872 . . . ib.

4. Report of the Sanitary Commissioner for Madras for the Year 1871. Madras, 1872 . . . . . ib.

5. Memorandum on the Cholera Epidemic of 1872 in Northern India. By J. L. Bryden, Statistical Officer. Simla, 1872 . . ib.

6. Traité de Climatologie Générale du Globe, Etudes Médicales sur tous les Climats. Par Le Docteur Armand. 8vo, pp. 808. Paris, 1873 ib.

Rev. VII.—Old Medicine and New. By Edward W. Lane, M.D. London, 1873 381

Rev. VIII.—A System of Midwifery; including the Diseases of Pregnancy and the Puerperal State. By William Leishman, M.D., &c. Glasgow, 1873 389

Rev. IX.—Lectures on Madness in its Medical, Legal, and Social Aspects. By Edgar Sheppard, M.D. London, 1873. Pp. 186 . . 401

Rev. X.—1. De la Pneumonie Caséeuse. Par le Dr. R. Lepine. 1872 . 404

2. The Anatomical Relations of Tubercle; a Debate at the Pathological Society of London, during the Session 1872-3 . . . ib.

Bibliographical Record.

Art. I.—Guide to Trefriw and the Vale of Conway Spa. By John W. Hayward, M.D. Second Edition. London, 1872. Pp. 64 . . 422

Art. II.—First Annual Report of the Supervising Surgeon of the Marine Hospital Service of the United States for the year 1872. Washington. Pp. 103 . . . . . ib.

PAGE

ART. III.—Notes on Asthma, its Nature, Forms and Treatment. By JOHN C. THOROWGOOD, M.D., &c. Second Edition. London, 1873. Pp. 166. 423

ART. IV.—On the Functional Diseases of the Renal, Urinary, and Reproductive Organs, with a General Review of Urinary Pathology. By Dr. CAMPBELL BLACK, M.D., Edinb. London, 1872. 8vo, pp. 321 . 424

ART. V.—A Practical Treatise on Urinary and Renal Diseases, including Urinary Deposits. By WILLIAM ROBERTS, M.D. Second Edition. London, 1872. Pp. 621 425

ART. VI.—1. Studi sulla Difterite. (Studies on Diphtheria.) Rapporto del Dottore G. FARALLI 426

2. Sul Ciclo Termico della Difterite. (On the Thermic Cycle of Diphtheria.) Nota del Dott. G. FARALLI . . . . ib.

ART. VII.—The Retrospect of Medicine. Edited by W. BRAITHWAITE, M.D., and JAMES BRAITHWAITE, M.D. January to July. London, 1873 . 430

The Half Yearly Abstract of the Medical Sciences. Edited by W. D. STONE, M.D. January to July. London, 1873 . . . ib.

ART. VIII.—University Oars; being a Critical Inquiry into the After Health of the Men who Rowed in the Oxford and Cambridge Boat Race from the Years 1829 to 1869, based on the Personal Experience of the Rowers themselves. By JOHN ED. MORGAN, M.D., M.A. Oxon., F.R.C.P. London, 1873. Pp. 397 . . . . ib.

ART. IX.—The Comparative Anatomy of the Domesticated Animals. By A. CHAUVEAU, Professor at the Lyons Veterinary School. Second Edition, revised and enlarged by S. ARLOING. Translated and edited by George Fleming, F.R.G.S. London, 1873. Pp. 957 . . . 432

ART. X.—Fistula, Hæmorrhoids, and other Diseases of the Rectum; their Diagnosis and Treatment. By WILLIAM ALLINGHAM, F.R.C.S. Second Edition. London, 1873. Pp. 263 437

ART. XI.—Dei Solfiti ed Iposolfiti nella Cura delle Febbri Intermittenti. Memoria del Dott. GIOVANNI FARALLI. (On the Sulphites and Hyposulphites in the Treatment of Intermittent Fevers. A Memoir by Dr. GIOVANNI FARALLI.) Milan, 1872. Pp. 128 . . ib.

Original Communications.

ART. I.—On Anæsthesia, Hyperæsthesia, Pseudo-Æsthesia, chiefly as met with among the Insane. By W. A. F. BROWNE, M.D., F.R.C.S.E., late Commissioner in Lunacy for Scotland . . . 441

ART. II.—On Puerperal Convulsions. By EDWARD COPEMAN, M.D., F.R.C.P., Senior Physician to the Norfolk and Norwich Hospital, &c. . 464

ART. III.—On the Morbid Histology of the Brain and Spinal Cord, as observed in the Insane. By J. BATTY TUKE, M.D., F.R.C.P.E., Visiting Physician, Saughton Hall Institution for the Insane; late Medical Superintendent of the Fife and Kinross District Asylum. Part III . 485

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

PAGE

Report on Materia Medica and Therapeutics. By ROBERT HUNTER SEMPLE, M.D., Member of the Royal College of Physicians, Physician to the Bloomsbury Dispensary, London 493

Report on Surgery. By HENRY A. REEVES, F.R.C.S.E. Assistant-Surgeon to the London Hospital, and Surgeon to the East London Hospital for Children and Dispensary for Women . . . 502

Report on Obstetrics and the Diseases of Children. By W. S. PLAYFAIR, M.D., F.R.C.P., Professor of Obstetric Medicine in King's College, and Physician for the Diseases of Women and Children to King's College Hospital; Examiner in Midwifery, &c., to the Royal College of Physicians 514

BOOKS RECEIVED FOR REVIEW 526

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1873.

Analytical and Critical Reviews.

I.—On Air and Rain.[1]

"You must pass the winter in a more genial and warmer climate"—or, "You should live in a more bracing atmosphere"—or, "You will not be able to resist the heat of the tropics"—or, "Town air must be avoided in a case like yours:"—We may safely conclude from the fact that such recommendations as the foregoing are constantly given by medical men to their patients, that there is a deep and general belief both in the therapeutic and in the pathogenic influence of climate.

It is no trifling thing, however, to recommend a man to leave home or change his residence. Cart loads of mixtures and pills would cost less, and few prescriptions are so difficult to follow—in so far at least as regards the multitude. It may be assumed, therefore, when a physician counsels his patient to seek in change of climate a restoration of health, that he is fully convinced in his mind of the value of what he advises, and that he has *at least* as thorough a knowledge of this, as he has of the other remedial agencies in which his faith is habitually declared.

There are those, perhaps, who think that the physician does not know much about any of them, and who regard the art of healing as little better than a make-believe; but with such wrong-headed and very objectionable people we have nothing here to do. We are not asking whether we know much or little about such things as quinine and strychnine, but whether we know as much about the constitution and action of climate as we do about them—whether, in short, we have studied climate in an equally careful and scientific manner.

[1] 1. *Air and Rain: the Beginnings of a Chemical Climatology.* By Robert Angus Smith, Ph.D., F.R.S., F.G.S. London, 1872.

2. *On Chemical Climatology.* By R. Angus Smith, F.R.S. 'Journal of the Scottish Meteorological Society,' January, 1870, No. xxv.

It is needless to say that we should have a good reason for every medical faith that is in us; but, in a special sense and measure, it seems desirable that our faith in climate, which sends men hither and thither over the face of the whole earth, which breaks up homes, and which leads to large outlays of money and time, should, if possible, be well grounded in knowledge. It is, indeed, but common honesty that we should do our very best to avoid mistakes in such a matter. In little things, of course, as well as in big, we ought to be alike honest—that goes without the saying—but it is clear that we may conscientiously and easily advise a patient to try a purgative or a tonic (hoping if not believing, that it will do him good), though we may hesitate and feel a special call to make sure that we are counselling wisely before we recommend what he may not be able to obtain without much difficulty.

In considerations of this nature there is surely a reason for making our knowledge of climate as full and as accurate as possible. But there are other and strong considerations which point to the same end. Has not Hippocrates himself said—"Whoever wishes to investigate medicine properly should proceed thus: in the first place to consider the seasons of the year and what effects each of them produces"?[1] Did he not mean by this to give climate almost the very first place as a producer, preventor, and healer of diseases, and who can refuse to admit that in this he was right? Climate, therefore, is something worth knowing about, because it is an agent of great and undoubted power. We can use our knowledge of it both to avert and to relieve suffering. It kills as well as cures, and may be used for either purpose. Yet is it not as true now, as it was when Brady said in a letter to Sydenham, that "No physician, hitherto, has attentively considered the force and influence of the atmosphere upon human bodies: nor yet sufficiently ascertained the part it plays in prolonging human life."?[2]

There is reason to fear that what was true in Sydenham's day continues to be true in ours, and that the belief in the action of climate, though it widely shapes our counsels, is nevertheless founded on very loose and imperfect information. It is not too much to say that it is mainly derived from works, the chief object of which was the bringing into repute localities in which the writers practised their art. Such works have often no claim whatever to be regarded as expositions of scientific research, and they too frequently betray a lamentable ignorance of well-known discoveries in meteorology. Medical men, in-

[1] Works of Hippocrates, 'Syd. Soc. Ed.,' i, 190.
[2] Works of 'Syd. Soc. Ed.,' ii, 3.

deed, instead of being more interested, appear to be less interested in the study of climate than lay men are—when that study is conducted according to hard scientific methods. They are certainly less interested in it than merchants, shipowners, and agriculturists, if we may judge by the fact that of the 600 members of the Scottish Meteorological Society,—on the publications of which this article is partly founded,—only fifty-six are doctors. The best workers in chemistry, botany, and zoology have always been drawn from the medical profession, but it is not so as regards meteorology, in reference to which medical men display a singular indifference, resting contented with loose and scanty information, and giving this, in their counsels to the sick, a precision and certainty to which it is by no means entitled.

Yet there is nothing more constantly acknowledged by physicians, and nothing more directly felt by the multitude than the influence of the weather in the production and relief of disease. Medical men are satisfied, however, with a vagueness in this matter, which would not satisfy them in others. They have a traditional way of speaking of climate, about which there is a certain air of learning, deceptive alike to themselves and to others; but is it not true, with some notable exceptions, that, as a rule, they know little of what has been, or is being done to advance meteorology? How few of them have any correct idea of what constitutes climate. To many, indeed, it is nothing more than an affair of temperature. To the meteorologist, however, it is temperature and a dozen other things of equal importance. We should say a *growing* dozen, if we might be allowed the expression, for our views are widening on this subject, and new elements are being added to the assemblage of elements which together constitute *climate*. This remark seems to indicate that however medical men may treat it, meteorology is not at present a neglected science. This, indeed, is true in a fuller sense than is generally understood. During the last ten or fifteen years a great impulse has been given to this branch of study, both in this country, on the Continent, and in America, and at this moment, it is probably safe to say that, no road of inquiry is followed by so many with such a healthy zeal, or with a better prospect of good results. In Great Britain we have now three journals wholly devoted to meteorology, and there are many others on the Continent and in America. These stand apart from the numerous annual publications on the subject, and the increasingly large space allotted to it in serials dealing broadly with physical or medical science. In the work which all this indicates, the great object, of course, is to discover the laws of weather; but it is clear that we

we cannot learn these laws without learning more about weather in every sense, and in the medical sense among others. In this last direction, indeed, there is good evidence that progress is being made, and an increasing interest taken. Perhaps this results from the fact that progress in the first direction leads to a more plentiful, a more accessible, and a more trustworthy supply of material to those who engage themselves in the study of the second—that is, in the study of medico-climatology. Inquirers start with broader views of what constitutes climate. They know more of the inter-dependence of barometric pressure and temperature and of the relations of both of these to winds and rain; they know more of the different physical actions of a dry warm, a dry cold, a moist warm, and a moist cold atmosphere; they know better what still air and air in motion means, in so far as plants and animals are concerned; they see that the elements of climate must not be looked at singly, but in their various combinations; they are aware that occult constituents of climate exist which we have not yet measured, but which are clearly so powerful as to claim a respectful consideration. With these and countless other advantages the medico-climatologist is now able to start in his investigations. On the other hand, he is, perhaps, more than ever deterred by the feeling that if he is to adhere to strict scientific methods in his researches, there is a labour of Hercules before him, so numerous are the sources of error, and so difficult is it to eliminate and avoid them. The whole subject, however, is now in a very different position from that, for instance, which it occupied when Sir James Clark wrote his classical work on climate; and the difference, if all secrets were known, might be found to be not a little due to the enlightened interest he took in the promotion of meteorological investigations, both in this country and on the Continent—an interest which bore fruit through his wide intercourse with the thinking and influential men of Europe.

In what has been said, it has been repeatedly hinted that narrow views may exist as to what goes to form *climate*, and that there may be misunderstandings regarding some of its constituents. Temperature, pressure, moisture, motion, &c., are never forgotten, though often very unintelligently considered, but there are many to whom it never occurs that there may be a *chemistry of climate*, and that airs may differ not only in such things as heat and moisture but in the proportions of oxygen, nitrogen, and carbonic acid which they contain, as well as in the presence in them of special substances either held in solution or in mechanical suspension. That such is probably the case every one is ready to admit, yet practically in works on

medico-climatology air is just air all the world over. It is true that for many years back experimentalists have laboured to show that there are airs and airs, as well as dukes and dukes; but still, even at so late a time as in the last year of his life, Dalton said that chemical experiment could not distinguish the air of Manchester from the air of Helvellyn, and it must be remembered that Dalton was equally distinguished as a chemist and as a meteorologist. Cavendish, too, could not decide that the air of London differed chemically from that of the country.

Dalton was far wrong, however, as is clearly shown by his distinguished pupil Dr. R. Angus Smith in his recent work on 'Air and Rain.' Dr. Smith, we think, originated the phrase—*chemistry of climate*—and in this work he gives us an extraordinary contribution of facts to the phrase. We only wish there had been more system in their arrangement, and that the index to them had been at least ten times as copious. This is almost the only unfavourable comment we have to make on the book, which, on the whole, deserves such praise that we should have been silent even as to these its faults, were it not that they are of such a nature as seriously to lessen its usefulness. The book is a great mine of information—the record of many thousands of experiments—and we feel safe in predicting that the quarriers in it will be legion, and as safe also in predicting that many a quarrier will lose his temper in the task, and will fail suitably to bless the intelligent industry which has placed such a mass of facts at his disposal.

It was while acting for the Royal Mines Commission and the Board of Health, or as Inspector under the Alkali Act, that Dr. Smith made most of the researches which are detailed in this work. Nearly all of them have previously appeared in various blue books, and they are brought together here without being to any large extent recast. Hence, probably, the confused arrangement of which we complain; but in all probability hence, also, the fulness of the record, since it gave less trouble to the author to insert than to omit.

What Dr. Smith has done, and what it is the great object of his book to show is this:—(1) That there are important and constant chemical differences between the airs of different localities; and (2) that these are capable of precise determination. He has done all this most conclusively, and yet after all he has only done what our senses did before.

"Whilst I have succeeded," he says, "in doing much of that which I intended to do, I have not got beyond the limits which earlier observers obtained by the mere fineness of unaided sense, and by sound reasoning without experiment. Still I hope I shall be found to have put their suspicions into plainer language, to have

proved that which they only imagined, and to have given in detail that which they only in a general, and, we may add, in a vague, manner, had attained."

The meaning of this is that by our senses alone we were able to speak correctly of the atmosphere of certain places as pure and invigorating, and of the atmosphere of other places as oppressive and enfeebling, and that the author has now shown the nature, and has measured the extent, of the variations which thus affected the senses. In many other things the senses are in a similar way ahead of the chemist. Taste, for instance, easily reveals a difference between the eggs of hens fed on inland and of those fed on seaside farms, but no chemical difference has yet been shown. That it exists, however, there can be no doubt, and perhaps at no distant day it will be found possible to determine its nature in the laboratory.

It was Regnault who first showed the fine distinctions between the amount of oxygen in pure and in tainted air.

"My work has been," says Dr. Smith, "so far as the gases are concerned, to carry this out further and to give minute details. These show that the mountains and great plains have an atmosphere different from that of great cities." "It is true," he goes on to say, "that in figures this appears small; but what is the meaning of small? If we measure size by percentage it will appear small, but still smaller will appear the strychnine that destroys us if we estimate the amount as a percentage of the weight of our bodies."

He is at much pains to point out the value of seemingly small differences, and he is clearly correct in this. Half a per cent. may look little and yet be really great. It would be disgustingly small as a railway dividend, and as disgustingly great as assafœtida in a cup of tea. In the impure air of a crowded theatre towards midnight the oxygen is not diminished by anything like half a per cent., and there is not more than one third of a per cent. of carbonic acid present, yet this is an extreme case of change, or, in other words, of impurity, and no one could live long in such an atmosphere and remain in health. Before the oxygen in the air is reduced by 2½ per cent. a candle burns dimly. Vastly smaller percentages than any of these, however, are determinable, and are clearly of great importance in their influence on health and life.

"Some people will probably inquire," says Dr. Smith, "why we should give so much attention to such minute quantities—between 20·980 and 20·999—thinking these small differences can in no way affect us. A little more or less oxygen might, perhaps, not affect us; but supposing its place occupied by hurtful matter, we must not look on the amount as too small. Subtracting 20·980 from

20·999 we have a difference of 190 in a million. In a gallon of water there are 70,000 grains; let us put into it an impurity at the rate of 190 in 1,000,000; it amounts to 13·3 grains in a gallon, or 0·19 grammes in a litre. This amount would be considered enormous if it consisted of putrefying matter, or any organic matters, usually found in waters. But we drink only a comparatively small quantity of water, and the whole 13 grains would not be swallowed in a day, whereas we take into our lungs from 1000 to 2000 gallons of air daily."

Nitrogen is usually taken as the *remainder*, after estimating the oxygen and carbonic acid, and it is said that nothing is known which can diminish or increase its amount to any appreciable extent. When the percentage of oxygen falls, its place is usually filled up by carbonic acid or by something worse. Suppose the oxygen to be replaced by carbonic acid, then if we wash out the carbonic acid we should have an atmosphere with reduced oxygen and increased nitrogen. We should expect to find an atmosphere of this kind in towns after rain, but, in fact, it is not so; and the probable explanation appears to be that the rain both absorbs the carbonic acid and gives out oxygen.

There seems good reason for believing that we have "a distinct variety of air on mountains—differing from that on plains." The analyses of Frankland, Miller, Schlagentweit, Dumas, and others appear to support this view, and to show that mountain air has an abnormally low percentage of oxygen and an abnormally high percentage of carbonic acid. But Lewy's analyses of Bogota air (2645 metres above the sea level) are not quite in accordance with those of other experimentors as regards this point, for he found both oxygen and carbonic acid in excess. The usual results of the analysis of mountain air—showing oxygen low and carbonic acid high—are variously explained. De Saussure thought it due to the action of vegetation on the plains, decomposing the CO_2, and fixing the carbon, but setting free oxygen. Dr. Smith, however, suggests that it may be due to the fuller oxidation, at high levels, of organic matters in the air; and this theory would give as the characteristics of mountain air (1) more carbonic acid, (2) less oxygen, and (3) no organic matter, constituting purity in a certain sense, and constituting purity in a very full sense, if it be correct that ozone is condensed oxygen, and that ozone is more abundant at high than at low levels.

It would appear that a favourable specimen of air may be assumed to contain—

Oxygen	20·96	vol. per cent.
Nitrogen	79·00	„ „
Carbonic acid	0·04	„ „

When air was taken from the front of Dr. Smith's laboratory and air from behind it, near an ashpit, an important difference was found, which maintained itself steadily in one direction through the thirty repetitions of the analysis. The air from the front yielded 20·94° of oxygen against 20·70° in the air from behind. In the latter carbonic acid was also in excess, but not sufficiently so to fill the place of the deficient oxygen, "leading us to look for other gases also that tend to increase the impurity." This he thinks important, since it is certain that gaseous emanations from foul places do not consist solely of carbonic acid. "We see putrid matter laid on the ground, and find it disappearing rapidly, and yet we are told that it is not accompanied by loss of oxygen; it is not credible, and the results given show it to be incorrect." The diminution of oxygen is thus accounted for, but we know that the putrid matter is not altogether resolved into carbonic acid, but partly into other and more deadly gases.

So far as London is concerned, there is less oxygen in the east, than there is in the west end in open places. Hyde Park shows the best results. Numerous analyses were made of air from Scotland, and they show that the seashore and open heaths contain the highest amount of oxygen; but it would seem that the air of the Scotch hills is not, as of hills usually, deficient in oxygen; perhaps, as Dr. Smith supposes, because they are not high enough, and also because they draw their supplies from the sea, which is never far away from them on any side. Scotch readers may possibly feel that Dr. Smith speaks somewhat irreverently of their mountains when he calls them hills and accuses them of want of altitude; but, we suppose that he had to take them for what they are, and that, in a scientific inquiry, fancy was forbidden to add even one cubit to their stature.

After all, this *chemistry of climate* is no new thing, for in 1828 De Saussure wrote—"The quantity of carbonic acid in the open air in the same place is subject to almost continual change, equally with the temperature, the winds, the rain, and the atmospheric pressure," and he even concluded that at Chambeisy the winter months of December, January, and February were as clearly distinguished from the summer months of June, July, and August by the proportion of the carbonic acid in the air, as they are by temperature, rainfall, or winds—the quantity of carbonic acid in the first three months being to

that in the second as 77 to 100. Experiments have not been made to establish or refute this remarkable statement. De Saussure's observation, however, to the effect that the percentage of carbonic acid in the air of mountains is greater than in that of plains has been amply confirmed; while, as we have already stated, it has been also shown that this excess of carbonic acid is attended with a diminution of oxygen. This leads to the remark that we appear to have no certain knowledge that a small excess of carbonic acid is of itself productive of much evil. In mountain air, indeed, it might be regarded as an evidence of purity, if its excess were due to a full combustion of all organic matters in the air. But as a general rule, any excess of carbonic acid should be accepted as a sign and proof of impurity, if for no other reason, because of the bad company it generally keeps. Sulphuretted hydrogen, marsh gas, hydrogen, and a host of other things appear with it as products of decomposition or putrefaction, and many of these are very deleterious. Its presence, too, betokens an active consumption of the oxygen, and a consequent diminution of its quantity. The atmosphere of a room is rendered unpleasant by ·1 per cent. of carbonic acid, and fair ventilation means air with less than ·06. Yet we can scarcely believe that the unpleasantness and unwholesomeness of an atmosphere with ·1 per cent. of CO_2 is due to the CO_2, for Schlagentweit found ·09 in the air of the Himalayan mountains, which the senses pronounced pleasant and invigorating.

One cannot help wondering that the air of large manufacturing towns contains so trifling an excess of carbonic acid; but Dr. Smith shows by calculation that in such a town as Manchester, which consumes 2,000,000 tons of coals annually, and which has half a million of breathing people, the increase of carbonic acid in the air should not be more than seen in the second decimal place, and experiment confirms the calculation. Differences, however, which are seen only in the second decimal place, are by no means insignificant or contemptible. In the case of some gases, as, for instance, chlorine, sulphuretted hydrogen, or sulphuric acid, quantities which might thus be represented would render the atmosphere intolerable. Indeed, these small amounts cannot be utterly insignificant, since there is no practical difficulty in measuring them.

The section of Dr. Smith's work, which treats of the air of mines is extremely interesting, and it is with unqualified horror that we read of the extremely degraded or impure airs in which some miners work. A candle will not burn with less than 18 per cent. of oxygen, when 3 per cent. of carbonic acid is present, but men *occasionally* work where candles will not burn,

and *often* work where candles burn feebly. "If we leapt," says the author, "from the pure air into a *close end* we should recoil with horror," but usually the miner passes gradually from the pure to less pure air, acquiring a toleration of the bad as he goes on, till he reaches an atmosphere in which all kinds of impurities are present, the result of exploded gunpowder, burning candles, expired air, mechanical operations, &c. In the experiments made in a close chamber, the air was rendered so impure that the candles and a spirit lamp carried by those who entered it were soon extinguished, and it was found impossible to rekindle them with matches—still gas burned brightly, and human beings breathed without difficulty at first, though discomfort and distress were soon felt. After the air became so impure as to extinguish the gas, and to extinguish candles as quickly as if they had been plunged into water—the oxygen being reduced to about 17 per cent.—still human beings entering the chamber were able to breathe without any immediate distress. In these cases the impurity consisted chiefly of CO_2, and to the senses there was less unpleasantness than is often felt in a crowded schoolroom, in the air of which organic impurities also abound. The senses, indeed, though very delicate and acute in detecting impurities, cannot always be trusted, and are clearly quite unable to measure degrees of closeness—0·1 per cent. of CO_2 being sometimes more disagreeable to the senses than 4·0, though the latter amount has rapidly caused alarming symptoms. In detecting the mere presence of *certain* substances, however, the nose appears to be greatly more delicate than chemical reagents. For instance, "one sniff" (4 cubic inches) of air containing only ·003 per cent. of muriatic acid reveals the presence and nature of the impurity, but we should have to operate on 60 cubic inches at least with a silver solution before we could reach the same conclusion.

"It has been a question," says Dr. Smith, "if CO_2 really does any active harm, or if it is only negative to life. I have always considered it remarkably innocent in small quantities, and been accustomed to look on the organic substances as the real evils; but the experiments in the close chamber have so far changed my mind that I am inclined to think carbonic acid has an (actively) injurious influence."

This is a point which is difficult of proof, but it appears to be supported by such a fact as that air which extinguishes a candle may cease to do so when the CO_2 is washed out. Doing this, however, would change the percentage of oxygen, and thus, perhaps, explain the phenomenon. Indeed, we know that $2\frac{1}{2}$ per cent. of CO_2 in air, when the oxygen of the

air has gone to form it, will extinguish a candle, while 4 per cent. at least is needed, when the acid, elsewhere formed, is poured into the air. The smaller amount of CO_2 in the first case involves a much greater reduction of the oxygen, than the greater amount does in the second, and the extinction of the candle is not due solely to the excess of carbonic acid, but also, and perhaps chiefly, to the defect of oxygen.

Dry air, the author tells us, stimulates the skin, because it removes moisture, and the skin must set to work to renew it. Its first action, therefore, should be cheering or bracing, and its last, perhaps, irritating. Moist air again, or soft air, as it is called, is soothing—it calms both the mind and the body, as it does the burning of a candle or a fire. There is a quaintness in this association of the effect of warm moist air on body and and mind with its effect on candles, and Dr. Smith's meaning will, perhaps, be better understood if we let him speak in confirmation of the popular belief that fires burn most brightly in cold dry weather:

"The warmth," he says, "cannot do otherwise than assist the combustion; we learn this on the grand scale by the effects of the hot blast. In cold weather a fire burns brightly, even though the cold is an agent of an obstructive kind to the combustion. In warm weather the fire burns less clearly, though the heat is an assistance to combustion. The reason seems to lie in this fact, that the cold air is deprived in a great measure of its moisture. Its effects on the candle are such as the least observant eye may remark, and the changes on a fire are of the same kind."

In this notice it would be impossible to give the scientific explanation of these phenomena, which the author attempts.

There is another popular belief, to which also he gives his support—the notion, to wit, that ventilation is more needed in summer than in winter, and what he says on this point seems worth the quoting.

"I was led," he observes, "to consider one of the effects of heat when working on the gases of putrefaction. It was then perfectly clear that the putrefaction proceeded exactly as the temperature rose, not ceasing at a little above 130°, perhaps approaching nearly 140°—which marks another point in organic substances of importance, as being that at which albumen coagulates. At any rate, according as the temperature rises within any limits natural to this country, the necessity for ventilation is felt. The organic matter becomes especially annoying, even with a small increase of carbonic acid. The substances that annoy us are often volatile, and we may suppose that they begin to decompose in the air when the temperature rises. Indeed it is scarcely a supposition; it is a fact that after a time they become entirely changed, whether they be organic or inorganic. Sulphuretted hydrogen becomes oxidized,

and the unpleasant odours in mining galleries become the smell of apples. In a house, the organic odours, if shut up, become mouldy or putrefactive, according to circumstances; but if the last they are more rapidly remedied by abundance of air, which oxidizes and washes them away. At a low temperature they will lie unaltered on every substance, and start out again when they are warmed. This action is more distinctly seen when the quantities of oxidizing matter are large; then the eye can perceive the amount daily diminishing until it is all carried away.

"If this result be caused by the action of warmth it is not right to demand as much air for ventilation in a cold day as in a warm; it is not right to break the windows of patients in winter, and to tell them that fresh air is better than heat. The chemical action, and with it the feelings, demand warmth first above all things. It is the very first demand as no function can go on without it. In the railway carriage as well as in the house, the great instinct of man is first to be warm enough, and he is quite right."

There is surely both error and truth in all this. Of course we are injured by low temperatures as well as by foul air. But why be injured by either? Can we not get warmth in pure air? It is true, as he says, "a draught of cold kills like a sword," while, as a rule, foul air kills slowly and insidiously; but we need not strip the last of its bad character, because we choose sometimes to put up with it for a time, in order to escape injury from the first.

We cannot part company with this quotation without pointing to it as a specimen of the author's peculiar and perplexing style, which often renders it difficult to reach his full meaning, and which would look like carelessness, if it were not manifestly what may be called *a characteristic*. Perhaps we may accept the general plan and arrangement of the book as an outcome of the same *characteristic*, for between the book as a whole, and many of the chapters, there is, in this respect, a striking resemblance. The author's reputation, however, rests solidly on the good work he has done, and not on his skill as a bookmaker. At page 231, in justification of his mode of expounding his subject, and of communicating information, he frankly says—"It does also save a good deal of trouble in compiling"—but trouble so saved to the writer is trouble given to the reader; and it would certainly have been well if Dr. Smith had spared himself as little in the arranging as he has done in the collecting of his facts and information.

For the interesting and instructive results of the numerous examinations of rain collected in localities nearly as numerous, and for the conclusions which may be drawn from these as to the impurities in the air through which the rain has fallen, the work itself must be consulted, as the space at our command has been

already exceeded. Nor can we deal otherwise with the section devoted to the consideration of organic matter in the air.

'Air and Rain' might be the name of a romance, so odd are the names which now-a-days are chosen by novelists. We know, indeed, of a sea-story, for which the title of 'Wind and Water' was contemplated. If any one, however, happens to send to the library for the work under notice, in the expectation of getting back a piece of light reading, it will be unfair to examine too strictly the words in which his disappointment is likely to find expression. Ordinary blue books are altogether Braddonish in comparison with this great record of ill-assorted, half-digested, dry facts,—a record which, notwithstanding its faults, will find an honourable place in every good library of reference.

It falls to few men to raise such a pile of original observations —even though the structure does resemble that of the cairn more than that of the well-coursed and well-jointed pyramid—yet the raiser of the pile in question modestly calls it "*this beginning.*" Does he himself hope and intend to give us the *ending* of *this beginning?* We can scarcely think so, but we are ready to believe anything of the boldness of a man who is able to say—"I have an actual pleasure in east wind."

II.—On the Physical Theory of Murmurs, Vascular, Cardiac, and Respiratory.[1]

THE second volume of the 'Lancet,' published in the year 1828, opens with a short essay, by Dr. (now Sir Dominic) Corrigan, entitled "An Inquiry into the Causes of 'Bruit de Soufflet' and 'Frémissement Cataire.'" The views propounded in this paper have quite recently received corroboration and extension at the hands of certain eminent French physiologists, and have been applied to a large number of auscultatory phenomena. We think, therefore, that our readers can hardly fail to be interested in a brief *résumé* of the subject.

---

[1] 1. *Théorie des Bruits Physiologiques de la Respiration.* Par le Dr. L. BERGEON. Paris, 1869. Pp. 20.

2. *Des Causes et du Méchanisme du Bruit de Soufflet.* Par le Dr. BERGEON. Paris, 1868. Pp. 103.

3. *Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques.* Art. "Auscultation." Par ALFRED LUTON. Paris, 1866. Pp. 122.

4. *Étude sur la Respiration; Recherches Physiologiques sur le Méchanisme des Bruits respiratoires.* Par M. le Dr. BONDET, Médecin de l'Hôtel Dieu de Lyon. 'Gazette Hebdomadaire,' 1863, p. 798, et seq.

5. *Études Pratiques sur les Murmures Vasculaires, ou Bruits de Souffle et sur leur valeur Séméiologique.* Par M. A. CHAUVEAU, Sécrétaire adjoint de la Société Impériale de Lyon. 'Gazette Médicale de Paris,' 1858, p. 247, et seq.

Laennec had maintained, as the result of his observations and experiments, that bruit de soufflet is owing to a true spasmodic contraction of the heart or arteries. This view is opposed by Corrigan, partly by reasoning, partly by the following experiment:

"Apply," he says, "the stethoscope under the outer third of the clavicle, not allowing it to pass on the subclavian. In a strong healthy man, not agitated, the mere impulse of the vessel is felt. Now compress the artery *above* the clavicle, so as to diminish the flow of blood through it; a loud bruit de soufflet is heard. Make strong pressure so as effectually to stop the flow of blood; no sound is heard. If the sound in this experiment arise from the arterial tube being excited into muscular action by the stimulus of the pressure, why does it cease when the stimulus is increased? If it be owing to spasm, it should be expected to continue at least some short time after the stimulus has been removed, or, the artery becoming accustomed to the pressure, if continued, its effect as a stimulus should cease. Neither takes place."

And a little further on he gives the following explanation of the production of the bruit in the experiment just described:

"When an artery is pressed upon, the motion of the blood in the artery immediately beyond the constricted part, looking from the heart, is no longer as before. A small stream is now rushing from a narrow orifice into a wider tube, and continuing its way through surrounding fluid. The motion is that of a current; the sides of the artery, instead of being acted upon by a body of fluid moving forward almost as a solid mass, receive the impulse of a stream whose particles are in motion with different degrees of velocity. The rushing of the fluid is combined with a trembling of the artery, and the sensation to the organ of touch is the *frémissement* of Laennec, the *bruissement* of Corvisart, and to the sense of hearing '*bruit de soufflet*.'"

And he goes on to show that, in aneurism, and in narrowing of the auriculo-ventricular communication in the heart, similar conditions are met with, that the effect on the blood's motion is the same, and that the bruits heard in those diseases are fairly referable to the same causes, as in the experiment above described.

Before concluding his paper, Corrigan feels it to be necessary to meet by anticipation the objection that he has "fallen into the errors of the mechanical physiologists, in ascribing to physical causes phenomena taking place in the living body." How different must have been the tone of physiological thought then from that of the present day! We should now feel it to be the crowning proof of the adequacy and completeness of a physiological theory to include it within the scope of a physical law. This has since been done, in the case of the arterial bruit, by

several French writers, who have shown that it is a particular instance of a phenomenon long ago fully investigated by Savart, and designated by him the "*veine fluide*" or "*veine liquide*," terms for which the German equivalent is "Pressstrahl," while the expression "fluid vein," is used by some English writers. And, perhaps, the best account of the matter is found in the work of M. Bergeon, which we have placed at the head of the present article.

"Whenever a liquid flows from a reservoir, through a vertical or horizontal orifice, the stream takes the form of a jet, to which Savart gave the name of a 'fluid vein.'

"The 'fluid vein' is essentially composed of two parts, the first calm, transparent, like a stem of crystal; the second agitated, without transparency, but so far possessed of regular form that it can be seen to be divided into a certain number of elongated swellings (of which the maximum diameter is always larger than that of the orifice), separated by narrower portions. This nodal appearance of itself shows that the liquid vibrates, and by means of the electric light Savart showed that the flow is not really continuous. The 'fluid vein' in part consists of separate drops of the fluid, succeeding one another at regular intervals, each drop changing its form in passing from point to point."

Thus, then, whenever a liquid escapes from a narrow orifice with a certain amount of force, its flow is intermittent and periodic instead of being continuous, and sonorous vibrations are generated.

The cause of this phenomenon has excited much discussion among physicists, and even now cannot be regarded as settled. Savart showed that it is not due to the vibration of the margin of the aperture through which the jet flows, and that it can be attributed only to oscillations of the entire mass of the fluid within the reservoir, causing that portion of the fluid which is opposite the centre of the orifice to rise and fall periodically, in alternation with the portions of fluid corresponding to its periphery. Bergeon has somewhat amplified this view, but without adding essentially to it.

Now, it is well known that a "fluid vein" is formed not only when fluid is allowed to escape through an orifice into the open air, but also when this orifice opens into a space containing fluid. In other words, if a tube through which a full stream of fluid is noiselessly flowing be constricted at any point, a vibrating "fluid vein" will at once be generated in the wider space beyond. Bergeon gives diagrams, by which the explanation of this is made manifest. By means of manometers it can be shown that above the constricted part the pressure of liquid is much higher than when no obstruction exists. Immediately below the seat of constriction the pressure suddenly becomes

nil; it then very rapidly rises again, but not so high as before, and after this falls suddenly towards the free end of the tube. In passing through the narrowed orifice, therefore, the fluid is in fact passing into a space in which there is no pressure.

It did not escape Corrigan's notice that the conditions which he believed to be requisite for the production of the *bruit de soufflet* might be imitated experimentally, by passing a stream of fluid through a tube, and he relates the following experiment:

"One end of a length of small intestine was attached to a pipe, and a current of water of considerable force was allowed to run through it. While the intestine was kept full and tense, the finger laid upon it received no sensation, any more than if it were laid upon a portion of the same intestine containing fluid at rest; but constriction being made upon any part, then immediately above the narrowed part where the intestine was no longer tense, and where, for the reasons already gone into, the motion of the fluid became very different from that through the upper part of the tube, a sensation was felt precisely like that of the *frémissement cataire*. No similar sensation was felt above the constriction. The same thing took place with the sound heard by the stethoscope. While the intestine was tense no sound, or a murmur exceedingly indistinct, was heard, but any part being constricted, so as to produce an alteration in the motion of the fluid, a very loud bruit de soufflet immediately became evident."

By Bergeon similar experiments have been repeated and variously modified. And he shows that, whatever the form of the constriction in a tube through which fluid is passing, a "fluid vein," and with it a bruit, is always produced in the space immediately beyond the narrowed point.

But to the physician these experiments will probably seem to be less important than those made on horses by Chauveau, and described by him in a memoir which forms the starting-point of the modern literature of this subject. One of these is the following:—A horse was pithed and the heart was exposed, artificial respiration being kept up. A thread was next passed loosely round the base of the pulmonary artery. Chauveau then introduced his finger into the artery through a small cut in its walls. Not the least vibration could be felt. The blood flowed so softly over the finger that only a vague sensation of its current was perceptible. But the moment that the thread was tightened, so as to narrow the calibre of the vessel sufficiently to produce a bruit de soufflet, he instantly found that in the axis of the artery a "fluid vein" was established, the vibrations of which he could plainly feel.

Chauveau sums up the results of his experiments in the following law:

"Bruit de souffle[1] is always produced by the vibrations of an intravascular *veine fluide*, and such a *veine* is constantly formed whenever the blood passes with a certain force from a narrowed into an actually or relatively dilated part of the circulatory system."

The author of this theory, therefore, is not satisfied with showing that the *veine fluide* is a frequent or even a principal cause of the bruit de souffle. He will have it admitted to be the sole cause. We must now inquire what evidence is brought forward, for the purpose of excluding the operation of other agencies, such as roughness of the surface over which the blood flows, alterations in the quality of that fluid, &c.

And we may first take *roughness of surface*, which has been supposed (in the words of Dr. Walshe[2]) to "cause unnatural friction between the blood and the surface," and so to generate murmur. For the last thirty years this has been placed in the foreground among the causes of bruits. By Gendrin,[3] indeed, all valvular murmurs were placed in the same category as pericardial friction sounds under the name of "*bruits de frottement endocardiaques*." Bouillaud and almost all subsequent writers have maintained a similar view. Now, Chauveau lays it down as a law that a bruit de souffle cannot be generated by mere roughness of the internal surface of a vascular channel, unless the calibre of the tube is modified. Having exposed the carotid artery of a horse, he drew a stout ligature lightly round the vessel at four or five points near one another. The internal and middle coats were torn through, and created a marked roughness of its surface; and this could be still further increased by seizing the vessel with a pair of forceps, and tearing its coats irregularly. But on auscultation afterwards he found that the blood flowed through the artery quite noiselessly; there was no trace of a bruit de souffle. In another experiment Chauveau made the blood of the carotid artery flow through a metallic tube, of the same diameter as the artery itself, with its inner surface roughened; still no murmur showed itself. It must, indeed, be allowed that in experiments of this kind negative results are never in themselves perfectly conclusive, since the conditions cannot be precisely the same as those met with in disease. But for our own part we think that the question

[1] It will be observed that some writers quoted in this article use the expression "bruit de soufflet," others "bruit de souffle." It may, therefore, be well to explain that they have exactly the same meaning. The former term was invented by Laennec, and is used by all the writers (as, for instance, Corrigan) whose works appeared soon after Laennec's. Afterwards the latter term was substituted for it, and is now universally employed in its place.

[2] 'A Practical Treatise on Disease of the Heart and Great Vessels,' 3rd edition, 1862, p. 87.

[3] 'Leçons sur les Maladies du Cœur,' 1841-2.

now hardly needs any elaborate discussion. When once the mind grasps the idea that a bruit de souffle is due to the regular vibration of a jet of blood passing into a wide space from a narrow one, it appears very unlikely that a similar bruit could be generated by a mere accidental roughening of the interior of the blood channel.

We may now consider, secondly, what evidence there is that the *quality of the circulating fluid* is concerned in the production of *bruit de souffle*. Some years ago, De la Harpe,[1] of Lausanne, made some experiments on corpses, in which he injected different liquids into the femoral arteries. When he employed oil for this purpose, no bruit was produced; whereas with either alcohol or water a marked murmur was generated. But, as is remarked by Chauveau, in these experiments all the conditions for the production of a sonorous *veine fluide* were afforded by the passage of the fluid from the canula into the artery. Chauveau himself made a series of experiments on horses, from some of which he withdrew ten kilogrammes of blood by bleeding, while he injected into the veins of other animals ten kilogrammes of tepid water. The state of the blood must have been greatly altered in both cases, yet no bruit was produced.

It is well known, however, that in the human subject an anæmic state is frequently attended by marked murmurs of two distinct kinds—one audible at the base of the heart and along the course of the great arteries, especially perhaps the pulmonary artery; the other (commonly entitled the *bruit de diable*) heard over the large veins at the root of the neck. Now, it is one of the main recommendations of Chauveau's theory that it professes to explain the occurrence of both these murmurs, the cause of which has hitherto appeared difficult of interpretation.

Chauveau's account of the anæmic murmur heard at the base of the heart and along the large arteries may be stated as follows:—He shows, in the first place, that in anæmia the mass of blood is much less than in health. Now, the capillaries, the small and medium-sized arteries, the veins, and the heart itself, with its orifices, are capable of accommodating themselves to this diminution in the quantity of the circulating fluid; but the two main arteries cannot retract in the same degree. Their calibre, therefore, remains proportionately larger than that of the orifices through which the blood enters them; and the conditions required for the generation of a bruit de souffle are fulfilled. This result is further favoured by the greatly increased force with which the blood is driven into the arteries by the heart in anæmic as compared with healthy subjects, in con-

[1] 'Archives Générales,' 1838.

sequence of the great diminution of the arterial pressure, which belongs to that morbid state.

This explanation is certainly ingenious, and perhaps satisfactory; but it appears to us less conclusive than that which Chauveau gives of the venous anæmic murmur, or *bruit de diable*. Hamernyk long ago pointed out that this remarkable murmur is never heard in any other veins but the cervical; and he seems to have shown that this is due to the fact (first demonstrated by Bérard) that the roots of the innominate veins have peculiar connections, by which they are enabled to resist suction, without being flattened. Their coats are in fact adherent to the cervical fascia over them, and so are fixed to the solid parts around.[1] The consequence is that they form on each side a sort of ampulla, the size of which is uniform whatever the state of other vessels. Hence, when the jugular veins retract to meet the diminished mass of blood in anæmia, these ampullæ remain dilated. This of itself satisfies the conditions required for the generation of a *veine fluide*. But in many cases the narrowing of the veins produced by the anæmia is not of itself sufficient to lead to this result; the application of the stethoscope is then needed to reduce their channel still further. And in some subjects a *bruit de diable* can be created by the pressure of the stethoscope alone, without their being anæmic or out of health in any way.

Thirdly, it is shown by Bergeon that there is no reason to suppose that a *bruit de souffle* can be generated, either by increased rapidity of the stream of blood, or by uniform augmentation or diminution of its tension.

It would appear, therefore, that there remains no cause to which the production of a *bruit de souffle* in vessels at a distance from the heart can be attributed, except the generation of a "fluid vein" in a stream of blood passing from a narrow into a dilated part of the circulating system, in accordance with Chauveau's theory. The conditions under which the blood flows in these vessels are in no way different from those which existed in Chauveau's and Bergeon's experiments, and we are now prepared to admit the universal applicability of their views to all vascular murmurs.

With regard to murmurs generated within the heart and at its orifices, however, we are by no means sure that this explanation holds good to the same extent. There are, indeed, some cardiac murmurs to which it is strikingly applicable. This is the case with the bruits that belong to mitral and aortic stenosis.

[1] This "canalization" of these veins (to anglicise the name commonly given to it in French works) renders them especially liable to the dangerous entrance of air during surgical operations, &c.

No one acquainted Chauveau's theory could hear a presystolic bruit, without being struck with the reasonableness of a view that would attribute it to a vibrating jet of blood forced through a narrow mitral ring by an hypertrophied auricle, and impinging on the interior of the ventricle at its apex, close to the very spot where it is heard, and where its thrill can often be so plainly felt.

But for regurgitant bruits (whether mitral or aortic) this theory is less completely applicable. Chauveau himself pointed out, and Bergeon has especially insisted on the same fact, that the murmur caused by a *veine fluide* is audible only in the dilated space beyond the constriction, and not on its proximal side. Thus, in an experiment, if water be passed through a tube which is narrowed in its calibre for a considerable part of its extent, no murmur is heard, either when the fluid enters the narrowed portion, or when it is flowing through it, but only when it passes into the wider space beyond. Now, it is evident that this applies very well to that part of a regurgitant bruit, which is heard in the direction of the stream; to that part, namely, of a mitral systolic bruit which is heard in the back, near the left scapula; and to that part of an aortic diastolic murmur which is heard below the valves, over the left ventricle and at the apex of the heart. But a mitral systolic murmur is much louder at the heart's apex than in the back; and an aortic diastolic murmur is almost always audible to some extent above the valves. These facts are inexplicable by Chauveau's theory, but Bergeon has supplied a very simple and complete explanation. He has shown that if a tube through which water is flowing be suddenly narrowed in a certain part of its course, and if the narrowed part be made to project backwards into the wider part, so as to form a lip or rim with a circular cul de sac facing the current, a murmur is always produced, which is audible on the proximal side of the obstruction, and transmitted backwards, or against the stream. Now, it is evident that in nearly all cases of valvular incompetency the conditions requisite for the production of such a murmur are fulfilled. The valves, although unable to close, are scarcely ever completely destroyed, but almost invariably project inwards to some extent, and thus form culs de sac, in which the murmur in question can be generated.

This extension, by Bergeon, of the very narrow limits within which Chauveau's theory would have confined the conditions capable of generating the *bruit de souffle* appears to meet all the facts that require explanation as regards murmurs that are known to be due to obstruction of, or regurgitation through, the cardiac valves. Are we entitled to say that all cardiac

bruits de souffle must be placed in this category? It appears to us that an affirmative answer to this question cannot yet be given. It must be borne in mind that all the physical experiments made by the French writers whose views are under consideration have dealt simply with the flow of liquid through tubes that are either rigid, or at most elastic. No similar experiments have as yet been made, or perhaps could be made, with cavities possessing contractile walls. Is it not very probable that were such experiments to be carried out, it might be found that something more had still to be learnt as regards the physical conditions capable of generating a *bruit de souffle?*

In certain cases, as is well known, cardiac and vascular murmurs present remarkable modifications in their quality, and we may now inquire to what causes these are due. One of these modifications consists in a peculiar roughness or harshness. Instead of being a soft blowing sound, the murmur resembles the noise produced by a file or rasp, or even by a currycomb. Hence the older French physicians, who devoted much pains to the discovery of the varied morbid changes leading to the development of the murmurs in question, designated them by special names, such as "*bruit de scie,*" "*bruit de râpe,*" "*bruit d'étrille.*" If the hand be placed over the spot where one of these bruits is audible, a thrill can generally be felt, such as Laennec termed a "*frémissement cataire.*" The two are so constantly met with together that there can be no doubt of their arising from the same cause; and the question as to the mode of origin of the murmurs under consideration may be taken as identical with that as to the causation of frémissement or thrill.

Now, the latter phenomenon is evidently due to vibrations of the solid tissues, transmitted directly to the surface of the body. It is, therefore, very natural to suppose that these vibrations are its determining cause. According to this idea, an ordinary soft *bruit de souffle* would differ from a harsh murmur attended with thrill in this—that while the former is due simply to the formation of a *veine fluide* (as described above), the latter is dependent on the simultaneous vibrations of the orifice at which the *veine fluide* is generated.

It seems, however, that this opinion is not tenable, although passages might be quoted from the writings both of Chauveau and Bergeon which would, at first sight, appear to support it. Among the points to which Savart directed his attention in the experimental investigations to which we have several times referred, was the question whether the sound produced by a *veine fluide* is due solely to the vibrations of the liquid or to those of the orifice through which it passes, and he came to the conclusion that the former was the case. For, were it otherwise—

"the nature of the material forming the orifice, the existence of an extremely thin edge to it, the degree of polish in the surrounding metal surface, and, above all, the character of the liquid and its temperature, ought to be capable of changing the number of vibrations. But no one of these conditions appears to exercise any influence on the state of movement of the *veine fluide*, nor is any effect produced when, the orifice being pierced in a brass plate, this is rubbed over with a fatty substance, so as to change completely the state of the surface.

"Lastly, if the circumference of the aperture be touched with a solid and resisting body, which would arrest its vibrations (if such existed), or at least affect their amplitude, no change whatever is observed in the state of the vein."

The point, besides its theoretical interest, has some real importance. For, if a rough bruit, with its attendant thrill, be due to the independent vibrations of the solid walls of the orifice at which it is generated, the occurrence of such a bruit may enable us to draw an inference as to the character of the orifice. Now Bergeon, in speaking of the causes of frémissement, says that this can be felt "when the souffle is intense and the tube" (he is referring to simple physical experiments) "thin and elastic. . . . . Walls which are thick and rigid do not allow of transmission, whereas supple, thin walls present the most favorable conditions." The use of the word "transmission" in the sentence which we have quoted seems to show that Bergeon is referring rather to the conduction of thrill and murmur from the fluid within than to the development of sonorous vibrations in the tube itself, otherwise we should have understood the passage differently.

It appears to us, however, that the facts of clinical experience fail altogether to support Bergeon's statement. No bruit is so frequently rough and attended with thrill as the presystolic bruit of mitral stenosis. But in this affection the mitral valve, at the orifice of which the bruit is generated, is generally extremely hard, thick, and inelastic; presenting, in other words, conditions the very opposite of those which Bergeon would lead us to expect. But, if we think for a moment, we shall surely have very little difficulty in understanding why the presystolic murmur is so harsh and so commonly accompanied by frémissement. Not only does the *veine fluide* that generates it flow into a comparatively wide and empty space, but it impinges on the wall of this space, close to the very point to which the finger is applied on the exterior of the thoracic parietes. The full shock of the jet of blood is transmitted directly to the hand of the physician who is examining his patient. It is probable, we think, that in other cases in which frémissement is felt, conditions more

or less similar may often be traceable, as, for instance, in aneurisms.

We must conclude, then, that the rough grating quality of a murmur and the accompanying thrill are not due to any special state of the orifice at which the murmur is produced, but rather to the intensity of the murmur itself, and to the particular direction taken by the stream of blood which causes it.

Another modification of the *bruit de souffle* is that which is commonly spoken of in England as the "musical" murmur. It is a high-pitched sound, varying greatly in different cases, and resembling sometimes a whistle, sometimes the mewing of a cat, or the cooing of a dove, or, again, the note of some musical instrument. Savart studied this subject, but came to the conclusion that it is difficult to determine the conditions under which the sound produced by a vibrating liquid assumes a musical character.

According to Bergeon, there are at least two kinds of these murmurs—(1) Musical bruits produced by the vibrations of the fluid alone. (2) Musical murmurs produced in part by the vibrations of the fluid, in part by those of some solid body placed in the course of the sonorous *veine fluide* which it generates.

(1) Savart found that when a *veine fluide* is made to impinge upon a resisting surface with but little capacity for generating sound, a cube of wood, for instance, it spreads out, and radiating in all directions produces a musical sound. Now, Bergeon suggests that an arrangement is not unfrequently met with in the circulatory system which offers an approach to these conditions. It is that in which a vessel immediately beyond a narrowed point is bent in the form of an S. The *veine fluide* then impinges on one side of the vessel, and may fairly be supposed to radiate as in Savart's experiments. The spot to which Bergeon specially refers, as presenting the required curve, is the jugular fossa at the base of the skull. In anæmic subjects, he says, a *veine fluide* is often generated at this point, as had been shown by M. Bondet, and, reinforced by the S-shaped curve of the vessel, it gives rise to an intolerable singing in the ears.

(2) A more frequent cause of the musical bruit is probably the vibration of a membranous substance suspended in the blood stream. This kind of bruit appears to be very easily imitated. Bergeon found in his experiments that by introducing a small tense membrane into a tube just beyond a narrowed point, so that the *veine fluide* might impinge upon it, he could throw this membrane into vibration, and make it produce a sound which was sometimes louder than that of the *veine* itself. By Chauveau musical bruits have even been produced experimentally within the jugular veins of the horse. He first nar-

rowed a vein at one spot, so as to generate a vibrating *veine fluide*, and afterwards seized the free edges of the valves with small bent pins, and according as he allowed the valves to float in the blood stream, or kept them applied to the wall of the vessel, he found that a musical bruit was present or disappeared again.

The theory of the *veine fluide*, which we have been tracing in its applications to cardiac and vascular murmurs, is by no means confined to them. It has an equally important bearing upon the sounds that are heard with the respiration under healthy and morbid conditions. The French writers, whose works we are reviewing, have not failed to study this branch of the subject as well as the former. And we may now pass on to consider the conclusions to which their experiments and observations have led them.

But first it may be asked, whether Savart's laws are applicable to gases as well as to liquids. To this question a sufficient answer might, perhaps, be found in the fact that the sound made by an ordinary bellows is so exactly like the cardiac and vascular murmurs that we have been considering that it has given a name to them, in both the French and the English languages—bellows murmur—*bruit de soufflet*. The point has, however, been specially investigated by Sondhauss.[1] He experimented with a jet of common air, mixed with tobacco smoke, to make it visible. This was forced by means of a bellows into a glass vessel through a narrow orifice. For a distance of three centimètres from the orifice the jet retained as nearly as possible its size, being, in fact, somewhat smaller. Beyond this it became wider than the orifice, and appeared to be the seat of a continual agitation. When made to impinge upon a spherical or plane surface the jet spread out and formed a sheet. In other words, Sondhauss's experiments with gases afforded results identical, so far as they went, with those of Savart on liquids.

It may, therefore, be assumed that whenever a stream of air passes from a narrow into a wider space a sonorous *veine fluide* may be generated.

Now, during the act of inspiration, the air drawn into the mouth and nose enters successively passages that are smaller and smaller until it reaches the space between the true vocal cords. Through this comparatively narrow space it passes into a much wider one, the trachea. The conditions for the production of a sonorous jet are thus fulfilled. And on listening over the larynx and trachea of a healthy person one hears, each time he

[1] Poggendorff's 'Annalen,' 1852, p. 58.

inspires, a loud blowing sound, which evidently is due to this cause. It is carried for some distance downwards along the respiratory passages, and thus follows the law of transmission of a *veine fluide*.

In the act of expiration, again, the air traversing the upper opening of the larynx escapes from a narrow into a wider space, so that a sound is in all probability produced at this spot. And if the lips be partly closed, while the expiration is at all forcible, another sonorous jet is generated as the air passes out of the mouth. Now, it has been shown that the sound produced by the vibrations of a *veine fluide* is heard chiefly in the direction taken by the jet itself, and is but slightly, or not at all, audible behind the narrowed point at which the *veine fluide* is produced. Bnt on listening with the stethoscope over the upper part of the trachea of a healthy person, one finds that an expiratory sound is plainly audible, and is, in fact, louder than the inspiratory sound heard at the same spot.[1] The explanation of this appears first to have been given by Bergeon. He shows that the expired air, in passing through the larynx, meets with the edges of the superior or false vocal cords, which, with the base of the epiglottis, form projections directed against its stream. The result is the same as in those experiments of his, in which a current of water flowing through a tube was made to pass into a narrowed part that projected backwards, forming a lip or rim, and giving rise to a circular cul de sac, facing the current. Under these conditions, as we have seen, a sound is produced, which (unlike that of a simple *veine fluide*) is heard *behind* the seat of obstruction. It has already been shown that the results obtained by Bergeon in these physical experiments are paralleled by the murmurs that are audible over the aorta in aortic regurgitation, and over the left ventricle in mitral regurgitation, respectively. The expiratory sound heard over the trachea appears to be another striking instance of the same kind. Indeed, as Bergeon remarks, there is no other way of accounting for the fact that the tracheal expiratory sound should be louder than the inspiratory. For, since the stream of air in expiration moves more slowly than in inspiration, the former ought to produce a more feeble murmur than the latter, did the two murmurs arise in the same way. But it is an ascertained fact that the sound produced by a current impinging on a solid edge turned against it is proportionately louder than that due simply to a *veine fluide*.

Now, if the stethoscope be placed over the surface of a healthy

[1] All authors appear to admit this, so far as the human subject is concerned. M. Bondet however, speaking of the horse, says that the expiratory murmur over the trachea is often less accentuated though longer than the inspiratory.

person's chest, a soft rustling sound is heard during inspiration, but during expiration either no sound is heard at all, or one which is comparatively much less loud. By Laennec this and the other respiratory sounds that have been already mentioned were attributed to the friction of the inhaled air against the walls of the trachea, bronchial tubes, and pulmonary vesicles, respectively. Most subsequent writers have expressed similar views.

But, as some of our readers may be aware, a very different theory was promulgated by the late M. Beau.[1] As far back as the year 1834 that ingenious but eccentric physician maintained that the chief cause of the various respiratory sounds heard by auscultation is the transmission downwards of a sound which he termed the "*bruit guttural*," and which he believed to be due to the friction of the in- and out-breathed air against the parts at the back of the throat, and especially the *velum palati*. Subsequently, in a second paper on the same subject,[2] M. Beau attributed the formation of the sound in question mainly to the orifice of the glottis, and accordingly named it the "*souffle glottique*."

Soon after the promulgation of M. Beau's theory, it was critically examined by Dr. Stokes and Dr. C. J. B. Williams. The former[3] rejected it, mainly on the ground that a natural respiratory murmur can be heard in patients who do not breathe through the mouth or nostrils, but through a tracheal fistula. The latter[4] also refused to accept it, using the following arguments:—(1.) That the sound of the voice, which is much stronger than the inspiratory murmur, does not reach many parts of the surface of the lungs. (2) That vesicular respiration may be so exaggerated in parts as to become as loud as the tracheal, without this having sustained any proportionate increase. (3) That when laryngeal rhonchus supplants the ordinary laryngeal blowing we still have the vesicular murmur, with the addition of the distant laryngeal rhonchus.

Raciborski, also, appears to have combated M. Beau's theory, showing that in the rabbit after tracheotomy the respiratory sounds are heard as before. But, as was said by Dr. Spittal[5] (in

---

[1] "Recherches sur la Cause des Bruits Respiratoires perçus au moyen de l'Auscultation," 'Archives Générales de Méd.,' 1834, p. 557.

[2] 'Archives Générales de Méd.,' 1840.

[3] 'Treatise on the Diagnosis and Treatment of Diseases of the Lungs,' 1837, p. 252.

[4] 'Pathology and Diagnosis of Diseases of the Chest,' fourth edition, 1840, p. 22 (note).

[5] 'Edin. Med. and Surg. Journ.,' 1839, li, p. 99. The statement in the text with regard to Raciborski is taken from this paper, as we have had no opportunity of consulting Raciborski's work.

a paper defending M. Beau's views), this experiment can hardly be regarded as conclusive, for the open extremity of the tube may have been itself sufficient for the production of a murmur, which would be transmissible downwards exactly like the *bruit guttural*. The same remark evidently applies to Dr. Stokes's observation. Indeed, with the views that we now hold as to the causes of the development of the *veine fluide*, it is evident that a tracheal fistula presents all the conditions requisite for it.

The modern French writers, however, whose works we have under review, appear to have demonstrated conclusively the truth of the opinion generally held, that the respiratory sound heard on auscultation over the surface of the lungs is developed within the organs themselves, and not merely transmitted downwards from the larynx; and this by two distinct lines of argument. In the first place, they have repeated Raciborski's experiment under such conditions as to exclude the possibility that a *veine fluide* could be generated in the tracheal wound. The trachea was completely divided between two of its rings, and its open end was drawn outwards either by a hook or by a metallic suture. The result was that the "*bruits glottiques*" disappeared entirely. On the other hand, the inspiratory murmur still remained audible over the surface of the chest, and its intensity was but little, if at all, diminished. The laryngeal sounds may therefore be abolished, while the pulmonary inspiratory murmur persists.

In another series of experiments, made by M. Bondet, exactly converse results were obtained. The pulmonary murmur was extinguished, the laryngeal sounds remaining unimpaired. This result was effected, in the horse, by the division of the pneumogastric nerves. After the performance of this operation, the acts of respiration were generally found to be less frequent, but the respiratory movements, nevertheless, more ample. The amount of air drawn in was increased, as was shown by the spirometer; being in fact, double the normal quantity, or even more than this. Now, on listening over the trachea the respiratory sounds were found to be very loud and prolonged, especially the inspiratory. *But the pulmonary murmur was completely abolished.* M. Bondet states that he repeated this experiment several times, and always with the same result. On listening to the chest of animals after section of the pneumogastric nerves, there was absolute silence.

Of the fact just stated M. Bondet gives the following very ingenious explanation. It is well known that an immediate effect of section of the pneumogastric nerves is paralysis of the muscles of the bronchial tubes. One of the first consequences

of such paralysis must be relaxation and dilatation of the tubes. Now, their ultimate ramifications open into the air-cells by apertures which are of less diameter than the air-cells themselves. Thus, under normal conditions it may be said that the air-cells form as it were an ampulla at the extremity of every twig of the bronchial tree; and this arrangement evidently affords the conditions requisite for the generation of a sonorous *veine fluide* at each of these points. But when the pneumogastric nerves are divided, the relaxed and paralysed bronchial tube opens into the air-cells by a funnel-shaped aperture. Hence, the respiratory murmur is no longer audible.

It is evident that this view with regard to the origin of the pulmonary inspiratory murmur is not essentially different from that which was propounded in this 'Review' in the year 1865, by Dr. Waters.

"The air-sacs," says Dr. Waters, "consist of somewhat elongated cavities, which communicate with a bronchial ramification by a circular opening which is usually smaller than the cavity to which it leads, and has sometimes the appearance of a circular hole in a diaphragm, or as if it had been punched out of a membrane which had closed the entrance to the sac. . . . . I believe that in the passage of the air through this constricted opening the main elements of the respiratory murmur consist."

According to this theory, therefore, the sounds heard during respiration have two distinct seats of origin:—(1) The glottis; (2) the termination of each minute bronchial tube in the corresponding air-sacs. During expiration, on the other hand, the conditions requisite for the development of a *veine fluide* within the lungs do not exist. In escaping from the pulmonary tissue into the bronchial tubes the air passes from a wider into a narrower space. And although each division of the bronchial apparatus opens into a larger tube, it must be borne in mind that the section of every tube is always less than the combined sections of the smaller tubes that open into it from below. Bondet and Bergeon, accordingly, agree that in their experiments, when the trachea was freely incised and drawn out through a wound in the neck, all expiratory sound ceased over the surface of the chest. We can, therefore, understand why in the human subject the expiratory murmur should either be entirely absent, or should at any rate be much less intense than the inspiratory under normal conditions; in fact, no expiratory murmur exists, except that which is transmitted downwards from the larynx.

Before quitting this part of our subject, we must add that M. Luton, while admitting the accuracy of M. Bondet's facts, is, nevertheless, disposed to doubt the validity of his explanation

that the pulmonary inspiratory murmur is due to a *veine fluide* formed by the air in passing from the extremity of a bronchial tube into the relatively larger air-cells. "It is doubtful," he says, "whether we can apply rigorously the law of analogy to such extremely minute canals." M. Luton goes on to point out that in respiration the air is energetically drawn into the lungs by the movements of the thorax, and thus, as it were, into a vacuum. Thus, in each part of the respiratory passages the air has a tension greatly in excess of that which exists in the parts beyond. And he conceives that this inequality of pressure may lead to the development, in the whole extent of the air-passages, of a series of alternate waves of condensation and rarefaction, by which sound may be generated. Indeed, just as we have maintained that no physical experiments have as yet reproduced the conditions afforded by a contracting cavity (such as the cardiac ventricle), so we may admit that the phenomena attendant on the suction of air into organs like the lungs are, after all, imperfectly imitated by simply forcing gases through tubes. It may be that in the act of inspiration physical laws, by which sound is generated, are called into action, altogether apart from those which regulate the development of *veines fluides*.

We may now pass on to consider how these views with regard to the respiratory sounds may be made to bear on the auscultatory phenomena presented by the air-passages and lungs under various diseased conditions. And first we must remark that the theory of M. Beau, as originally enunciated, was intended to include "all the respiratory sounds, not complicated with râles," and was by no means limited to the normal inspiratory murmur heard over the surface of the lungs in health, to which murmur alone Dr. Stokes and Dr. Williams directed their critical remarks on the theory. Indeed, the observation which originally led M. Beau to doubt the correctness of the commonly received views was the following:—He was examining a case of pleuritic effusion, in which the subspinous fossa presented marked tubal respiration, so that it seemed to blow into his ear. At the same time the patient was making a loud noise in breathing. But when he was told to breathe quietly so as to lessen this noise, the tubal respiration diminished in the same proportion. M. Beau goes on to say that in several other cases in which a bronchial souffle was audible he found that it could be diminished or suspended by making the individual diminish or suspend the respiratory sound in the throat. He, therefore, concluded that the morbid sound in question is solely due to the transmission downwards of his "*bruit guttural*;" it being of course supposed that hepatized lung, or lung com-

pressed by fluid, conducts sound better than the healthy pulmonary tissue.

As we have seen, the investigations of MM. Chauveau and Bondet have demonstrated the incorrectness of M. Beau's theory as applied to the normal inspiratory murmur. But it would seem that these observers have proved the same theory to be true, so far as concerns the morbid sound, known as bronchial or tubular breathing. A horse was affected with pneumonia, and an intense "*souffle tubaire*" was audible. MM. Chauveau and Bondet cut across the trachea below the glottis. The air then entered the lungs without passing through the glottis, and the *souffle* at once disappeared.[1]

It is most desirable that the accuracy of this observation should be tested by further observations in the human subject; that it should be ascertained, for instance, whether, after tracheotomy for laryngeal phthisis, bronchial breathing hitherto audible over consolidated parts of the lungs disappears. Perhaps this may not necessarily be the case, for the diameter of the canula used in tracheotomy is much less than that of the trachea, and the conditions, therefore, still exist which are required for the production of a *veine fluide*. But the experiment of MM. Chauveau and Bondet certainly appears to show that when there is an artificial opening into the larynx it is quite possible that pulmonary consolidation may exist without revealing its presence by the ordinary signs. Perhaps, indeed, this difficulty may not be confined to cases in which tracheotomy has been performed. Ulceration of the larynx, destroying the attachments of the vocal cords, may sometimes prevent the formation of a *veine fluide* in the air which passes the glottis, and so remove one of the conditions necessary for the development of bronchial breathing in diseased parts of the lung below. In this way we may find an explanation of the fact, on which Dr. Stokes[2] formerly laid so much stress (and for which he admitted that he was unable to account), that it is often difficult or impossible to determine whether the lungs are or are not healthy in cases of laryngeal disease. The fact itself, must, we think, be well known to every practical physician.

The theory of the origin of bronchial breathing in the transmission downwards of sounds produced in the larynx has another practical application of some importance. We believe that most auscultators, in investigating the state of the lungs,

[1] We feel bound to state that we have not been fortunate enough to find this experiment recorded either by M. Chauveau or M. Bondet, in their respective papers. We give it on the authority of M. Bergeon, who mentions it in both his works—in the one at p. 18, in the other at p. 15.

[2] Op. cit., p. 253.

direct the patient to breathe as noiselessly as possible. And when he is making a blowing noise with his mouth they attach but little importance to any bronchial breathing that may be audible. Now, our own observations have repeatedly shown us that this is a mistake, although we were not until lately aware of the physical theory which explains why it is so.

It is important to note that M. Beau's theory of the cause of bronchial breathing is not necessarily applicable to the sounds heard over a pulmonary cavity, communicating freely with a bronchial tube. The orifice of the tube then presents the conditions requisite for the development of a *veine fluide* in the air passing into the cavity. The same remark applies to the fistulous opening which leads into the pleura in cases of pneumothorax. Thus, both amphoric and cavernous breathing may be produced in the spaces over which they are heard. It is, however, necessary for the generation of morbid sounds in this way that the walls of the cavity should not be too rigid, to allow of its undergoing changes of size during respiration, and so permitting the entrance and exit of air. Moreover, it is only during inspiration that a *veine fluide* can be generated in a cavity in the way we have supposed. According to M. Beau, indeed, the cavernous souffle, equally with the bronchial, disappears when the patient breathes quietly, so that no noise is produced in the throat.

It remains for us to consider how far the theory which we have been considering applies to the auscultatory phenomena presented by emphysema of the lungs. The writers whose works are under review have, however, made but little reference to this disease, perhaps because they have found some difficulty in dealing with it. Dr. Waters, indeed, finds an argument in favour of his view with regard to the nature of the respiratory murmur, in the fact that in emphysema of the lungs the inspiratory murmur is extremely feeble. The air-sacs, he says, become much altered in character, in consequence of distension, rupture, and absorption; and the membrane guarding the entrance to them entirely disappears as the disease progresses. Bergeon, again, attributes the feebleness of the inspiratory mumur, which Bondet observed in animals after section of the pneumogastric nerves, to the fact, pointed out by Bernard and Longet, that by this operation the lungs are rendered emphysematous. Hence, says Bergeon, the air which enters the chest, finding the pulmonary vesicles already distended, cannot penetrate them with the same degree of force as before. But it is to be observed that the inspiratory sound is equally deficient during an attack of asthma. Now, in this disease, according to the theory current in England, the bronchial tubes are narrowed by spas-

modic contraction; and one might, therefore, suppose that they would fulfil better than tubes in the natural state the conditions required for the development of a sonorous *veine fluide.*

A more serious difficulty seems to be presented by the prolonged expiratory murmur, that is well known to be so marked a feature of emphysema and of capillary bronchitis. Luton, indeed, attributes this to the transmission downwards of a sound formed within the larynx. It is, he says, the first phase of the same phenomenon, of which an extreme degree constitutes the *souffle tubaire.* The expiratory sound of emphysema, prolonged as it is, is not more sonorous than natural, unless the laryngeal murmur is itself increased in intensity. We cannot but regard this explanation as extremely unsatisfactory, and as failing to meet the facts of the case.

III.—A System of Dental Surgery.[1]

Perhaps there is no speciality in surgery which has made greater advances within the last ten years than that concerned with diseases of the dental organs, and the issue of a second edition of a book which has been generally accepted as an authority for those engaged in the study of dentistry presents a fitting opportunity of reviewing the progress made. The treatment of the teeth was for many years to a great extent empirical, and too often carried on by men who had but little general education and who were for the most part medically unqualified; but a noble effort on the part of those whose exertions in its cause have made odontology a science caused the College of Surgeons to recognise its claims by granting a special licence in dental surgery. This diploma is the more valuable inasmuch as it implies that its possessor has spent two years in study at a general hospital, attending the same lectures, and engaging in the same curriculum as that which is necessary for those who desire to obtain the full qualification in medicine and surgery. The higher qualifications required have already materially improved the class of men who enter the profession; and although it is a question whether it would not have been a wiser measure to have given the degree of licentiate in dental surgery only to those who had qualified themselves by becoming members of the College, the necessity of spending two years at a hospital amongst those who are qualifying themselves for higher degrees stimulates the

[1] *A System of Dental Surgery.* By John Tomes, F.R.S., &c., and Charles S. Tomes, M.A., &c., &c. Second edition, revised and enlarged with 268 engravings. London, 1873.

ambition of many, and induces them to acquire the licence. It is a matter to be regretted, that, in no few instances, the public do not yet distinguish the qualified from the wholly unqualified members of the profession; but another decade will, doubtless, lead them to appraise at their true worth a class of men whose conduct brings a blush of shame upon the face of every respectable practitioner when he hears them called by the same title as himself, and it is deplorable that journals of reputation can be induced to insert the specious and lying advertisements of such men as those to whom we refer. Only by insisting on the highest standard of professional knowledge and the strictest code of social ethics in our educational establishments can we teach the public to discriminate between such men and those deserving their confidence.

No man has contributed more to the literature of dental surgery than Mr. Tomes, and it is with great pleasure that we see another edition of his book, in producing which he has been aided by his son, Mr. C. S. Tomes. The present volume is in every respect an improvement upon its predecessor, the important additions to its text proving the great advance which has been made in the science of which it treats since the publication of the first edition in 1859. All the sections of which the book is composed have been carefully revised, especially those relating to the development of the dental tissues and to the description of caries; whilst those on neuralgia, odontomes, hyperostosis, and those indirect affections connected with dental disease, are entirely new. Notwithstanding these additions, however, the book is not a ponderous one, inasmuch as the size of the print has been materially reduced. The first 250 pages of the book are devoted to the subject of teething, that is, the condition of the teeth and jaws at the time of birth, and the changes onwards until the temporary teeth have arrived at maturity. The subject matter here is much the same as in the earlier edition, which seemed even then to be almost exhaustive of the topic, as far as development was concerned; and, if a fault could be found, it would be that the latest improvements in the mechanical treatment of irregularities of the teeth have not been described quite so fully as they might have been; this, however, is a small matter where so much that is valuable is to be found.

The section on the dental tissues has been almost entirely rewritten, and the list of works referred to, given at the head of the chapter, alone prove how thorough has been the research after every available source of information. Here we must allude to a most interesting discussion as to the histological importance of the so-called membrana preformativa. Mr. Huxley was the first to discover that this membrane could be raised

from the enamel at any period of its growth by the use of hydrochloric acid, and, as he considered his discovery proved to him the existence of a well-defined membrane separable both from the enamel-pulp and the enamel, the question occurred whether the enamel organ really has any influence in the development of the enamel, Mr. Huxley maintaining that neither it nor the capsule "contribute *directly* in any way to the development of the dental tissues, though they may indirectly." Kölliker and Wenzel hold a different view entirely and assert that the enamel-cells do not undergo conversion into enamel-fibres, but that the enamel is secreted from their ends. Now, Mr. Tomes thinks that the enamel-cells are actually converted by calcification into the enamel-prisms, the periphery of the cell being calcified earlier than the central portion. Whilst calcification is thus proceeding inwards in each individual cell it unites the contiguous cells to each other. At this point, viz. the remotest line of calcification, the cells are easily withdrawn from the calcified fibres, leaving the surface of the latter with the appearance of a perforated membrane, these perforations being caused by the withdrawal of the inner uncalcified portions of the cells or columns. Now, it must be remembered that this membrane can only be demonstrated by the action of a strong reagent, viz. by an acid capable of dissolving calcium carbonate and phosphate, and we think that, considering this fact, its substantial existence is at least open to doubt. In fact, Mr. Tomes's clear explanation of the development of the enamel-prisms from the columns of the enamel organ almost conclusively proves, that this membrane is to be regarded only as the youngest layer of enamel but slightly impregnated with inorganic constituents, and as having no connection whatever with the formation of the enamel.

Some years ago, it may be remembered that Professor Owen entered into a warm controversy with Mr. Nasmyth concerning a membrane which has long been known by the name of its professed discoverer. "Nasmyth's membrane" is a tissue separated from the surface of the enamel, and has been therefore called the persistent dental capsule. Waldeyer considers it to be formed from the external epithelium of the enamel organ, metamorphosed into cornified tissue, as a proof of which theory he states that the remains of cells can be seen by staining with nitrate of silver. Huxley considers it to be identical with the *membrana preformativa;* whilst Kölliker, following out his favorite excretion theory, thinks that it is a protective covering thrown out by the enamel-cells over the enamel. On the other hand, Mr. Tomes, arguing upon the fact that this membrane is continuous with the cementum of the fang, and that it is thickened in the depres-

sions of the crowns of molar teeth, considers "Nasmyth's membrane" to be nothing more than a thin layer of cementum, the absence of lacunæ in its substance being owing to the fact of its extreme thinness. Mr. Tomes has made many observations on numerous specimens since the previous issue of his book, and they have led him to feel even more confident of the correctness of his views than before. Hence, if they are accepted,—and Mr. Tomes's explanation of the appearances presented certainly appears more lucid than any yet given, we must look upon this membrane as cementum either imperfectly calcified or not yet calcified at all; but, at the same time, it must be recollected that there are many analogies for assuming it to be a kind of finishing coat to the enamel, as Kölliker supposes it to be.

One of the most interesting chapters in the book is that on "Odontomes," and it is probably the most perfect memoir on the subject existing; for besides many original remarks it presents a most instructive *résumé* of the views held by various authors on the subject. The term odontome is applied to those masses of dentinal tissue which result from morbid conditions of the formative pulp, and may be divided into four classes, according to Professor Broca's classification, viz. "odontomes embryoplastiques," those which arise before the development of the *membrana eboris; odontomes odontoplastiques*, or those arising before the first layer of dentine is formed; and *odontomes coronaires* and *radiculaires*, or those which arise during the formation of the crown and root respectively.

This subject is of the greatest interest to the surgeon, for this is a class of tumours the character of which has hitherto been wholly misunderstood, so much so that terrible operations have been performed for their eradication under the supposition that they were osteal tumours, but which, had their true nature been appreciated, need never have taken place. The peculiarity of these tumours is that they are *encysted* in the bone of the jaw, having no continuity of structure with the surrounding tissue. On examination they are seen to be made up of a heterogeneous mass of enamel, dentine, and bone-like material. In the odontoplastic variety there is often, on a merely superficial examination, but little resemblance to a tooth, but in the *odontome coronaire*, which originates after calcification has commenced, the crown at least is fairly formed, mixed up, though it be, with subsequent outgrowths of the pulp. Now, large portions of the jaw have been resected under the belief that there were tumours connected with its substance, whereas, had their true character been known, only a small portion of the bone need have been removed, sufficient to expose the mass, which would then have been easily extracted, its removal being followed by the relief

of all the symptoms, the cavity in the bone speedily contracting and filling up, leaving but little trace of the operation behind.

What are the means by which these growths may be identified prior to removal? In every case one or more teeth have been noticed to be absent from the normal series; and in all those cases where there is enlargement of the maxilla, and teeth are found never to have been erupted, there is a strong antecedent probability that the absent tooth is the cause of the tumour, and there is but little doubt that, had this fact been always remembered, many jaws might have been saved and the odontome removed with very little destruction of the bone. We have not sufficient space to enlarge upon this interesting subject, but may briefly say that this chapter is as pregnant with interest to the general surgeon as to the specialist.

The next chapter is devoted to the consideration of those peculiar cysts found in the substance of the jaw containing a tooth or teeth, and, therefore, called dentigerous; and although more easy to diagnose than the odontome, they have still been at times mistaken by the surgeon. Much discussion has arisen as to their true character, and Mr. Tomes, though regarding the question as still in abeyance, seems to incline to the view—the correctness of which mainly depends upon the assumption of the presence of Nasmyth's membrane on the enclosed teeth—that the fluid contained in the cyst lies between the enamel and the tooth-capsules. This opinion has been promulgated by Mr. Salter, though we know not to whom the credit of its origin must be conceded.

The section on neuralgia is also, like the two preceding, one of general interest, and Dr. Anstie's views upon the subject are somewhat sharply criticised. The opinions concerning the pathology of neuralgia are very various, and Dr. Anstie maintains that the posterior roots of the spinal nerves are the real seat of mischief, and that the essential condition is atrophy, which is usually *non-inflammatory* in its origin. With regard to this view Mr. Tomes remarks that it scarcely seems tenable when experience daily teaches us that the extraction of a tooth, or even the destruction of a nerve by an escharotic may effect the instantaneous cure of long-standing neuralgia; for this fact seems of itself to prove that neuralgia is possible without much central degenerative change. With respect to Dr. Anstie's disbelief in carious teeth being a frequent source of the disease, it is remarked that few dental surgeons could endorse it, though Mr. Tomes excuses it by saying that many of the most typical cases of neuralgia, as produced by diseased teeth, are just those in which from the absence of all *local* pain the teeth are never subject to suspicion. We ourselves wonder that a careful observer like

Dr. Anstie could have hazarded such an assertion; and, indeed, not to mention innumerable other cases, we have just seen one where acute neuralgic pain over the left parietal bone, which had existed for nearly two years, was instantly cured by the extraction of a carious first molar which had never been suspected of being the *causa morbi*, inasmuch as it had never itself exhibited the slightest local pain. We are certain that if those who are called upon to treat seemingly intractable cases of cranial and facial neuralgia would pay more attention to the condition of the teeth they would often find the straight road to successful treatment of the malady. Likewise in the chapter succeeding that which we have just commented upon, many secondary affections are mentioned which had their primal origin in diseased conditions of the teeth, those affections varying from the simplest neuroses to amaurosis and paralysis.

We now come to the consideration of the pathology of dental caries, and here Mr. Tomes not only gives a most clear and lucid exposition of his own views on the subject, but has added an appendix at the end of the volume which enters fully into the opinions of other writers, and from which certain deductions are drawn which will be alluded to presently. On the causes of decay of the dentinal tissues various theories have been advanced; some maintaining that the destruction of tissue is mainly dependent upon chemical action, others regarding it as strictly a vital phenomenon, while others, again, argue that it is the result of combined chemical and vital action. In comparing the chapter in the first edition of this book relative to these different opinions with that in the present volume, it appears that Mr. Tomes's views have undergone some modification; he then seemed to side with those who argued that a combination of chemical and vital forces caused the disease, summing up his case by saying that whilst there could be but one opinion as to the mere chemical character of the processes by which the enamel and dentine are softened by the removal of earthy matter, that this process was to be regarded as a "consequence secondary to some preceding change in the part rather than as a primary condition." He now comes to a different conclusion, viz. "That caries is an effect of external causes in which the so-called 'vital' forces play *no* part," a conclusion which in the present state of our knowledge—which must as yet be deemed imperfect—does not seem to be quite warranted by facts, especially as the views of the chemico-vital party have been rather kept out of view in the controversy, the arguments in the appendix seeming to us particularly biassed and inconclusive. It would be out of place to discuss this question *in extenso* here, but some pathological phenomena which are described by Mr. Tomes appear to militate against the view

which he adopts. In teeth the subjects of decay a transparent zone, limiting the area of destructive action, may be seen, produced by the consolidation of the dentinal fibrils within the tubes. This zone of dentine being thus rendered hard and impervious, the diseased is isolated from the healthy portion of the tooth; hence, this condition is exactly equivalent to that which occurs in the case of inflammation or sloughing of soft tissue, where the parts which surround the inflamed area are infiltrated with serum or lymph, whereby the spread of the disease is arrested. This, at least, would show that vital action is at work, and cases are not wanting to prove that disease may not only cease, but that the carious surface may become hard and polished like the surrounding tissue. Again, in pregnancy the teeth often show signs of decaying suddenly, the tendency to disease ceasing after parturition. A like result occurs after certain severe diseases, and a change from one country to another may either develop or arrest carious action, the oral secretions continuing normal meanwhile; and we cannot but conclude that, although an abnormally acid condition of the mucus or saliva may be a source of disease, certain changes must take place in the structures of the tooth beforehand, and that therefore caries is the result of vital and chemical phenomena combined.

Other chapters are devoted to the consideration of ulcerations of the tongue, and of tumours seen in connection with the gums and other portions of the oral cavity, and merit particular attention, inasmuch as the dental surgeon often has the first opportunity of noticing diseases like epithelioma and cancer, which can only be cured when placed under treatment at an early stage of their development. It would not be within the limits of a review to enter very fully upon those portions of the book which are devoted to the correction of deformities by mechanical combined with surgical treatment, or to refer in detail to the various operations which are required for the cure or arrest of disease by plugging or stopping carious cavities. Wonderful advances have been made in these branches of dental art, many of the requisite operations being unsurpassed by any in general surgery in the neatness and skill which they require. Indeed, it is now rarely necessary to extract teeth which only a few years ago would have been infallibly condemned. But with regard to the treatment of this most practical portion of the subject we think that —well described as is every operation mentioned—it is not quite abreast with the advances made, for many ingenious operations and novel means of treatment are not even mentioned here. Still, while making these remarks, it must be conceded to Mr. Tomes that what he has described has been described in a way

which could not be excelled in clearness and perspicuity of style. The book is a most valuable contribution to physiology and surgery, and the author will not feel it otherwise than complimentary when we give the chief praise to that portion of his work which treats of that minute anatomy to the elucidation of which he has so successfully devoted himself, and with regard to which he has obtained a world-wide reputation. The only suggestion we could make would be that, at some future time, the book might be conveniently divided into two volumes, one on anatomy and physiology, the other on dental surgery, which latter subject, as has been remarked before, might have been much more fully entered into; but, as it is, there is no work we are acquainted with which, taken as a whole, can be compared with Mr. Tomes's well-written and instructive manual.

IV.—Cooper's Dictionary of Practical Surgery.[1]

Cooper's great work has found a worthy editor in the veteran surgeon Mr. Samuel Lane. Though the value of the book has been considerably diminished by the long interval which has elapsed between the appearance of the first and second volume, this has probably been unavoidable, from both the great labour involved and the difficulty of keeping a team of *collaborateurs* in order, but it will prove our excuse for devoting more attention to the second than the first volume, the contents of which are, of necessity, somewhat behind the day.

The last edition of the 'Dictionary,' edited by Mr. Cooper, contained 1500 closely printed pages in one volume. The present edition consists, as we have said, of two volumes, each containing over 1000 pages, of the same size and type as before. Antiquated material has been excised, so as to leave some 700 pages for new matter, which has been apportioned as follows:—249 have been allotted to articles on new subjects, 280 to re-written articles, and the remainder to emendations of existing articles, these being conveniently placed within brackets, so that the reader can easily ascertain whether the author or the editor is writing. The names of the contributors include many of the leading surgeons and writers of the day, and

[1] *Cooper's Dictionary of Practical Surgery and Encyclopædia of Surgical Science.* New edition, brought down to the present time. By Samuel A. Lane, Consulting Surgeon to St. Mary's and to the Lock Hospitals; formerly Lecturer on Surgery at St. Mary's Hospital; Member of the Court of Examiners of the Royal College of Surgeons. Assisted by various eminent surgeons. In two Volumes, Vol. I, 1861; Vol. II, 1872. London.

since it would be impossible in one article to do justice to all their contributions, we propose to select one article by each as a specimen of the kind of work to be found in this veritable Encyclopædia of Surgery.

The article ANÆSTHESIA, by the late Dr. John Snow, which did not see the light until after the author's death, gives an excellent account of anæsthetics up to the time at which it was written, and the writer was able to speak with the authority of great experience respecting the administration of ether, chloroform, and amylene. Whilst giving a general preference to chloroform, Dr. Snow allowed, candidly enough, that ether possessed one important advantage in its greater safety, but he pointed out its greater difficulty of administration, owing to the lowering of temperature due to evaporation—a point which may be well borne in mind during the present, probably temporary, reaction in favour of this agent. The great point in Dr. Snow's article appears to us to be the way in which he insists that if a patient is in a condition to be submitted to an operation at all, he ought to have the advantage of anæsthesia, since the pain he would otherwise suffer would depress the system more than the inhalation. This is a point too often lost sight of.

Mr. Erichsen is well known to have paid great attention to the subject of ANEURISM, the article on which is edited by him. As a bibliographical record of the various views held at different times, Mr. Cooper's original article is unrivalled in English surgical literature, and Mr. Erichsen has made such additions on the subject of treatment by pressure, &c., as were needed. The article is now, however, of course, somewhat antiquated, and should be supplemented by the perusal of Mr. Erichsen's chapters on the same subject in his 'Science and Art of Surgery,' or Mr. Holmes' article in his 'System.' The impetus given to the treatment of intra-thoracic aneurisms by the distal ligature by Mr. Heath's successful case of double ligature, the treatment by genuflexion, the improved methods of applying galvanism, and last, but not least, the revival of the catgut ligature by Mr. Lister, have all occurred since the date of this essay. It is very instructive to read how we work in a circle, and how many modern improvements are but revivals. Take, for example, Mr. Porter's suggestion to apply a temporary metallic compressor to an exposed artery, and we have but the revival of Assalini's forceps and other metallic instruments, of which, as Scarpa remarks, "they are liable to all the inconveniences which are inseparable from the presence of hard bodies introduced and kept for several days in the bottom of a wound, especially when this is recent, in which case they cannot be retained in a proper direction without difficulty, or exactly at such a depth as will not be

attended with hurtful pressure upon the wound itself and important parts in the vicinity." The catgut ligature, again, was used by Sir Astley Cooper with perfect success in one case, without the addition of the carbolic acid now so much relied on to perfect the cure, but it failed in other cases, both in Sir Astley's and Sir P. Crompton's practices; and in the last volume of the 'St. Bartholomew's Hospital Reports' is recorded a case of fatal secondary hæmorrhage following ligature of the femoral with catgut, followed by antiseptic dressing.

The article Cancer has been considerably expanded by Dr. Handfield Jones, who has given the views of Paget, Walshe, and Rokitansky, as generally held at the date of writing, but, necessarily, does not refer to the growing belief in the local origin of cancer, as maintained by De Morgan and Arnott. The question of the prolongation of life by operation does not receive any extended notice in the article in question, and it may be well, therefore, to remind our readers of Mr. Sibley's and Mr. Morrant Baker's recent papers in the 'Medico-Chirurgical Transactions,' in which statistics are brought forward, which confirm Paget's general statement that "the average duration of life in unoperated cases is something more than two years, in operated cases is something more than twenty-eight months." Mr. Sibley, whose statistics are drawn from the records of the Middlesex Hospital, says, "in the cases of cancer of the breast, those who have been operated on lived fifty-three months, whilst those in whom the disease was allowed to run its natural course lived only thirty-two months." Mr. Baker, whose data were drawn from Sir James Paget's practice, says, "the average length of life in scirrhous cancer is forty-three months when the primary disease is not removed, and fifty-five months when the operation is performed; whilst in the case of medullary cancer the results are even more striking, being twenty months without, and forty-four months, or more than twice the time, with an operation."

Caries has received some small additions at the hands of Mr. Spencer Smith, who has, however, omitted the long details formerly given of the methods of removing carious bone. In fact, Mr. Smith deprecates operative interference altogether, and though we should not go to this length, we agree with him in thinking surgeons are too apt to look to the local mischief alone, and to ignore constitutional causes. Many cases of caries of internal parts would doubtless heal if they were not constantly irritated by probing, and were gently stimulated by the injection of some simple lotion.

Cataract, by Mr. White Cooper, occupies five-and-twenty pages, and might have been said to be quite out of date were it

not that ophthalmic surgery has come round in fifteen years again to some of its ancient practices. The old operation of extraction by the large corneal flap is the one form of extraction here described, and it will be remembered that this gave way to Von Gräfe's method of a limited corneal incision, followed by an iridectomy, and to the spooning out of the lens by Schüft or Waldau. Then it was found possible to extrude the lens without the spoon, and iridectomy was abandoned by some ophthalmic surgeons, who returned either to the large section or to Liebreich's incision through the sclerotic, a structure hitherto totally proscribed. In congenital cataract the introduction of the operation of "linear extraction" has been of great service by freeing the eye, at an early period, of irritating particles of lenticular matter and diminishing the number of needle operations, and thus Tyrrell's great feat of operating upon children has been perfected.

The two articles Dislocations and Fractures have been edited by Mr. James Lane, who has devoted to them and to his other contributions an amount of labour quite remarkable. The old and almost universally received views of Sir Astley Cooper on these subjects have of late received some rude shocks, more particularly in regard to the action of the muscles, in both classes of affections. Thus, the late Mr. Syme was strong in maintaining the non-necessity for extension in cases of fracture of the thigh, though he did not go to the extent of Jobert de Lamballe in doing without apparatus altogether; and Bigelow has done much to disabuse surgeons of the idea that muscular contraction is the great impediment to the reduction of dislocations, by showing that it is sometimes necessary to tear the hole in the capsule of the hip-joint larger, before reduction of the head of the femur can be effected.

The treatment of dislocations by manipulation rather than by forcible extension is yearly making progress, and will soon as completely supersede the old plan with the pulleys as modern anæsthetics have the bloodletting and nauseating doses of tartar emetic of bygone ages.

Hamilton's standard work on fractures and dislocations has appeared since one, at least, of Mr. Lane's articles written, and so also the work of Bigelow on the hip, to which we have referred above, and there is a lack, therefore, of reference to American views which we find in more recent authors.

The classical works of Malgaigne and R. W. Smith, of Dublin, have, however, been largely referred to in both articles, and Mr. Lane has gone carefully through each of the numerous forms of fracture and dislocation described by Cooper, and has added such illustrative cases and new methods as to increase

materially the value of the articles, which each occupy nearly one hundred pages.

The article on Diseases of the Ear has received a few additions and emendations from the pen of the late Mr. G. Pilcher, but it cannot be said to represent either the most modern views or practice in this department of surgery.

The experience of the Crimean war has been fully brought to bear by Mr. Blenkins on the old Peninsular article on Gunshot Wounds, and full reference is made to the various professional writers of that period, and also to the manuscript Jacksonian Essay on the subject, by the late Alfred Poland. The abolition of venesection, the employment of anæsthetics, and the preference for primary over secondary amputations, which were established among army surgeons in 1854—56, have been fully borne out by the experiences of the great American war and the more recent Continental struggles. The records of the American civil war, which are the most elaborate which have ever been published, have, we believe, settled in the affirmative another vexed question, the propriety of closing all chest wounds at once, even if it should subsequently become necessary to open the wound or perform paracentesis for collections of fluid. The Crimean preference for excision over amputation in the case of the upper extremity has likewise been maintained everywhere, but the most modern experience in the Franco-Prussian war has condemned excision of the knee for gunshot injuries, and has not given a more favorable result as regards excision of the head of the femur than was experienced in the Crimean or American wars, when the deaths averaged 90 per cent.

Hæmorrhagic Diathesis is one of a series of articles on the blood, contributed to the present edition of the 'Dictionary,' by the late Mr. Henry Ancell, of which the editor makes prominent mention in his preface to the first volume. We have selected the article on Hæmorrhagic Diathesis as a specimen of the author, because it is obviously more suited to a surgical dictionary than many others of the series, which embraces such subjects as *Cholæmia, Hydræmia, Hyperinosis, Hypinosis, Heterochymensis,* &c., but we cannot express a high opinion of his labours. The article on Hæmorrhagic Diathesis is founded principally upon a paper by Mr. Lane, published in 1840, and evinces little research or personal experience. The lack of these latter qualities is evident also in the other articles from the same pen, Mr. Ancell having produced a series of extracts from the works of others rather than a well-digested *résumé* of the matter in hand, supported by his own investigations.

Injuries of the Head have been entrusted to Mr. John Adams, who has brought the experience of many years, while

surgeon to the London Hospital, to bear upon this interesting subject. In a chapter on Injuries of the Scalp, which he has added to Cooper's article, Mr. Adams recommends the use of sutures in scalp wounds, and denies that their presence tends to produce erysipelas. His remarks, too, on the general treatment of these cases, and especially the avoidance of exposure to cold winds, are most judicious, and the lessons of long experience. Mr. Adams agrees with Mr. Hewett in believing that Pott exaggerated the frequency and pathological importance of his well-known "puffy tumour" as indicative of suppuration on the dura mater, for it often happens that the scalp has sloughed before matter has formed beneath the calvaria.

Mr. Adams agrees with most modern surgeons in believing that fracture by *contre-coup* is extremely rare, if, indeed, it ever occurs; and the cases of fracture of the base of the skull resulting from falls on the vertex, which were formerly ascribed to *contre-coup*, are clearly the results of the direct force applied to the base of the skull by the trunk through the vertebral column, and have nothing in common with the laceration of the brain from indirect violence, which undoubtedly occurs, and, as well pointed out by Hilton, especially at those points where the brain is not furnished with the water-cushion formed by the subarachnoid fluid. The fluid which escapes from the ear or into the pharynx, in cases of fracture of the temporal bone, is undoubtedly, in the majority of cases, cerebro-spinal fluid, though a case has been recorded by Holmes, in the 'Pathological Society's Transactions,' in which the fluid was solely from the internal ear. In the treatment of these cases there has been a reaction from over-active treatment to doing nothing, and we are glad, therefore, to note that Mr. Adams recommends the administration of calomel, so as to affect the system as a prophylactic against the meningitis which comes on but too surely in the majority of these cases if left untreated.

The remarks on concussion of the brain are judicious and to the point, though we should be ourselves inclined to lay more stress than is done on the after-care required by patients who have suffered from concussion of either brain or spinal cord. There are unquestionably remote changes in the nervous centres which can be best avoided by the greatest quietude and absolute rest, and the general prescription to go to the sea-side for a week or two to get strong is much too vague, and not seldom leads to disastrous consequences. As a symptom of compression of the brain, Mr. Adams rightly lays great stress upon paralysis as being most constant. He recommends operative interference when it is present, and gives cases supporting the practice of trephining when no depression of the skull exists, for the evacua-

tion of blood or matter. To puncture the brain itself for the evacuation of matter is, doubtless, a bold practice, but one which offers the sole chance of saving a patient's life in some circumstances, and a recent case in St. Bartholomew's Hospital would show it not to be so dangerous as supposed. With the present comparative rarity of trephining the occurrence of hernia cerebri has become almost unknown; but Mr. Adams believes it to be generally connected with an abscess and some disintegration of brain-substance, and recommends shaving off the protruding mass, and the subsequent application of pressure, when, if recovery ensues, he believes an increased quantity of fluid in the ventricle restores the equilibrium of pressure.

Mr. Acton's article on Impotence is an epitome of the views put forth at greater length in his well-known work on the reproductive organs. His remarks on the moral and medical treatment of the various forms of impotence are worthy of all attention, and on the question of marriage he makes the following suggestive observations:

"The writer's experience is that, as a general rule, there is little need to dissuade those from marrying who ought not to do so. Our task is rather in the other direction—to encourage those nervous hypochondriacal people who labour under the delusion that they are unfit to undertake the rational duties of husbands and fathers."

The elaborate article on Inflammation by Dr. Druitt is one of the most valuable *résumés* of the entire subject we are acquainted with. In the space of 100 pages Dr. Druitt has been able to display a very profound acquaintance with the literature of his subject, and to discuss all the theories from time to time held by men of authority, while towards the conclusion he has examined the several modes of treatment proposed, giving his own views for and against them. The first section is devoted to a definition of inflammation, which presented great difficulties, and to the question whether inflammation should be regarded as a disease, which Dr. Druitt answers in the affirmative. An analysis of the local and constitutional symptoms of inflammation follows next, and then, in Section 4, we have a general account of the progress and results of inflammation. To this succeeds an exhaustive description of the minute anatomy of inflamed parts, taking the several tissues and membranes in order. The causes of inflammation are discussed at length, which their variety fully justifies, and this section leads naturally to the next equally lengthy one, in which the varieties of inflammation are considered, and a classification is attempted. Lastly, we come to the most interesting portion of the essay, viz. a history of the various theories of inflammation, with the treatment adopted from early days down to the present

time. For this purpose Dr. Druitt has made a rough division of the surgical doctrines of inflammation into seven classes:

"The first is that of ancient surgery, more particularly as represented by Galen, whose doctrines, classification, and very words were copied by succeeding writers, and only became obsolete in the time of Cullen and Hunter. The second began with the beginning of modern science in the seventeenth century, and adopting the vitalistic or pneumo-pathological basis, continued down, through Cullen and Hunter, to Cooper, Abernethy, and the great surgeons of the last generation. A third may comprise the early microscopic school, by which the capillaries were chiefly studied. A fourth coincides with the development of the cell-doctrine. A fifth includes the later ideas of the activity and power of protoplasm or 'germinal matter,' independently of the formation or function of 'cells,' formally so called. A sixth will treat of chemical theories. A seventh of the now popular neuro-pathology."

In the section on treatment Dr. Druitt has discussed all the various systems of treatment with fairness and impartiality, and the whole is well worth perusal. Beginning with preventive treatment, including rest, narcotics, and the antiseptic treatment of Lister, he passes on to expectant treatment, which is summarily dismissed in favour of curative treatment. The remedies for inflammation are subdivided into those which (1) diminish the quantity or the velocity of the blood or its heat; (2) purify it by acting on the excretory organs; (3) allay nervous irritation; (4) support the strength; (5) act specifically in a way we cannot explain; (6) counter-irritants and neurotics. Space will not allow of our following the author through these most interesting pages, which form the conclusion of his able article; but we will venture to place before our readers his own summary in the form of twenty-four general conclusions, which are as follows:

"1. Inflammation cannot be defined as to its essence, so that the best plan is to define it by its accidents as 'a diseased process attended with hyperæmia and exudation,' as we have said above, or with pain, heat, redness, and swelling.

"2. There are certain natural processes which resemble it in some respects, as erection, ovulation, menstruation, lactation, salivation, and the action of the intestinal mucous membrane during digestion.

"3. There are certain morbid processes into which inflammation passes by insensible gradations—to wit, neuralgia, hypertrophy, tumours, benign and malignant, mucous flux, and hyperæmia.

"4. But as neuralgia, hypertrophy, tumours, fluxes, and hyperæmia may (like the best examples of repair) occur without inflammatory symptoms, so it is clear that the term 'inflammatory' applies

to the manner in which these changes take place, that is, if they take place in a rapid, violent, and painful manner (W. Moxon, 'Analytical Pathology,' 'Med. Times and Gaz.,' 1870, vol. ii, p. 441). The diagnosis of inflammation is chiefly founded on hyperæmia, heat, exudation, and progressive change.

"5. The superaddition of inflammatory characters to morbid processes brings into play a new order of phenomena, requiring special treatment.

"6. The essential seats of inflammation are tissues in their minute structure. The vessels, nerves, and lymphatics are instruments, but not essential agents. Yet some inflammations are diseases *in* a part rather than *of* it, beginning with capillary embolism or afflux of morbid blood-elements (Beale), or with the intrusion of morbid leucocytes.

"7. Theories of inflammation must be comprehensive, not exclusive. All theories heretofore in vogue have some partial truth, but no one can be accepted as a view of the whole truth. The doctrine of the state of the blood, of humours, of the influence of the nervous system, of the action of vessels, the action of 'cells,' of exudations, of embolism, and of leucocytes, are not incompatible, but may be held as so many parts of a harmonious system.

"8. Inflammation of any part must be considered an expression of irritation or wronged vitality, of defective resistance to causes disturbing the processes of nutrition, the liability increasing in proportion to the weakness and sensibility. Injury to living tissue 'renders it incapable,' as Goodfellow has well expressed it, 'of exercising its proper affinities'—of growing in harmony with the organism of which it forms a part. The same rule holds good, as Virchow has shown, with regard to vegetals as to animals.

"9. It is the characteristic of high health and vigorous life that injuries are healed without nerve-irritation and inflammation, and that morbid states of the blood, from improper food, cold, or the like, are got rid of by the normal processes of oxydation and excretion.

"10. It is under conditions of weak health and lowered vitality that fevers and inflammations are most likely to occur.

"11. There is nothing benevolent nor conservative in fever and inflammation. They are grievous wastes of force and substance, and imply a prodigal production of the lowest amœboid forms of organization.

"12. It is a pernicious doctrine to hold that 'Nature, feeling herself injured, sets up inflammation to restore the damage or recover the health.' If there be damage, it is the duty of the practitioner to remove causes of irritation and to quiet the nerves, till natural processes of oxydation and elimination shall have got rid of any material cause, and till an injured or fatigued organ can recover its nutrition.

"13. The best result of inflammation is that it sometimes produces a new organ of oxydation or elimination, as in gout, mucous flux, and critical abscess. But as these may risk life and health, it

is the surgeon's duty to render them needless by the means just mentioned.

"14. Of the causes of inflammation some reach the part from without, as wounds; others through the blood. In the latter case the blood is sometimes the mere vehicle, as of cantharidine to the kidneys. But the real blood diseases are those produced by some modification of that fluid, whether spontaneous or resulting from chemical substances acting as ferments. (6 supra.)

"15. This last-mentioned class of cases constitute the 'fevers' of which local inflammations are products and symptoms. In a true 'local' and traumatic inflammation the feverishness is secondary and symptomatic.

"16. But in all great idiopathic inflammations, and in the cases of injuries in which septicæmia has occurred, the local symptoms are as secondary as are the lesions in typhoid or scarlatina.

"17. For the great idiopathic inflammations, just as for acute rheumatism, we ought to restore the term 'fever.' Just as we speak of rheumatic fever, so we ought to speak of gouty, erysipelatous, pneumonic, pleuritic, and peritoneal fevers. So the greatest physicians always did—Hippocrates, Sydenham, Hoffmann, Hexham, and Fordyce. To speak of erysipelas, gout, or pneumonia, as *local* inflammations is absurd. Pathology in this instance must *recueillir pour mieux sauter*.

"18. Just so inflammation, tumours, malignant and other, hypertrophy, 'tubercle,' elephantiasis arabum, and the like, ought to be taken as members of one series.

"19. We cannot attempt to give, even in the most general form, the treatment adapted to each kind of inflammation. For, in truth, that of every case must be adapted to the cause, the degree, the constitutional peculiarity, the organ or tissue involved, and the stage. Remedies that would be useful in an early stage may be useless or pernicious afterwards.

"20. It is preventive treatment which should be the surgeon's great study. In all cases of injury or threatened inflammation, by rest; if there be a wound, by providing against putrefaction and germs: in many cases after exhaustion and exposure, by a moderate use of stimulants, with a warm bath and nutritious food. In cases of wound or parturition, the first impressions on the nervous system should be neutralised by opiates. In cases where excess of food and a gorged condition of the alimentary canal exist, an incipient case may often be cut short by purgatives. In malarious cases, quinine; in gout, colchicum seems to have the power of suppressing that nervous condition which permits of local mischief; and it is for 'specifics' of this kind, as preventives, that the practical surgeon should pray.

"21. If an inflammation be already lit up, a different order of remedies may be requisite, just as fire-engines must be had to check a conflagration the first spark of which might have been put out by a housemaid's mop. Bleeding should be practised if the condition of the *patient* seem to render it expedient. If not, a labouring

organ may be freely reached. Purgative, saline, and opiate remedies, and wine, may be added according to the needs of each case.

"22. In the fully-established inflammation, when the effusion or other result has taken place, the surgeon will think of giving vent to discharge, of supporting the strength, and repairing the mischief which he has not been able to prevent. There is nothing unreasonable in believing that a bath and brandy-and-water may prevent an attack, which may require bleeding if fully developed, and wine at its close.

"23. In the reparation of injuries, and in diseases whose course is known and regular, the treatment must be chiefly expectant.

"24. In every instance the surgeon at the bedside will do well to divest himself of all abstract and metaphysical notions, and to see before him, not a 'case,' but a brother, a being of flesh and blood, whose body and soul require to be dealt with by moral and material agents, according to the needs of each."

Mr. Maunder has rewritten the article on Intestinal Obstruction, and given the researches of Brinton, Gay, and others, together with some considerable experience of his own. He rightly insists upon the free administration of opium to allay peristalsis, and speaks guardedly of any operative measures. We find no mention of the injection of olive oil, which we have known act very successfully in some cases of obstruction, nor any reference to the use of belladonna to relieve local pain and stimulate the bowel to contract.

The articles Diseases of Joints and Excision of Joints have been carefully emendated by Mr. Thomas Wakley, who has made copious additions to them, consisting principally of extracts from the works of Brodie, Erichsen, Coulson, Barwell, and others. By this means, Cooper's originally somewhat scanty articles have been brought well up to the practice of the day, some subjects not originally treated of, *e.g.* hysterical affections of joints, being added. In the article on Excision Mr. Wakley has given an excellent *résumé* of the history of the operations on the several joints, and has supplied some valuable statistics as to the results obtained, having himself had considerable experience in this department of surgery.

Sir Duncan Gibb has contributed a short article on the Laryngoscope, an instrument the practical utility of which he did much to popularise in this country.

The Surgical Diseases of the Larynx are from the pen of Dr. Macleod, who has also undertaken the articles on the Pharynx and Trachea, Tonsils, &c., and to whose article on Wounds we shall have occasion to refer further on.

Lithotomy and Lithotrity are written by the veteran William Coulson, who has in these articles brought to bear a long experience and a matured and calm judgment, which

render the opinions expressed especially valuable. The article on Lithotrity is entirely new, for in the forty years which have elapsed since Cooper wrote the whole subject has been revolutionised by Civiale, Brodie, and their successors. Mr. Coulson is not a blind advocate for lithotrity in every case, but holds the scales very justly between the two operations, appreciating fairly the advantages and disadvantages of each. For large, hard stones he would prefer lithotomy, nor does he hesitate to recommend recourse to lithotomy after lithotrity has been commenced, when symptoms of great irritation arise and the patient's health is giving way under repeated operations. The lateral operation, with limited incision, is the form of lithotomy preferred by Mr. Coulson, who, in the operation of lithotrity, uses the instrument and follows the method of Civiale. The use of injections after the operation of lithotrity is recommended by Mr. Coulson, who thinks them of great advantage.

Mr. James Lane has contributed a short article on LITHOTOMY IN THE FEMALE, and has especially called attention to the valuable vesico-vaginal operation practised by Mr. Baker Brown, himself, and other surgeons, with the greatest success.

The article on DISEASES OF THE MAMMA was entrusted to the late Mr. Alexander Ure, who has made a few additions to it, and has supplied the sections on Cysts, True Hydatid Tumour (in contradistinction to the term employed by Sir A. Cooper for all forms of cyst), Chronic Mammary Tumour, &c.

NEURALGIA is hardly a subject for a surgical dictionary, but Cooper's original article has been edited and added to by Mr. Charles Hunter, who has for some years paid attention to the subject, especially in connection with the subcutaneous injection of remedies. Mr. Hunter has also contributed the article on NEUROMA.

The article on the OPHTHALMOSCOPE is one of a series of able papers on ophthalmic surgery by Mr. Bader. In drawing it up the author acknowledges his indebtedness to Zander's work on the subject, translated by Mr. R. B. Carter, and then sketches the various forms of instrument invented, their uses, and respective advantages. The article VISION, also by Mr. Bader, is well worth perusal by all practising surgeons, explaining, as it does, briefly and intelligibly, the modern views and researches on abnormalities of vision and their correction by appropriate glasses. Few men, not professed ophthalmologists, have the courage to face Donders' great work, translated for the New Sydenham Society, bristling, as it does, with mathematical formulæ; but they will find here, in a short compass, all that it concerns them to know of hypermetropia, myopia, astigmatism,

&c., with plain directions for choosing lenses to suit each variety of sight.

Ovariotomy could not have been placed in better hands than those of Mr. Spencer Wells, who, though at the time the article was written he could speak of no fewer than 200 cases, has, as is well known, recently brought the statistics of 500 cases of his own before the Royal Medical and Chirurgical Society. Mr. Wells traces the history of ovariotomy from Mr. Dowell's first case, in 1808, to the present time, and justly claims to have himself done much to bring the operation into repute. Mr. Wells has unquestionably had the largest experience of ovariotomy and the greatest amount of success of any living surgeon, and he would confer the greatest benefit on fellow-labourers in the same field if he would publish all the cases in which he has made exploratory incisions and has failed to complete ovariotomy, either from errors in diagnosis or other cause. In this article, and also in his recently published work, Mr. Wells lays down excellent rules for the diagnosis of ovarian tumours; but, as all practical ovariotomists know too well, all these rules are open to exceptions which render them nugatory, and the fact is, as was broadly laid down by the late Mr. Baker Brown (whose labours deserve every recognition), that it is impossible to be perfectly certain as to the nature and attachments of any abdominal tumour until the operator's hand is within the abdominal cavity. Mr. Wells gives, in this essay and in his practice, the preference to the clamp for the treatment of the pedicle. He appears to be a little prejudiced against other methods of treatment, which is, perhaps, natural; but we cannot see on what grounds he can claim more perfect and complete recoveries after the use of the clamp than after one or other of the intra-peritoneal methods of treating the pedicle. We have seen not a little trouble with the stump of the pedicle, both immediately and for months after the operation, whilst the recovery in numerous cases of "tied and dropped" pedicle has been rapid and complete.

Mr. Sercombe contributes a short paper on Fissures of the Palate, in which he details the operations of Fergusson and Pollock for the closure of split palate by operation, and enters more at length into his own special department, and the means for closing mechanically fissures in both the hard and soft palate. However successful surgeons may be in closing a fissure of the palate, it is undoubted that the improvement of the voice is often slight and unsatisfactory. This arises, we believe, from the shortness of the palate, by which the nasal and oral cavities are not properly separated during articulation, and if the dentist can overcome this difficulty by the adaptation of an artificial palate the result will probably be more satisfactory.

Pyæmia is contributed by Mr. Henry Lee, who has also written the article on "Diseases of the Veins." Mr. Lee takes a somewhat mechanical view of pyæmia, believing that a distinct *materies morbi* finds its way into the veins, where, if coagulation is not induced, it necessarily enters the systemic circulation, leading to decomposition and secondary deposits in various organs of the body. But even if coagulation does occur, there is no security for the patient, for the clot may break down or be mechanically carried on, producing the well-known effects of embolism. A good sketch of the morbid appearances found after death, and of the symptoms seen during life in a well-marked case of pyæmia, is given; and, in speaking of treatment, Mr. Lee proposes to arrest phlebitis which might be creeping towards the trunk and thus threatening to produce pyæmia, by applying two acupressure needles above the seat of mischief, and dividing the vein between, so as to produce adhesive inflammation at a healthy spot, and thus seal up the vessel.

Scurvy is a short article contributed by Mr. Harry Leach, who is well known to have devoted much attention to the subject. The utility of lemon juice and fresh vegetables in preventing and curing scurvy is vigorously supported by the author.

The original and learned article on Surgery by Cooper has had tacked on to it a very imperfect sketch of the "Progress of Surgery," by Mr. Ernest Hart, of which the concluding sentence will give the best idea:—"I will conclude by asking indulgence for this rapid summary, which the necessity for extreme condensation (amongst other reasons) has, in my hands, rendered even more imperfect than it might otherwise have been."

Diseases of the Skin are treated of by Mr. Erasmus Wilson, who devotes a large portion of the article to the enumeration of the varieties of skin disorders described by various authors. Thus no fewer than twenty-nine varieties of eczema are described! Dermatologists appear to be rather pedantic in their choice of terms, if one may judge from the following under the head of Erythema:

"The hyperæmia following a blow is termed erythema ab ictu, that occasioned by riding on horseback is erythema in equitantibus, and that which results from the pressure of the weight of the body in bed-ridden persons is the erythema ab decubitu, or erythema paratrimma; a scorch by the fire is erythema ab igne; and the hyperæmia following exposure to cold erythema ab gelu."

Mr. Wilson is a firm believer in the constitutional origin of local skin diseases, and, while not neglecting local remedies,

directs his attention especially to constitutional treatment. His long experience has shown him that arsenic is a most useful remedy, and that the drug sometimes requires to be pushed beyond the regulation doses if a cure is to be effected.

Mr. William Adams has contributed the articles Curvature of Spine and Talipes, in the latter of which is included an account of all the congenital and non-congenital distortions of the feet. Mr. Adams is well known as a leading supporter of what is commonly known as "orthopædic surgery," and these articles give a good *résumé* of that practice, which consists mainly in the use of mechanical apparatus and the division of contracted tendons. Whether a more physiological view of the nature and treatment of these affections might not be followed in some examples of deformity by better results than are now obtained, it is not for us now to discuss.

Mr. Gascoyen, in addition to several articles relating to diseases of the bones, has edited the article on Diseases of the Testicle. In this due reference is made to the researches of Lawrence and Syme on hernia testis, and the labours of Curling, Gross, and other writers on the subject. Many new and useful sections have been added to the original article, which now fully represents the knowledge of the day.

Tetanus has been rewritten by the late Mr. Poland, whose industry and research on any subject he took up are unrivalled. The article is, however, neither so complete nor so modern as the same author's essay on the subject, in Holmes's 'System of Surgery,' and is, in particular, wanting in reference to more recent researches as to pathological changes in the spinal cord.

Mr. George Lewis Cooper has edited the article, Diseases of the Tongue, to which he has added a good account of the modern operations for removal of the whole or part of that organ; and has also contributed a biography of his uncle, Samuel Cooper, which is placed at the beginning of the second volume of the Dictionary. From this we learn of Cooper's indomitable industry dating from early life, of his military experiences, his literary successes and temporary hospital failures, and, lastly, of his career at University College, where he probably never was estimated as highly as he deserved, from being brought into close contrast with the more brilliant Liston, and also from the fact that he clung to office longer than his age warranted. Cooper's 'First Lines of Surgery' and his 'Surgical Dictionary' are the best monuments of his fame.

Dr. Robert Barnes has contributed a short article on 'Transfusion, of which he speaks hopefully, believing that it will gradually come more and more into employment and esteem, and

has also edited the various articles relating to the uterus. The article Inversion of Uterus is rewritten, and is replete with references to valuable cases, but that on Prolapsus of Uterus is the original one of Cooper, with merely an addendum by Dr. Barnes, who remarks—"The article by Cooper is so excellent that I have feared to mar it by interpolation. I simply add such information as subsequent progress demands."

Tumour has been undertaken by Mr. John Birkett, who has contented himself with making some additions to Cooper's original article. Now, if there is one article in the whole work which of necessity demanded entire rewriting it is this, since the whole subject has undergone such alterations at the hands of Paget, Virchow, Billroth, Broca, and others, that to quote Abernethy, Carswell, and Warren, in the present day is hardly fair either to them or to the reader. Mr. Birkett quotes Broca's classification of tumours with approval, but does not attempt to follow it, and is content with inserting brief notices of the myeloid, recurrent fibroid, and myxomatous tumours. He subsequently gives another classification of tumours, presumably his own, which is "based upon their elementary composition, and has been arranged especially with regard to facilitating their diagnosis at the bedside;" and yet a third quoted from Billroth; but the article is unsatisfactory as a whole, from being simply a piece of patchwork.

Ulcers and Wounds (besides several articles earlier in the work) have been entrusted to Dr. Macleod, of Glasgow, who has made many excellent additions to both. The varieties of ulcers are well described, and good rules for their treatment are laid down. In the article on wounds the various modes of healing as laid down by Paget are given, and an excellent sketch of the history of surgery in the treatment of wounds is appended. Perhaps the most interesting portion is the sketch of modern practice in relation to wounds, which Dr. Macleod divides into—1. Methods of treatment which aim at excluding the air altogether. 2. Plans of disinfecting or preventing putrefaction. 3. The thorough drainage of wounds. 4. The use of irrigation. 5. The employment of certain gases. Speaking of Mr. Lister's antiseptic dressing, Dr. Macleod writes, "That very much can be done by the careful development of this system of practice, and that much more may be expected of it than has even yet been accomplished, no one who has read Mr. Lister's able papers can doubt." We should have been glad, however, to have had Dr. Macleod's own experience on the subject, and some reference to what he had personally witnessed in the wards of the Glasgow infirmary.

Sir Henry Thompson has revised the articles relating to the

URINARY ORGANS, including the PROSTATE, and he has so completely exhausted these subjects in his well-known treatises upon them, that it would be vain to look for any new information in the articles before us. They are, in fact, excellent epitomes of the author's larger works, and may be, therefore, recommended to those who have neither time nor opportunity to devote to literary labour. We are a little surprised to find the operation of tapping the bladder above the pubes in cases of retention spoken of as "easily performed and without danger," in cases in which the bladder can be felt above the pubes; nor do we agree with the dictum "no doubt this is the best situation in which to retain an instrument afterwards." We are not aware of any published cases proving tapping above the pubes to be superior to the operation *per rectum*, and have known disastrous results follow the operation in the most skilful hands.

A short article by the late Mr. Weedon Cooke on the surgical uses of preparations of ZINC concludes the work.

We have purposely left all reference to the labours of Mr. Samuel Lane, the editor of the 'Dictionary,' to the last, and have now to congratulate him on the satisfactory completion of his work. The book would, doubtless, have been of more uniform value had the two volumes appeared more nearly together, but only those who have had to drive a team of medical writers knows what the difficulties in the way of punctual performance of promises are. Mr. Lane's own labours must have been very great, since the revision of a multitude of small details has fallen on his shoulders. He has not, however, shrunk from undertaking several lengthy articles, all of which show evidence of a thorough acquaintance with the literature as well as the practice of surgery, and give proofs if any were required of Mr. Lane's fitness for the editorial office to which he was appointed. From among Mr. Lane's articles we select that on VENEREAL DISEASE for comment as being of general interest, and on a subject upon which that gentleman is especially entitled to speak with authority.

The earlier part of the article is a reproduction of Cooper's historical summary, together with a few interpolations by the editor. In this we have in detail the views both as regards pathology and treatment of Hunter, Carmichael, Wallace, the anti-mercurialists Rose and Fergusson, &c. This, though interesting, will, we fear, tend to confuse those who plunge into the article without any distinct ideas as to modern teaching; and for students, therefore, we should recommend the perusal of the latter half of the article first. This latter portion consists of an elaborate, candid, and fair statement of the views of modern writers on the nature and treatment of venereal disease.

Mr. Lane is an uncompromising "unicist," and has no belief in the existence of two poisons, the one producing the simple chancrous sore and the other the true infecting chancre.

"We of course admit," he says, "that the separation of venereal ulcers into two classes, the indurated or infecting, and the unindurated or non-infecting sore is within certain limits of great practical value. We admit freely that the indurated sore will, as a rule, be followed by constitutional infection, and that the unindurated sore will, as a rule, produce no such results. But both rules, the latter especially, have occasional exceptions, and we believe it to be impossible to predict *with certainty* of any given sore, however typical in appearance, that it will or will not be followed by secondary infection. There is no *certain* proof of the infecting nature of the sore, but the fact of infection itself."

Again, upon the important question of infection of the system by a soft sore, Mr. Lane says:

"We are certain that sores that have never shown induration at any period of their progress, and which we have ourselves carefully watched throughout, have, nevertheless, been followed at the usual period by a well-marked secondary eruption."

We need hardly say that Mr. Lane has no faith in the so-called "*chancre mixte*" which is put up to explain the undoubted occurrence of constitutional symptoms after a soft sore, and which he speaks of derisively as "this double-barrelled sore."

The section on modern treatment is full and satisfactory. Mr. Lane speaks in no uncertain tone of the use of mercury given with judgment, and thinks it of secondary importance by what means it is introduced into the system. He regards iodide of potassium as rather antagonistic to mercury, and therefore characterises the common practice of giving the two drugs in combination as "blowing hot and cold." Sections on syphilis affecting internal organs taken principally from Dr. Wilks' essay; on 'Mr. Hutchinson's views on inherited syphilis;' and on syphilisation, quoted principally from Mr. James Lane's and Mr. Gascoyen's paper in the 'Medico-Chirurgical Transactions,' are followed by a transcription of the greater part of the valuable report of the Government committee on venereal diseases published in 1867.

We have thus endeavoured to do justice, so far as the space at our disposal will permit, to the labours of the editor and staff of *collaborateurs* of the last edition of 'Cooper's Surgical Dictionary.' We recommend the work to the notice of all those students of surgery who are not content to accept the last new view as true because it is new, but desire to know something of the history of their art, and to form their opinions upon those of the leading thinkers of the past and present generation.

V.—Letheby on Food.[1]

THE work which we have before us, and which ought to have received earlier notice, is by the well-known medical officer of health of the City of London, and consists of four lectures delivered by him at the Society of Arts in 1869, in his capacity of Cantor Lecturer. It is characterised by the chemical knowledge and general ability of its author, and has been so well appreciated by the public as to have already reached a second edition.

In proceeding to describe the contents of this work we may remark that there are three qualities which run through it. It does not lay claim to originality of observation as to food and dietaries; it acknowledges fairly the source whence the extracted materials have been derived, and it possesses numerous tables, which render it a very useful work of reference. We may also add that it is written in a terse, clear, and attractive style, and is excellently well adapted to the intelligent auditory, to whom it was originally addressed.

The subjects discussed by the author are large and important, viz. 1st. Varieties of food—their chemical and nutritive values; 2nd. Comparative digestibility of foods, and the functions of different foods; 3rd. Construction of dietaries and preparation, and culinary treatment of food; and, 4th. Preserved unwholesome and adulterated foods. The works most largely laid under contribution are those of Dr. Edward Smith, and particularly his reports to the Privy Council, his lectures at the Society of Arts, and his papers in the Philosophical Transactions.

Taking the quantity of nitrogenous matter which Dr. Smith had found to be necessary to sustain life in the Lancashire operatives, viz. 1220 grains, he supplies a table which shows the very varied quantities of carbon which would be associated with it in different foods, viz. from 2723 grains in skim-milk cheese to 15,062 in bread, and 1,110,000 in beer or porter, and for greater convenience of comparison supplements it with another in which human milk is taken at 100. This method, however, yielded no satisfactory results, and it became necessary to select foods which by combination would yield a suitable proportion of nitrogenous and carbonaceous matter, and to take a standard quantity of nitrogen and carbon as a daily supply. By making an average of many observations recorded by Dr. Lyon Playfair and Dr. E. Smith he found that the daily requirement

[1] *On Food: its Varieties, Chemical Composition, Nutritive Value, Comparative Digestibility, Physiological Functions and Uses, Preparation, Culinary Treatment, Preservation, Adulteration, &c. Being the Substance of Four Cantor Lectures by* H. LETHEBY, M.B., M.A., Ph.D, &c.

of different classes of persons was 3888 grains of carbon and 181 grains of nitrogen, but he adopted Dr. Smith's estimate for the Lancashire operatives, viz. for an adult woman 3900 grams of carbon and 180 grains of nitrogen, and for an adult man 4300 of carbon and 200 grains of nitrogen, which Dr. Smith had shown were contained in about 2 lbs. and 2 lbs. 4 oz. of bread.

He then cites a table by Dr. Smith which shows the quantity of carbon and nitrogen in 1 lb. of different foods, with the cost of each, and the quantity to be obtained for one penny, and, consequently, the cost of the weekly supply, on the assumption that one food alone could be selected; but as that is manifestly an erroneous assumption, a very valuable table is compiled which gives the proximate and the ultimate elements in nearly all ordinary foods, with the proportion of the carbon to the nitrogen of each, from which dietary tables may be readily constructed.

We shall now proceed to consider his observations on the most important vegetable and animal foods.

In reference to wheat the author adverts to the greater quantity of gluten which is contained in the produce of hot climates and seasons, and shows the economic value of brown bread and the different layers of the bran. The gluten ranges from 8 to 15 per cent. in the whole wheat, and the nitrogen from 1·70 per cent. in fine flour to 2·58 per cent. in coarse sharps, whilst the mineral matter, including the phosphates, varies in yet greater proportion, for it is only 0·71 per cent. in fine flour, against 7·0 per cent. in bran. Notwithstanding the chemical advantages of bran over fine flour, he points out the greater nutritive value of the latter, and asserts that "seconds flour is the best for domestic use." The important property of cerealine, which M. Mège Mouries has shown to exist in bran, may, however, be obtained without mixing the bran with flour, "by treating bran with warm water, and then using the water in the manufacture of bread." He states that a sack of flour should yield 95 four-pound loaves, but by the art of the baker, and the addition of alum and "a gummy mixture of boiled rice," the quantity may be increased to 100 loaves.

In reference to barley meal he makes a statement which is startling, and needs correction, viz. that "it is employed by about 90 per cent. of the out-door labouring populations of England," but it is very doubtful if 9 per cent. of that class ever eat barley bread, whether in whole or in part. He makes a quotation from McCullock shewing that in the time of Charles I (1626)—

"It was the usual food of the ordinary sort of people, and that as late as the middle of the last century hardly any wheat was used in the northern counties. In Cumberland the principal families used

only a small quantity of wheaten bread about Christmas time. The crust of the everlasting goose-pie which adorned the table of every country family was invariably made of barley meal."

No doubt important changes have occurred in the direction indicated, and wheaten flour has replaced barley-meal in pie crusts, but, unfortunately, the goose-pie has itself disappeared from our tables.

Rye was at one time as generally eaten by the northern nations of Europe as barley, and still retains its position in some of them, but it is rarely eaten in England. Oatmeal has, however, remained as the food of Britons to a greater degree than either barley or rye, and although "its taste is peculiar, being first sweet, then rough and bitter;" "the grain is very rich in gluten and fat," and is a food of far higher value. A preparation of the latter, with which we are too well acquainted, in certain states of ill health, viz. water-gruel, was offered as a luxury to Londoners by advertisement in the 'London Gazette' for Friday, Aug. 13, 1695, to such as chose to purchase it between the hours of six and eleven, a.m., at the Marine Coffee-house, Birchin Lane.

Maize, or Indian corn, is stated to be "rich in nitrogenous matter and fat," but does not make good bread. It is, however, baked into cakes or made into puddings, and when "deprived of its gluten and harsh flavour by means of a weak solution of caustic soda, and then dried, it forms the expensive food called oswego, maizena, or corn flour, which is now largely used for puddings." Dr. Letheby regards it as "almost if not altogether the cheapest food for the poor."

The proportion of gluten in rice is "only about 6·3 per cent., and it rarely exceeds 7·," and "is, in fact, one of the least nitrogenous of all the cereals," yet it "is the principal food of eastern and southern nations," and "gives nourishment to not less than one hundred millions of persons." Such a fact cannot be gainsayed, yet it is doubtful whether rice could become the staple food of the people of this country, even if it could be obtained in sufficient quantity, and it is quite certain that in nutritive and economic value it cannot compete with wheaten flour. Millet is said by the author to be little better than rice, for it contains but about 9 per cent. of nitrogenous matter, 74 of starch and sugar, 2·6 of fat, and 2·3 of minerals, yet it is eaten over a very large part of the earth's surface, and maintains life.

The leguminous foods, as pulses, peas and beans, and the dahls and grams of India are shown to be so rich in nitrogenous matter that the proportion to carbonaceous is as 1 to 2·7, whilst in wheat it is as 1 to 7, and in rice 1 to 12·9; but

they are not equally well digested, and M. Sorbiere remarked, in 1698, "that peas be too windy for supper meat."

In reference to the potato, the author quotes the statement of Dr. Edward Smith in reference to the Irish peasants, at the time of his inquiry, viz. "when the season arrives and the potatoes are plentiful, as much as 3½ lbs. are consumed, three times a day, by an adult. This, indeed, is the regular allowance of an Irishman, who finds no difficulty in consuming his ration of 10½ lbs. of potatoes daily." Dr. Letheby remarks that "they go well with meat and fish, and are considerably helped with a little dripping or butter, but the great adjunct is milk." The latter fact is, however, due less to the fitness of the combination than to the circumstances of the Irish labourer, who, as Dr. Smith observed, cannot get meat or fish, or dripping, but has an allowance in a farmhouse of an Irish pint of butter-milk three times a day. If this food could be obtained at ½d. per lb., he regards it as the most economical food, but it cannot, and at double that price potatoes must be a very dear food as compared with wheaten flour.

Succulent vegetables and fruits are consumed for their medical rather than dietetic qualities, and he commends the use of sea-weeds, stating that "these results place sea-weeds among the most nutritious of vegetable substances; in fact, they are richer in nitrogenous matter than oatmeal or Indian corn."

The proportion in which animal food is eaten by the labouring classes of England is shown by the report of Dr. Edward Smith to the Privy Council, and quoted by the author under the different kinds of food.

In reference to milk, it appears that 76 per cent. used new milk, 83 per cent. butter-milk, and 53 per cent. skim milk; but the quantity consumed varies from about one pint per head daily in Scotland and Ireland to less than half a pint weekly in London. The proportion of nitrogenous to carbonaceous matter in cow's milk, when taken in the solids (which are 14 per cent.), is as 1 to 2·2. Cream contains 34 per cent. of solid matter, of which 26·7 per cent. is butter.

In reference to cheese, he remarks that a mixture of cream and new milk is used in the manufacture of Double Gloucester and Stilton, whilst milk with one eighth to one tenth of its cream removed "produces the quality of cheese which is most sought after as Single Gloucester, Chester, American, &c."

M. Husson showed, from the 'Octroi Returns,' that the quantity of meat consumed in Paris per head was 7 oz. a day, whilst Dr. Wynter computed it to be only 4½ oz. in London. Dr. E. Smith's report gives only 14·8 oz. per week for the in-door low-fed operatives in London, which is a much larger quantity than

that obtained by the out-door ill-fed labourers' families in South Wales and Ireland. It appears, however, that a larger proportion of persons obtain it in England than in other parts of the United Kingdom and Ireland, the proportion being 70 per cent. in England, 60 per cent. in Scotland, 30 per cent. in Wales, and 20 per cent. in Ireland. The small quantity of food which is obtained by the working peasantry of this country might lead us to consider whether a reduction could not be made in the dietary of the better-fed classes during this period of scarcity of meat, for although a so-called poor dietary may not in itself be desirable, there can be no doubt that the agricultural labourer compares favorably with other classes in both longevity and health, while the proportion of nitrogen in his food is much below that of the richer classes.

Our space will not allow us to refer further to this subject or to enter more at large into Dr. Letheby's work, but we may add that there are many interesting particulars on other kinds of animal foods.

When discussing the digestibility of foods the author quotes the well-known results of Dr. Beaumont, and gives tables of animal and vegetable foods, which may be advantageously consulted. They show a fallacy which too commonly prevails among educated people, viz. the belief that vegetable food is more readily digested than animal food. Thus, whilst a piece of cabbage required 4 hours, and a potato and wheaten bread 3½ hours, for digestion, beef was digested in 2¾ hours. There are, however, some anomalies in the tables which cannot be easily explained, for whilst beef required 2¾ and mutton 3 hours, venison steak was digested in 1½ hour. Such a disparity in substances which are identical in physical and chemical properties seems to indicate an error of record or observation. It is, however, interesting to note how much more readily raw egg—that is, uncoagulated albumen—is digested than cooked egg, for whilst whipped egg was digested in 1½ hour, roasted egg required 2¼, and fried egg 3½ hours. This is of great interest in reference to the over-cooking of meat, for not only may such meat be rendered hard and tough by the highly coagulated albumen, but rendered far more difficult of digestion, and require a period for the process beyond that during which the food could remain in the stomach.

The author enters at some length into a very interesting part of his subject, viz. the functions of food as a source of muscular power, and shows in how great a degree the generalisations of Liebig have been shaken by the researches of Dr. Edward Smith in 1857 and 1861, Traube in 1861, Haidenham in 1864, and Fisk and Wislicenus in 1866.

Dr. Smith, in his paper in the 'Philsophical Trans.,' 1859, showed that the carbonic acid evolved in respiration was in direct proportion to muscular exertion, and that the test was so exact that it was possible to measure the effect of the combined movement of the fingers only. At a later period, and by researches amounting to thousands, he corroborated the experiments made at the same period by Bischoff and Voit, which showed that the excretion of nitrogen bore no relation whatever to the amount of muscular exertion, so that a man might work the treadmill form any days, and the effect would be so small as an average increase of 19 grains per day—an increase due probably to unexplained causes—whilst there was a close relation between the nitrogenous food eaten and the nitrogen eliminated.

Traube asserted that "all muscular force was derived from the oxidation of fat and hydrocarbons, and nerve force the oxidation of tissue." The result of the experiments of Fisk and Wislicenus whilst ascending the Faulhorn was such that "the work actually performed exceeded the energy of the oxidized muscle by more than as much again," although this included the oxidizable non-nitrogenous as well as the nitrogenous materials.

The contrast of these later observations with Liebig's earlier speculations is worthy of present consideration. The author quotes the remarks of Liebig:

"We see as an immediate effect of the manifestation of mechanical force that a part of the muscular substance loses its vital properties—its character of life—that this portion separates from the living part, and loses its capacity for growth and its power of resistance. We find that this change of properties is accompanied by the entrance of a foreign body (oxygen) into the composition of the muscular fibre; and all experience proves that this conversion of living muscular fibre into compounds destitute of vitality is accelerated or retarded, according to the amount of force employed to produce motion. Nay, it may safely be affirmed that they are mutually proportionate—that a rapid transformation of muscular fibre, or, as it may be called, a rapid change of matter, determines a greater amount of mechanical force, and, conversely, that a greater amount of mechanical motion (of mechanical force expended in motion) determines a more rapid change of matter." "The amount of azotized food necessary to restore the equilibrium between waste and supply is directly proportional to the amount of tissue metamorphosed." "The amount of living matter which in the body loses the conditions of life is in equal temperatures directly proportional to the mechanical effects produced in a given time." "The amount of tissue metamorphosed in a given time may be measured by the quantity of nitrogen in the urine." "The sum of the mechanical effects pro-

duced in two individuals in the same temperature is proportional to the amount of nitrogen in their urine, whether the mechanical force has been employed in voluntary or involuntary motions, whether it has been consumed by the limbs or by the heart and other viscera."

The author quotes the interesting observations of Professors Haughton and Frankland on the mechanical power of the internal organs, and of muscles when consumed; and these, added to the direct observations of the enquirers above mentioned, prove conclusively that this speculation was not based upon observation, and that it lacked the first evidence of sagacity or forethought, viz. truth. But however disastrous the result it may be to the reputation of Liebig, the assertion has had the merit of inducing others to perform experiments on man and animals of almost unprecedented duration, and to add greatly to the sum of physiological knowledge. Let it be well understood—

1st. That instead of the assertion of Liebig, that "the amount of tissue metamorphosed in a given time may be measured by the quantity of nitrogen in the urine," there is no connexion whatever between them.

2nd. That the elimination of carbonic acid is the measure of muscular exertion.

3rd. That the relation with excreted nitrogen is the ingested nitrogen.

On this ground the consumption of fat, and of the hydro-carbons generally, assumes its proper position, and the fact of the great consumption of hydro-carbons by workmen who make exertion is rationally explained. The author unnecessarily quotes from Dr. Parkes the opinion, that it does not thence follow that nitrogen is of no service in the system. This element of food may surely have another *rôle* to play, seeing that it is found in so many, and in those which are reputed to be strong foods. This was shown by the experiments of Lawes and Gilbert on manures to be that of a stimulant to vital action; and that, with its use, the increase of hydro-carbons in the crop was out of all proportion to that of the nitrogenous element. Such, it is probable, is one of its actions on the body; and as exertion is a prime excitant of the transformation of the hydro-carbons, so in the absence of exertion nitrogen may play the same part. Why is it necessary to give a highly nitrogenised food to infants? surely not to sustain muscular action, but to stimulate vital action, and to cause a larger appropriation of the hydro-carbons of the milk by the system.

The necessity for mineral ingredients in food is too evident to require any detailed observations, and the author has given

due weight to the action of potash in muscular juice and to soda in the blood. The quantity of these substances which are required must be determined by that which leaves the body daily under different circumstances, as well as that which becomes fixed during the period of growth. Dr. Edward Smith is again quoted as stating that an adult man requires daily from 52 to 79 grains of phosphoric acid, from 51 to 175 grains of chlorine (equal to from 85 to 291 grains of common salt), from 27 to 107 grains of potash, from 80 to 171 grains of soda, from 2·3 to 6·3 of lime, and from 2·5 to 3 of magnesia.

Dr. Letheby, in his observations on the action of coffee and tea, quotes the experiments of Lehmann on the one hand, and of Dr. Edward Smith on the other, as showing opposed results, but not with his usual sagacity, for in fact they were not opposed. Lehmann's experiments were made on the emission of urea; and as he found some diminution in that excretion, he asserted that these substances lessened muscular waste. But it will be observed that this was asserted under the influence of the erroneous *dictum* of Liebig; but now that it is shown that there is no necessary relation between urea and muscular waste, and that there is a relation between urea and ingested nitrogen (which was not then known or noted), Lehmann's facts may remain, but the inference must be discarded.

Dr. Smith's experiments had reference to the evolution of carbonic acid by the respiration, and showed that, as there was uniformly a large increase of that product (a much larger one than could be explained by the composition of the tea which was consumed), it followed that tea increased vital action and the transformation of the hydro-carbons. These experiments were exceedingly numerous, and are detailed in his paper in the 'Phil. Trans.,' 1859. Hence tea increased the consumption of food material, and did not supply food material in any appreciable quantity.

The use of fermented liquors and the action of alcohol receive suitable notice at the hands of the judicious author, and here again he finds antagonism between the speculations of Liebig and the experiments of Lallemand, Perrin, and Duroy. He also quotes the remarkable series of researches made by Dr. Smith on himself, including all the ordinary kinds of alcohols, which showed that, whilst alcohol was common to all, there were other ingredients which accounted for the observation of Hogarth when he prepared his caricatures of Beer Court and Gin Lane.

Nothing is more remarkable than the conservative character of scientific literature. Let observations or speculations of a

man of repute gain access to the text-books of science, and they will continue to be quoted long after later researches have demonstrated their fallacy. This is evident as to food by the Report of the Gelatin Commission, and the action of tea and alcohol; but it is, nevertheless, remarkable that the explosion of the theory of muscular force by Liebig is not followed by the omission of all deductions based upon it, and that it should be necessary, even yet, to insist upon the evident fact that alcohol alone is not alcohol in the popular sense of the word.

It is too large a question to be discussed in this article, or it would be useful to show that alcohol, and, indeed, some alcohols may lessen vital action without being conservative of vital force, for it would be easy to prove, from the minute statement of the effects of moderate doses made by Dr. Smith in his paper in the 'Phil. Transactions,' that the action is that of a disturber of the vital functions and of a poison rather than a food; and hence, if the result be conservation, whether of nitrogenous or carbonaceous excretions, it is not a conservation consistent with vital force, but allied to disease. It is also quite clear that no investigation of these substances can be satisfactory which does not include their physical as well as chemical effects, and that some of their actions, as, for example, the local effects upon the stomach, heart, and skin are of the former class.

The construction of dietaries is a somewhat rough-and-ready process, and less based upon recondite scientific researches than we are accustomed to allow, for it is little more than ascertaining how much and what kind of food is actually eaten. Thus, as to quantity, the author quotes the dicta of Dr. Edward Smith and Dr. Playfair as to the food required by different classes of persons and under different conditions, but these were simply the expression of actual quantities eaten by persons so circumstanced. Such are the reports of the former observer on the food eaten by the Lancashire operatives at the time of the Lancashire distress; of needle-women, tailors, silk-weavers, and other indoor operatives; of farm labourers throughout England, Scotland, and Ireland; and of prisoners undergoing various degrees of exertion; whilst the tables of the latter include soldiers and sailors of different nations, English navvies, blacksmiths, and prize-fighters in training. Having ascertained that people living under given conditions consume a certain amount of food, it is assumed that the quantity has been ascertained which should be supplied to others similarly circumstanced.

Inquiries of this nature, when made in an accurate manner, and on a sufficiently large basis, afford tolerably sure ground on which to construct dietaries, and may carry conviction to the

reader; yet there must be a difference between the amount which is obtained and that which is really requisite for the fullest health and strength in the conditions assigned. To say that a London needlewoman obtains 950 grains of nitrogen and 22,900 grains of carbon weekly, and an English navvy 3374 grains of nitrogen and 58,075 grains of carbon in the same time, does not prove that in either case the quantity was the most fitting for them, but only that they did obtain it and lived upon it. How much defect of proper nourishment may be tolerated for months or even years is seen in a half-starved town population, whilst an amount of food which all admit to be an excess is eaten by the well-to-do classes with impunity for a similar period. The body is so tolerant of adverse influences that the line between disease and health is a very broad one, and as it gradually shades off the changes are not very perceptible.

But whatever may be the advantages or disadvantages of this mode of reasoning, the facts on which it is based seem to have been tolerably well established. Thus, in Dr. Smith's inquiries, extending over some 600 families in different parts of Great Britain and Ireland, carefully selected, and with inquiries made in the most minute and accurate manner possible, it was found that the total weekly average quantity of nitrogen in the food was 1500 grains, and of carbon 34·167 grains, or as 1 to 22·71, whilst that obtained by the Lancashire operatives in times of poverty, but whilst they appeared to be in good health, was 1295 grains of nitrogen and 29,214 gains of carbon, or as 1 to 24·8. The latter has been adopted as a basis of calculation of late years, and it is probable that it fairly represents the minimum quantity of food which should be obtained by persons of both sexes combined when not very actively employed; but, to be on the safe side, the quantity actually named by Dr. Smith was 1400 grains of nitrogen and 30,100 grains of carbon weekly for men and one tenth less for women.

Such, however, are only generalities and averages, admitting of wide variation, under even the same conditions; for, as already indicated, the quantities actually eaten depend more upon opportunity to obtain than choice. Yet it is reassuring to find a very close approximation amongst numbers of persons living in a similar manner; as, for example, in the quantities eaten by the agricultural labourers on the average, so variously circumstanced as they are in the several divisions of these kingdoms. Thus the weekly average quantity of carbon consumed in Wales and Scotland was so nearly the same that there was a difference of only about 500 grains weekly, and the extreme averages in the three kingdoms showed a difference of only 8000 grains weekly, as indicated in the following table.

	Carbon. grains.	Nitrogen. grains.
England	40·673	1594
Ireland	43·366	2434
Wales	48·354	2031
Scotland	48·980	2348

The more recondite mode of estimation is that which ascertains the effect of exertion upon some great vital function, and then assumes that a supply of food equal to that amount is necessary to supply the waste. There is an air of precision about such an investigation which is very attractive, but it may be doubted whether the results are even as reliable as those obtained from observation of the quantity of food actually consumed; and we have already seen that the established basis—the excretion of nitrogen as a measure of waste—is an exploded fallacy. From such premises Dr. Smith has affirmed that with idleness we need 3816 grains of carbon and 180 grains of nitrogen daily, whilst with ordinary exertion and active labour the quantities are increased to 5688 and 6825 grains of carbon, and 307 and 391 grains of nitrogen, or, to quote the carbonic acid, 7·85 oz., 9·11 oz., and 12·9 oz. in the several conditions named.

Having regard to the degrees of digestibility of foods and the variation in the act of digestion at different periods, it is clear that such an addition should be made to these quantities as may represent the amount passing off as fæces, and we should be careful to allow a somewhat excessive amount.

The relation which the nitrogenous should bear to the carbonaceous elements is less certain now that the true value of the nitrogenous excretion as a test of nutrition has been ascertained, and the least erroneous mode of ascertaining it is to refer to the actual composition of our leading foods. Thus, as to vegetable foods, it is in bread as 1 to 6·8, Indian meal 1 to 7·7, rice 1 to 12·9, and potato 1 to 10·7, whilst in animal foods it is in new milk as 1 to 3·6, skim milk 1 to 2·5, the whole egg 1 to 1·6, Cheddar cheese 1 to 2·7, fat beef 1 to 5, fat pork 1 to 12·5, and dried bacon 1 to 20·8. These proportions are very varied, and in practice the proportions actually consumed lie between that of potato, bread, milk, and meat. So far as the fattening properties of food are concerned, Lawes and Gilbert found a proportion of 1 to 5½ or 6 as the best, but as this implies an excess of carbonaceous in relation to the daily wants of the system, the proportion must be somewhat higher.

It is to be observed that some make their estimate on the nitrogenous and carbonaceous elements and not on the nitrogen and carbon only; and when the latter are taken the proportions to each other are much smaller than in the former. Thus in bread they are as 1 to 22·7, in potato 1 to 35·4, in fat beef 1 to 17·5, in new milk 1 to 13·4, and in Cheddar cheese 1 to 10·9.

There is no difficulty in applying these statements to the dietary reports, for there can be no doubt that the proportion should be that of woman's milk, which is less than that of cows' milk, since there is only 1·52 per cent. of caseine in the former, whilst there is 4·41 per cent. in the latter, although the percentage of butter is somewhat larger (3·55 against 3·13), and of sugar considerably larger (6·5 against 4·77).

As age advances the proportion in the food actually eaten, and, therefore, the proportion required diminishes, but at no period is that of bread sufficient. It is also to be observed that when an increased quantity of food is required to meet wants due to exertion the proportion diminishes; and having regard to the probable action of nitrogen as proved by Lawes and Gilbert in vegetables, and Dr. Smith in animals, viz. that of a vital stimulus, we shall find the proportion required higher as other vital stimuli (as exertion) are diminished. Hence it is the greatest at the extremes of life, and the least at middle age. These proportions are well shown in the table extracted by the author from Dr. Smith's work on the cyclical changes of the human system, where the comparison is with age and body weight.

Quantity required per pound of body weight.	Carbon. grains.	Nitrogen. grains.
Infancy	69	6·78
Aged 10	48	2·81
„ 16	30	2·16
Adult	23	1·04
Middle age	25	1·13

Hence, in the construction of dietary tables, it is necessary to consider the quantity of food required, the proportion of the nitrogenous to the carbonaceous elements, the cost in relation to these questions and to the means of the consumer, as well as the question of facility of acquisition and the tastes of each person. These are duly discussed by our author, and a table is given which shows the dietaries which Dr. Smith prepared for the Lancashire operatives, at a cost of from 1s. 11¾d. to 3s. 3½d. per adult weekly, besides the special dietaries adapted to children and fat persons.

We may also add the estimate for the different meals, as quoted from Dr. Smith, as follows:

	Carbon. grs.	Nitrogen. grs.	Equal to Carbonaceous. oz.	Equal to Nitrogenous. oz.
Breakfast	1500	70	6·62	1·04
Dinner	1800	90	7·85	1·34
Supper	1000	40	4·52	0·59
Total in the day	4310	200	18·99	2·97

This, in round numbers, gives one part to supper, one and a half to breakfast, and two to dinner.

We cannot afford space to discuss the author's observations on more general questions, as the time for taking meals, ancient dinners, and the Roman habits of dining, all of which are interesting and instructive, but it would startle our modern economists to be told that a dinner for a dozen guests cost £48,500, and that 2000 different dishes of fish and 7000 of fowl were supplied at one entertainment by Vitellius for his brother Lucius.

The author's remarks on the treatment of foods in their preparation are interesting, whilst those on the preservation of food are amongst the most useful of the whole work.

The value of Liebig's Extract was discussed in the 'Times' and 'Standard' newspapers of October, 1872 (to which we call the attention of our Swedish and Russian readers), and the author makes the following observations:

"False views have been entertained of the nutritive power of this extract, for as one pound of it represents the soluble constituents of from thirty to thirty-four pounds of lean meat, or from forty-five to forty-eight pounds of ordinary butcher's meat, it has been assumed that its nutritive power is in the like proportion, but Liebig has taken care to correct this error, by showing that the extract, when properly prepared, merely represents the soup or beef tea obtainable from the quantity of meat, and as it is deficient in albumen, it must be conjoined to substances which are rich in this material."

But besides the removal of the albumen from the soluble parts of flesh, there is also that of nearly all the gelatin and of all the fat, besides every portion of solid fibrous flesh, so that the elements of meat must be added to it before it can be called soup-meat or beef tea. The researches of the northern chemists, as Professor Almen, of Upsala, and Professor Ritter, as well as of many in this country, have shown the very small nutritive value of this substance, and how utterly insufficient it is to supply the place of soup or beef tea. Even Liebig himself, in the discussion of last year, stated that "it is not nutriment in the ordinary sense," and Dr. Smith affirmed that its action was analogous to that of tea or coffee. This is also the view of our author, for he adds—

"No doubt the physiological action of the extract is due to the alkaloids which it contains, and as these are very similar to the active principles of tea and coffee (theine and caffeine) in their effects on the body, it must be concluded that extract of meat is more of a vital restorative than a nutritious food."

Do those who give beef tea made in the ordinary manner

from fresh meat consider that a cupful of it is only equal in nutritive value to a cup of tea, or do they not rather regard it as something of infinitely greater importance to the patients for whom the administration of it is regarded as necessary? If so, how can they be content to supply its place by using Liebig's Extract, which is thus, on good authority, regarded as deficient in nutriment as a cup of tea, and, in Liebig's words, "is not nutriment in the ordinary sense."

The part of the work which treats of the effects of diseased meat has special importance, for the author, as Officer of Health of the City of London, is called upon to condemn a larger quantity as unfit for human food than any other person, and has, therefore, his attention constantly directed to the subject. He informs us that within seven years he has seized and destroyed 1,567,810 lbs. of meat, of which 805,653 lbs. were diseased, 568,375 lbs. putrid, and 193,782 lbs. from animals which had not been slaughtered. After all this experience, by which as much food was destroyed as would have supplied a good meal to every inhabitant of a state as large as Sweden, he is compelled to express doubts as to the propriety of such a course. His remarks are well worth the attention of medical officers of health and inspectors of nuisances, now so recently appointed, and of justices upon whom the responsibility of rejection ultimately rests. He says:

"I feel that the question of the fitness of such meat food is in such an unsettled state that any action in the matter is often very uncertain, and I should like to have the question experimentally determined, for as it now stands we are either condemning large quantities of meat which may be eaten with safety, and are, therefore, confiscating property and lessening the supply of food, or we are permitting unwholesome meat to pass almost unchallenged in the public markets."

The truth seems to be that we have been too ready to regard food as unwholesome which is simply repulsive to our sense of propriety, and have too much ignored the two facts so strongly insisted upon by Dr. Smith, in his 'Report to the Medical Officer of the Privy Council on the Dietary of Low-fed Populations,' that braxy mutton is eaten universally and with impunity in the North of Scotland, and that fœtal calves are commonly eaten by labourers in many counties of England and Wales. The author has, however, brought together a valuable mass of facts on this subject, as well as on the effects of eating mouldy food, decayed cheese, and ergotized grain, which will well repay perusal.

The characteristics of good meat, on which he relies, may be quoted:

"1. It is neither of a pale pink colour nor of a deep purple tint, for the former is a sign of disease, and the latter indicates that the animal has not been slaughtered, but has died with the blood in it, or has suffered from acute fever.

"2. It has a marbled appearance from the ramifications of little veins of fat among the muscles.

"3. It should be firm and elastic to the touch, and should scarcely moisten the fingers; bad meat being wet, and sodden and flabby, with the fat looking like jelly or wet parchment.

"4. It should have little or no odour, and the odour should not be disagreeable, for diseased meat has a sickly and cadaverous smell, and sometimes a smell of physic. This is very discoverable when the meat is chopped up and drenched with warm water.

"5. It should not shrink or waste much in cooking.

"6. It should not run to water or become very wet on standing for a day or so, but should, on the contrary, be dry upon the surface.

"7. When dried at a temperature of 212° or thereabout it should not lose more than 70 to 74 per cent. of its weight, whereas bad meat will often lose as much as 80 per cent.

"Other properties of a more refined character will also serve for the recognition of bad meat, as that the juice of the flesh is alkaline or neutral to test-paper instead of being distinctly acid, and the muscular fibre, when examined under the microscope, is found to be sodden and ill-defined."

The remarks of the author on adulterated food carry with them the weight due to the reputation of a skilful analyst, and are very valuable for reference. He, however, complains of the insufficient use which is made of the public analysts by the poor, who are the chief sufferers from adulteration, and could obtain his services free of charge. His observations on the "Act for Preventing the Adulteration of Articles of Food and Drink" (23 and 24 Vict., c. 84, 1860), that "no good has resulted from it, and it really stands upon the statute book as a dead letter," imply a want of education on the part of all classes, but at the same time we feel constrained to admit that adulterations of an injurious character are probably fewer than at any previous period in our history, and that the simple possibility of such an examination has a deterrent influence.

VI.—The Principles and Practice of Veterinary Surgery.

It must be a matter for congratulation that, while medical science is steadily advancing and yearly acquiring new ground,

[1] *The Principles and Practice of Veterinary Surgery.* By W. Williams, M.R.C.V.S., F.R.S.E., &c. Edinburgh and London, 1872, pp. 685.

veterinary science is endeavouring to keep pace with the spirit of the times, and to make up for the almost stand-still attitude it had until recently maintained in this country. Within the last few years it has shown an eager desire to atone for past indifference and apathy, and to render itself worthy of the claim Vegetius sought for it:

"Ars veterinaria post medicinam secunda est."

The visitation of that terrible bovine and ovine scourge—the cattle plague—in 1865, which so disturbed our agriculture, hampered trade, ruined scores of farmers, and cost the nation something like eleven millions of pounds, seems to have given it an impetus which it appears nothing else could afford, and to have testified to its value in these days, when horses, cattle, and sheep form so large a portion of the national wealth, and are likely to increase still more as the demands for them become more urgent.

Before the invasion of the cattle plague, it must be confessed that the advantages veterinary science is capable of conferring were badly appreciated, and that it had fallen into a very narrow groove of routine and supineness. Whatever energy and enthusiasm may have inspired its exponents in the early days of its existence in England—for there can be no doubt that under the excellent teaching of the clever surgeon and able veterinary professor, Mr. Coleman, much was done to popularise it and to promulgate sound doctrine—comparatively little was done in after years to give it a claim to attention or endow it with the attributes of a progressive science. Almost the last to establish veterinary schools, Britain has, strange to say, done less to promote the interests of veterinary medicine than any other country in Europe. France, towards the middle of the last century, filled with dread at the havoc wrought by the same terrible malady which so appalled us eight years ago, founded the three splendid schools which have since done that country so much service and so much credit. Other governments in time followed this example, and established national schools for instruction in veterinary medicine. Those of Vienna, Berlin, Hanover, Stuttgart, St. Petersburg, Munich, Dresden, Belgium, and other places, have been always directly under government control and supervision, and the greatest care has been exercised with regard to the appointment of teachers, the subjects taught, and the thorough training of the students. This national solicitude for the promotion of veterinary science has, of course, proved eminently beneficial, not only to its progress, but to the countries which have so wisely fostered it. The professors have been, and are,

generally distinguished for their scientific attainments, and many of them have gained a world-wide reputation. We need only refer to such names as Chauveau, Dupuy, Lafosse, Bouley, Colin, Hertwig, Gurlt, Hering, Roloff, Franck, Röll, and a host of others, in proof of this.

In England, previous to 1785, the medicine of the lower animals was, it may justly be said, in a deplorable condition; for though it could boast of such men as Blundeville, Markham, the Duke of Newcastle, Snape, Gibson, and Bracken (both surgeons), Bartlett, Osmer, Clarke, and Stubbs, yet it must be confessed that their instruction had but little influence on the daily practice of the ignorant and illiterate farrier and cow-leech, to whose tender mercies the life and welfare of sick and lame creatures were consigned. The ignorance prevailing as to the nature of disease and its appropriate treatment was most lamentable, and to read the barbarities perpetrated by these men makes one almost sick with horror. The most absurd surgical operations were performed to relieve animals from diseases which really had no existence; while simple injuries, such as those due to improper shoeing or to wounds in the region of the foot, were, for lack of a knowledge of anatomy and of an acquaintance with pathological processes, often led to permanent unsoundness or the destruction of the patient. Superstition had a large share in the devising of remedies, which were in nearly every case as impotent as they were fantastic. As an instance of the stupid and cruel operations resorted to, we may adduce one which was designed to prevent a horse from stumbling, and a description of which appears in a treatise on farriery published before the above-mentioned date. We are told:

"With a lancet or a sharp penknife slit the skin from his (the horse's) nose to the upper lip, that is, downright, just between his nostrils, about two inches, which when you have done, part it as wide as you can, and under it you will find a red film, or second skin; that likewise cut and part, beneath which you may discern a flat, smooth, white sinew, the which take up with your cornet horn point, and twist it round about; after a little space twist it again round, so another time; then have regard to his legs, and you shall see him draw by degrees his hinder legs almost to his forelegs, the which, as soon as he had done, with your lancet divide the sinew at the part which grows to the lip, the which as soon as you have effected, untwist it and it will shrink up into the head, and then his legs will withdraw back again; for note that this sinew is the cause of his stumbling, and that it goes quite through his body, and spreads itself into his two hinder legs; after which you must close up the slit, and put into it fresh butter and a little salt beat fine; then take a cere-cloth made of burgundy pitch, or stock pitch, and lay upon it, and afterwards you may trust your horse for stumbling.

This is a rarer secret, known by few, but worthy to be observed in any the like occasions."

This is a fair specimen of the anatomical knowledge prevailing in those days, as well as a good example of the object the untutored farrier and horseman had in view in the great majority of their operations on the unfortunate horse or ox.

As an evidence of what their knowledge of disease was, at so late a period as 1775, we may quote from the 'Farrier's and Horseman's Complete Dictionary' (third edition), published by Thomas Wallis, surgeon, in that year. Alluding to "planet-struck or shrew-running," we are told that it—

"Is a distemper in horses, being a deprivation of feeling or motion, not stirring any of the members, but that they remain in the same form as when the beast was first seized with it. It proceeds sometimes from choler and phlegm, superabundantly mixed together; sometimes from melancholy blood, being a cold and dry humour which affects the hinder part of the brain; sometimes from extreme heat and cold, or raw digestion, striking into the veins suddenly; or, lastly, from extreme hunger, occasioned by long fasting. If the disease proceeds from heat, it may be known by the hotness of the horse's breath, and the free fetching of his wind; but if from cold, by a stuffing and poze in his head. For the cure, some prescribe to hang a flint-stone over his head, or some cold iron, as an old scythe, &c.; others, to give him fifteen seeds of single piony; others prescribe exercise before and after water; to mix hemp-seed in his provender, and to cause him to sweat, by giving him mistletoe of the oak, mustard-seed, seed of black poplar, cinquefoil, germander, hyssop, and St. John's wort. The ancient farriers, and many of the country people to this day, when they see a horse or bullock have his limbs suddenly taken from him, and not being able to think what should be the cause of such an unexpected change, believe him either to be planet-struck or shrew-run."

It is needless to say that the barbarities and absurdities of those times were productive of great loss and the infliction of an immense amount of cruelty on useful and valuable animals; and yet, wonderful to relate! nothing was done in the way of amendment until an agricultural society (the Odiham), moved, doubtless, by the example set by France and other countries, held a meeting on the 29th July, 1785, when the following minute was recorded:

"Resolved, that farriery is a most useful science, and intimately connected with the interests of agriculture, and it is in a very imperfect, neglected state, and highly deserving the attention of all friends of agricultural economy. That farriery, as it is commonly practised, is conducted without principle or science, and greatly to the injury of the noblest and most useful of our animals. That the society will consult the good of the community in general, and

of the limits of this society in particular, by encouraging such means as are likely to produce the study of farriery upon rational and scientific principles."

Though the subject was frequently brought under notice, nothing was done until 1789, when it was resolved "that, for the complete establishment of farriery on rational and scientific principles in this country, such institutions for education are necessary as have been established in France and other places." The praiseworthy exertions of this society at length culminated in the establishment of the Royal Veterinary College in London, with M. St. Bel, a distinguished French veterinarian, as Professor. The commencement was very inauspicious, the course of lectures being attended by only four students. In two years, however, there were fourteen students, and infirmary stables having been erected, there was accommodation for no less than fifty horses. At this time St. Bel unfortunately died, and some difficulty was experienced in repairing the loss caused by his premature decease; but at last Mr. Moorcroft, who had been an apprentice to a surgeon in Liverpool, and Mr. Coleman, whose career commenced most promisingly as a surgeon, were jointly elected teachers. The first-named gentleman soon after went to India, where, distinguishing himself as an adventurous and intrepid explorer in Central Asia, he was murdered during one of his journeys. The responsibilities attending the advent of the new science, and the task of establishing it on a sound basis therefore devolved upon Mr. Coleman, who, holding the office of Principal for nearly half a century, may be said to have devoted his whole life to the perfecting of veterinary medicine and surgery in this country. By his writings, which were marked by careful observation, originality, and zealous research, no less than by his teaching, he did much to elevate and improve the science of which he was so talented an exponent, and it must be admitted that at this early period those whom he taught were generally distinguished, no less for their practical ability than for their enthusiasm in their new profession. The sister branch of medicine generously took compassion upon the as yet feeble bantling, and besides manifesting the most tender regard for its welfare and interest in its progress, lent its aid in sustaining and directing its efforts. Foremost among the distinguished surgeons and physicians to whom this credit is due, must be mentioned Sir Astley Cooper, who, as an intimate friend of Mr. Coleman, took the greatest interest in promoting the success of veterinary science, not only for the advantages it was likely to confer on the community at large in a pecuniary sense, but for the benefits human medicine might derive from the study of comparative pathology. Marked changes took place

in the opinions entertained with regard to some of the diseases peculiar to animals, and particularly those of the horse. Coleman soon perceived what other continental veterinarians had previously endeavoured to inculcate and enforce, but which has only too frequently been lost sight of since: viz. that to prevent the maladies of animals is more profitable and reasonable than to attempt to cure them. His efforts were chiefly directed to the suppression of that formidable equine and asinine malady, "glanders," a contagious disease transmissible to mankind, and which was terribly prevalent among army and other horses at the commencement of his career. This prevalence was due to the utter neglect of hygienic measures—foul, badly ventilated stables, and the association of the diseased with healthy animals. By his influence, as Principal Veterinary Surgeon to the Army, the malady was all but completely banished from the cavalry barracks, while his teaching in the class-room tended to suppress it in civilian stables. Thus was inaugurated a new era in stable management, which is, perhaps, carried to greater perfection in Britain than any other country.

But though veterinary science had so far obtained a footing in England, it must be acknowledged that its primitive rate of progress has not been maintained. On the contrary, it is to be feared that on more than one occasion it has retrograded, and the position it at first promised to assume has not been achieved. Several reasons may be given for this regrettable state of affairs. There has been no proper supervision as regards the technical education of the students, owing to the schools which in time were instituted as rivals to that in London having to compete with each other and support themselves. This competition was not of a healthy nature, inasmuch as it extended to the fees for teaching; and as these became reduced, so the amount of instruction was also limited. Two sessions of six months each were deemed sufficient to teach a student the rudiments of a profession which presents great difficulties in its acquirement; but even this was not so pernicious in its effects as the admission to the schools of youths who were lamentably deficient in the elements of an ordinary education, many of them being actually unable to speak and write their own language correctly.

The struggle for existence which the schools have had to maintain has militated most powerfully against any improvement; and the system of teaching, being also based on uncertain pecuniary advantages, which depended upon the number of students admitted and the rapidity with which they could be pushed through the schools, has had to suffer from the same cause. In a country which possesses the finest breeds of horses and cattle, not to mention sheep, in the world, and which has

every right to claim a large share in the advancement of science in general, but more especially medical science, it is perfectly incredible that veterinary medicine should have received so little attention until within the last few years; and that, scourged since 1839 with contagious, but preventable, maladies whose ravages have cost tens of millions of pounds, the rulers of a mighty empire whose wealth is mainly centered in the services and profits derived from these animals, should have utterly ignored its existence, is almost beyond belief. In modern times it would appear that a political crotchet of no value whatever to the community is of far more significance to the existence of a party than attention to a subject which may certainly have no great hold upon the imagination, but which nevertheless will confer a far greater and more permanent benefit than can ever attend the ordinary hustings cry for popularity and political ascendancy.

On the Continent, these things are better managed. There are no private or other schools which rely mainly for their existence on the number of pupils admitted, and the rapidity with which they can be turned out; and which have no supervi- in teaching, and no interest in the success of the graduates. The schools are national; the teachers are elected publicly and on their merits only, and their salaries do not depend upon the number of young men to whom they lecture, but upon their own abilities; the graduates study for at least four years, and prior to being received into the schools they must demonstrate that they have received a good education, and one that qualifies them to comprehend the lectures which they must attend. In Germany more particularly, not content with their position as veterinary students only, many graduates subsequently take a medical degree. It is scarcely necessary to say that this at once places the veterinary science of the Continent far beyond that of insular Britain, and that the Continental veterinarian is a well educated and scientific practitioner. It is not at all rare to find medical men who are veterinarians, and veterinarians who are medical men. Indeed, we are carried back to the infancy of veterinary science, and find in this only what we note in the writings of Hippocrates, Aristotle, Marcus Portius Cato, Celsus, Galenus of Pergamus, and other great medical authorities of antiquity, who saw in the medicine of the lower animals only an extension of what more particularly applied to the human species. Even Vegetius Renatus, when collecting materials for his classical work, 'De Arte Veterinariâ,' written some fourteen hundred years ago, not only sought the advice and opinions of the veterinary surgeons of his day, but also the assistance of the best physicians. *Nam (inquit) mulomedicinæ*

doctrina ab arte medicinâ non adeo in multis discrepat, sed in multis plurimisque consentit.

From the earliest times the medicine of man and that of animals have been looked upon as almost the same, if not identical; and if in recent times this identity or analogy has been more or less lost sight of, and the latter has consequently suffered from neglect, it is none the less true that for a long period in the infancy of medical science, veterinary medicine stood on a level with, and was sometimes even in advance of, that of man. Indeed, it is only towards the fifteenth and sixteenth centuries that we find the latter beginning to acquire a degree of development which was in less than two hundred years to leave the other far behind. On the Continent the distance has been greatly diminished within the last fifty years, but for the reasons stated it has not been so in this country, though there are signs that an effort is being made to retrieve past errors and make up for lost opportunities. As this is done the public in general, and human medicine to a larger extent than may at first sight appear likely, will be benefited. Human and veterinary medicine have many points of contact, and can advantageously meet and mutually aid each other in the elucidation of numerous obscure or disputed subjects connected with the nature of disease and its treatment, no less than on the common ground of sanitary science. The maladies of the lower animals are, many of them, closely related to those which affect man, and the health and welfare of the latter only too frequently depends upon the freedom from disease of the domesticated creatures. The maladies which are generated in the latter may so injure their flesh that, if consumed by mankind, serious disturbance may be the result; while some of the most deadly affections known to our species are directly transmitted from pestiferous animals. We have only to refer to glanders, rabies, malignant carbuncle, aphthous fever, tuberculosis, trichinosis, and the many parasitic diseases now known to be due to transmission from parasite-bearing creatures; and we have reason to believe that medical men are daily becoming more impressed with the fact that a knowledge of animal diseases and of special morbid processes in them, is of paramount importance with regard to the perfecting of their own science, and will ultimately prove of the greatest value. This has long since been recognised by several of the highest authorities in medicine, though unfortunately it has never been universally accepted or acted upon. Even one of the most distinguished members of the veterinary profession this country has produced—himself a surgeon also—the late William Percivall, thus wrote of veterinary science half a century ago:

"To the enlightened practitioners of medicine of the present day we need say but little to convince them that veterinary inquirers may, from time to time, by extensive opportunities in comparative anatomy, add something to the general fund of medical knowledge; and that, so far, they may now and then consult veterinary works with advantage. Indeed, so much analogy is there throughout, between the structure and economy of the horse and those of the human subject, and so alike are the chief phenomena of pathology, that the surgeos had but little to do to become the veterinary surgeon; though the converse of this, unfortunately for the latter, by no means obtains. And where is the surgeon who would not himself direct the treatment of his sick or lame horse, in preference to calling in the village blacksmith?—a man whose knowledge is necessarily confined to that of nailing the shoe to the foot? The *theory* of medicine in the human subject is the theory of medicine in the brute; it is the application of that theory—the *practice*—alone that is different. Whether we prescribe for a man, or a horse, a dog, a cat, the laws of the animal economy are one and the same in all, and an unerring system of principles, built upon ascertained and established truths, is to dictate our practice in all. We might as well, in reference to the principles of each, attempt to separate surgery from medicine, as insist that either of these arts, in *theory*, is essentially different from the veterinary. Every day's experience only seems to confirm this our belief, in showing us how often the diseases of a horse arise from the same causes as those of a man, exhibit the same indications, and require a similar method of cure."

This allusion to the history of veterinary science in Britain, and the relation of this science to that of human medicine, is necessary when we come to notice its literature, which, scanty in the extreme, gives but a feeble idea of the development of comparative pathology in other countries. Until within the last few years the works published in this country pertaining to the diseases of animals, and which could be reckoned of any value, might easily have been counted on the fingers. This fact alone is, perhaps, a more certain index of the absence of enthusiasm and zeal, as well as of a low standard of education, among the members of a profession, than any other could well be. Veterinary writers have been few and far between, and the small encouragement these have obtained has not been conducive to the increase in their number. With the exception of Blaine, Youatt, Coleman, Percivall, Clark, Mayhew, Gamgee, and one or two more recent authorities, it may be said that veterinarians have not ventured in this kingdom to give us the results of their experience, nor yet to epitomise the knowledge of their science in a form which might be utilised by those who were desirous of forming some idea as to the status of veterinary medicine.

There is all the more reason, therefore, to welcome any serious

attempt to repair this deplorable omission of years gone by, and to see in every sincere endeavour to extend and popularise veterinary science an augury of a better and more creditable epoch in the future. Mr. Williams' 'Treatise on Veterinary Surgery' is a decided step forward, and is likely to prove of much assistance to the student and practitioner, in affording them a satisfactory notion of various pathological processes, and a rational method of treating surgical diseases. As a manual of veterinary surgery it is not nearly so complete as it might have been made, had the author possessed a more extensive experience in his profession, or acquaintance with the excellent French and German works on the subject which have appeared within the last twenty years.

For the reasons just stated veterinary science is yet in its infancy in this country, and its literature most meagre; it is, therefore, necessary, in order to produce a tolerably complete treatise on either surgery or pathology, to have recourse to text-books on these subjects published on the Continent, which are generally models of method and store-houses of most valuable facts. Mr. Williams has also over-crowded his pages with quotations, one third of which could easily have been dispensed with. Only too frequently he gives us the different opinions of two or three authorities relative to a single disease, but declines to favour us with his own. This of course detracts from any originality the work may possess, while it encumbers it and makes its reading tedious. And the nomenclature is not always very happily chosen; in some instances it sounds strangely of the last century farriery, and is not well placed in a book intended for the "practitioners and students of veterinary science." We have for example—"bog-spavin," "thoroughpin," "capped-hock," "seedy-toe," "curb," "thrush," "canker," "quittor," "foul in the foot," and other terms which are neither expressive, scholar-like, nor yet elegant. We also observe, too, that Mr. Percivall's name is persistently spelt Percival throughout the book—a small mistake certainly, yet one which misled us for some time. The work is illustrated with a number of woodcuts and photo-lithographs,—the former very indifferent, the latter excellent.

The first chapters are, of course, devoted to a consideration of inflammation, its symptoms and terminations, and contain the generally received doctrines as to this condition. Among the terminations are included the peculiar pyogenetic fever of young horses, known to horsemen as "strangles"—a very interesting disease which has attracted much attention from the earliest times. In its simplest form it may be said to be a catarrhal fever accompanied by, or rather complicated with,

inflammation and suppuration of the submaxillary lymphatic glands; and in its more serious or malignant form it offers an irregular course, with the formation of secondary abscesses in various parts of the body, and not unfrequently ends in death. Mr. Williams gives it a brief and superficial notice, and defines it to be a non-contagious febrile disease, and expresses himself as "clearly of opinion that it is non-contagious. The whole course and nature of the disease points to its non-contagious nature." He also adds, "I think it may be laid down, as a rule, that it rarely attacks the same horse more than once." With regard to its contagiousness, it is curious to note that the older hippiatrists firmly believed in its transmissibility. For instance, Solleysel, in his 'Parfait Maréchal' (published in 1664), after saying that it was derived from the north, adds:

"Il est très essentiel de séparer le cheval qui jette ses gourmes, des autres chevaux, parceque, non seulement elles se communiquent, mais un cheval peut prendre la morve de celui qui ne jettera que la gourme, quand même il ne lècherait pas ce qui sort par le nez à son compagnon . . . l'odeur seule est capable de lui communiquer ce mal, qui se peut prendre aussi en buvant dans un même seau."

Subsequent authorities, up to the end of the last century, were convinced that the malady is contagious. At the beginning of this century, however, when the Broussain doctrine began to prevail in medicine, doubts were expressed as to the contagiousness of adenitis (as the German hippopathologists have designated this malady), and it was finally denied by such writers as D'Arboval, Vatel, Rodet, and Delafond. Nevertheless, in France, among the veterinarians who were located in horse-breeding districts and in the army remount establishments, the belief that it was transmissible was not shaken by these authorities, inasmuch as their almost daily experience demonstrated it to be so, and now there are very few indeed who do not coincide in this opinion. The clinical evidence is overwhelming, in fact, that the disease can be propagated both by a fixed and a volatile virus, and our own observation among hundreds of young horses has amply satisfied us that its contagiousness varies at different times, like other virulent maladies; our experience also coinciding with that of Professor Reynal, of Alfort, who asserts that foals, and young horses of three, four, and five years of age, newly imported, more certainly and rapidly communicate the disease than older animals. This authority knew of numerous instances which went to demonstrate that a single foal, introduced into a stable, may infect all the horses therein; and also that mediate or immediate contact was not absolutely necessary for this transmission, as it has frequently happened that those horses which stood nearest the diseased one were not always the first attacked. To deter-

mine by experiment whether the disease was contagious, Viborg, of Copenhagen, Toggia, of Piedmont, and Gohier and Reynal, in France, resorted to inoculation, and were successful in producing it. According to M. Charlier, it is even transmissible to the bovine species, the malady having manifested itself in a badly ventilated cowshed in which a diseased horse was kept.

We have seen it occur twice, and even a third time in the same subject; and Reynal has observed it no less than four times. M. Riquet likewise informed him that during his stay at Hamburgh in 1848, he had often occasion to observe that newly purchased horses were affected with the malady, and that after their recovery they were sent to Hanover, where they were attacked a second time, and when finally forwarded to France to join their regiments they suffered a third attack. At the remount depôt of Saint-Avold, Reynal has noted the same occurrences. So that not only is there abundant testimony in favour of this disease appearing in the horse oftener than once or even twice, but there is incontrovertible proof that it is contagious and inoculable. This is a matter of much importance from a sanitary point of view, as the malady does not always appear in a benignant form, and even when recovery does follow the animal is only too frequently permanently depreciated in utility and value.

The chapter on "Fractures and the Diseases of Bones" is interesting, if incomplete. Fractures, and particularly those of the limbs of animals, are seldom satisfactory cases to treat, the expense and the chances of more or less lameness remaining, generally proving insurmountable obstacles when horses are concerned. Indeed, utilitarian and pecuniary considerations are omnipotent in the medical or surgical treatment of animals. Nevertheless, the amputation of portions of limbs are not extremely rare with the smaller quadrupeds, and valuable mares have been preserved for breeding purposes after the loss of a leg. Mr. Williams tells us he once saw a cow in a drove *en route* from Wales to London with a wooden leg, amputation having been performed above the knee.

Some of the observations on the treatment of fractures are judicious, and give a good idea of the difficulties to be overcome by the veterinary surgeon.

"When a fracture fails to unite, the causes of such non-union may be looked for in a variety of circumstances; but the most common of these—in the lower animals—is the inability to prevent motion in the limb. Temperament has much to do with this. A calm good-tempered horse may make a good recovery, whilst an irritable, fretful, animal will keep an injured limb in a state of continual motion, will take unkindly to the slings, and thus render union an impossi-

bility. In addition to, or independent of, this want of repose, even in a simple fracture, a piece of muscle or other soft tissue may be imprisoned between the broken ends of the bones, and if this is not removed, union is not likely to take place. When a fracture is oblique, this complication is not at all uncommon. . . . . Surrounding circumstances and food have an effect upon the repair of fractures. Where stables are overcrowded and ill-ventillated, necrosis and suppuration will most likely occur; and if these be associated with improper food, it will be useless for the practitioner to undertake the treatment. I have seen cases where union of the pelvis did not commence until the food had been changed."

A particular and abominable form of cruelty is connected with compound fracture of the occipital crest.

"In ponies that work in coal-pits this is a very frequent form of fracture, arising from the tunnels being made so low that the animals are continually striking their heads against the roof. The force of the blows is here nearly strong enough to break the skin and the surface of the bony crest. It is my opinion that in some cases the detached pieces of bone result from the continual blows producing necrosis. It is lamentable to think that these poor animals are worked from week to week, and month to month, without an effort being made to prevent or cure such injuries. I have seen the skin of the forelock detached for several inches—easily raised up as a flap, swollen, inflamed; with an ugly unhealthy wound underneath it, containing several pieces of necrosed bone, causing the poor brute to be nearly mad with pain, and dangerous when any one approached his head."

Surely such barbarity needs the correcting influence of the Royal Society for the Prevention of Cruelty to Animals. At any rate, if the tunnels cannot be made larger for the passage of the unfortunate ponies, the wealth-acquiring pit-owners should be compelled to provide each animal with a defence which Mr. Williams informs us is now in use at the Bowling Iron Company, near Bradford. This consists of a plain bridle having a piece of strong harness leather extending over and covering the forehead and pole, from about two inches above the eyes to a distance of two or three inches behind the ears, pierced with holes, and properly adjusted to the peculiarity of each head. This must be kept pliable by frequent oiling, or it is apt to become hardened, and to cause irritation at the base of the ears.

In speaking of fracture of the pedal bone, the author recommends that all detached horn and fragments of bone be removed, and afterwards says:

"It is curious to observe how soon a portion of foot stripped of its crust (hoof) is sheathed and coated over by the horny secretion of the sensitive laminæ; in a very few days, if the laceration of the soft tissues has not been great—where the horn is merely stripped

off—it will be found that the soft structures are covered over by a layer of protecting horn, which prevents further irritation, and enables the patient to use his foot with comparative ease. Had the advocates of the hypothesis, that the horny laminæ are secreted by the coronary substance, only remembered what they must have observed in the course of their practice, they never would have propounded such an absurdity."

Mr. Williams has evidently not made himself acquainted with the character of this corneous secretion thrown out on the surface of the vascular textures of the foot. Gourdon of Toulouse, Bouley of Paris, and other careful observers, have demonstrated, experimentally and histologically, that this material is only of a temporary character, and bears no more resemblance to the ordinary horn of the leaves on the inner face of the hoof-wall than does the provisional callus of bone resemble that of bone tissue. The designation of hypothesis and absurdity applied to what has been proved by observation and experiment is quite unjustifiable; but this is not the only instance in which the author displays an amount of acridity which his knowledge of the subject at times certainly does not compensate for.

The chapter on dislocations is well written, but like the preceding is incomplete. No mention is made, for instance, of luxation of the phalanges in the horse, though this accident is not very rare, nor yet always irremediable.

The non-inflammatory diseases of bones have a peculiar interest at this time, for the chapter dedicated to this section is almost wholly made up by the observations of Professor Varnell, who appears to have been the first veterinarian to describe the malady known as osteo-porosis, osteo-malacia, &c., a disease now affecting thorough-bred horses in South Australia. Varnell had previously made himself acquainted with the malady in the United States of America, where it is known as "big head;" so that when, in 1860, he met it again at Reading, it was no novelty to him. Though, of course, no reference is made to its continental history, yet the disease has long been known, and French and German veterinary literature is particularly rich in materials relating to it. Brugnone, in his 'Traité des Haras,' describes it in the last century; Roupp devoted some attention to it in 1825; Lecoq, of Bayeux, reported upon it in 1828, Strauss in 1831, Bénard in 1832, Noll in 1840, Darreau in 1842, and recently the distinguished veterinarian, Roloff, who has furnished a most exhaustive essay on it. It has been witnessed in the equine species in Normandy, Poitou, and in the North of Germany; in the bovine species in the Bourbonnais and Switzerland; in sheep, in Hungary, Saxony, Prussia, and in the South of France. In all these countries it may be said that the dis-

ease is enzoötic, though it is more frequent in some years than others, and is generally considered as allied to scrofula. It is usually fatal, and appears to be incidental to youth. It has been designated "arthritis" and "the paralysis of young animals," from the joints of the limbs being most frequently involved, though all parts of the skeleton are disposed to take on the abnormal condition. Varnell's description of the cases he examined in this country is most instructive and interesting. It appears that attention was first attracted to the horses (six in number) by their defective action, perhaps in one joint or limb only, in which, upon examination, tenderness would be evinced, with inordinate heat, and in a few days visible enlargement would be observed to exist. In the course of a week or thereabouts, another of the limbs would become affected in the same way, when the acute symptoms of the first attacked would, perhaps, have partially passed off. In this way all the four extremities, one after the other, eventually became diseased. The appetite was generally good, and for a time the animals did not lose flesh very fast; but after a while, from the great constitutional disturbance set up, they fell off in their appetites, and began to waste very rapidly. The pulse seldom rose much above the natural standard, and the excretions appeared to be natural.

It appears that these horses had been, all but one, bred on the same farm, and were the offspring of different mares and sires. They had been fed on the same food as other horses on the farm which had never shown any symptoms of the disease. The owner had been in the habit of feeding his horses in the same way for years without any previous ill effects. The food was good. Male animals only were attacked; none of the fillies had ever manifested any tendency to become affected; and a farm only a short distance from the one on which these cases occurred was never visited by it, though the horses thereon were bred from the same parents, and fed in the same manner with the same kind of food. Of the symptoms, Varnell says:

"I was first shown a brown cart-horse, five years old. He was standing in the cart stable with other horses; was in very fair condition, and at first sight appeared as if nothing were the matter with him; the pulse was only a little above the ordinary standard, and the excretions, as far as I could see or learn, were natural. I ordered him to be led out of the stable, when I observed that he stepped short, flexed his limbs with difficulty, and apparently with much pain, particularly the near-fore and the off-hind legs, the knee and hock joints of which were hotter than natural, slightly swollen, and tender when pressed upon. These symptoms were also present in the other two limbs, but in a much less degree of intensity. On causing the horse to be turned round, or moved in a backward

direction, a decidedly rigid state of the loins was observed, and by pressing upon any part of the back, considerable pain was evinced. On examining the animal's head, I was particularly struck with the enlarged and roundish appearance of the facial region. Each ramus of the lower jaw, the upper maxillary, and the nasal bones, were evidently enlarged, and pressure on them caused some amount of pain. On looking into the mouth, I observed that the mucous membrane was of a purplish tint, except at the margin of the gums, where it was of a pinkish colour, crossed here and there by bluish lines (veins). The lining membrane of the nasal passages and the conjunctival membrane were also paler than natural."

A post-mortem examination yielded the following results:— The internal organs, with the exception of a peculiar pallor, showed no marks of disease. One horse which was sent to London by rail, was found on arrival at the station to be so lame that it could not walk from the horse-box to the van which was to convey it to the Royal Veterinary College. On its attempting to move, the fore-legs gave way at the elbow-joint on one side and the shoulder-joint on the other, in consequence of the articular and capsular ligaments and the tendons of the muscles which are attached to these joints becoming detached (as was found to be the case afterwards) from their bony connections, by the tearing away of portions of diseased bones. This horse was destroyed on the spot, and the body taken to the College for dissection.

"The soft parts generally were paler than is natural, especially the muscles of those limbs which had been thrown out of use from the pain produced whenever any movement was attempted to be made. But in these structures I did not detect the slightest traces of fatty degeneration. The fat generally had a mottled, watery appearance, which is very common in animals that are rapidly losing flesh, and this had been the case in a very marked degree for a short time before the horse was destroyed. In disarticulating the limbs from the trunk, and also the several bones of each limb one from the other, the appearance of the interior of each joint was remarkable. In most instances, although not in all, the articular cartilage was of a dark slate colour, much thinner than is natural, and in many places it was entirely lost. This was especially the case round the margin of the articulations, leaving the bone at that part quite exposed. The synovial membrane was considerably thickened, especially in those parts where it is most vascular. The quantity of synovia in each joint was small, of a dark colour, and in some cases mixed with blood. The character of some of the articulating surfaces, however, was quite different. In such the articular cartilage was pale-coloured, and in some places of a palish yellow, velvety to the feel, and evidently containing fat; thereby indicating that the cartilage-cells had disappeared and fat had become deposited in their place. The ends of the bones were so much softened that, by apply-

ing a slight degree of force to the capsular or articular ligaments, small portions of the bone could easily be detached. The periosteal covering of the flat and irregular, and also some parts of the long bones was very vascular, and could easily be stripped off. The bones, generally, were likewise so very soft that they could be cut with a knife in any direction with the greatest ease; and if pressure were applied to the cut surfaces, or where the periosteum had been removed, blood would ooze from numerous points. In the interior of the bones the cancelli were filled with a red gelatinous substance. The ribs, the vertebræ, and indeed all the irregular and flat bones, were in the same condition. The shafts of the long bones of the extremities were not visibly increased in size, nor was the shell or compact structure much altered. The ends of these bones, however, were enlarged and soft; and on making a section through them, in their long diameter, the medullary canal, and especially the cancelli near to the extremities, had a singular although not a uniform appearance. In some of them the whole of the interior was of a dark red colour, from congestion of the vessels and effusion of blood into the areolar interspaces; in others, one half only of the interior was in this state, the other part being filled with a peculiar fat, and, consequently, very pale in colour. It was at the end of the bone affected in this way that the articular cartilage was of a palish yellow colour, velvety to the feel, and also slightly greasy. Even the teeth did not escape the malady, one of their constituents being evidently affected, which was evinced by the crusta petrosa being much thicker and more spongy than natural."

The bones, on being examined by Dr. Harley, presented two well-marked varieties of morbid change, one of which was most characteristically seen in the bones of the head, the other in the long bones of the extremities. Of the bones of the head the lower jaw was most diseased; it was considerably hypertrophied in its transverse diameter, the periosteum readily became detached, the osseous tissue was of a pink colour, and on pressure a quantity of blood oozed from its surface as if from a sponge. The osseous tissue was elastic to the touch, and so soft that it could with facility be cut with a knife. The surface of the section had a somewhat fleshy appearance, but to the nail it conveyed rather the impression of cartilage. The osseous tissue was expanded into a network of fine fibres, and the Haversian canals so enlarged that they might have been mistaken for bony tissue, while that tissue resembled the canals. Examined by means of higher powers, the osseous tissue was seen to be rarified, but the canaliculi proceeding from the lacunæ or bone-corpuscles were in many cases obliterated. The enlarged Haversian spaces were filled with a gelatinous matter, mingled with a small portion of fat, and here and there pervaded with fine, scarcely visible fibres. Varnell describes the bones, after maceration

and drying, as exhibiting many peculiarities which were not apparent in their fresh state.

"The ulceration of the articular surfaces, which seemed to be altogether confined to the cartilage of incrustation, was found on their removal to affect the bone beneath; and the loss of structure, however small it might be, in the cartilage, extended more or less deeply into the bone, the size of the cavity increasing with its depth. This fact, I think, proves beyond disputation that the loss of cartilage depended primarily upon the diseased condition of the bone, and that the general thinning of the cartilage arose from the same cause. Scarcely an articulation of the whole body was free from this 'worm-eaten' condition of both bone and cartilage. It was found, likewise, in cleaning the bones after maceration that, from their extreme softness, great care was necessary to prevent their different processes (epiphyses) from being detached, and the application of the slightest force to a portion of ligament or tendon that was not sufficiently separated by decomposition, would be certain to effect their removal. The external surfaces, also, of the bones, as contrasted with those in a normal condition, had a very singular aspect, especially in the flat and irregular bones. The ends of the long bones were similarly affected, but the shafts of them deviated but little in external appearance from a healthy state. A transverse section, however, of the middle of the shaft showed very distinctly that the osseous laminæ surrounding the Haversian canals were very much thinner than is natural, thus necessarily leading to a considerable enlargement of the latter. In the first named, however, and in the flat bone, instead of the surface being smooth and dense, it had a character not very unlike a fine sponge. The foramina for the passage of the blood-vessels, and especially those belonging to the periosteal membrane, were much dilated, thus giving the bone a kind of honeycomb appearance."

From this excellent and interesting description, which we have quoted at such length, there can be no doubt as to the disease being osteo-malacia—an affection whose intimate nature has been so well illustrated in the pathological histology of Rindfleisch, and the writings of Virchow, Volkmann, Rokitansky, Fœrster, and others.

In animals the disease is peculiar to youth—the period of growth and its causes are not yet well understood. Roloff, who has given us the best description of it in animals, and whose symptomatology of the malady is a model of conciseness and lucidity, is inclined to attribute it to a deficiency of the earthy salts in the food, or to a mal-assimilation of these in the body; he recommends local and general treatment, though he lays the greatest stress on hygienic measures. Mr. Williams abstains from recommending treatment of any kind.

We cannot venture to notice at any length the succeeding

chapters devoted to diseases of the joints, lameness, tumours, wounds, the diseases and injuries of particular regions, &c. In speaking of lameness in the posterior extremity, no mention is made of the peculiar injury arising from rupture of one of the attachments which binds the external gastrocnemius tendon to the apex of the calcis, and which permits the tendon to slip to one side of that bone; but this is only one of a large number of omissions which are of more importance.

When treating of disease of the feet we find a remarkable instance of confusion and muddle arising from an imperfect knowledge of the anatomy and physiology of these very important organs.

Attempting to describe the pathology of the formidable malady of the horse's foot, which is located in or upon the lower face of the navicular bone, and which is technically designated "navicularthritis," Mr. Williams states that it "is due to the rheumatoid diathesis and to concussion, and the liability to suffer from the first-named cause originates in hereditary predisposition and accidental circumstances." Elsewhere it is designated "rheumatoid bursitis." Now, it is a fact that the malady is very rarely, if ever, witnessed in slow-paced heavy draught horses, which are particularly liable to diseases of tendons, bones, and bursæ, but is almost special to fast-moving saddle or harness horses. It is also a disease of the fore feet, never the hind ones; and we must remember that the thoracic limbs of the horse are the chief sustainers of the weight of the body, and whatever additional weight may be imposed on the animal's back. Why should the rheumatoid diathesis so frequently manifest itself in this situation, and never in the hind feet? Such a convenient term to explain what cannot be comprehended, reminds one very much of the farrier and groom's tendency to localise the same malady in the shoulder. With these gentry, in fact, the cause of every lameness exists there, and we fear that the rheumatoid diathesis may become equally abused. The cause of this far too common and ruinous disease is to be found in the bad management to which horses' feet are exposed, and more particularly to the faulty—nay, cruel—way in which the majority of them are shod. By exposing a horse which has no trace of the rheumatoid diathesis to hard, fast work on artificial roads, and shod in the fashion so common now-a-days, we can almost produce the malady at will.

The author is no more fortunate in his attempts to elucidate the pathology of the disease. He appears to believe it may commence in the interior of the bone, and indeed asserts that it does so. He evidently overlooks the fact, that until a very late stage of the malady—really the last—the under or posterior

surface of the bone alone is involved; the upper or anterior becoming affected only when the whole piece is carious. If the disease originated in the cancellated tissue of the bone, why should one surface only be constantly affected? Another matter he strangely does not take into consideration; and that is the circumstance that the cartilage which covers the upper surface is an articular or hyaline cartilage, while that on the lower face is fibro-cartilage—as may easily be seen by the naked eye in a fresh specimen—whose fasciculi of fibrous tissue are intimately blended with, and, indeed, assist in forming, the ligaments which attach this bone to the os pedis. The mistake is all the more marked by Mr. Williams giving three drawings, after Redfern, representing the changes occurring in hyaline cartilage, to show the morbid alterations taking place in navicular-thritis. An acquaintance with the anatomy of this region would surely have preserved him from such a palpable blunder. He need not have invoked the rheumatoid diathesis to account for the presence of the malady, when he had already noticed that—

"race-horses, so long as they are shod with racing shoes, rarely suffer from navicular disease. The pace with them tells upon the column of weight-bearing bones and upon the ligaments and tendons, but when put to harness-work in carts, omnibuses, &c., and shod with a thick shoe, they soon become unsound."

Surely the thick shoe does not induce this hypothetical diathesis. Concussion is another of the favorite terms employed to account for the disorder; but it is no more satisfactory or reasonable than the diathesis, as no bone is better protected from concussion than this sesamoid. The truth is that, as in so many other instances in surgery, a fair knowledge of the anatomy and physiology of the organ would lead to a clear and correct idea of pathological processes, and indicate with a much greater amount of certainty than is exhibited in this work the causes in operation to bring about such definite changes. We are cognisant of numerous cases in which the disease has been produced in young horses during a gallop over uneven ground. There is nothing extraordinary in this; indeed, casually glancing at the disposition of the structures within the hoof, we can scarcely expect them to be any more exempted from injury than the same structures in other regions of the limb; and if we consider that the great flexor tendon plays over this bone at a greater mechanical disadvantage than perhaps anywhere else, and that this disadvantage is immensely exaggerated by the vicious manner in which the foot is generally treated by the farrier, our only wonder is that the disease is not more common.

This instance, from among many which are afforded by Mr.

Williams's book, will suffice to indicate the necessity there is of greater attention being paid to the anatomy and physiology of the domestic animals, and instead of a few months' loitering in the dissecting-room, cutting up they don't know what, veterinary students should have a thorough knowledge of these subjects. Their reputation as men of science, and their success in life, mainly depends upon it; while the value of the property entrusted to their care renders it absolutely necessary that the public should possess a sure guarantee that they do possess this knowledge.

The directions for treating the disease of the foot-membranes, vulgarly termed "canker," are good; but the author is scarcely justified, from our experience of the affection, in stating that "when the laminæ are involved the case may be generally looked upon as hopeless." We have seen such cases make a good recovery.

In describing the diseases of the generative organs, Mr. Williams is silent with respect to the existence of a contagious affection of the equine species, which, now that we are importing horses in such large numbers from the continent, may only too soon find its way into our studs. This malady, which has received a number of names—such as the *épizoötie chancreuse, maladie du coit, typhus vénérien, syphilis du cheval, maladie vénérienne* by the French veterinarians, and the *beschälkrankheit, lähmungskrankheit,* and *bösartige chankerkrankheit* of the Germans—was unknown, it appears, before 1796, when Ammon observed it in North Prussia. It was believed, however, to have been seen in Russia before that period, and Renner, who thought it originated in that country, witnessed its effects in the Imperial stud. It always prevails with most severity in Southern Russia. Since it was first observed by veterinarians, it has spread to various parts of Prussia (though from its contagious character being now fully ascertained, its ravages have lately been promptly checked by legislative measures), Hanover, Bohemia, Hungary, Pomerania, Silesia, Styria, Switzerland, Wurtemberg, and France, becoming enzoötic in several regions where it has been allowed to prevail unhindered. Since the French occupation of Algiers it has shown itself in that country, and the Arabs, who designate it "El dourine," maintain that it was introduced by their invaders. The symptoms are at first local, and not unlike those of human syphilis; the secondary effects are characterised by cutaneous eruptions, nervous derangement, paralysis, and a lingering death.

Surely such a serious and interesting disease merited some notice.

Our review of this work has greatly exceeded the limits we

had assigned to it, and nothing but the importance and attractiveness of the topic could excuse the length to which our remarks have carried us. We are deeply impressed with the value of the facts which comparative pathology may furnish to the science of medicine, and in taking leave of Mr. Williams's book we have to express our sincere regret that he has not done more justice to his subject. The many excellent treatises which have appeared in Germany, France, and other countries have not been utilised; neither has the immense mass of facts accumulated in the various continental journals devoted to veterinary science been made in the slightest degree available. This neglect has operated most prejudicially in the compilation of his work, which gives us but a poor idea of the advance veterinary pathology and surgery has made in recent years. It is sadly lacking in originality, and about one half the book is made up of quotations from medical authorities whose writings have for years occupied our book-shelves. This is most unsatisfactory and disappointing. A little discernment, a moderate amount of ability, and a slight acquaintance with veterinary literature, must have provided us with a treatise which would have interested medical practitioners, extended our knowledge of many subjects connected with animal diseases, and proved a boon no less to the veterinary surgeon than to the agricultural and horse-loving public. We can but repeat that the volume is a step forward; it will not mark an epoch.

VII.—Army Hygiene in France.[1]

"It is easy," said Marshal Bugeaud, "taking troops to fire, but difficult to maintain and preserve them there." To preserve them in health and take charge of them when struck down by sickness or wounds are among the duties of the army surgeon, but, as indicated very clearly in the works we have quoted in connection with this article, these duties involve conditions the nature of which is in many respects unfamiliar to the professional man in civil life; army hygiene is, in fact, a speciality. Thus, heretical as the observation may seem to many, the scientific hygienists of the schools would find their *science*, if unaided by practical knowledge of military requirements and conditions, of but little, if any use, with an army actively employed. The

[1] 1. *Traité de Chirurgie d'Armée*, par L. Legouest. Paris.
2. *Le Chirurgie Militaire*, par Leon le Fort. Paris.
3. *La Mortalité dans l'Armée*, par Le Dr. J. C. Chenu. Paris.
4. *Code des Officiers de Santé*, par P. A. Didiot. Paris.

science, for example, which would enable a manipulator to ascertain the minute composition of impurities of water is but so much superfluous knowledge while armies are rapidly traversing territory where only one kind of water is obtainable, and where they are glad to use whatever can be got. It signifies little to know the rules according to which, for sanitary purposes, barracks should be erected, the space that should be allowed per man, the rate at which the air of occupied buildings should be changed, the relative dimensions of inlet and outlet shafts, and all the other minutiæ connected with the subject, so long as an army is in the field operating against an enemy, and the men glad to get what shelter they can; neither is it of principal importance to have the means of testing the precise proportions of albuminates, fats, starches, and salts in food when the main question is how to obtain enough of any kind to satisfy the appetites and requirements of the troops. No! the knowledge which is required on service is not so much that of pure science, although that is, of course, valuable in its sphere, as the particular training which enables a man to turn the most unfavorable conditions to the best possible account, teaches its possessor to meet emergencies as they arise, and indicate such measures, and such alone, as are practicable of application with reference to the military conditions of the forces and objects which they are expected to attain. This particular kind of hygiene is not to be learnt in the laboratory, in the library, nor in the lecture room. He who would study it must do so with soldiers on service and in the field. His knowledge gained in such a school may seem rough to the ultra "scientific" members of his craft, but it will have this advantage, that it is practical and practicable.

But while army medical officers are expected and *required* to extemporise means whereby to mitigate, as far as is possible, the causes of inefficiency which act upon soldiers on active service, it would appear reasonable to suppose that the regulations of the service should be so framed as to ensure the fulfilment of measures seen by the nominally responsible officers to be necessary for the preservation of the health of the troops. Unless such were the case the very name of *service de santé* is a misnomer. The obvious purpose of a *health service* is to preserve health; nevertheless, as we shall presently see, this most important function has hitherto been absolutely neutralised, in France, by the regulations under which the members of the *service de santé* are obliged to *restrict* their functions.

M. Chenu discusses the conditions which curb and limit the usefulness of medical officers. He observes that while the proportion of veterinary surgeons to horses in the French army

was $4\frac{1}{2}$ to 1000, there were only 2 medical officers per 1000 men, a disproportion extraordinary in times of peace, but frightful in time of war, inasmuch as in the latter case horses severely wounded are forthwith destroyed; whereas the soldiers who are wounded demand and ought, upon grounds of expediency, if from no higher object, to receive the greatest and most constant care. On service, while the forage of horses is carefully adjusted with reference to the extra work and fatigue to be undergone by them, the food of soldiers, at all events until very recently, was denounced by medical officers as insufficient under similar circumstances. Professional competence is unappreciated, and held to be subordinate to "administration," however misdirected and incapable; the sphere of medical duty is limited, in a great measure, to the actual bedside treatment of soldiers when sick or wounded, surrounding arrangements and conditions which come within the sphere of hygiene being looked upon as beyond his province. The medical officer has little, if anything, to do with the soldier so long as the latter is out of hospital; he has not sufficient power to ensure all his requirements being supplied while in hospital, and the results are that in late wars the losses by disease in the French army, as compared to those in battle, have been as seven and eight to one, and the mortality among the wounded far beyond what occurs in other armies, where the medical services enjoy a better status. Is it, therefore, to be wondered at, if we read accounts of riots and disturbances among the populace while conscription is being enforced, and that Frenchmen press forward to emigrate in large numbers, as compared to what formerly was the case? How can it be expected so long as the *service de santé* continues to have assigned to it so low a status as it has heretofore occupied and still occupies in France, that its members will assume responsibility? Is it likely that, so long as their views upon scientific and professional matters may at any time be ignored by the unscientific and non-professional members of the *intendance*, medical officers will trouble themselves with what brings them no credit, but, on the other hand, discredit? Here it is that the boasted "unity of direction" fails, that "centralisation" paralyses the hands and energies of a class of officers who above all others ought to enjoy, within certain evident limits, freedom of action commensurate with the high functions they should fulfil towards the military forces.

Unhappily, great as these evils are in regard to the interests of the army, they are not the only ones that have arisen from the subordinate position assigned to the medical service in the *hierarchy* of the French army. For years prior to the Franco-

Prussian war the *recruitment* of this service had been becoming more and more difficult. Improvements such as experience proved to be needed had been introduced into the army medical services of America, Prussia, and England; still the defects in and attendant evils of the *service de santé*, demonstrated as these had been during the wars in the Crimea and Italy, continued and were in existence in 1870, when war was declared. What were the consequences? Qualified medical men had kept aloof from the *department;* its ranks were only partly filled; a sudden cry had to be raised for assistance wherever it could be obtained; young, inexperienced men were gladly accepted whose skill was questionable, and long before the war had terminated the several societeies of aid under the *Red Cross* had, fortunately for the sick and wounded, almost monopolised their professional care. It was no part of the programme of the surgeons belonging to *volunteer ambulances* to extend their services to the hygiene of the troops, unconnected as they were with the army; nor is it for a moment to be supposed that intendants or generals would have paid much attention to their suggestions had they submitted any. The exhausted condition of the military service, however, has given the French society an opportunity, of which it is availing itself, to endeavour to substitute that service by an organization of its own, a measure which can only have for effect the further weakening of the functions of the army surgeons in regard to sanitation, while for the general purposes of distant war it can scarcely be expected that good men and those of experience will desert their private practice and connections to take their risks in a campaign of the kind in sufficient numbers to be of real service to the sick and wounded. Young and inexperienced men will be worse than useless, and many of those whose interests and prospects are not bound up in the army will find causes for withdrawing when the real pressure of field service arrives. Nor must the fact be forgotten that for war against other than a *civilised* enemy *Red Cross* associations are unsuitable. What would Arabs, negroes, or even the mild Hindoo care for the Geneva Convention? Their principle would be again, as it has been on former occasions, to destroy the white man, whether wounded or not; hence in wars against such enemies it will be well to remember that philanthropic establishments must hold a very subordinate place, if, indeed, any at all, in the advance or in the actual scene of hostilities; and that under such circumstances dependence can alone be put upon the regularly organized military establishments. The *service de santé* must, therefore, be rendered efficient, and this may be done very readily. Let the regulations recently promulgated with regard to the corresponding services

in Prussia and Italy be applied to France; let those in force in America during the war of the secession be adopted. We had almost said let those of the British medical service be taken as a model, but that it, like the army generally, is in the throes of *reorganization*—a process to which, by the way, the former ought to be tolerably well accustomed by this time, seeing how often it has of late been subjected to it. Indeed, there is much reason to fear that, should the retrogressive provisions introduced in the warrant of 1st March not be modified or abrogated, and England be again engaged in a European war, the remarks already made in reference to the medical science of France, and the consequent great sickness and mortality among her troops will be equally applicable to those of our own country.

The several codes of instructions published for guidance in regard to different branches of the French military services are remarkable for the minute and explicit directions they contain. To judge from them alone, the great machine of the army is so beautifully regulated in all its parts that serious error seems impossible. It is unnecessary to further allude to the sad break down of that army in 1870-71 than to observe, in connection therewith, that while the instructions in regard to matters hygienic are in themselves as perfect as it seems possible to make them, their execution most certainly leaves much to be desired, not from want of knowledge on the part of medical officers, but from the fact that the *intendant*, not the medical man, has the power to carry out or veto whatever is proposed to him by his *subordinate*, the man of scientific training, the army medical officer. This state of affairs is so extraordinary, it has been proved by experience to be so opposed to the interests of the soldier, that the persistent way in which it is retained strikes all who are not French administrators as unaccountable. As observed by Dr. Chenu,[1] "medical officers belong to the *matériel*, not to the *personnel* of the army." As an illustration of *hygiene* under the direction of the *intendance* we would mention a circumstance that occurred during the siege of Paris. Typhus and typhoid fever had broken out among some of the German prisoners. It was necessary that those affected should be sent to hospital. The *intendance*, without reference to the medical officer most concerned, simply despatched them to a particular *ambulance* for the purpose of being taken in, irrespective of the fact that a large number of wounded were accommodated there. It so happened, however, that the ambulance in question was one of those under the *red cross*, and its medical director a man of energy and determination. He point blank

[1] 'De la Mortalité dans l'Armée,' p. 115.

refused to receive the patients. But what would have been the case had he been a mere agent and subordinate of the Intendance—in other words, a medical officer of the French army? In the event of his compliance a terrible disease would have been introduced among the wounded; his refusal to obey the orders of his superiors would have brought upon himself military degradation and personal ruin. Accounts of the Crimean war relate how the *Intendance* selected the most *sheltered* position for the hospitals there, how the emanations from the sick, unremoved by free ventilation, produced fatal disease; how, in other respects, the hygienic measures deemed necessary by the French medical officers were unapplied; and how, as a consequence, the death-rate from all causes during the winter of 1855-6 was 155 per 1000 among our allies as against 12 among our own troops.

Dr. Chenu enumerates in brief the principal conditions which led to this great difference. The English troops were provided with good and ample huts, the French were not. Our troops had the benefit of good roads and means of communication, the French had not. Such measures as were seen by the principal medical officer of our army to be necessary were taken in advance; the recommendations on the same subject submitted by *M. l'Inspecteur Baudens* were not attended to. The hospital service with us had the advantage of absolute control by the medical officers; it was not so with our allies. The sick and wounded of our forces had the advantage of trained orderlies and attendants; it was not so with our allies. In our camps the food left nothing to be desired in regard to quality, variety, or quantity; it was otherwise in the French army, "because the regulations there in force were too inelastic to accommodate themselves to the necessities of the time." Similarly, during the war in America the authority of the medical department was supreme in all that concerned the preservation of the health of the effectives and the care of the sick and wounded. The Minister for War was very explicit on this subject, and it is a pity that the views expressed by him had not penetrated the War Office in Pall Mall.

"Instead of placing at the head of hospital establishments instituted for treatment of sick and wounded officers who, whatever their other merits may be, cannot possibly have perfect knowledge of patients, and who, therefore, with the best possible intentions, embarrass the action of the medical men, our Government, *wisely inspired*, has placed a medical officer at the head."

And he adds—

"Never in the history of wars has the mortality in hospital been

so small, never have such establishments escaped so completely the diseases which usually occur in them."

Hospitals, it was added, were instituted for the benefit of the sick and wounded, and to this end all considerations were held to be subordinate. In French military hospitals *administration* seems to be the main object, the requirements of patients altogether a secondary affair.

As we have observed, army hygiene has a twofold sphere. It includes all that conduces to the physical efficiency of the effective forces, all that secures the comfort and management of the sick and wounded. Let us consider very briefly these two great purposes, and, first, as regards the *hygiene of effectives*. The first object at the beginning of a campaign is to place the largest possible number of effective men in the field, the second to maintain them there in the greatest possible state of efficiency. For these purposes the regulations contain very definite orders, provided only that they are thoroughly carried out.

"When troops are ordered to take the field the first duty is to eliminate the incapable, including the weak, the spiritless, the very young, the old, and the untrained recruits."

So soon as the army is put in movement it becomes the more necessary to place all questions of hygiene under a strict code of regulations, for then the troops composing it are exposed to increased risk of disease, to the combined effects of fatigue, absence of due protection from the elements, improper and insufficient food, besides the casualties incident to such service as they may be engaged upon. During long halts dangers incident to the conditions present themselves. If they remain long upon the same ground injury to health will arise from mere accumulation of men upon a limited space and the saturation of the ground by their evacuations, &c., more especially if the site has been lately occupied in the same way; these are calculated to give rise to epidemic disease. Health is, moreover, liable to suffer from the effects of season and climate. From all these and other causes a continual decrement takes place in the "strength" of a force from the very time it proceeds on service, partly by deaths from sickness and wounds, partly by the transfer of men to hospitals in the rear, and partly by invaliding and sending back to the mother country such as become permanently incapable. For each of these classes special arrangements have to be made, and in carrying out these arrangements the *Service de Santé* performs an important part so long as it is fully organized for the duties.

French writers forcibly point out the necessity, when troops on service are halted for a considerable time in any one locality, to observe the strictest watch over their sanitary con-

dition, adding, with truth, that much depends upon this whether relaxation from the fatigues and anxieties incidental to a campaign shall prove beneficial or injurious to health and discipline. They further add that the benefits to be obtained from such temporary halts include restoration of physical strength, good supplies, cleanliness, refitting clothing and equipment, regular living, and avoidance of the risks of exposure; the dangers include temptations of various kinds to luxury, irregularities and vice, the risk of infection and disease from impregnation of the ground, the plague of insects which soon springs up in camp, and so on. The experienced commander and medical officers understand all these, and should take their measures accordingly. If, as unfortunately has occurred on occasions of no distant date, their recommendations are ignored by reason of the existing *system of administration and centralisation*, the fault must be left upon the proper shoulders—soldiers may be swept away by *avoidable* causes, yet the nominally responsible officers be powerless to avert the evil.

It has been well observed by a writer on hygiene of the French army that "to enumerate the inconveniences of military service is to prepare for their occurrence, and thus to guard against them . . . . It is the duty, not only of medical, but of the military officers, to be watchful over their men, to see to their tents, cooking, cleanliness, proper care of their persons and property, for, unless continual and strict supervision be exercised, the men soon abandon themselves to irregularities, and are apt to give themselves up to laziness. Mutual help now becomes essential for the well-being of the whole. This is true of squads, companies, and individuals."[1]

The means provided for the shelter of the French soldier on service are usually not only imperfect, but of such a nature that recent experience has led to the belief that they would be better dispensed with altogether. This shelter is nothing more than the *tente abri*. In severe weather it is useless, and according to not a few observers it proved at times an absolute incumbrance, the enemy having been able the more completely to effect a surprise of the men employed in "striking" and packing them. The Germans carried no tents, but on some occasions they bivouacked in the open field; for the most part they were quartered upon the inhabitants of the districts and towns through which they advanced. In the late War of the Secession in America the soldiers had no tents; their only protection was a *poncho*, which formed their tent and bed while halting, a cloak while on the march, and with the sudden rapidity of

[1] 'Nos Armées en Campaigne,' par F. Puiguet.

movements in war it is doubtful if the use of tents will henceforward be practicable in Europe. In tropical countries they must continue to be indispensable.

In the bivouac, however, great will be the relative comfort of the old and "knowing" soldier as compared with the inexperienced or careless. The former has learnt the necessity of turning to account even the rough means provided in the open field, and how much comfort, efficiency, and health depend upon his doing so. The experienced officer also knows how necessary it is on his part to personally see to the arrangements being made by the men. Unfortunately, the sanitary knowledge of the medical officer can seldom be applied in such cases. Soldiers, as a rule, only bivouac in times of actual war, and then military exigencies supersede all other considerations.

But if soldiers may go through a campaign without tents, they certainly cannot do so without food. In all campaigns the arrangements for maintaining a good and sufficient supply in this respect have occupied much attention, and in the late war those made by the Germans show a degree of care and completeness such as may well serve as a model for future occasions. The food supplied must not only be good and abundant, it must be properly cooked, so that health and strength may be maintained. But it is obvious that in war there are often greater difficulties attending the cooking of food than in obtaining the supplies. The Germans fell upon the excellent plan of preparing their food over-night, for which purpose one party was told off in turn, while the others rested. Before starting in the morning they had a good hot meal, taking in their mess tins a portion of the meat to be eaten with bread or biscuit throughout the day. The experience of most wars has proved how necessary it is that every soldier should have a knowledge of cooking, so that he may be able in emergency to prepare a savory meal for himself. Various measures have been made to introduce an ambulatory cooking apparatus in the French army, and as we write the *Société des Secours aux Blessés* is occupied with this subject for the use of the effectives as well as for the wounded. Suitable arrangements of the kind would be of the greatest advantage in times of war; nevertheless, they will not supersede the necessity of cooking vessels being carried by soldiers themselves, and of their being able, if need be, to prepare their own *soup*, for that is incontestably the form of meal best suited for field service. *C'est la soupe qui fait le soldat.* It is well known that, as a class, French soldiers are adepts in the double art of foraging for a meal on service and of preparing it. In the instructions for campaigning directions on these points are given in detail; the greater

number are inapplicable as regards British soldiers, as, for example, that for gathering *snails* (*escargots*) and preparing them as food when all else fails. As for firewood, the French soldiers are careful to lose no opportunity when on service of having with them a supply of this kind of fuel.

If minute instructions in regard to small details were alone sufficient to preserve the health of an army, disease ought to be unknown in that of France; unhappily, however, mere regulations are of themselves neither sufficient to compensate for the notorious insufficiency of food hitherto allowed to the French soldier, alike in time of peace and in time of war, nor to meet the exigencies of modern war. The actual quantity of food given to the French soldier is altogether insufficient, and the interval between his meals too long. On this point M. Didiot[1] observes—

"Two meals only in the twenty-four hours are insufficient for good alimentation and for the proper maintenance of the digestive functions. Between the afternoon *repast* and that of the following morning there is an interval of sixteen to seventeen hours—an interval too prolonged, which gives rise to painful gnawings of an empty stomach and fainting of the person from weakness under any exertion."

M. Didiot and other medical officers have advocated an increase in the amount of food and frequency of meals. A small increase in the former has of late taken place, that is, ten ounces of meat with bone, instead of eight ounces; still, this is quite inadequate. In our own army the soldier receives, as Government ration, twelve ounces of meat; but then it must be remembered that various articles, in the shape of extra messing, are obtained with the 3½d. per day stopped from his pay for the purpose.

The French soldier has no "extra messing," and no money, as a rule, to enable him to supplement his ordinary ration. The *centimes de poche*, to the value of one halfpenny, allowed daily to him, would go a sorry way in this direction. This insufficiency of food virtually neutralises all powers of hygiene. It disables the soldiers for the fatigue incidental to war, and renders them a ready prey to such diseases as are incidental to war. All writers on hygiene of French troops allude to and condemn the miserably insufficient food allowed to them. Contrasts are drawn between the luxurious rates at which, during the War of Secession, the soldiers of America were fed, as compared to the French, and comparisons drawn between the rates of mortality by disease among them, that of the Americans being 3·9 per 100, that of the French 10. As well, in fact, might we

[1] 'Code des Officiers de Santé,' p. 533.

expect a steam engine to perform its full amount of work with an insufficient allowance of fuel as soldiers inadequately fed to undergo the same amount of exertion as those who receive full rations.

Another point in regard to which regulations and fact are at times in disaccord in the French army is that of *clothing*. It is often difficult beforehand to realise the actual necessities in this respect that occur from climate, season, and the exigencies of service, or, it may be, the great variations that become necessary by a range of temperature such as sometimes occurs. This has on several occasions been amply illustrated on actual service. In the expedition to China in 1860-61 the soldiers who had to withstand the severity of the winter at Tientsin were clothed precisely as they had been in France and as they were on board the "Impératrice" transport, in which they passed the Red Sea and Indian Ocean. In the late war the troops of France who defended Paris were ill-provided with clothing; many were thus unable to withstand exposure on the outposts and bivouacs, and it is not to be questioned that much military incapacity was the result. Of course, such conditions as those of that siege upset all ordinary plans, and the rapidity with which troops had to be raised exceeded all arrangements that could be made to provide them with sufficient clothing; but what is desired to be indicated is that on campaigns the attention necessary for purposes of health is not always bestowed upon them—a remark that is applicable from the days of the first Napoleon to the day of Sedan.

The formation and arrangement of camps rest with the officers of Engineers, yet those of the *Service de Santé, if consulted*, should, from their special knowledge of sanitation, be in a position to afford valuable counsel on this subject. To enable them the better to do so the instructions laid down for their guidance assume them to be acquainted with the general principles of encampment, and with the medical topography of the various districts or countries in which French troops are liable to serve. It is pointed out that the choice and form of a camp depend upon its purpose. If on an ordinary march, comfort or convenience are alone considered; if, on the other hand, it be formed for military purposes, sanitary considerations must necessarily be limited to such as may be observed under the special circumstances, and are in accord with strategic objects. With regard to ordinary camps, the following is a summary of instructions, namely—

"Clear roads to the market and other places of supply, prepare reservoirs and suitable drinking places, see that the means of drainage of the camp are sufficient, and that all refuse be regularly

removed. If the ground be stony, irregular, or covered by shrubs, let it be cleared as much as possible, each battalion attending to its own. If necessary, divert a stream or part of it for the use of horses and cattle, as well to avoid risk from the animals swallowing leeches as to leave the water clean for the troops. Appoint a place for washing clothes. The latrines should be conveniently situated, deep, and well protected, to prevent the occurrence of accidents at night and afford shelter in bad weather, all such arrangements being made as speedily as possible by parties of men detailed for the purpose."

The French regulations direct that parties of men be detailed for the purpose of going in search of firewood, and afterwards cutting it up; in the British service, however, such instructions would be unnecessary, as everything is provided by our excellent control-department.

Experience has shown that it is much more economical for a state to protect the health of armies than to treat such men as fall sick, and that the surest way to effect this is to avoid the causes which are known to produce them. This becomes a principal part of the duties of medical officers, who not only are expected to do all that is calculated to avert disease, but to mitigate the sufferings of the troops in every possible respect, to diminish the mortality among them, to prevent mutilations when that can be done, and to prevent the outbreak of epidemics. They have also to bear in mind the fact that although, so long as a campaign lasts, the usual excitement attending it often resists the physical fatigue, after it is over sickness in many instances breaks out, and therefore the measures of precaution, to be completely effectual, must be continued for some time after hostilities have ceased. Illustrations of this occurred among some of our own troops after the Indian mutiny and in the French after the Italian war. But, unfortunately, it may be well said that "France has brave soldiers, but she neither knows how to look after or take care of them." It is, moreover, said of them,[1] "that neither in peace nor in war do they receive their just rights, and that they are less well treated than either the American or English soldier." The subordinate and unsatisfactory position held by the *Service de Santé* is at the bottom of much of this. Ameliorations proposed by medical officers were unattended to save in a manner offensive to their proposers; reference was not made to them in regard to arrangements having a direct bearing upon the health of troops, as, for example, sites for and erections of barracks, clothing, food, means of cleanliness, and so on; hence, they naturally ceased to continue representations or remonstrances which did no good. It may also be added, members of the medical profession kept aloof

[1] 'Mortalité dans l'Armée,' p. 234.

from the army, and thus an important branch of the public service has in a great measure ceased for the present to be really effective for its high and important duties.

With regard to the *hygiene of the wounded*, the late campaign on the Continent has furnished several important lessons, some of which may be thus epitomised, viz.:

1. The first help in the field should be of no greater extent than is sufficient to permit of the transport of the wounded to the ambulances.

2. When they have been received into suitable establishments, the speedier the necessary operations are performed the better.

3. Accumulation of wounded in one locality and overcrowding in particular buildings or apartments are alike to be avoided. Both are fertile sources of epidemics and of great mortality.

4. The transfer of wounded from hospital to hospital and their transport to a distance should be conducted as far as practicable by railway or river, and not, if it can be avoided, by ordinary wheeled conveyances. Not only are these with difficulty protected and covered, but delay becomes necessary by the necessity for cooking food, performing the needful dressings, and so on.

Nor are instructions less minute in regard to the hygiene while the wounded are undergoing treatment in ambulances or hospitals. The dissemination of wounded, the evils of overcrowding, insufficient ventilation, and absence of thorough cleanliness, are forcibly dwelt upon in published codes of instructions. The necessity of performing operations as early as possible after the patient has been brought to hospital is pointed out, and details given in regard to the construction and interior arrangement of hospitals and buildings used temporarily for the accommodation of wounded.

M. Legouest considers that the ambulances which follow the active portion of an army on service ought to be capable of affording all necessary help to *one fifth*, at the very least, of the effective force; it being understood that after an action the wounded shall be transferred as rapidly as possible to establishments in the rear, and, if practicable, sent to their own country. The *materiel* of ambulances is arranged and packed in "regulation" waggons in such a way as to admit of being readily got at when needed, and to be capable of division in case of small bodies of troops being detached. These waggons have often been declared to be too heavy for their purpose, and their arrangement too complicated. It has been proposed to replace them by carriages of lighter make; but at the time of the late war, as they had been in 1831, so they remained. For cam-

paigns in hilly countries, and in Algeria, the *materiel* is conveyed upon mules; but it needs only to be stated that for a division of 10,000 effectives 364 of these animals would be required for this purpose alone, to indicate what a train is thus caused, without taking into account such bedding, clothing, and means for cooking as are left under the charge of the administration by way of reserve.

It is curious to notice that the means of transport for the wounded have remained absolutely unimproved from what they were in the time of the first Larrey. Contrasting with those in use by some of the associations for help during the Franco-Prussian war, those provided by the administration looked almost barbarous. In the first place, the *brancardiers* or stretcher-bearers of former days did not seem to have had an existence, the only men available for this duty being some of the *infirmiers* or Army Hospital Corps, some supernumeraries from other corps under the Intendance, and a few drafted from the ranks of regiments engaged, all without particular training for the duty, and the latter sometimes showing unmistakable signs of disapproval of the occupation to which they were put, besides being worse than useless. The hand-litter or *brancard* was the only form of such contrivances used by the French, and next to it the *litier* and *cacolet*, as in the olden time, carried on the backs of mules. Neither had conveyances for the transport of wounded been improved according to modern ideas, nor railway carriages fitted for a similar purpose. It is not too much to assert that, in regard to the former, if it were possible that any contrivance could be worse suited to its intended purpose, than the French pattern *voiture*, the carriage for transport of wounded according to the English model might, perhaps, be accorded that proud pre-eminence. Both are built upon principles directly the reverse of those that are indicated by experience. They ought to be as light as is consistent with strength, and to be capable of being rapidly driven. They are so heavy as to be veritable machines of torture to the wounded while in transit along bad roads or across fields, and are no more suited for rapid movement than is an ordinary waggon for merchandise.

The American carriages invented respectively by Evans and Howard are by far the best for the removal of wounded that have heretofore been used in war. They or modifications of them under different names were used in 1870-71, but only by the various societies of aid working under the red cross, no attempt being apparently made by either of the contending parties to adopt them, although, doubtless, this will be done on a future occasion. But these arrangements or any others specially fitted up for wounded formed but an insignificant part of those re-

quired to remove the victims of the great battles that took place at short intervals throughout the campaign. Omnibuses, carts, and waggons of all kinds, had to be put in requisition, abundance of straw being placed in the bottom of such as were unprovided with springs, and thus the wounded protected in a measure against the effects of jolting.

Two questions, before the war, had been discussed in regard to the transport of the wounded to which allusion may here be made. It has been asked, ought the *brancards* of a conveyance itself be taken to the field in order that wounded may be brought back and without further danger be placed in the carriage? And whether the carriages containing them ought to be drawn at a slow or rapid pace to the hospitals? Experience has resolved both these points. It has been found that if the men and *brancards* of a particular carriage once leave it, the chances are much against their finding it again; therefore, that they should remain with it and receive such wounded as are brought to them from the front. As to the question of pace, the more quickly a wounded man is conveyed to the rear the better for himself; only in order that he may be so conveyed it is necessary that the carriage by which he is transported shall be light and well finished, very different from those according to French or British official model in use during the late war.

With respect to an advancing or otherwise actively engaged army the question of evacuating or transferring sick and wounded to the more distant hospitals in the rear becomes one of great importance. On the one hand, care must be taken so that the ambulances or field hospitals shall not become overcrowded, and thus diseases attributable to that cause be developed; on the other, that only such as are likely to be for a lengthened period unfit for service shall be sent, as by want of sufficient attention to this point the effective strength of an army would speedily be impaired.

During the late war transport by river was only needed to a very insignificant extent, and that by sea not at all. In other wars, however, more especially those of the Crimea, Algiers, China, and America, one or other or both these means had to be extensively used, and in some cases arrangements made by which the necessary discomforts of the patients were reduced to their minimum. Whenever and wherever practicable, the disabled should be speedily sent to their mother country. The effect of this measure in a physical and *moral* point of view is immense; it proves to them and to their friends that they are being taken care of by Government, and the Germans carried out the measure to the utmost extent.

M. Legouest and M. Le Fort both discuss the means of

transport of sick and wounded by railway. This subject is of recent creation. Like many others of the greatest improvements in the sanitation of soldiers, we are indebted for its development to the American War of the Secession; but during the Franco-Prussian War so far behind were the French that this valuable auxiliary had not been adopted by them, and we learn that even after Paris had capitulated the evacuation of sick and wounded which then took place from the city to the provinces was performed by the Société des Secours aux Blessés, to whom the duty was delegated by the Intendance. In 1867 the Prussians had arranged and fitted up after the American plan some of their fourth-class carriages, these, like the former, having a door at either end, thus admitting of one or more tiers of beds being placed along either side, a passage being left along the centre of the carriage. The space of one tier of beds is usually occupied, at least in winter, by a stove, so that each carriage may thus carry ten patients. Twenty carriages of this kind usually formed a sanitary train, it being further provided with separate ones as a kitchen, pharmacy, store, &c., and, in addition, carriages for the staff and for sick or wounded officers. For carriages built according to the French, and, it may be added, the English plan, having transverse partitions and doors in the sides, fittings such as are used in America and Germany cannot be put up; M. Fischer, of Heidelberg, however, has invented a means of suspending a bed from the partition on either side of third-class carriages, but not only is the method inconvenient, but as no more than one patient can be conveyed in each compartment the number of carriages required becomes inconveniently large, and it is difficult to attend upon the wounded while in transit. But notwithstanding the improvements made in regard to railway transport, there are certain inconveniences still connected with it. Cooking becomes difficult while a train is in motion, so is the distribution of meals, and the performance of the more important dressings impossible; hence, for all these purposes somewhat protracted halts at stations must be made, and it becomes necessary to telegraph in advance to such stations to have the necessary preparations in readiness, thus, of course, necessitating the maintenance of *employés* at those places for the service.

The installation of temporary hospitals in time of war becomes an important part of military hygiene. No doubt the use of railways gives facilities, heretofore unknown, for the removal of wounded to hospitals and other buildings of a permanent nature situated at a distance from the scene of hostilities, yet establishments are still required where the first care can be given to men wounded in battle. As far as prac-

ticable, such temporary establishments are conducted on the same plan as the permanent hospitals. The selection of buildings for the purpose, the distribution of beds, the means of ventilation and necessary conveniences, the arrangements for conducting professional details, for obtaining and preparing food, have all to be considered, and for the most part can be speedily arranged. Some buildings are evidently better suited than others for the purpose, but it may be stated, as a rule, judging from the experience of the late war, that the more imposing the appearance of the building, the more *luxurious* its interior fitting up, the less adapted it is for the accommodation of wounded. Public buildings in which large assemblies had congregated should be absolutely avoided—as churches, chapels, ball-rooms, &c.

Much has of late years been written on the subject of huts and tents for the accommodation of sick and wounded. Army medical officers—at first from necessity, afterwards from choice—have had recourse to both these means of temporary shelter before the subject attracted the notice of hygienists and others in civil life. Drs. Bell and Henner, of the British army, made use of tents during the Peninsular War; in 1830 they were used by the French in Algeria; in 1854-5 in the Crimea; in 1861 M. Levy read a report on the subject before the Académie de Médecine, pointing out their advantages, and some of the defects which needed to be remedied. The following year the Americans perfected and extended their use; but as regards priority in their adoption, they have only the credit, but it is a great one, of having brought them to a condition hitherto unknown. In 1864 tents were used in Berlin as supplementary to the permanent hospitals; and in 1868, on the urgent representation of M. Le Fort, a tent constructed according to a plan of his own was erected near the Hôtel Cochin. The same tent was subsequently used for the accommodation of wounded during both sieges of Paris, and with more favorable results than usually occurred during normal years in the standing hospitals. Similar results were obtained by the Germans during the siege of Metz. Nevertheless, it has been shown beyond question that neither tents nor huts are, under all circumstances, "proof" against the outbreak in them of *hospital diseases.*

In comparing the relative advantages of huts and tents, it is observed that huts, from their nature, form a hospital more or less fixed; tents, from the readiness with which they admit of removal, are suited for mobile and temporary hospitals. They supply the means of making emergent provision for wounded, and afford shelter even, if necessary, upon the field of battle,

On the other hand, tents are permeable to the great heat of summer and cold of winter; and although capable of being readily and effectually heated by means of a stove and flue led under the floor, as adopted in the American ambulance in the Avenue de l'Impératrice in Paris, yet the establishment of such means of heating requires more time than is consistent with a completely *moveable* hospital.

M. Le Fort describes some of the hut hospitals used during the late war. He points out that they were elevated upon pillars from the ground to such a degree that the cold winter wind, being thus permitted to circulate completely around them, produced a temperature within so low as to be severely felt by the patients, and a similar observation is made by others. He also alludes to the existing tendency there is to abandon regularly built hospitals, and use in their stead huts such as those constructed in America and Germany, pointing out, at the same time, certain obvious difficulties in regard to them, such as that of regulating the interior temperature in extreme winter and summer climates, the continual danger of fire, the difficulty of obtaining, in or near cities of importance, ground of sufficient extent for their erection, and in the event of their being urgently required, as in the case of war, the difficulty of erecting them sufficiently rapidly to meet the emergency.

After what has already been said we are in a measure prepared to learn that no distinct pattern of tent for hospital or ambulance purposes existed with the French Intendance during the late war. MM. Legouest and Le Fort observe that conical tents, such as are usually issued for the use of effective troops, were alone available for ambulances at Metz, but that so ill-adapted were they for the purpose that they ought to be abolished. The latter-named author contrasts the defects in this respect of his own army administration as compared to the excellent models used in America in 1862 to 1864, and that adopted in Prussia since 1869. Towards the latter end of the siege of Paris a committee was formed, under the presidency of M. Husson, for the purpose of determining upon a form of hospital hut calculated to fulfil, as far as may be possible, all the requirements of hygiene.

Whatever be the nature of accommodation afforded to sick or wounded soldiers, it must be obvious that attendants are, perhaps, in only one degree less necessary for their well-being than medical men. Unfortunately the requirements of ambulances and hospitals during 1870-71 very far exceeded the means in this respect available, the result being that of the soldiers employed as such many were utterly untrained for the duties, others in no way suited for their performance. What

was wanting under the "administration" was to a great extent supplied by philanthropy, and undoubtedly in the great majority of instances were supplied; yet M. Le Fort does not hesitate to express himself very freely in regard to some of the "infirmiers et infirmières volontaires."

VIII.—Mind and Body.[1]

Works with the title and scope of those before us are very liable to be regarded as sensational, as magazines of the marvellous, and to be read by those who prefer truth when clothed in mystery, and by the dyspeptic and hypochondriacal, who seek a resemblance or identification of their own sensations and symptoms with those of other and more notorious sufferers. All this may be entirely in opposition to the designs and desire of the authors; yet it is, perhaps, natural and legitimate that such treatises should find a ready acceptance with the class of subjective invalids, as a very large proportion of the illustrations of the influence of mind in its various manifestations over the structures and functions of the body are drawn from the histories of individuals affected with nervous diseases, real or imaginary, with proteiform constitutional conditions, with states of excitability, irritability, and departures from robust health, which, although they cannot be classed nosologically as diseases, are its prodromata or preliminary stages, or who were placed in circumstances calculated to act strikingly and prejudicially upon hereditary or innate predisposition and whatever feeble organs or morbid tendencies might exist. If we examine the 430 cases narrated or alluded to by Dr. H. Tuke in his volume, we cannot fail to be impressed by the large number which belong to one or other of the two categories enumerated; in other words, we perceive that the most prolific source of psycho-pathology is within the domain of disease itself, and disease not necessarily connected with the more obvious relations of mind to body. To Dr. H. Tuke belongs the merit, however, of having attempted, not merely to adapt his speculations to scientific inquiries, but to confine them within the regular and rigid limits of professional and philosophical investi-

---

[1] 1. *Illustrations of the Influence of the Mind upon the Body in Health and Disease.* By Daniel Hack Tuke, M.D., M.R.C.P. 1872.

2. *The Power of the Soul over the Body, considered in relation to Health and Morals.* By George Moore, M.D. 1846.

3. *Theory of the Influence exerted by the Mind over the Body in the Production and Removal of Morbid and Anomalous Conditions of the Animal Economy.* By John Glen, M.A. 1855.

gations. Although his work be a repertory of very curious and interesting illustrations, it is not addressed, although it may be acceptable, to the general reader, but is couched in scientific language, is arranged in accordance with certain psychological principles, many of which, although current and popular, are not recognised, and it has evidently for its grand object the solution of the difficulties which surround the subject upon known physiological and pathological laws. We do not absolutely demur to the incessant effort to explain every problem by reference to recent supposed discoveries; but although the last to receive a dogma or a fact because it was inexplicable or impossible, we would prefer to admit a doubt or a difficulty to the reception of a solution which itself requires solution or is founded upon mere hypothesis. Dr. Tuke appears to be conscious of the habit, if not the vice, now prevalent, of over or strained elucidation, enters a gentle caveat as to the admissibility of antagonistic doctrines, as where he endeavours to reconcile the hyperæmia of Kussmaul with the anæmia of Van der Kolk, as observed in epilepsy, knowing that what the actual condition of the blood-vessels was prior to or at the time of the convulsion, or even subsequently, cannot be inferred from that seen after death, by the supposition of the latter being the result of malnutrition and degeneration, and by the theory of vascular centres or areas (p. 174); but, occasionally, rushes courageously into discrepancies where different results are attributed to the same cause, or different causes are assigned for the same result. When quoting the words in order to satisfy "our craving for explanations," and describing Brown-Séquard's opinions, he very opportunely remarks that "the influence of one portion of the brain itself free from disease may affect another portion and produce paralysis, and that the irritation is in these cases transmitted through nerve-fibres to some cells at the base of the brain, and from them reflected upon the neighbouring blood-vessels by means of other nerve-fibres." (P. 217.)

The apparent confusion—we shall not venture to call it contradiction—in tracing out the causation of various phenomena may in part depend upon the minute subdivision of mental factors, and the endeavour to connect them specially with structural or functional changes. Thus, the mind is represented in its operation upon the body as consisting of (1) intellect, (2) emotions, (3) will. Intellect is secondly held to act in the form of (1) excess of study or mental strain; (2) voluntary attention and contemplation; (3) involuntary attention, dominant ideas; (4) recollection and memory, association of ideas; (5) imagination and expectation; (6) imitation and sympathy.

Then, under each of these genera or species of mental influence, sometimes constructive or reparative, sometimes degenerative and destructive, are grouped the multiform diseases, abnormal sensations and phenomena which they have been supposed to produce, cure, alleviate, or modify. Again, the emotional powers are enumerated, as (1) joy and its various forms; (2) grief and its various forms; (3) hope and faith; (4) despair; (5) self-esteem; (6) humility; (7) fear, fright, terror, anxiety, &c.; (8) calmness; (9) anger; (10) love and benevolence; (11) hate and malevolence; (12) general emotional excitement. Then follow, under each of these sentiments or passions, long catalogues of very heterogeneous effects, which have been attributed to their interference with the established laws of the economy by various authorities of different degrees of trustworthiness. When we discover among these that joy is adduced as the cause of death in Irish convicts on the announcement of their liberation (p. 317), in the Roman mothers whose sons returned after the battle of Thrasymenus (p. 118), although we have no evidence as to the state of the heart or large vessels in these individuals, and that fright is regarded as leading to a similar result, the force of an observation made above may be appreciated; and, lastly, under volitions there are placed numerous affections, such as hallucinations, trance, simulated hemiplegia, and tetanus, and cure of epilepsy, with which the will is conceived to be in some way associated. We are prepared to expect that in a more advanced stage of knowledge such an exhaustive classification as we have now epitomised may prove a useful, even a necessary guide in judging or dealing with the *modus operandi* of the nervous system, and that it should be regarded even now as an important contribution in guiding our inquiries in the right direction; but, examined from a practical point of view, we are inclined to prefer the course pursued by Dr. Moore, who, in a much more general and simpler fashion, confines his observations to the power of attention, abstraction, memory, will, passion, in affecting for good or for evil different corporal conditions. In fact, although presented under different aspects, all the mental manifestations arranged by Dr. Tuke under intellect are resolvable into acts of attention. His text or starting-point is the well-known saying of John Hunter, himself of a susceptible and sensitive nature:—"I am confident that I can fix my attention to any part until I have a sensation in that part" (p. 5). We have not the most remote notion of treating either John Hunter's little finger or anything connected with our celebrated countryman with disrespect when we recall the equally far-famed experience of Ben Jonson, who related to Drummond that he distinctly saw Turks, Tartars,

Romans, &c., in conflict around his great toe, but wisely attributed the vision to his imagination.[1]

"This experiment (Hunter's finger) raises several questions, as, (1) might sensations always be felt in the part from the changes which are constantly going forward in the tissues, but are unobserved except when the attention is directed to them? (2) Or does the act of attention excite increased vascularity of the sensory ganglia, and cause subjective sensations? (3) Or, lastly, do the sympathetic centres become excited and the vaso-motor nerves influenced, so as to cause temporary vascular changes in the finger which involve sensation?" (p. 30).

The responses to these learned queries may be curt and conclusive. There is no proof whatever that sensation or alteration in structure occurred in the finger experimented upon, either by or without the instrumentality of the attention or determination of blood. In fact, there is a large mass of proof to the contrary. If any sensation, tingling, tickling, painful or whatever its nature, occurred during the experiment, and if any physical change accompanied or followed that perception, both must have been confined, if a local habitation must be found, to the vesicular neurine. It would be well for science were philosophy and physiology united in permanent wedlock. Hunter's suggestion, upon which such a lofty superstructure has been based, and which was supposed to unveil the mysteries of animal magnetism, is not a simple experiment. It presupposes a theory; it involves a belief and expectation; it demands that the intellect shall select a test for the solution of a doubt—that it shall will the attention be directed and concentrated upon a certain spot, and shall watch intensely for certain impressions, and reveal whatever may appear to be such; in other words, consciousness passes through various phases, one of which, in our apprehension, is the reference to the spot experimented upon of a preconceived mental condition depending upon the strain of attention or the entire chain of subjective mental acts. It is obvious that Hunter must have preformulated and anticipated the sensation, even the kind of sensation, as it must have been some modification of pleasure or pain, and that he must have willed as well as located its origin, transmission, and reception into consciousness. The trite example of the reference of sensations to a toe or a foot which has been removed from the body for years, and this in defiance of a knowledge of the real facts, and the most steadfast attention to the parts involved, and which, though depending upon irritation of a cut extremity of a nerve, must be central, cerebral, mental, is an apt illustra-

[1] Brierre de Boismont, 'Sur les Hallucinations,' p. 47.

tion of the fallacies which may be interwoven with conclusions upon the subject.

In connection with this subject there should be introduced the observations of Professor Gregory (p. 31), who reports that, in consequence of suggestion, a person experienced a variety of sensations, such as burning, from knives, chairs, the floor, with which his body came into contact, alternating with sudden cold; and the assertion of Gratiolet (p. 34), that a "law student, witnessing a surgical operation for the first time, consisting in the removal of a tumour from the ear, felt so acute a pain in the same place as to cry out." But not only does attention or suggestion exalt or pervert sensation, but it is held to suspend or annihilate it altogether. The most marked instances of this effect are derived from the works of magnetisers, or, as Dr. Tuke would designate them, psycho-therapeutists, of whose honesty we have no suspicion, but as to the accuracy of whose reasonings and deductions we entertain grave doubts. There are Cloquet's operations for the removal of the mamma during anæsthesia; there are Esdaile's cases of painless operations performed on the natives of India, and many others authenticated by equally creditable witnesses, but we have been always astounded that the number was not much greater. It is not necessary at this point to enter upon the discussion of the obscure relation between the non-existence and the non-perception of pain; but after knowing intimately the details of grave operations, such as lithotomy, iridectomy, &c., where neither psychical nor physical anæsthetics were resorted to, but where the patients were as quiet and still and undemonstrative as if they had been dead, we arrived at the conclusion that the tolerance of suffering, the capacity to feel pain, and the power of the will to subdue expression, differed as widely in different individuals, according to character, sex, temperament, as the power and activity of the external senses. Much instructive information upon these points may be obtained from the history of the Convulsionnaire de St. Medard, who underwent and even courted every modification of wound, bruise, blow, injury, not merely with impunity, but with feelings of pleasure and intense enjoyment, and where the theory that the dominant idea of miraculous interposition seems inadequate to explain the protection secured, unless we have recourse to theories as to abstraction, syncope, unconsciousness.

It cannot be questioned that, under the influence of certain passions and propensities, new or unusual conditions of the nervous system are created, that the rapture of orgasm, the strength or force developed to meet and overcome resistance, that the exquisite tact in apprehension, that the impressions of

self-satisfaction and imaginary strength accompanying the use of opium or stimulants, and the sudden and total and sometimes permanent abolition of muscular power through shock, differ as widely from each other as they all do from pain; but that, notwithstanding this distinction, they must all be traced to states of consciousness, acting, it is true, through nervous matter, but that they cannot be identical with the parts of the body or to the condition of the cerebro-spinal nerves to which they are referred. In speaking of such an identification or of the direction of attention or will to a particular region or organ, Dr. Tuke and others appear to conceive that something, which they would, perhaps, call an act of volition, passes from the brain to the spot indicated; but there is no reason to believe that thought *per se* does traverse nervous tracks, except when the co-ordination of muscles takes place, and we then regard will, not as an impulse or blind mandate of thought, but as thought itself, or that the region, organ, or spot, exemplified by Hunter's finger, stands in any other relation to the attention than any external object, than, for instance, the finger of any one else. It is highly probable that the views with regard to innervation, the necessary connection between the nerve-centres and the exercise of the ordinary functions, the nutrition, waste, the health, disease of distant parts, and the more demonstrable relation between volition and muscular contraction, have suggested the notion that mentalisation, whether under the form of intellect, attention, or emotion, passes as a current between two points. The varied nature of the influences passing to and from the nervous centres has evidently suggested, not merely the theory as to the identity of the electric and nervous power, but Brown-Séquard's opinion that, in addition to the four special senses of vision, audition, olfaction, gustation, there are distinct conductors of impressions of touch, tickling, temperature, muscular sense, and pain (p. 127), included, of course, in the nerves of common sensation, and has further sanctioned the conclusion that their remote effects are of a varied and sometimes opposite character. Countenance has, perhaps, been recently given to the supposed transmission of something; it may be in pulses or vibrations from one point to another, from the circumference to the centre of the nervous system, by the experiments instituted for the measurement of the passage of nervous influence. In the application of the results recorded by Helmholtz and others to the subject now under consideration, it should be kept in mind that pain, electric shock, &c., were the media by which sensation was excited, and that it would be as justifiable to conclude that the same conditions of the so-called nervous current existed when so acted on, as when the brain is poisoned by alcohol or otherwise, as

that either of these states should be accepted as an exposition of the ordinary laws regulating nervous influence.

In his analysis of faculties or states of consciousness, acting either concretely or separately upon the corporeal frame, Dr. H. Tuke defines "intellect as the result of impressions made upon the senses from without, or consisting of purely ideal states, whether these be formed by recollective or creative imagination (the simple remembrance of sensations excited by the outer world, or so combined as to construct new forms), cause sensation, motion, and important changes in the organic functions of the body" (p. 100). This is not the place to take exception to the vagueness or to the metaphysical heresies included in this paragraph, as it has been quoted mainly for the purpose of adding that of the "intellectual states or ideas included under the very comprehensive generic term intellect, is attention," which is, however, admitted to be under "one form an act of the will;" expectation with belief, which appears to us not merely allied to but identical with faith, whether in things natural or supernatural, and imagination, or those "ideas which arise without any external stimulus" (p. 16), are described as associated with "expectation, belief, faith, imitation, sympathy, and hope—some of these involving the feelings more than others" (p. 18). Ample warning is given of the difficulty of disentanglement of these complex conditions from one another, and it might have been prudent to eschew the scheme of following out their individual and independent influences over particular organs. We are further cautioned against assuming that "because effects are produced and cures performed by means of a mental condition called the imagination, these results are imaginary, or, in other words, 'all fancy'" (p. 18), meaning that imagination has a real, direct, substantial influence over organization and function, and may impart efficacy to remedies generally held in contempt in removing morbid conditions of these. But the author goes further than even this, and affirms that "in the ideal states the bodily changes *correspond* to the ideas present in the mind, and are themselves involuntary, illustrating the automatic action of the hemispheres upon the sensory, motor, and sympathetic centres" (p. 106). Here it may be convenient to cite a few of the more interesting and novel instances of this correspondence. The following is given as an illustration of the effect of the intellect on the organic functions:—"The influence of intense study, long continued, in causing Diabetes will not be questioned. In one of the most rapid cases which have fallen under our notice this was apparently the cause. Dr. Richardson refers to three cases in which the first excretion of

sugar and the profuse diuresis were sequential to severe mental strain, and observes that 'they constitute a hopeless class; the danger sudden, the course rapid, the fatal end sure'" (p. 101). The next quotation is as to the effect of expectation with belief:

"The confident assertions that a person subject to epileptic fits will have an attack has frequently proved sufficient to produce one. Madame de St. Amour attained great reputation in France within the last half-century for the power she exercised over various diseases. It is related that on one occasion a young woman was brought to her, when she demanded, 'What is your complaint?' 'Epilepsy,' replied the girl. 'Then, in the name of the Lord, have a fit now!' exclaimed Madame de St. Amour. The effect was instantaneous; the patient fell backwards, and had a violent attack of epileptic convulsions. Without expectation, the simple thought or remembrance of previous attacks suffices with some epileptics to cause a recurrence of the fit; and still more potent is the recollection of the cause, if the cause has been of an alarming character" (p. 65).

Setting aside the possibility that coincidence—the sentiments of awe or veneration—might have accounted for the event in the above case, much more apposite illustrations of Unger's "expectation and foreseeing of the action of a remedy often causes us to experience its operation beforehand" (p. 4), which is confessedly the foundation of these views, may be found in Dr. Rostan's experiments in the Hôtel Dieu, where fainting, purgation, emesis, &c., were induced by the intimation that the means resorted to would produce them; in the potency of Dr. Lisle's purgative pills; and in the relation by Dr. Durand of vomiting being produced on 80 of 100 hospital patients to whom sugared water had been administered as an emetic, or in the influence of the dream of the daughter of the Hanoverian Consul, that she had swallowed an aperient pill, in causing brisk purgation, although it would be equally legitimate to conclude that the overloaded state of the bowels acted as a natural stimulus to the involuntary muscles. The power of imagination over (1) sensation, (2) voluntary, (3) involuntary muscles, is pointed out in the following examples:

1. In an article in the 'Fortnightly Review' on Charles Dickens, there is stated an interesting fact in reference to his brain-fictions, namely (what the novelist had himself stated), that "every word said by his characters was distinctly *heard* by himself." The narrator felt puzzled at first to account for the "fact that he could hear language so utterly unlike the language of real feeling and not be aware of its preposterous-

ness, but the surprise vanished on his recollecting the phenomena of hallucination" (p. 53).

1. "A case reported by Professor Bennett, who was told by a clergyman that some time ago suspicions were entertained in his parish of a woman who was supposed to have poisoned her newly born infant. The coffin was exhumed and the Procurator Fiscal who attended with the medical men to examine the body declared that he already perceived the odour of decomposition, which made him feel faint, and, in consequence, he withdrew. But on opening the coffin it was found to be empty, and it was afterwards ascertained that no child had been born, and, consequently, no murder committed" (p. 48).

2. "I (Dr. Percival) met with an instance of this kind lately in which it was very difficult to prevent a person from rendering himself completely hydrophobic. Himself and his wife had been bitten by a dog, which they supposed to be mad. The woman thought herself well, but the man, a meagre hypochondriacal subject, fancied that he had uneasiness in his throat, and that he could hardly swallow anything. When he first applied to me a medical friend who was present asked him whether he had any sensation of heat at the pit of the stomach. He answered in the negative, doubtfully, but next day I found him in bed, complaining of heat at the pit of the stomach, difficulty of swallowing, tremors, and confusion in the head. He continued to persuade himself he was ill of rabies and confined himself to bed, expecting death for nearly a fortnight. At last I remarked to him that persons who were attacked by rabies never survived more than six days; this drew him out of bed and he began to walk about. By a little indulgence of his fears this might have been converted into a very clear case of hydrophobia, and the patient would probably have died" (p. 67).

As instructed by the symptoms presented by Dr. Percival's recital, and by those in the numerous instances of spasmodic contraction of the œsophagus, we are inclined to object to the whole rationale (p. 68) of hydrophobic convulsions—

"Whereby, from the analogy between the cerebral nerves and the grey matter of the spinal cord, the spectrum of a cup of water is supposed to pass along the optic nerve to the grey matter in the brain, where it produces ideogenous changes corresponding to the idea of water, and from thence the said idea is propagated to the analogue of the grey matter, exciting another series (kinetic changes), by which, through the medullary matter, the necessary groups of muscles are combined in action."

Would it not be as feasible and true to suppose the transmission of an irritation from the original wound or virus to the muscles of deglutition as to all other muscles, the recollection of such irritation being called forth by the sight of a cup of water; or, dispensing with the ideation of water, sound, or the impression of air upon the surface altogether, as not essen-

tial to the production of the spasm, are we not entitled to conjecture that the terror inspired by the recollection of a previous paroxysm should prove sufficient, as in many cases of epilepsy, to throw the muscles into convulsive action, through the ordinary sensory motor tracks? These expositions are introduced, not as exhaustive or trustworthy, but in order to show the facility with which pretentious theories may, in the present imperfect state of our knowledge of the general physiology and local relations of different parts of the nervous system, be propounded and accepted without any suspicion of obscurantism as valid and valuable facts.

3. Imagination acting upon the involuntary muscles may be followed by fatal consequences.

"There is a case on record of a man who was sentenced to be bled to death. He was blindfolded, the sham operation was performed, and water allowed to run down his arm in order to convey the impression of blood. Thinking he was about to die, he did actually die. Imagination had the same effect as the reality. But it is impossible to say how much fear had to do with it; probably a good deal as in the instance of the man reprieved, after his head had been laid on the block. and the fatal axe was about to fall. The reprieve came too late. The anticipation of death had arrested the action of the heart" (p. 78).

It is obvious, as our author admits, that fear or the anticipation of coming doom may have proved as instrumental as imagination in bringing about the results recorded, but, as we are left absolutely ignorant of the health or unhealth of the criminals, we lack an important element in forming a judgment as to the nature or seat of death. We have come into contact with an instance which seems less open to such questionings. A robust uninstructed imbecile employed as a scullion and errand-boy was arraigned, tried, and condemned to decapitation by his fellow-servants. The boy knew nothing of such executions, did not understand nor participate in the frolic, he betrayed no apprehension, but knew simply that he was to die by the order of his superiors. He allowed his eyes to be bandaged, as directed, placed his head upon the kitchen block, and when a wet towel was drawn along his neck drew a long sigh and died. The direct modes by which such disastrous events are induced, as well as all the disturbance in the heart's action attributed to the emotions, are—

"Through the acknowledged sympathetic and probably through the pneumogastric, by the reflex action which it may be supposed to exert when excited centrally by certain emotional states, just as it is alleged to do from the state of the heart at the periphery, or, if Moleschott's views be adopted, directly through motor fibres of this nerve" (p. 244).

These views are perfectly orthodox and consistent with pre-

valent creeds, but grave commentaries on their permanency are supplied by Dr. Tuke's own remark—

"That so long as such wide differences of opinion exist among physiologists as to the functions of the nerves supplying the heart, we cannot speak with any precision of the distinct modes of action by which the emotions influence this" (p. 243).

And by a still higher authority:[1]

"Although the sympathetic nerves also undoubtedly carry the influences of impressions, the direction of these cannot be ascertained, from their numerous anastomoses, as well as from the ganglia scattered over them, all of which act as minute nervous centres. But there *are* cases where certain psychical stimuli (as the emotions) act on organs through these nerves, and where certain diseases (as colic, gallstones, &c.) excite through them sensations of pain—not, be it observed, simple sensation, but pain."

Our author, in treating of the phenomena following emotion, while confessing that, "as an idea may instantly excite emotion, and *vice versâ*, and as the emotions form motives which are rapidly followed by acts of will, ideational, emotional, and volitional states are intimately bound together," and declaring "that we cannot get rid, if we would, of an ideational element, one which determines the character and direction of the purely emotional feeling which it generates" (p. 108), attempts to guard himself against metaphysical questions, and, accordingly, avoiding a definition, gives the following description of our sentiments and instincts:

"Every one is conscious of a difference between a purely intellectual operation of the mind and that state of feeling or sentiment which, also internal and mental, is equally removed from (though generally involving) a bodily sensation, whether of pleasure or pain, and which, from its occasioning suffering, is often termed passion; which likewise, because it moves our being to its very depths, now with delight, now with anguish, is expressively called emotion—a true commotion of the mind, and not of the mind only, but of the body" (p. 107).

As these feelings form the mainsprings of character and conduct, they constitute, as might be expected, important agents in the operation of mind upon body, and occupy a large portion of Dr. Tuke's attention. In treating of fear, when acting upon the involuntary muscles, he says—

"The rupture of the cutaneous capillaries, or the transudation through their walls of blood so as to occasion 'sanguineous perspiration,' should be enumerated here among the results of emotional excitement. I can, however, only refer to one well-marked case of the kind—that of a sailor, aged 30, who was so alarmed by

[1] 'Text-book of Physiology.' Professor Bennett. Part II, p. 287, 1871.

a storm that he not only fell on the deck speechless, but on going to him Paulini observed large drops of perspiration of a bright red colour on his face. At first he imagined that the blood came from the nose, or that the man had injured himself by falling; but on wiping off the red drops from the face, he was astonished to see fresh ones start up in their places. This coloured perspiration oozed out from different parts of the forehead, cheeks and chin, but was not confined to these parts, for, on opening his dress, he found it formed on the neck and chest. On wiping and carefully examining the skin, he distinctly observed the red fluid exuding from the orifices of the sudoriparous ducts" (p. 267).

It might be inquired whether, under such circumstances, the exudation from the skin-pores was the result of the emotion or of the physical condition indicated by the fall, speechlessness, &c. The action of the same instinct on the organic functions is traced in the following incident:

"A woman, apparently miserably poor, with a sick child in her arms, had attacked some ladies who were driving in a carriage, and, finding her petitions unheeded, she flung her infant into the carriage, exclaiming, with a fearful imprecation, that her little one was ill with the smallpox, and she prayed Heaven to send the malady to these hard-hearted ones. It was only too true; the fearful disease was raging in that locality at the time, and whether from contact with the child, or terror at the woman's words, I know not, but one of the ladies speedily sickened and died . . . . A medical friend informs me that a near relation of his, a young lady about seventeen years of age, took the smallpox under the following circumstances:—She was walking along the street, and saw not far from her a child in the smallpox much disfigured. It was a disagreeable object, and made a strong impression upon her mind. She was taken ill, and suffered from as severe an attack of (confluent) smallpox as my informant had ever attended" (p. 313).

To the various doubts expressed by Dr. Tuke as to whether emotion was in any degree effective, we are constrained to add our conviction that contact or contagion were perfectly adequate to propagate variola in both these instances. An example of the power of fear in arresting nutrition in a particular organ is thus recorded:—"I have recently known," says Marshall Hall, "the teeth to decay in an extraordinary manner, in a few weeks, as the effect of painful emotion, more allied to fear than any other" (p. 292). When fear, sympathy, and vivid and intense attention, directed to one spot, act in concert, the following results are supposed to have arisen:

"I shall first give the following example—the case of a highly intelligent lady, well known to myself. Although the emotion had for its object another person, it none the less acted upon her own system. One day she was walking past a public institution, and

observed a child, in whom she was particularly interested, coming out through an iron gate. She saw that he let go the gate after opening it, and that it seemed likely to close upon him, and concluded that it would do so with such force as to crush his ankle, however, this did not happen. 'It was impossible,' she says, 'by word or act to be quick enough to meet the supposed emergency, and, in fact, I found I could not move, for such intense pain came on in the ankle, corresponding to the one which I thought the boy would have injured, that I could only put my hand on it to lessen its extreme painfulness. I am sure I did not move so as to strain or sprain it. The walk home—a distance of about a quarter of a mile—was very laborious, and on taking off my stockings I found a circle round the ankle as if it had been painted with red-currant juice, with a large spot of the same on the outer part. By the morning the whole foot was inflamed, and I was a prisoner to my bed for many days.'"

Our attention has recently been directed to a case much less open to cavil. Dr. Aitken writes, in the 'Report of the Inverness Lunatic Asylum,' published in 1872:

"Among the patients no mental disturbance could be traced to the vaccination beyond such discomfort and irritability as is not unfrequently manifested in healthy individuals, though a phenomenon occurred in one of them deserving of special remark. A man, a chronic maniac, remarkable for his sense of dignity, yet good humour, his grotesque manner and bizarre gesticulations, after being vaccinated seemed to have his memory roused to the remembrance of the cicatrix of his first vaccination. He was incessantly taking his coat off, tugging up his sleeves, looking at them apparently with a sense of admiration, patting them with his fingers, exhibiting them to every one who came into contact with him, and bending down at such a distance from the fire that the part of the arm on which they were might be gently warmed. In a short time an abscess formed of considerable size, which it is believed may be fairly taken as an instance illustrative of the effect of attention directed to a particular part" (p. 16).

The illustrations of the influence of the depressing emotions on the organic functions may be closed by quoting those of fear and grief in the production of gangrene, and of the same emotions, with the addition of jealousy, hatred, &c., in the production of cancer:

"I. Guislain mentions two cases bearing on this subject; one in which a woman, who had seen her daughter violently beaten, and was much frightened, suffered in consequence from gangrenous erysipelas of the right hand; the other in which a woman, æt. 24, saw her brother die, and was greatly affected. A wen which she had on the head became gangrenous in a few days. II. Descuret reports the case of a young woman who had cancer of the breast requiring operation, which he attributes to the maleficent action of

jealousy, hatred, and chagrin." "Romberg says he attended a lady, æt. 40, whose right mamma had, four years previously, after violent mental excitement, become attacked with scirrhus, which was being gradually developed. Bichat maintains that cancer of the stomach frequently owes its origin to powerful emotions" (p. 288).

According to Romberg, "hypochondriasis can only be said to exist if the mind creates new sensations, which, in their turn, give rise to nutritive derangement" (p. 135); and a similar mode of argument has been employed when such cases as those now described are treated of, even when taint is presupposable. But instead of asserting that anxiety, despondency, and gloom predispose to cancer, and that anxiety, directed to some part or organ, may lead to disorders in nutrition, and ultimately to this degeneracy, would it not be more consistent with the facts —as depression invariably follows cancer, whether it has preceded its appearance or not—to hold that the local disease may be dynamically the cause of the despondency, or that both may be part and parcel of that original constitution called hereditary?

Dr. Tuke, apparently failing to attach sufficient value to the differences in the susceptibilities of different persons, sexes, ages, and what would now be popularly called the inhibitory influences created by intellect, will or self-command, and temperament, expresses surprise that sudden joy and fear or grief should excite similar influences, and, dissatisfied with the narrow limits created by particularising the sphere of each individual feeling, has established a section entitled "General Emotional Excitement," under which it is clear all combinations of sentiment or appetite, especially when violent, may be comprehended. As, however, Dr. Tuke avoids all sharp distinctions between mental states, and reiterates that these are interdependent and often inseparable, there is a close approximation between his views upon emotional and volitional excitement and influence.

After giving Reid's interpretation, "Volition, therefore, signifies the act of willing and determining, and will is put indifferently to signify either the power of willing or the act" (p. 327), he adds that, in treating "of the intellect and emotions, he has, under other terms, treated of what by many is regarded as belonging to the province of will" (p. 327); and "Should this be the correct mode of regarding its nature, it is obvious that the will is not a special faculty, independent of the other mental faculties, but that it is composed of an emotional or active element and an intellectual or regulative element, the balance of which results in a volitional act" (p. 330). The grand desideratum or difficulty, however, is not to prove that volition is of intellectual

or emotional origin, but to determine the power by which we move our muscles in walking, &c., that by which we adapt the apparatus of the external sense in observing the movements of a monade or a star, that by which we resist the instigations of certain propensities or substitute one motive, or, to use Dr. Tuke's phraseology, neutralise by another, that by which we recall images from the depth of memory into present consciousness, and that by which these images, according to Sir D. Brewster, "are again pictured upon the retina," are all identical or essentially different in nature, scope of operation, and psychical relations. These marked contrasts in the action of what has been supposed to be a distinct faculty, and our author's conceptions as to the influence of this faculty over the muscular and mental systems, may be fairly indicated by the following cases:

1. Esquirol maintained that no one could successfully feign an attack of epilepsy, not even those who were thoroughly familiar with all the symptoms. One day Drs. Calmeil and Trousseau were talking with Esquirol on this subject at the asylum of Charenton, when suddenly Dr. Calmeil fell down on the floor in violent convulsions. After examining him for a moment Esquirol turned round, exclaiming, "Poor fellow, he is epileptic;" but he had no sooner said so than Dr. Calmiel got up and asked him whether he still insisted on thinking epilepsy could not be feigned (p. 337).

2. "Hyacinthe Langlois, a distinguished artist, and who was on intimate terms with Talma, told M. Brierre de Boismont that this great actor had informed him that when he entered on the stage he was able, by the power of will, to banish from his sight the dress of his numerous and brilliant audience, and to substitute in the place of these living persons so many skeletons. When his imagination had thus filled the theatre with these singular spectators the emotion which he experienced gave such an impulse to his acting as to produce the most startling effects."

Such a narrative affords an apparent countenance to Sir David Brewster's opinion, for it does not deserve a nobler title, who, when combating Dr. Abercrombie's view—

That memory and imagination acted independently of the organs of perception—held that the images recalled or recreated were reproduced upon the retina, &c., or its parts, through which they were originally transmitted, and were examined there, as they followed the motions of the head and eye. This he explained by supposing that the recollection of an object previously seen acts by retransmission from the brain along the nerves to the same points of the retina as had been acted upon by the original object, when the impression there had been transmitted to the sensorium. Spectral illusions, according to him, resulting when the impressions were very

vivid and powerful, the nerves excitable, and the faculties, memory, and imagination powerful (p. 50).

Putting aside the lack of all proof of such causation, in what manner can we explain the reproduction of such images in the 40,000 blind people, and the much larger number of deaf people, in whom the retina and auditory pulp have been destroyed, and which cannot again be called into the same condition as when they first received or vibrated, if we must assist our comprehension by physical motion or change, under the glorious colours of a sunset or the music of a Handel? The capacity claimed by certain individuals of calling up or creating in the sphere of vision images such as flowers, and by others of projecting these representations upon a wall or curtain, so as to be seen by them objectively, might be regarded as affording support to Sir D. Brewster's opinion; but both Goethe and Muller, who are the best accredited possessors of even a limited exercise of such a faculty, say that the forms contemplated by them were, "not natural flowers, but composites and of fantastic forms, although symmetrical as the rosettes of sculptors,"[1] and that they varied infinitely in proportion and colours, so that the process to be explained is not an act of recollection of former images, but one of conception or ideation. The facts as well as the theory require substantiation, and will not be elucidated by the transcendental views as to—

"Ideas or conceptions being cerebral (hemispherical) states, which seem to recall the same condition of the sensorium as that which was originally excited by the sensory impression . . . That subjective sensations and objective sensations occupy the same seat cannot be doubted; but the difficult question is whether the definite remembrance of a particular object passing beyond a mere notion does or does not cause a true sensation, however faint" (p. 51)—

quoted and to a certain extent sanctioned by Dr. Tuke.

In connecting the mental influences with the different parts or functions of the body upon which they are said to act, the author has arranged the various proofs, or illustrations as they may be considered, in distinct sections, as they may be observed in organs of sensation—voluntary muscles, involuntary muscles, organic functions. This is a lucid and legitimate classification, and a great advance upon all previous schemes of a similar nature, but, unfortunately, the intimate and invariable connection of the psychical cause and the physical consequence is not demonstrable, and the effects might in many instances be as fairly ranged under one or other cause, or under all the causes combined. To the scientific reader, who has gone over the work continuously and attentively, such inconsecutiveness will

[1] Muller's 'Physiology of the Senses.' London, 1848, p. 1394.

not be detectable, but to untrained minds the minute subdivisions, where no broad distinctions are palpable, will be found to be cumbrous and perplexing. When studied as a philosophical disquisition ought to be, and where the physiological and psychological expositions are applied to the pathological changes, the author's labours assume a comprehensive character which is worthy of much praise, even when perfect concurrence in his conclusions may be withheld. While Dr. Moore repudiates phrenology, and Dr. Glen ignores it altogether, Dr. Tuke avoids the subject, unless it may be occasionally detected under the masquerading guise of electrobiology and a certain reliance upon the experience of Dr. Elliotson. The latter, however, adopts a general localisation of the mental powers in assigning to the hemispheres the functions of intellect, emotion, &c.; to the sensory ganglia that of the conveyance of impressions to consciousness; and to the motor tracts the expression of volition. To such conclusions it is not necessary to object; but when he exceeds this generalization—when he endeavours to describe the process, even the structural alterations in these organs, which eventuate in psychical phenomena—or, in other words, ideas, emotions, will—grave inquiries arise as to whether he may not have passed from the region of knowledge into that of fancy. We find in these pages such statements as "Every thought, every sensation, is accompanied by a change in the composition of the substance of the brain" (p. 102); in memory "substrata are reawakened into activity by affinitive impressions, and it follows the law of association of ideas" (p. 17); "but the condition of my mind when I perceive the sun may be considered to involve ideational, as well as excited by, sensorial changes—including, in short, the mental state which, as an idea, remains after the external object or stimulus is withdrawn. A change has been induced in the grey matter of the hemispheres by the upward action of the sensory ganglia, excited by impressions from without, so that we have three distinct, though continuous, portions of the nervous system acted upon—the peripheral expansion of the optic nerve on the retina, the corpora quadrigemina, and the hemispherical ganglia" (p. 28). The sensorial condition following the admission of light, and with it colour, form, &c., and, it may be, of heat, through the external senses, and the conception which these impressions go to make up (ideational), but which is not in consciousness separable from them, may be received as a fair exposition of the process by which we acquire and retain the idea of the sun; but the change supposed to be induced in the grey matter by the upward action of the sensory ganglia is altogether an hypothesis,

and meriting the term "fanciful," which Dr. Tuke holds in great abhorrence. *It is possible* that a knowledge of the external world may pass along the route traced out, that it may reach and rest in the vesicular grey matter; it is possible that such afflatus or influence may produce physical changes in this matter—that these changes may affect or be associated with changes in the psychical relations of the impressions thus received, and convert them or combine them into the concrete idea of the sun, which it is possible may remain there as a spectrum "registered for reproduction" (p. 2) or recollection; but all this is sublimated or rather materialistic assumption, and we are not writing of possibilities, but attempting a grave and philosophic exposition of the ascertained causes of phenomena, and in no way entitled to rest satisfied with such data. It would be well to bear in mind the aphorism in Tyndall's 'Scientific Materialism,' "that the passage from the physics of the brain to the corresponding fact of consciousness is unthinkable."

Again, we have the following passages:—"Although the hemispheric cells are insensible to pain, or to any other known stimulus, they have a sensibility of their own to ideas, and the sensibility which thus declares the manner of their affections is what we call emotional" (p. 120). "Emotions may so injuriously affect the nervous system that the will can no longer direct or control the muscles when these become the sport of the sensori-motor centres" (p. 194). "In those cases violent mental excitement must have produced such changes in the nervous tissues that the normal superior power of volition over muscular movements was suspended, and the sensori-motor apparatus left to its uncontrolled automatic action" (p. 179). "Frequently, then, as vascular changes, occasioned by emotion, may cause the morbid condition of the nervous tissue which entails paralysis, it seems very probable that a mental shock may directly produce molecular changes in the brain and motor system, independently of those which arise from congestion, anæmia, or rupture of a blood-vessel" (p. 218), and such like, which, although sometimes the emanations of other and differently constituted minds, are adopted and employed without protest by our author in the explanation of changes in structure or function. It is true that, in using these views and the language in which they are couched, he merely harmonises his labours with certain of the vague and untenable speculations of the most distinguished writers upon cognate subjects, and we seize the occasion in order to record a doubt and demurrer as to the soundness and the safety of the course pursued. For example, in Professor Laycock's valuable work on 'Mind and Brain' there are found the following sentences:

"The fundamental energy of the law of design is the *realisation* of the teleiotic ideas in time and space. In living and conscient organisms these teleiotic ideas, as derivations of the fundamental idea, are both biotic and intellectual. They have, therefore, a twofold operation—*First*, as vital forces acting on the material substratum, so as to shape and arrange its ideagenic and kinetic *substrata*, the changes in which will be coincident at some future time with states of consciousness, but which do not necessarily enter into the consciousness at the time of formation and arrangement (latent consciousness); *secondly*, on the consciousness—or mental forces proper—exciting its various states, known as feelings, desires, intuitions, beliefs, faculties" (p. 60).

In Hammond, on 'Diseases of the Nervous System,' the following *non-sequitur* occurs:

"It is probable that there are at least two kinds of nerve-cells in the grey matter of the cord, which, though alike in anatomical characteristics, differ essentially in their functions. One set is motor and one sensory. In those cases of spinal paralysis involving motion, and in which there is atrophy of the nerve-cells, the motor cells are diseased; in those in which sensation is affected, and in which atrophy of nerve-cells is discovered, the sensory cells are the ones affected. Now, progressive muscular atrophy, pure and uncomplicated, is unattended by derangement of sensation, and unaccompanied by paralysis, except such loss of power as is directly due to the diminution of the volume of the affected muscles. The presumption is, therefore, that neither the motor nor the sensory cells have disappeared or become atrophied, and yet, on post-mortem examination, we find that nerve-cells of *some* kind have been diseased. The presumption is, and it is reasonable, that there are cells which are specially connected with the nutrition of muscles—trophic cells" (p. 675).

In Professor Bennett's recent volume on 'Physiology' we read—

"For the reception of smell, taste, touch, vision, and hearing, nerves with peculiar endowments are provided, and to them are added a spinal structure or organ adapted for the purpose. It is possible, as previously noticed, that there may be tubules possessing endowments for conveying influences from other impressions than those just referred to, but these are not yet known" (p. 327).

These are extracts from text-books by eminent and trustworthy teachers, but we humbly submit that the propositions enunciated are unintelligible and illogical; or, where intelligible, undemonstrable, and that of "ideagenic and kinetic substrata," "trophic nerves," and of "special sensory tubule," we know nothing except hypothetically or inferentially. Professor Bennett, with his usual caution and clearness, guards against rash notions as to the nature of the impressions conveyed along these tubules by writing—

"*Sensation* may be defined to be *the consciousness of an impression;* and that it may take place, it is necessary, 1st, that a stimulus should be applied to a sensitive nerve which produces an impression; 2nd, that, in consequence of this impression a something should be generated we designate an impression, which influence is conducted along the nerve to the hemispherical ganglion; 3rd, on arriving there it calls into action that faculty of the mind called consciousness or perception, and sensation is the result" (p. 288).

Much of what has been here complained of is to be attributed to microscopical inquiries *sub judice,* and to the transference of actual discoveries by the microscope to regions with which they have no connection, and into which it is highly improbable that physical science will ever be competent to penetrate. But supposing all that has been so grandiloquently written as to the "tiny maelstroms" seen by Huxley in the representative protoplasm of the nettle-sting were proved to exist but which has been discredited, if not disproved, by Stricker, who describes the protoplasm "as a homogeneous substance in which any granules that may appear must be considered of foreign importation, and in which there are no evidences of circulation,[1] or supposing again that granules or atoms of nerve-tissue, which we conceive to mean the smallest possible size of the materials of which brain is composed which the human mind can realise or the microscope reveal, were seen moving in vortices or oscillations in the fluid taken from the ventricles from the substance of the cerebral mass, or in any extraneous liquid, and it were proved that these movements did not originate in currents established by temperature or impulse from surrounding objects, which it has not been, or even supposing further that alterations of form or position of the contents of the brain-cell or tubule have been observed and that they were not the result of endosmose or exosmose in the walls; or of the various physical influences inseparable from new and altered circumstances and from removal beyond the influence of vital forces; the gulf between such conclusions and the recognition of molecular changes in the grey matter is enormous and impassible. Yet, Dr. Tuke seems throughout his work to accept such conclusions as applicable to the solution of the problems surrounding the act of mentalisation, or, as the school to which he belongs would affirm, the elimination of ideas and emotion or, as Professor Bennet had it:

"The brain thinks as a muscle contracts, or emits thought as a piano emits sound.[2] True, it is not the will or sensation which is the principal agent of movement, but the material changes in the vesicular neurine or grey matter" (p. 69).

[1] As regards protoplasm in relation to Professor Huxley's essay on the "Psychical Basis of Life," by James Hutchinson Sterling. Edinburgh, 1862. P. 22.

[2] 'Bennet's 'Text-Book of Physiology.' Part I, p. 180.

Assuredly this is a leap in the dark; but we cannot believe that Dr. Tuke has cleared the gulf to which allusion has been made. Of the will, the sensation, and the movement, we have or may have a clear knowledge; but of any operation of the vesicular neurine we have no knowledge whatever. It is probable that an organ which grows is nourished, is broken down, and sometimes repaired, through which the circulation goes on, sometimes under the ordinary, sometimes under special arrangements, in which endosmose and exosmose take place, in which there is a constant flux and reflux of health, disease, temperature, electricity, chemical relations, &c. &c. should present molecular as well as more appreciable alterations in all parts of its structure. In fact, we may fairly infer the existence of such changes without any departure from sound reasoning; but we do leave a sound and stable standpoint when we argue upon such inferences as established facts, when we declare all mental conditions as produced by changes which are merely inferred, and connect the explanations of various vital phenomena with their existence. It would be quite as legitimate to advance the postulate that, given the existence of such changes, they must be not merely the concomitants, but the consequences of different acts or stages of consciousness, whether these consist in ideas, emotions, or will. Nay, were it desirable to construct a rival hypothesis, ample materials might be found in the influence of study, excitement, the preliminary stages of insanity, in inducing actual and perceptible alterations in the brain. But we shall leave this edifice to some more ingenious architect, and conclude by remarking that dependance upon mere inferences in so grave an inquiry reminds us of the profane prayer of the wretched Thistlewood immediately before his death:—"Oh, Supreme Being, if there be a Supreme Being, have mercy upon my soul, if I have a soul!" We are inclined to admit, as adumbrations of impending revelations, the molecular basis of animal solids, movements in chyle and pigment-granules, when removed from the body; the chemical and electrical actions in dead or even living nerve-fibre and the deductions of Du Bois Raymond, "that nerves must be composed of peripolar molecules; of molecules, that is to say, with the negative electricity gathered around the poles, which point to the ends of the fibre, and with the positive electricity arranged as an equatorial belt between the polar regions;" but such an admission is the full extent which experience justifies. It would be unreasonable to object that physiologists should elect to remain dwellers in a *terra incognita*, or even of the debatable land which connects the demonstrable from the undemonstrated, or that they should place the mechanism and modes of nervous action among open

questions, or even that they should cling to creeds which have the sanction of long experience and little more in their favour; but it seems a deceptive and dangerous policy, though now very prevalent, to reason inferentially from certain analogies, and then to hold the inferences as discoveries, as established facts, as dogmata to be accepted. This is not merely apostacy from the inductive method, but the creation of a vague and misty speculation, which, unworthy of the name of hypothesis, deserves that of the Romance of Physiology. It would not be profitable to criticise the chapter on psycho-therapeutics, nor to comment upon the semi-credence of the author in the efficacy of Mesmerism and Braidism as remedial agents. Due care has, perhaps, been taken in the following paragraph :—"If cures of disease are performed by a magnetic influence passing from A to B, they are not (as has already been intimated) illustrations of the influence of A's mind upon A's body, the phenomena with which alone we are now concerned" (p. 408). To guard against the more exaggerated development, of these doctrines, but even allowing that subjective phenomena did issue from any possible relation of A to B, and cheerfully conceding the incalculable benefits which may flow from high intellectual endowments and moral qualities, and the faith and reliance reposed in these in the amelioration of human suffering, and in the inspiration of the strength of hope, we would be reluctant to prescribe or prefer any such means when precise and trustworthy, though not specific, physical remedies are accessible, or to admit into the pharmacopœia, as Dr. Glen appears to do, ternary or quaternary compounds of courage, confidence, hope, &c.; while we contend that coincidence or spontaneous absorption would be more legitimate alternative theories in the following cases :—A. "Opacities in the cornea of the eye have been frequently made to disappear. I am acquainted with a woman whom this disorder, produced by smallpox, had deprived of the use of one eye, and who recovered it while being magnetised for another disease" (Delenge, p. 407); and while B, in the case of the cure of deafness in the deaf and dumb by hypnotism we would conclude that the patient was not deaf, that his attention had been roused and directed to the development and education of a dull and imperfect, but not a non-existent sense, and that the *opinion* of the head master as to the complication of deafness was not sufficient evidence upon the point. We are bound to admit the appropriateness of reviewing and reventilating the whole subject in such a work.

We have passed over many questions open to doubt and dispute, and have scarcely adverted to the imperfect authenticity and accuracy of many of the illustrations from which important

deductions are drawn; but we have examined this volume rigidly and carefully, because we regard it as a valuable contribution to medical literature, as the first scientific attempt to systematise and elucidate the vast number of facts or narratives connected with the interdependence of mind and body, accumulated during long periods of observation, or scattered through various and dissimilar treasure-houses, and because the efforts to make clear what has hitherto been obscure and to reduce within the confines of philosophy what has hitherto been mainly left within the domain of crude and vulgar speculation are, in every instance, painstaking, honest, and supported by the testimony of others, even when they are not satisfactory nor successful.

IX.—The Pathology of Bright's Disease.[1]

At a meeting of the Royal Medical and Chirurgical Society, on the 10th December, 1872, during the discussion which followed the reading of the second paper whose title we have given below, it was moved by Dr. Sibson, seconded by Dr. Quain, and carried unanimously—"That it be recommended to the Council of the Society to appoint a Committee to inquire into the condition of the walls of the heart and arteries in relation to the state of the kidneys in chronic Bright's disease."

At the annual meeting on the 1st March the President, Mr. Curling, stated that "the motion for the appointment of a Committee had been under consideration, with a view to carry it into effect, but it had been found impossible to secure the co-operation of Fellows whose decision would carry sufficient weight." It would be a waste of time to speculate on the circumstances which led to the failure of the attempt to obtain the co-operation of a sufficient number of influential Fellows to form the proposed Committee. It is probable that those who were nominated to serve on the Committee were of opinion that by the terms of the resolution they would be required, not merely to examine and report upon the specimens which had been exhibited by Sir William Gull, Dr. Sutton,

[1] 1. *On the Pathology of the Morbid State commonly called chronic Bright's Disease, with Contracted Kidney* ("*Arterio-Capillary Fibrosis*"). By Sir W. Gull, Bart., M.D., D.C.L., F.R.S., and Henry G. Sutton, M.B., F.R.C.P. 'Med.-Chir. Transactions,' vol. lv, pp. 273—326.

2. *On the Pathology of Chronic Bright's Disease with Contracted Kidney; with especial reference to the Theory of "Arterio-Capillary Fibrosis."* By George Johnson, M.D., F.R.S. 'Proceedings of the Royal Medical and Chirurgical Society,' vol. vii, No. III, pp. 101 to 105.

and Dr. George Johnson, but also to investigate for themselves the condition of the heart and arteries in relation to Bright's disease; and this inquiry, it may have been thought, would involve a greater expediture of time and labour than the proposed members of the Committee were able or willing to devote to the subject.

Failing, then, an authoritative report by a competent Committee, we propose to give a brief history of the controversy between Sir William Gull and Dr. Sutton working conjointly on the one hand, and Dr. George Johnson on the other; and we will endeavour to set forth the facts, or what appear to be facts, in such a manner as shall assist our readers in forming a judgment upon the questions in dispute.

So long ago as the year 1850, Dr. Johnson, in a paper published in the 33rd volume of the 'Medico-Chirurgical Transactions,' stated that in the advanced stage of chronic Bright's disease, in all its forms, but more especially in the contracted granular kidney, he had found the muscular walls of the minute renal arteries much hypertrophied. In a woodcut illustration he represented the hypertrophied renal artery with an inner longitudinal and an outer circular layer of fibres, both apparently muscular, and of about equal thickness. In that paper no reference is made to the condition of the minute arteries in any other tissue than that of the kidney. Many years afterwards, in the 51st volume of the 'Medico-Chirurgical Transactions,' the same author published a paper, in which, restating the fact of hypertrophy of the muscular walls of the minute renal arteries, he made the additional announcement that, when in the advanced stages of chronic Bright's disease there is hypertrophy of the left ventricle of the heart without disease of the valves or of the large arteries, the cause of the cardiac hypertrophy is to be found in the fact that the muscular walls of the minute arteries in the various tissues and organs have undergone a marked degree of hypertrophy. His theory is that, in consequence of degeneration of the secreting tissues of the kidney, the blood is deteriorated, partly by the loss of its normal constituents, but mainly by the retention of urinary excreta; that this abnormal quality of the blood excites undue contraction of the minute arteries, and, as a result of this, an impeded circulation; the left ventricle of the heart, therefore, beats with increased force to drive the blood through the resisting arterioles; and the physiological result of the long-continued over-action of the two antagonistic muscular forces is that the walls of the heart and those of the minute arteries become simultaneously and in about an equal degree hypertrophied.

This theory, it must be admitted, is in accord with the most recent physiological doctrines relating to the forces concerned in the circulation of the blood; and in confirmation of his theory the author exhibited specimens of minute arteries from various tissues, the walls of which were apparently much thickened by an increase of normal muscular tissue. Five of the specimens are represented by chromo-lithographs appended to the paper in question.

The last volume of the 'Medico-Chirurgical Transactions' (vol. lv) contains a paper by Sir William Gull and Dr. Sutton, in which they deny, or speak doubtfully, of the existence of hypertrophy of the arterial walls, and give an entirely novel interpretation of the cardio-vascular changes, and their relation to chronic renal disease. To this paper, again, Dr. Johnson has replied by another, which will probably be published in the next volume of the 'Transactions.' Meanwhile, from the abstract which appears in the 'Proceedings' of the society, we have an outline of the reply which he makes to the criticism and the pathological theories of his opponents.

We will endeavour, as briefly as possible, to set forth the arguments on either side.

Sir William Gull and Dr. Sutton maintain that there is a diseased state characterised by what they designate a "hyalin-fibroid" degeneration of the minute arteries, this morbid condition occurring in the arterial wall external to the muscular coat, and a similar degenerative change also affects the capillaries. They therefore propose to call the disease "arterio-capillary fibrosis." They consider it probable that this morbid change commonly begins in the kidneys, but there is evidence of its also beginning previously in other organs. The contraction and atrophy of the kidney are looked upon as "part and parcel of the general morbid change," and it is believed that the kidneys may be but little, if at all affected, whilst the morbid change is far advanced in other organs. The morbid changes in the kidney and in the tissues, "though allied with senile alterations, are probably due to distinct causes not yet ascertained." Their explanation of the manner in which the vascular disease causes the cardiac hypertrophy we give in the authors' own words:

"The hyaline-fibroid material in the walls of the arterioles must be an impediment to elasticity, and it can be experimentally shown that greater force is required to propel a fluid continuously through a non-elastic than through an elastic tube. The left ventricle, therefore, owing to this diminished elasticity of the arterial walls, has of necessity to contract with greater force to carry on the circulation."

This is, in brief, the theory of Sir William Gull and Dr. Sutton; to which Dr. Johnson objects that the appearances which are described as a "hyalin-fibroid" change in the minute arteries are a post-mortem physical result of the distension of the fibrous tunic of the arteries by the mixture of glycerine and camphor-water, in which all the specimens were mounted before they were examined by the authors of the new doctrine. He maintains that the hyaline appearance in question is never seen in the vessels examined immediately after their removal from the body, or in vessels preserved in dilute spirit, or in a solution of salt having a specific gravity of 1030.

Dr. Johnson states that the "hyalin-fibroid" appearance produced by the imbibition of glycerine may be seen in vessels from subjects at the two extremes of infancy and old age, and when death has resulted from disease having no relation with Bright's disease, and unassociated with hypertrophy of the heart.

After the reading of his paper he showed specimens mounted in glycerine and camphor-water, and presenting the hyaline appearance, from the pia mater of a woman who died of diabetes, and whose heart weighed only six ounces and three quarters; from another woman who died of cancer of the ovary, and whose heart weighed eight ounces; from an infant, æt. 11 months, who died from spasm of the glottis—the kidneys, heart, and all the viscera being quite healthy; and from a boy, æt. 15, who died from typhoid-fever, having been in good health until the attack of fever which killed him. There was no post-mortem evidence of disease, except such lesions as resulted from the fever.

In order to show that the hyaline appearance of the arteries was a post-mortem result of maceration in glycerine, corresponding arteries from the same subjects, mounted, some in dilute spirit and others in salt and water sp. gr. 1330, were placed side by side with the others, and, being unchanged by the liquid in which they were immersed, the vessels presented a perfectly normal appearance.

It is shown that a fluid slightly acidulated rapidly passes in and distends the fibrous tunic, rendering it "hyaline;" then the neutralization of the acid by ammonia occasions a rapid shrinking of the before-distended tunic, which again assumes its normal fibrous appearance. On the occasion referred to two specimens of arteries from the same pia mater were placed side by side. In the one specimen the arteries had their fibrous tunic distended and rendered hyaline by the action of a slightly acidulated fluid; in the others the vessels, after undergoing distension by the acid fluid, had been made to resume their normal appearance by neutralizing the acid with ammonia.

The physical conditions which favour the imbibition of the simple unacidulated mixture of glycerine and camphor-water would, it is suggested, be influenced by various circumstances, such as the mode of death and the period after death at which the examination is made; and obviously it is incumbent on those who maintain that the appearances in question are the result of pathological processes to demonstrate them in vessels which have been unchanged by artificial agents after their removal from the body.

Referring to fig. 7, plate xi, in the paper by Sir William Gull and Dr. Sutton, Dr. Johnson declares that they ignore the fact that in the renal arteries there is an inner longitudinal and an outer circular layer of muscular fibres, and in a transverse section of such an artery they describe the inner longitudinal layer as alone muscular, while the outer circular layer of muscular fibres they erroneously suppose to be identical in structure and position with the so-called "hyalin-fibroid" layer in the arteries of the pia mater! So gross a blunder as this is sufficiently damaging to the reputation of its authors. It is also affirmed that, while they deny the existence of hypertrophy, some of their own specimens and drawings which were exhibited at the conversazione of the Royal Medical and Chirurgical Society, soon after the reading of their paper, were, in fact, good examples of hypertrophy of the muscular walls of the arterioles, and we can bear testimony to the fact that this was the opinion of some highly competent observers, who carefully examined the specimens on that occasion.

Again, Dr. Johnson maintains that Sir William Gull and Dr. Sutton have mistaken other appearances produced by glycerine on healthy vessels for pathological changes; for instance, the imbibition of glycerine often renders the muscular coat of the arteries pale and confused, and this is described as atrophy of the muscular tissue.

Minute arteries in the pia mater, having their walls distended by the glycerine fluid, are described as thickened capillaries; and the separation of the inner from the muscular coat by the imbibition of glycerine gives an appearance represented in figs. 3 and 4 of their fifth plate, which is erroneously described as a morbid thickening of the inner tunic of the artery.

Sir William Gull and Dr. Sutton believe that the hypertrophy of the heart is not a result of the renal degeneration, but rather that the cardiac hypertrophy and the atrophy of the kidney are joint results of an antecedent "arterio-capillary fibrosis."

In proof of this position they refer to six cases in which it is said that the cardio-vascular changes were present while the kidneys were either healthy or but little changed in structure.

Commenting upon these cases, Dr. Johnson maintains that in each case the hypertrophy of the heart was due, not to changes in the minute blood-vessels, but to other obvious causes. Thus, in one case (No. 7) there was emphysema of the lungs and bronchitis, and so the impeded systemic circulation resulting from long-continued partial apnœa may probably have caused the cardiac hypertrophy. In one case (No. 10), the patient being sixty-nine years of age, there was senile gangrene, which obviously implies extensive degeneration of the larger arteries. In one case (No. 20) there was disease of the aortic valves, which is usually considered a sufficient explanation of hypertrophy of the left ventricle. One patient (Case 2), who was sixty-three years old, may probably have had senile degeneration of the arteries, but, in addition, her kidneys, weighing fifteen ounces, could scarcely have been free from disease. Another patient (Case 3) was seventy-seven years of age, and, therefore, probably had ossified arteries, while in a sixth case (No. 19), the age being sixty-two, there was atheromatous degeneration of the cerebral, and probably, therefore, of the other arteries, the lungs were very emphysematous, and the kidneys together weighed only eight ounces. So that, as Dr. Johnson remarks, "It is assumed by Sir William Gull and Dr. Sutton that kidneys weighing as much as fifteen ounces and as little as eight ounces were alike free from disease."

It is an old and a well-established doctrine that the loss of elasticity which results from atheromatous and calcareous degeneration of the larger arteries involves a loss of propelling power, impedes the passage of the blood, imposes extra work upon the left ventricle, and so explains cardiac hypertrophy; but Dr. Johnson maintains that the attempt to explain hypertrophy of the heart by degeneration and consequent loss of elasticity of the minute arteries is physiologically untenable, since in this attempted explanation the elasticity of the large arteries which co-operates with the heart as a propelling force is confounded with the muscularity of the smallest arteries which antagonises the heart. Degeneration of the muscular walls of the arterioles would involve, not an increase, but a decrease of resistance to the blood-stream, and, therefore, would not explain the cardiac hypertrophy.

It is admitted by all observers that hypertrophy of the heart is more frequently associated with the small granular kidney than with the large white kidney. Dr. Johnson explains this by stating that, in connection with the large white kidney, the blood is usually diluted and watery, while with the small granular kidney there is a state of uræmia; and it is suggested that this more concentrated form of blood-con-

tamination excites greater contraction of the minute arteries than the more watery blood which results from the large white kidney; and, further, it is pointed out that with the lardaceous degeneration of the kidney the walls of the minute arteries throughout the body have a tendency to undergo degenerative changes by which their contractile and resisting power is impaired.

In reply to the statement of Sir William Gull and Dr. Sutton, that the morbid changes associated with the contracted kidney are the result of "causes not yet ascertained," Dr. Johnson maintains that, in the majority of cases, the renal degeneration may be traced, with a high degree of probability, to an excessive consumption of food and of alcoholic stimulants, either with or without gouty symptoms, but he has seen many cases in which the disease appeared to be a result of long-continued dyspepsia in persons of strictly temperate habits. His doctrine is that the proximate cause of the renal degeneration is to be found in the excretion of abnormal products by the gland-cells, which, in the discharge of their excretory functions, undergo destructive changes. The renal degeneration, therefore, appears to be a result of the physiological function of the kidney as one of the main blood-purifying organs.

X.—Chapman on "Neuro-Dynamic Medicine."[1]

THE nature and cause of *pain* have long been subjects of anxious inquiry by physiologists and pathologists, but it cannot yet be said that the problem has received a satisfactory solution. *Causa latet vis est notissima*, and a sensation which is experienced more or less, at one time or another, by every human being, is almost inexplicable as to its origin. Regarding what is called its *final cause*—that is to say, the purpose for which pain appears to exist, or the object it is intended to accomplish—it is, of course, the warning it conveys as to the existence of danger, and as to the mode of obviating or escaping it; but considerations of this kind belong to the province of the metaphysician rather than to that of the physician, whose province it is to examine into morbid conditions only with a view to the alleviation of human suffering. Hence, in medical books, although pain is repeatedly referred to as a symptom of disease,

[1] *Neuralgia and Kindred Diseases of the Nervous System; their Nature, Causes, and Treatment; also a Series of Cases, preceded by an Analytical Exposition of them, exemplifying the Principles and Practice of Neuro-Dynamic Medicine.* By JOHN CHAPMAN, M.D., M.R.C.P., M.R.C.S. London, 1873. Pp. 512.

very little information is given, or, perhaps, can be given, as to the essential nature of pain itself, further than that it is some condition of a nerve or nerves which is accompanied by a disagreeable impression. If there were no nerves in the body there would be no pain, and on the other hand there would be no pleasure; but such remarks, again, belong rather to metaphysics than to medicine. The study of the nervous system, however, has led to such discoveries as to the respective functions and properties of different nerves as to reflect immortal honour upon the discoverers, and to place them upon the highest niche among the prominent benefactors of mankind. It is needless to refer to Sir Charles Bell, to Marshall Hall, and to Brown-Séquard, as among the foremost in the ranks of those who have successfully cultivated the physiology of the nervous system; but when we turn from what has been written upon the function of healthy nerves, to the theories and speculations and practice which have been proposed in reference to the diseases of the nervous system, we become lost in perplexity when attempting to reconcile conflicting views or to connect the results observed with the original influences from which these results have sprung.

Dr. Chapman, as far as we know, has himself made no original physiological researches in reference to the functions of the nervous system, but he deserves great credit for the industry he has displayed in examining the results obtained by other investigators, both English and foreign, and for the diligence with which he has collected from a multitude of sources a great array of facts and opinions bearing on the subject of neuro-pathology. That his selection is always judicious we are by no means prepared to assert, and it strikes us that he often quotes from books and from authors whose opinions are not founded upon sufficient acquaintance with facts, but whose views, if agreeing with those of Dr. Chapman, are paraded with a prominence equal to that accorded to the great leaders of physiological and pathological science. So, again, if he deserves approbation for the persistent and laborious manner in which he collects pathological and therapeutical phenomena observed by himself; on the other hand he lays himself open to animadversion for the somewhat dogmatic tone he assumes in maintaining his own views, the complacency with which he regards his own plans (or rather plan) of treatment, and the pertinacity with which he advocates, as the panacea for human ills, in season and out of season, intra-professionally and extra-professionally, that special method of treatment which, to make use of the mildest expression, is still *sub judice.* Hence, among the general public, if we are to judge by the opinions expressed by the newspapers, he has met

with a reception in various quarters of the most opposite kind, some regarding him as the great discoverer of a plan of treatment in comparison to which all others are but empirical gropings, and others looking upon him as a modern Sangrado, who recklessly recommends only one kind of treatment for every conceivable form of human disorder. We consider him neither a William Harvey nor a Sangrado, and in the following review we hope to do justice to his theory and his practice, though we may be excused for sometimes questioning the validity of the one and the universal applicability of the other.

We must remark, in the first place, that although the word "neuralgia" is the prominent one in the title of Dr. Chapman's book, he includes a great many affections in his pages which, in the ordinary language of medicine, are not comprehended under that name. Indeed, he seems to use the word in its etymological rather than its medical sense, for he evidently employs it to denote any or all nervous pain, whatever may be its intensity, manifestations, or associations; and accordingly we find described among its predisposing causes gout, rheumatism, cancer, pulmonary tubercles, and sleep (?); and among the exciting ones, teething and the diseases of the teeth, disorders of the bowels, piles, intestinal worms, uterine maladies, drunkenness, aneurisms, cancerous growths, enlarged glands, hernia, diseases of bones, syphilis, punctured wounds, the presence of foreign bodies, &c. &c. All these conditions and circumstances are or may be, of course, the causes, predisposing or exciting, of nervous pain, and we find no fault with Dr. Chapman for enumerating them, and our only object in pointing out the use made of them by the author is to show that the sphere of his inquiries and discussions passes far beyond the limits of what is known as "neuralgia" by the medical profession, and transfers the word to the category of general terms.

Having thus proposed for himself a very wide field of investigation, including, in fact, almost the whole range of medical and surgical pathology, it became incumbent upon him to investigate also the proximate cause of neuralgia, that is to say (adopting his wide definition of neuralgia) the essential nature and origin of pain in general. As he here enters the territory of doubt and speculation, he, of course, also approaches that of controversy, and as in former writings he has given sufficient evidence that he is a spirited antagonist, so in the present volume he exhibits no falling off in that respect. He is, however, especially opposed to the views of Dr. C. B. Radcliffe and Dr. Anstie, which he vigorously denounces, inasmuch as he holds doctrines diametrically opposed to theirs. Both those physicians are honorably known by their writings or investigations on the

physiology and pathology of the nervous system, and although they have not, perhaps, solved the difficult problems presented to their notice, yet they have attracted attention by the ingenuity of their reasonings and the value of their suggestions as to the treatment of disease.

With regard to the genesis of pain, these physicians hold opinions which, though not identical, are in accordance with one another. It is not easy to represent their views in a very brief and, at the same time, intelligible form, because some of the experiments on which they rely are rather complicated in their details and some of their reasonings are somewhat recondite. But Dr. Radcliffe assumes that during each act of normal feeling there is an electric change in the feeling nerve and in the sensory centre to which it is related; that this change consists in a temporary reversal of the ordinary electric state of the nerve and nerve-centre in question; that this change is accompanied by a discharge or loss of "natural electricity;" that while this reversal exists the vitality of the nerve and nerve-centre is lower than it is when the nerve and nerve-centre are not performing their function; that hence there is a diminution of the normal supply of arterial blood or of the circulation of venous blood in the nervous centres, or, in other words, that a lowering of their vitality increases their sensibility or irritability, and, therefore, constitutes the indispensable condition precedent of pain. The practical conclusion is that pain in general denotes a depressed condition of some nerve or nerve-centre, and Dr. Radcliffe points to the fact that pain of a neuralgic character may be associated with a very depressed condition of the circulation, and he considers, moreover, that such pain would seem to be antagonised rather than favoured by an over-active condition of the circulation.

Dr. Anstie arrives at pretty nearly the same conclusion, but by a different process, and he puts forward the proposition that the nerve-centres of sensory nerves affected with neuralgia are in a state of atrophy, which is usually non-inflammatory in origin; and, with regard to various painful disorders, he maintains that pain is not a true hyperæsthesia, but, on the contrary, that it involves a lowering of true function; that its existence is due to a perturbation of nerve-force, originating in dynamic disturbance either within or without the nervous system, and that the susceptibility to this perturbation is great in proportion to the physical imperfection of the nervous tissue, *until* this imperfection reaches to the extent of cutting off nervous communication (paralysis). Dr. Anstie thus agrees with Dr. Radcliffe in regarding pain as being due to a lowered condition of vitality.

Dr. Chapman, however, who, as he triumphantly announces, "fortunately holds in his hands a valuable instrument of experimentation and verification, which other labourers in the same field have not had the advantage of using" (p. 22), holds an opinion the very reverse of that supported by Drs. Radcliffe and Anstie. He does not entertain the slightest doubt that all functional disorder is expressive of material change; and although he admits that in some cases of neuralgia there are no visible evidences of morbid change at the seat of pain, yet he maintains that the seeming contradiction is easily explained by the fact that all pain is expressive of centric disease. In other words, he would argue that a pain—say of the eyebrow, or cheek—is due to some morbid change at the sensory root of the fifth nerve. The theory which Dr. Chapman believes "adequate to explain all the phenomena of neuralgia, which indicates a successful method of treating the disease, and the truth of which seems to be proved by the result of its practical application," is summarised in a series of propositions, which are as follows:—(pp. 23 and 24) 1. Pain, whatever may be its exciting cause, and whatever may be the structure in which it is felt, is like ordinary sensation a phenomenon of functional change in the sensory centre into which the affected nerve is rooted. 2. The nature of the functional change denoted by ordinary sensation, and the nature of that denoted by pain, are essentially identical, the difference in the two being only a difference of degree of rapidity or intensity with which the change occurs. 3. Pain, like ordinary sensation, is of various degrees of intensity, and whereas pain denotes a more rapid functional change in the affected sensory centre than occurs during ordinary sensation, the successively higher degrees of intensity of pain are expressive of successively higher degrees of rapidity of functional change in the functioning sensory centre. 4. Whereas an indispensable condition of these functional changes in the sensory centre which are comprised within what may be termed the ordinary sensory scale is a normal supply of arterial blood—in order to provide for those transformative changes which are at once chemical and nutritive, and which constitute the groundwork and possibility of functional change, so a supply of arterial blood greater than normal is an indispensable condition of those more intense functional changes in the sensory nerve-centre comprised within the wide range of what may be called the neuralgic, or, more generically, and perhaps more correctly, the *algic* scale. 5. Pain is not necessarily a morbid phenomenon: in its beginnings it is rarely, if ever so, in otherwise thoroughly healthy organisms; but if in such organisms, the operation of its exciting cause be long continued,

it will induce in the affected sensory centre a habit of morbidly intense functional activity so that at length, when that habit is generated, it will persist even after its cause is removed. 6. A neuralgic habit thus generated may be transmitted hereditarily; and althongh it may remain latent during a considerable time, it may be suddenly lighted up by some exciting cause so slight as to escape observation, and thus constitute in the second generation what is sometimes designated spontaneous or idiopathic neuralgia. 7. The general doctrine expressed in the above propositions in respect of pain, is, *mutatis mutandis*, applicable to the several phenomena constituting the complications of neuralgia, which consist generically in disorderly actions of muscles—voluntary and involuntary, of morbidly excessive actions of glands, and of disorderly processes of local nutrition.

We have transcribed the foregoing propositions literally, word for word, and have also carefully copied the punctuation, except in one instance, where a comma was obviously inserted after the wrong word; and this we have done in order that Dr. Chapman's views may be expounded by himself and in his own language. If a single word had been altered or a single sentence paraphrased, or if we had modified the punctuation in any way, we might, though quite unintentionally, have been thought to misrepresent, or perhaps imperfectly to represent, his opinions, and we therefore leave them as they are to speak for themselves. To our own mind they are not very clearly expressed, and the very precision of language obviously aimed at by the author seems to us to defeat its object, for there are so many parentheses and modifications in most of the leading sentences that it is very difficult in the case of some of them to apprehend their true meaning. However, it is plain that he differs from Dr. Radcliffe and Dr. Anstie as to the proximate cause of pain, and that he looks upon it as a result of *increased* vital activity, or hyperæmia in the nerves or nerve-centres.

It has been objected to the theories put forward by Dr. Radcliffe and Dr. Anstie that they are not supported by sufficient proof, and, in fact, neither the "electric" theory of the first physician nor the "atrophy" hypothesis of the second can be put to the test of actual demonstration; but Dr. Chapman tells us that "the proposition that there is hyperæmia of the affected nerve-centre in all cases in which pain is felt, is susceptible of decisive proof" (p. 24). This proof seems to be drawn from the effects of the ice-bag and of hot water to the spinal cord, but we are not quite sure that the profession will look upon such "proof" as more cogent in favour of Dr. Chapman's views than those brought forward by his two antagonists in favour of theirs.

After all, however, the great aim of the physician and the surgeon is to alleviate human suffering, and if Dr. Chapman has discovered a method by which this object is attained, he deserves the thanks of the profession and the community, whether the theory on which his treatment is founded be correct or not. If so excruciating a malady as neuralgia (even in the restricted and usual acceptation of the word) can be cured by the application of a spinal ice-bag, by all means let the fact be admitted and the remedy be received among the category of beneficial therapeutic agents; and if, as Dr. Chapman seems to assert, a whole host of maladies more or less related to neuralgia can be cured by the same method, the gratitude due to the discoverer of so beneficent a treatment is proportionally augmented. But without venturing to cast any doubt on the genuineness of the cases recorded by Dr. Chapman, we must take leave to observe that the domain of what are called "nervous diseases" has always offered a fertile field for experimental medicine; and while, on the one hand, some of these maladies have resisted all therapeutical measures brought to bear against them, others, on the contrary, have yielded, or have appeared to yield, to plans of treatment the most diverse, or to no treatment at all. How often has it happened that the most torturing and agonising attack of neuralgia has resisted all remedial measures and then has suddenly ceased of itself, leaving the patient in apparently perfect health? How often has it occurred that the same disease, after inflicting its horrible sufferings on a patient for a long series of years, has ceased entirely as age advanced, and has left no trace, at least apparently, of its former existence? And extending the view over other morbid conditions of the nervous system, how often has it been observed that epilepsy (for instance) has appeared or disappeared in consequence of some functional disorder or altered condition of the system, and that hysteria (to which word Dr. Chapman seems to entertain a very strong objection), with its hundredfold manifestations, has come and gone, no one knowing why or whence or whither? Whether or not the pain in neuralgia be really due to hyperæmia of the nerve-centres, or, in other words, of the sensitive roots of the nerves affected, is a question of small practical importance, and must necessarily be incapable of proof, because the symptoms only exist during life, and post-mortem investigation can throw no light on a phenomenon which is avowedly often transient even in its most remarkable manifestations. But if the application of the ice-bag to the spine really cures these diseases and a great many more of a similar nature, it requires very strong proof, not that the cure was really effected, but that the application of the ice-bag was the *cause*

of the cure. Dr. Chapman's own pages are too full of extraordinary instances of what may be called sympathetic phenomena (for want of a better term) to allow us to suppose that the induction of a change in the condition of the blood-circulation in the sensitive roots of the spinal nerves will infallibly remedy the disorders of the nervous system; nevertheless, if it does so in a sufficient number of cases, it is then a fair subject of inquiry whether there is any *proof* that the remedial measures and the removal of the disease are to be considered in the light of cause and effect, or as accidental coincidences. The connexion of the nervous system with the outer world, and with the inner world of thought and emotion, is a mysterious problem which may well employ the mind of the philosopher or the physician, but facts at present are too strong to countenance the theory that the psychical and physical phenomena of all nervous diseases are to be explained on the assumption that there exists a drop or two too much or too little blood in a nerve root and that those diseases are to be cured by diminishing or promoting a supposed hyperæmia.

Such, however, is the foundation of Dr. Chapman's so-called 'Neuro-Dynamic Treatment of Neuralgia and Kindred Diseases.' "In every case," he says, "in which spine-bags are employed remedially the chief and essential feature of the influence exerted is *force*—δυναμις. Expansion and contraction of blood-vessels, whether in the nervous centres themselves by direct action, or in the periphery of the body by indirect action, are the primary effects produced by modifying the temperature along the spinal cord; and in every case in which a patient is cured or benefited, the cure or melioration is a secondary consequence of those primary effects" (p. 283). It is only right to state that Dr. Chapman employs both *heat* and *cold* in his treatment, and he therefore justly repudiates the term "ice-treatment" as applied to it, and considers *neuro-dynamic* as the most accurately descriptive, the most simple, and the least objectionable he knows.

It is by no means easy to follow Dr. Chapman through the reasonings on which he founds his system of "Neuro-Dynamic" Medicine. Some of them appear to us to be purely hypothetical, while others, even if true, do not obviously lead to any definite conclusion. We can gather, however, that he attributes the chief pathogenetic influence in neuralgia and its allied diseases to the sympathetic nervous system and its function in regulating the diameters of the blood-vessels throughout the body. We have already shown that while other authorities regard spasm and convulsions and pain as the expressions of an adynamic or anæmic condition of the central portions of the nervous

system, Dr. Chapman considers that they indicate an opposite state, namely, one of excitement and hyperæmia. Then he states that the sympathetic ganglia and the spinal cord can be rendered hyperæmic or anæmic artificially by means of heat in one case and cold in the other, applied along the spine; and he maintains that cold applied in that region will subdue cramps or excessive tension of both voluntary and involuntary muscles, and will also lessen sensibility and secretion. The application of heat along the spine, he says, will in some cases induce cramps of both voluntary and involuntary muscles, and will increase sensibility and secretion, and will lessen the general circulation and the bodily heat. The truth of these propositions he believes to be fully established, and "*assuming it to be so,*" he expects that "by means of the proper application of cold and heat along the spine, a remedial power over neuralgia and its kindred disorders far greater than is conferred by any of the agents already passed in review is now within our reach" (p. 285). The "agents passed in review" are those generally employed in the treatment of neuralgia, such as quinine, opium, belladonna, arsenic, bromide of potassium, chloride of ammonium, aconite, nitrite of amyl, iron, strychnia, zinc, phosphorus, hydrate of chloral, chloroform, ether, alcohol, turpentine, the actual cautery, acupuncture and electro-puncture, cyanide of potassium, veratria, &c.

The chief "proofs," as it appears to us, on which Dr. Chapman relies in support of his opinions and his practice are the therapeutical results he has obtained, and he devotes no less than 138 pages to the record of a series of 100 cases treated by his method. All these cases are successful, and in the few instances where the results were not quite so satisfactory as might be wished the reason was that the patients were unable to pay for sufficient ice. The cases are of the most miscellaneous character, including all kinds of diseases of the uterus, as menorrhagia, leucorrhœa, uterine neuralgia, amenorrhœa, dysmenorrhœa; also epilepsy, diabetes, paralysis, diarrhœa, sciatica, dental neuralgia, headache, constipation, blindness (!), smallpox (!), anorexia, indigestion, flatulence, cutaneous irritation, cough, gout, &c. Happily for the patients, but unfortunately for the interests of science, not one death occurred, so that it is impossible to gather any confirmation or otherwise of Dr. Chapman's theory by means of pathological investigations pursued by the scalpel or the microscope.

As we have previously remarked, we do not doubt his facts, but we may ask whether there are any unsuccessful cases, and whether these have been recorded with the same fidelity as the successful ones? If, too, the effects of the ice-bags are so effi-

cacious in curing or relieving a large number of the diseases of the nervous system it would be desirable, as it certainly would be practicable, to obtain from our great public hospitals some extensive series of results of a treatment which, in Dr. Chapman's hands, appears to have been so beneficial.

The book has all the merits and all the demerits of an *ex parte* representation of facts, set forth with no mean amount of confidence by the author. Both the modes of treatment and the hypotheses upon which it is based have an apparently charming simplicity, calculated to attract the general public, who are always delighted to be admitted to the arcana of physic, and to fancy themselves on a level with the physician in apprehending the mystery of disease and its treatment. As just now remarked, it is a duty particularly incumbent on those who possess special opportunities for experiment and observation in hospitals to determine the value of the therapeutical method so vaunted by Dr. Chapman; but of this we may be sure, that those of the public who delight in medical marvels will, in their own way, put to the test the merits of hot- and cold-water spinal bags, so confidently recommended and so assiduously advertised.

Bibliographical Record.

Recent Medical Periodicals.—Within the past year or eighteen months several new periodicals devoted to recording medical doctrines and practice have been launched into the world; and as we have been favoured by copies of some of them, they claim so much notice in these pages as may suffice to indicate their purpose and scope. British periodical medical literature is enriched by a few new journals, but still when compared with the like literature of France, Germany, and (as we think may now be added) the United States of America, it remains far less copious. The first new journal we feel called upon to notice is *The Birmingham Medical Review*, a quarterly journal of the medical sciences. As the whole series has not reached us, we must form our opinion of it from the two numbers we have in hand, viz. No. 1, published in January, 1872, and No. 5, the first number of the present year. It is published in Birmingham, and may be taken as indicative of a healthy provincial feeling of self-help and independence, and of a desire on the part of the Birmingham practitioners to vindicate for themselves a status in medical periodical literature, and the ability to contribute in no mean proportion to the general stream of medical knowledge issuing from the press. We attribute these sentiments and motives to the Birmingham practitioners because, although the editor looks farther afield than Birmingham and its vicinity for contributions, yet the moving spirit, the scheme and, commercially, the speculation itself, are to be found in that enterprising town.

It was started with four departments, viz. original communications, reviews, and bibliographical notices, jottings at the societies, and a periscope. In the last example in our hands we notice that the reviews and notices take the first place, and the jottings at the societies have dropped out. To our apprehension these changes are not improvements, for we feel confident that good original communications, having practical issues, will be better and more largely appreciated than reviews of books. These latter, indeed, have a better field in other established periodicals, in which, too, either greater space affords more scope to make them more fully analytical or more frequent publication gives occasion for more speedily bringing books

under the notice of readers. If original communications, after having yielded precedence to reviews, suffer reduction in quantity or in quality, it may be legitimately urged that the Birmingham men have failed to establish their ability to maintain a journal representative of the scientific knowledge and practical experience they possess. In short, simply as a review of medical books and a periscope, it could not carry on an existence; and, in plain terms, could not justify its existence.

The 1873 number we also regard as less complete in the table of contents, for in it the matters noted in the periscope are not stated, as in the first part issued, and the reader must, therefore, turn over each page of the periscope to discover them. It strikes us, moreover, that the "jottings at the societies" ought to be a useful section of the journal, if well selected, for Birmingham has at least one large medical society, in connection with the British Medical Association, having sections for the special pursuit of microscopical and pathological science. It might, consequently, be fairly supposed that useful notes could be culled from the proceedings, for which the local medical journal would be the best medium of communication.

We shall not single out particular papers for remarks, but speak generally of the original communications as of a valuable character, creditable to the distinguished Birmingham practitioners who have produced them, and we may note, in passing, that the names of many of the leading medical men in Birmingham do not appear, as we should have looked for, as contributors.

Although we have thus freely animadverted on this new journal, we can confidently recommend it as a worthy addition to the English periodical press, and wish it success.

Food, Water, and Air, in relation to the Public Health, is a literary venture of Dr. A. Hill Hassall, the well-known veteran in chemical and microscopic researches respecting food and drink, than whom none can claim more attention in such matters.

Although the subject of food, water, and air includes all the elements concerned in public health, we cannot congratulate Dr. Hassall on the title he has taken for his paper, for to many less instructed minds its scope will, we apprehend, appear to fall within the range of sanitary medicine as but one section of it, and not of general importance.

It is a monthly publication, of quarto size, having a dozen pages, each of two columns. Its low price, threepence, puts it within the range of every man's pocket who wants to know what will improve and what deteriorate public health, and for himself what to eat, drink, and avoid.

Each number is made up of several "Leading Articles," and of a gathering of "Facts relating to the Public Health," selected from

the current literature, medical and general. Indeed, more or fewer of the "Leading Articles" themselves are transplanted productions; and, judging from the contents, Dr. Hassall has few coadjutors in contributing such articles, whether critical, analytical or original. Herein, therefore, is in our estimation a weakness of this periodical.

The next to notice is *The Medical Record*, which has made a vigorous start, and called to its aid many eminent physicians and surgeons, most of them practising in London. It is a weekly publication, but in plan departs from that of the usual periodicals. According to the editor's "proem," its "object is to supply medical men the means of receiving week by week the progress of physiological and pathological science, and of medical, surgical, and obstetric art, in all the countries in which they are successfully cultivated." The accomplishment of these objects is sought by brief and critical reviews and notices of published books and of essays and papers printed apart or published in volumes of transactions and reports. A notice of new inventions and a column or two of miscellaneous jottings and news fill up the remainder of each number.

We must, however, not fail to remark a very useful feature in connection with the analytical notices, viz. the appending of a list of the papers contained in various journals, chiefly foreign. This plan will be of great value to the student of any special branch of medicine, inasmuch as by looking through these lists for a year, or other period, he can at once learn what contributions have been made on the topic he is working at. It will, however, be incumbent on the editor to secure as complete a bibliography as practicable, and especially to see that the work done and recorded by English writers is duly catalogued. We make this remark because we have remarked lists containing only references to foreign articles. We doubt not this new periodical will be duly appreciated, as it occupies ground only partially cultivated by other journals. To maintain a successful career it must persevere in the same course, fulfilling its special mission as a faithful and full record of work everywhere accomplished, of hypotheses advanced, of questions calling for solution, and of practical improvement effected.

"Our American cousins" have produced several new journals of late years, as might be expected from the energy and the commercial activity so largely developed in them, and from their wide-spread attachment to periodical literature. The first we direct attention to is the *Archives of Scientific and Practical Medicine*, edited by Dr. Brown-Séquard, assisted by Dr. E. C. Séguin, of New York. The name of the editor is itself a guarantee for originality in research and for articles of scientific value. It is astonishing, indeed, to find him playing the part of an editor to another medical journal when he is, and has been for some time, a principal editor of the 'Archives de Physiologie,' and when we find his name attached

to lectures, to treatises, and to papers encountered on every side. All this displays extraordinary activity and industry; but it will also lead to the suspicion of hastiness and want of reflection in the work accomplished, and will be a constant source of danger of the writer repeating himself. His coadjutor, Dr. E. C. Séguin, is the son of Dr. E. Séguin who many years ago interested himself in the education of idiots, and not long since produced a small treatise on medical thermometry, reviewed in these pages.

It is a well got-up journal, creditable to the publishers, Lippincott & Co., of New York, and is to appear monthly. The first part was published last January. According to the prospectus, it is to be chiefly occupied by original papers in every branch of medical science; but besides these each part will also contain an *exposé* of the state of knowledge on some great medical question, together with translations of short foreign papers; reports on the results of experimental laboratory researches; reviews of books and bibliographical notices, and reports on the progress of medicine, surgery and obstetrics, and a miscellany.

The indefatigable editor contributes two original communications, one "On a New Mode of Treatment of Dyspepsia, Anæmia, and Chlorosis," the other on the "Effects of Injuries of Nerves;" and besides these he adds a report on the state of our knowledge "on the sudden arrest of many normal and morbid phenomena." For future numbers he likewise promises a long series of papers. Dr. Séguin appears as joint author with Dr. Sands of a paper detailing "a case of traumatic brachial neuralgia treated by excision of the cords of the brachial plexus."

We are pleased to see among the contributors the name of Dr. W. H. Draper, as author of a paper on "Cerebral and Visceral Neuralgia;" and, if for no other reason, the name of a lady medical authoress, Dr. Mary C. Putnam, will attract attention to the article on the "Significance of the Pulse in the Forming Stage of Generalised Puerperal Peritonitis." There is a pedantic smack about this title, but the subject-matter of the article appears to be an attempt to explain the clinical features of puerperal peritonitis by the pathological processes hypothetically concerned in their production.

We wish success to this well-devised literary undertaking, and hope from its contents an advancement of our scientific medical knowledge. If carried out rigidly, according to its programme, as mainly a medium for recording original work, it will occupy a hiatus in the current medical literature of America.

The Sanitarian is another new American venture, purposed to appear monthly. It is edited by Dr. A. N. Bell, of Brooklyn, and published by Barnes & Co., of New York and Chicago. Its special object is sanitary medicine, but it also gives space for brief "book

notices," apparently limited to treatises on sanitary science or on cognate subjects.

The contents of the first number include an address by Dr. Agnew on "The Importance of Sanitary Science," the outline of a bill introduced in the Senate of the United States, to establish a "Bureau of Sanitary Science;" a short paper on "Results of Sanitary Improvements of Towns," based on English returns; a brief quotation from the Vienna 'Wochensschrift,' "On Preventive Medicine;" a report of a committee of the Medical Society of the State of New York on "Infant Mortality;" a "History of the New York Quarantine Establishment," by Dr. Bell, the editor; a sensational communication, headed "School Poisoning," showing the accumulation of carbonic acid gas in some factories and schools visited by the city sanitary inspector, Dr. James; some observations on "The Necessity of Re-vaccination," and others on "Life Assurance. The most important and lengthy paper is that by the editor, who, after a sketch of the opinions held relative to quarantine during the last quarter of a century, goes on to describe the conditions and arrangements of quarantine adopted in New York and vicinity, illustrating his paper by a "Quarantine Chart of New York Harbour," and by plans of the hospitals and warehouses constructed on small islands in the harbour for the effectual carrying out of the system. So complete an account cannot fail to interest, in spite of defects in style. This sanitary periodical has assuredly a very wide field of usefulness, and should command a wide success in the United States, where sanitary medicine is, speaking generally, a recent innovation, and not as yet a matter of legislation by the general government.

A new French medical quarterly made its appearance in January last, under the title of *Revue des Sciences Médicales en France et à l'Étranger*. It is edited by Georges Hayem, an agrégé of the Faculty of Medicine of Paris, and in the prospectus sets forward the desirability of a more complete acquaintance with what is done abroad as a leading object. This aim, indeed, we regard to be a very desirable one, for French medical writers generally display a lamentable ignorance of what is achieved outside their own country, and, as is remarked by the editor, there is no journal which makes it its business to record the discoveries, the experience, and the prevalent opinions of foreign lands.

In character it does not accord with that of a review in the English sense. It presents no exhaustive critical and analytical essays of books, of their teachings, and of the professed objects of the authors in writing them, after the fashion we are accustomed to, but rather follows the fashion of compilers of medical retrospects, in making brief analyses and abstracts of the subject-matter of books, of pamphlets and of articles published in transactions, reports, and journals. Its more important articles give a critical résumé of the

facts and conclusions of the works analysed, whilst the others, the more numerous, state in a few lines the subject-matter of the production noticed, and are spoken of in the prospectus as "works for consultation." Besides the foregoing matters are lists of contributions, "Bibliographical Notes" in the several branches of medicine, indicating their source and the names of the writers.

We have looked through the 1039 pages of the first volume of this new journal, now complete in two parts, and are pleased to observe how fully it represents the work accomplished in the principal countries of the globe where medicine assumes the character of a cultivated science. We heartily wish this French and foreign review of the medical sciences a large success.

The next periodical we would call attention to is, *Lo Sperimentale*, an Italian journal, devoted to medicine and surgery, published at Florence, under the direction of Professors C. Ghinozzi, Buresi, and Landi. It is far from being a new publication, but it is new to us among the journals received, and we are glad to bring to notice this effort of our Italian brethren to advance and diffuse medical science. Its contents consist of original communications and of abstracts from other journals, along with some items of medical intelligence. The original memoirs are by Professor Morelli and Dr. Festi, Dr. Agostinelli, Dr. Casanova, Dr. Massei, and Dr. Zecchini, more or fewer of which names will be familiar to our readers who take note of the researches and teaching of our foreign professional brethren. The first paper is on diphtheria, as noticed in Florence and its vicinity, in the decenniad 1862—1872, and will be found interesting to our sanitarians. The same may be said of the second memoir which is occupied by a history of variola which prevailed epidemically at Castelfidardo in 1871-72, accompanied by comments on the necessity for revaccination, and by arguments in favour of animal vaccination. Dr. Casanova writes a physiological disquisition on the self-moving cell as the nidus of normal and pathological tissues. Dr. Massei gives a case of laryngeal stenosis, and is followed by Zecchini, who sketches the various views propounded on the origin of man, and shows himself opposed to the doctrines of the evolutionists by reference to an assortment of authorities and statements of very various weight.

The extracts from other journals and the reviews of progress are all of indigenous origin, as though medicine was uncultivated outside the Italian kingdom. The only departure from this adhesion to home-spun physic occurs in a brief bibliographical notice of an Italian version of MacCormac's 'Notes of an Ambulance Surgeon.' In recent times much scientific activity has reappeared in Italy, and its results are the appropriate material of the journals of the country, but when reviews of the state and progress of the several branches of medicine are undertaken the survey must be held as very inade-

quate which does not extend beyond the boundaries of the Italian kingdom.

Lastly, we have to introduce a greater novelty in the shape of a Spanish fortnightly medical periodical, started this year by Dr. P. G. de Velasco and edited by E. Garcia Perez. Each number bears on its face an allegorical picture, indicating the several departments of medical knowledge, which looks quaint enough in a nineteenth-century publication. The several numbers are further illustrated by portraits of eminent physicians and by woodcuts in elucidation of cases described or of other matters treated of. The woodcuts cannot be commended for accuracy of drawing or for artistic finish.

The matters treated are very various. Original articles appear on the requirements of medical schools for efficient teaching, on the method of teaching anatomy, also notes on the work done in public institutions, and papers on questions in the science and practice of medicine, surgery, &c. Besides such papers there are bibliographical notices of books, notes of cases and of pathological specimens, biographies of the eminent men whose portraits are engraved, extracts from foreign journals, reports of societies, meteorological memoranda, and news. The title selected for this journal is *El Amfiteatro Anatomico Español.'*

To make our review of recent medical periodicals more complete we will name one other, the *Correio Medico de Lisboa,* which does great credit to the profession in Portugal, and has been kindly supplied to us now for more than a year.

The growth of a vigorous periodical medical literature is a healthy sign. It is indicative of the recognition of something to impart and of something to acquire, of a pervading spirit of inquiry, of a generous rivalry in the diffusion of professional observation and experience. It moreover holds out the best prospect of so diffusing the knowledge acquired and the hypotheses advanced in one country among the other nations of the earth, that the observers and original investigators in each land may occupy a common platform, and proceed with their work in the full consciousness of what others have already accomplished in the same direction. This salutary end is especially advanced by those journals which present from time to time an abstract of the facts and opinions published in the current literary productions of the day, and we note with satisfaction the appearance of such abstracts or reports in all the principal medical papers published in different countries. It would not be without interest and instruction to produce a complete list of the medical journals now published in the several countries of the globe. Such a list would be some criterion of the medical activity existing in each, and, we take it, would exhibit the profession in England as less alive to the value of a periodical press than

other countries in Europe, and particularly so in regard to more strictly scientific journals.

Origin and Destiny of Mankind.[1]—This handsome volume, with many excellent illustrations, discusses a subject which must be of intense interest to medical as it is to other men, but the manner in which the subject is dealt with, apart from the matter, places it beyond the pale of the medical reviewer. A great portion of its contents consists of archæological matter,—the archæology of the religions of the world, and a comparative examination of their teachings and principles compared with those of the Christian faith. To a considerable extent also the volume is a critique of the facts and doctrines of the Bible, and substantially a theological one. There is a vast amount of valuable research and information in its pages; but the author, with regard to almost all his facts and statements, is only a borrower, and he appears to us to select only those that make for his purpose—that purpose is primarily and essentially a destructive one. In his view religion is an antiquated affair, a nature-worship in its primitive shape, but overlaid with an immense mass of sacerdotalism. He consequently would rid mankind of the incubus, and demand only a sort of reverence to some power in nature, one, however, which becomes a vanishing quantity when the mind attempts to seize upon it, and may, in our apprehension, for what it is worth, be relegated to the region of myths. From nature, personified as a power or force, we are told we spring, and to her must return; this nature is eternal; in chemical combinations resides her creative power, and no vital force, peculiar to living beings, has an existence; protoplasm is the foundation of all organic life, and between organic and inorganic matter chemistry knows no real distinction; and lastly, this world, and all its varied matters and creatures, are the result of evolution from some filmy, attenuated diffused matter, which many a long day since existed in "infinite space," but eventually found itself squeezed together into a more coherent mass, "which progressively condensed, and thus gave rise to heat, light, electricity, magnetism, and chemical affinity." Having got thus far, each and all of these several active factors set to work, and, as a result, concocted this fair world and all its inhabitants.

An account such as this of the origin of mankind, but duly expanded and dressed up in the much evolved scientific language of the day, may commend itself to some minds as worthy of acceptance, but to our humble conception is as mystic as the assumed myths it seeks to replace.

There is, as we set out by saying, a vast mass of information in

[1] *On Mankind, their Origin and Destiny.* By an M.A. of Balliol College, Oxford. London, 1872. Pp. 78.

this large volume, comprising a collection of facts not known and not accessible to many readers. Consequently, there is much to repay perusal; but the reader instructed at all in the matters treated of will soon perceive that it is very much the composition of a special pleader, who will only see one side, who raises doubts and difficulties which have been disposed of aforetime, or have no actual existence, who withholds facts and arguments that may be advanced against him, and uses hypotheses and speculations to serve his purpose as if they were demonstrated verities.

Frey on the Microscope.[1]—This is a translation of one of the best and newest Continental treatises on the microscope and its use in investigating the various tissues and parts of the body in their normal and in pathological conditions. The first section is devoted to the theory of the microscope. The theory of the magnifying lens, the simple microscope, the compound microscope, the cause and correction of spherical and chromatic aberration, are explained, together with the employment and uses of the different parts of the compound microscope, and the section concludes with a notice of some of the instruments of the best makers, *e.g.* Merz, of Munich; Nachet and Chevalier, of Paris; Zeiss, of Jena; Oberhauser and Hartnack, and Smith and Beck of London. With regard to the large instrument of the last-named makers, the author finds that its great advantage over Oberhauser's large instrument is that the stage permits of the introduction of a perfected condenser. In other respects it is, as judged by a Continental standard, too complicated. The second section is on apparatus for measuring and drawing, and touches at some length on micro-photography; the third treats of the binocular stereoscopic and polarizing microscope. With regard to the value of the stereoscopic microscope, the author is of opinion that by many it has been over-estimated. The future can only decide whether science is to derive real benefit from it. The fourth section, on testing the microscope, completes that portion of the work which chiefly refers to the instrument proper, as distinguished from its uses and discoveries. Then follow sections on the use of the microscope, microscopic examination, the preparation of microscopic objects, on fluid media and chemical reagents, on methods of staining, impregnation with metals, the drying and freezing processes, on mounting and injecting. The section on injecting appears to us particularly good, but all these chapters are full of practical directions and information which cannot fail to be of the greatest value to the worker.

From the eleventh section to the end, inclusive—a space of between four and five hundred pages—is allotted to the description of the

[1] *The Microscope and Microscopical Technology. A Text Book for Physicians and Students.* By Dr. Heinrich Frey, Professor of Medicine in Zurich, Switzerland. Translated from the German and Edited by George Cutter, M.D. New York, 1872. Pp. 658.

microscopy of the tissues, fluids, and organs of the body in health and disease. We know of no more complete and valuable guide to the practical histologist. The book is not a mere descriptive microscopic anatomy, but rather a guide to the investigation of the tissues and fluids. It bears the same relation to an ordinary work on histology that a "dissector" bears to a descriptive anatomy book. For instance, take the eleventh section, on blood, lymph, mucus, and pus. The author first tells how blood is to be obtained and examined; how it is best diluted; the appearance of the corpsucles; then how the movements and changes in the living colourless cells may be brought into view by the means of the warm stage and iodine serum; how the colourless cells may be stuffed (gefüttert) by injecting granulated colouring materials for several days into one of the large lymph spaces which lie under the skin of the frog; the mode of numerically estimating the number of the two kinds of cells; then of the plasma of the blood and the pathological changes in the blood. On this last-mentioned point we may observe that Dr. Frey does not attribute much value to the endosmotic changes in the form of blood-cells which have been described in processes of disease. The two pathological conditions on which the microscope has hitherto thrown valuable light are those of leucæmia and melanæmia. Of the latter condition he writes:—

"In malignant forms of intermittent fever the enlarged spleen has been seen to have a blackish appearance. The microscope shows as a cause of this change of colour, granulated lymphoid cells, often of a considerable extent, and which contain within them granules of the black pigment. Passing out through the splenic vein, they become mixed with the blood, and are seen in this fluid when it is subjected to microscopic examination. In consequence of their size they produce obstructions in certain capillary districts, especially in the brain and liver" (p. 233).

Then the author gives the mode of repeating Recklinghausen's discovery of the transformation of lymphoid cells into red corpuscles in the blood of the frog. The effect of electricity, heat, and chemical reagents on the blood-corpuscles are next given, the mode of obtaining hæmatocrystalline, hæmatin, hæmin, and hæmatoidin, and the portion of the section devoted to the blood concludes with the discussion of the movement of the blood in the living animal. The process of "stuffing" the lymphoid cells in the frog, to which we have alluded, was used by Cohnheim to establish the transmigration of those cells through the capillary walls and the fact that pus-cells come from the blood-vessels, for if a finely granular colouring material has been previously injected into one of the lymph-sacs of the animal a part of the pus-cells found on the surface of the mesentery (inflamed by exposure to the air) will be found to contain colouring matter. The lymphoid cells during health also migrate and appear as movable

cells wandering through the spaces in the connective tissue. The questions remain yet to be answered whether all migratory cells and pus-corpuscles originate in the blood, whether the cellular elements of connective tissue may be changed into pus, and whether the emigrants may not be transformed into other tissue elements. The latter, Dr. Frey thinks, is not to be doubted. Observers have affirmed the division of the lymphoid cells as well as their origin from the cells of the connective tissue.

This section, which we have chosen as giving a fair idea of the fulness of the range of the author's observations, concludes with an account of the microscopy of lymph, chyle, mucus, and pus. Pus-cells are the extravasated colourless blood-corpuscles which have collected at the point of irritation. But they are to be found at times inside epithelial cells, and the author gives an illustration of ciliated epithelium-cells containing pus-cells obtained from the respiratory mucous membrane. Remak, Buhl, and Rindfleisch assume that the pus-cells were formed in the interior of the epithelial cells. The author, however, inclines to the explanation that the included pus-cells are "those vagabonds of the body," the wandering cells which have penetrated from the tissue of the mucous membrane into the epithelial cells.

The remainder of the book the limitation of our space forbids us to analyse. Of the various sections, we may single out the one on the urinary organs as seeming to us—at least, as far as the anatomy of the kidney is concerned—especially full and valuable. The directions given for injecting and the admirable woodcuts of transverse and longitudinal sections seem to us admirably fitted to help the student. We may notice throughout that the work is profusely illustrated with clear and well-executed wood engravings, and that credit is generally given to the discoveries of English as well as Continental observers. The translation is avowedly a literal one. The translator, in his preface, apologises for the German character of the English text, but states that he found himself bound to a rigid adherence to the original by the nature of the subject, the minute descriptions, the frequent repetition of the same terms, and the impossibility of otherwise rendering justice to the author. We think, on the whole, that the work does Dr. Cutter great credit.

Fox on Ozone and Antozone.[1]—In this admirably finished work the author, whose name is already well known in the paths of both medicine and meteorology, has collected the scattered literature of a most interesting subject, and has systematised all that is at present known concerning ozone, and the still more mysterious antozone.

[1] *Ozone and Antozone, their History and Nature. When, where, why, how is Ozone observed in the Atmosphere?* By Cornelius B. Fox, M.D. Edin., M.R.C.P. Lond., Fellow Brit. Meteor. Soc., Fellow Obstet. Soc., Member Scottish Meteor. Soc., &c. London, 1873. Pp. 329.

The task undertaken in the compilation of this book was no light one, for, perhaps, no substance has been so much written about of late years as that which owes its rediscovery (at all events) and its name to Professor Schönbein, of Basle. At the same time, so contradictory and variable in merit, so dispersed and fragmentary, were the papers which treated of ozone, that to reconcile conflicting statements, to separate what was good from what was bad, and, finally, to reduce everything to order and system, would at first sight seem almost to be a hopeless undertaking. Yet we may without flattery congratulate Dr. Fox on the large measure of success which has attended his efforts. Should that success be regarded as not complete, its incompleteness is due not to any shortcoming on his part, but to the circumstances of the case, to the somewhat unsettled and unsatisfactory state of our present knowledge of ozone. In his book Dr. Fox presents us with a digest of the most important facts relating to this body which have been established by the investigation of observers in many countries, but more especially in Germany and France; yet he does not stop here, for a large portion of the work is filled with original matter of a high degree of excellence.

Having passed rapidly in review the chief points in the history of ozone, the author proceeds to consider *what* this body is. He appears to accept the hypothesis advanced by Dr. Odling, and afterwards experimentally established by M. Soret, namely, that ozone, having the formula $O_2O = 48$, is an allotropic modification of oxygen, with a varying amount of which it is always associated. The methods of preparing ozone and its properties are then enumerated. "*When* is ozone observed in the atmosphere?" is the question which next engages attention. Under this division of the subject the influence of season, of day and night, of certain atmospheric states and phenomena, as electrity, thunderstorms, halos, auroræ, phases of the moon, eclipses, asteroids, and earthquakes, is considered. The influence of the direction of the wind is illustrated by a beautifully executed coloured lithograph of an ozonoscopic windrose. In this the marked effect of sea-weeds in increasing the amount of ozone observed at a given place is well shown. "*Where* is ozone observed?" is the third question which Dr. Fox seeks to answer. He considers under this heading the influence of locality on the manifestation of "nature's great deodorising and purifying principle," also the influence of height above sea-level, of local circumstances in cities and towns, and of the neighbourhood of lakes, rivers, and the sea. In seeking to answer the question "*Why* is ozone observed?" occasion is taken to point out the uses of this body in nature, and especially to investigate its supposed relations to health and disease. To the physician this, perhaps, will be the most interesting part of the book, but it is to be regretted that so far our knowledge in this direction is very scanty and uncertain. *Meliora*

speremus. One reason why this is so appears incidentally from observations made in the following division, in which our author deals with the question. "*How* is ozone observed?" Speaking of the chaotic state of ozonometry, he says (p. 182) "The truth of the statement, that the estimation of ozone, as it has hitherto been conducted, is eminently unsatisfactory, will not be disputed even by the most inexperienced of observers." The portion of the book which immediately precedes and follows this quotation is of sterling value. The tests for ozone are fully enumerated and described. An account is given of the different ozonoscopes in use, and these are in most cases represented by woodcuts. The errors inherent in the old method of ozonometry are fully pointed out, and an improved method is suggested and explained at length. It is shown that the desideratum in determining the presence of ozone and in estimating its amount is a *sensitive* test, unaffected by any other body in the air, and capable of being readily employed. At present "there are only two tests which merit the attention of ozone observers, namely, the iodized litmus and the simple iodide of potassium test." To the former alone can the title of ozonoscope be properly applied. In explaining how ozonoscopes should be placed, so as to avoid the many possible sources of error, the author is, perhaps, rather inclined to repeat himself, but this is chiefly due to the nature of the case, and we have little doubt that in a second edition this trifling fault, if it be a fault, will disappear. When accurate estimates of the amount of ozone in the pure air of different climates and under varying atmospherical conditions have been secured, the author shows that—

"We shall be in a position to attempt the elucidation of the following and many other questions, which are of immense interest and importance to the human race;—(1) What are *all* the sources of atmospheric ozone? (2) How is it formed, and in what circumstances does it arise? (3) What is its precise action on animals and plants? (4) Has an excess or deficiency of ozone any effect on the public health? (5) If so, what is the nature of that influence? (6) What is the effect of the presence of epidemics on its amount, as calculated by the improved ozonometric method? (7) Does ozone oxidize one only or all of the different kinds of organic matter found in the air?"

In conclusion, a word of congratulation is due to the publishers for the admirable manner in which they have executed the task entrusted to them in the bringing out of this work. Dr. Fox and his readers are alike fortunate in this particular.

Handbook of Medical Information.[1]—As usual with books of this sort, this one is addressed to intelligent persons of both sexes

[1] *Handbook of Medical Information and Advice; containing a brief Account of the Nature and Treatment of Common Diseases*, &c. By a Physician. London, 1872. Pp. 352.

who may happen to be placed beyond the reach of a medical man, and these unhappy people are presumed to be able to find in its pages sufficient information to recognise and to treat disease. The author further hopes that the acquaintance with physic to which he introduces his readers will, in their case, "operate in exalting the reliance" they should place in medical men, and likewise diminish their eagerness "to take refuge in all varieties of quackery."

These hopes we conceive to be delusive. The superficial acquaintance with disease and remedies, which alone is obtainable from such a book, is prone rather to favour conceit in its possessors, and to induce them to play the doctor's part when a chance offers, and instead of encouraging resort to a regular practitioner and reliance upon him, it rather encourages mischievous delay, and annoying, meddling, and crude suggestions, and foolish criticisms upon medical treatment when adopted.

Medical men perpetually experience difficulty and annoyance in combating some preconceived notions the patient or his friends have formed of his case and its fitting treatment. In fact, the priggishness of a patient having the conceit of medical knowledge is about the worst variety of that pestilent feature of humanity.

Although, however, we cannot partake of the high hopes of material and moral advantages to follow acquaintance with his teachings, expressed by the author, the constantly recurring phenomenon of the publication of works similar to the present convinces us that the public will have such books; and it is well for the said public when no worse production than the one now under notice falls into their hands, for its compiler appears to have resorted to many of our best-known writers on medicine, and to have had sufficient sagacity and literary skill to extract much useful and instructive information, and to put it into a readable shape. It would be easy to show that the "physician"-author is at times in arrear in his knowledge, and doubtfully clear in his expositions of pathology, symptomatology, and treatment. If his readers are to carry out the treatment he suggests in all cases, without the oversight and direction of a qualified practitioner, we should anticipate frequent untoward results; for instance, local bleeding and tartar emetic (which latter he quotes as the remedy *par excellence*) are strong measures in the hands of the untrained when called upon to deal with what they may conceive to be bronchitis in its early stage.

Consumption is the longest article in the book, and in its composition the author betrays great confusion of ideas relative to its pathology, its nature, and varieties, and altogether has launched out into details out of place in a book such as he had in hand, and what, as we may tell him in his interest and for future consideration, will be little appreciated by those who refer to it, and who want to

find "recipes" and "cures," and not etiological and statistical disquisitions.

The general get-up of the volume is highly commendable, and its contents have not the quackishness and pretentious display of many like books.

The English Factory Legislation.[1]—A large proportion of the public, especially those outside manufacturing towns, are, we apprehend, little acquainted with the history, growth, and provisions of the Factory Acts now in operation in this kingdom. This much, however, is pretty generally recognised—that factory labour is controlled by Act of Parliament, and that the results of inspection have operated advantageously to the physical and, in some degree, to the educational condition of factory operatives.

The factory laws, however, exhibit in so wide a degree the principle of paternal government, and involve interests of such enormous magnitude, that they deserve to be more fully known than they are. Their early history, moreover, was attended by so considerable parliamentary and extra-parliamentary agitation and discussion that it possesses a spice of romance, and may be read both with curiosity and instruction. Like that of most other political developments, the history of those Acts exhibits, in one party, highly wrought accounts of the evils to be remedied and faith in the sovereign remedy proposed; and in the other, in forebodings and prophecies of dreadful consequences of the contemplated legislation, and a *couleur de rose* representation of the existing state of things, needing no amendment. However, the principle of Government control of labour gained the day, and the results have on all hands been admitted to be excellent.

This little work relates the history of the rise and gradual extension of factory legislation, noting the variation and growth of public opinion on the subject, and the purpose and working of the several enactments put into force to the present date.

The point of view taken by the author is essentially that of the statesman and politician. The purpose and value of the Factory Acts, considered as sanitary measures, are less apprehended, and the work effected by the medical officers appointed under the Acts taken little note of. This circumstance, however, is not much to be wondered at, seeing that the work of those officers is not a subject of official recognition, and, indeed, comes very imperfectly within the range of official knowledge.

The lesson to be drawn from all this is, that the factory medical men should make known what they are worth, what they do, and

[1] *The English Factory Legislation.* By Ernst Edler von Plener. Translated from the German by F. L. Weinmann. With an Introduction by A. J. Mundella, M.P. London, 1873. Pp. 175.

what they are capable of doing. Official want of recognition soon passes into official indifference, and this again into opposition to a body which manifests neither soul nor spirit, but confides in its own persuasion of its utility and in its self-knowledge of the work it accomplishes, giving, however, no external signs thereof to enlist attention to it.

Those who have watched factory legislation and its results in this country will be much pleased to find its principles and its main provisions progressively recommending themselves to other countries.

To what extent this progress has been made is shown in the appendix to this work. In fact, we find from it that several foreign kingdoms have not only been copyists of our measures, but have gone beyond us, and will soon become examples to us of more advanced views of protection as regards especially the labour of women and children.

The treatise under notice is the composition of the First Secretary of the Austrian Embassy in London, who has evidently devoted great pains and research in the uninviting department of literature represented by blue-books and reports, and gathered facts to make a very readable, interesting book.

Teratological Catalogue.[1]—Our forefathers would have rested content with the announcement of a list of monsters and malformed parts, but the present age demands information, whatever it chance to be, served up as an o-logy, and our scientific men have consequently to extract from their well-thumbed Greek lexicons some recondite word which shall be duly tacked on to the o-logy, and make a term of due length and of the abstruse, mysterious signification sought for by the transcendental minds of the nineteenth century.

The word Teratology, we presume, has the same sort of comfort about it to the scientific mind as 'Beelzebub' had to the religiously minded elderly female of the tale; and we should be sorry to disturb the comfortable sensations it may call forth by any critical remarks upon its character and parentage. And happily Mr. Lowne takes occasion in an excellent introduction to the catalogue to define teratology as having "for its domain the consideration of abnormal conditions of development."

In arranging the examples of such conditions he adopts six classes:—Variation; Duplicity; Excess of Growth; Arrest of Growth; Arrest of Development; and Disease; and these classes, again, are made subservient in their application to abnormal conditions of plants; of the axis in animals; of the limbs; of the skin

[1] *Descriptive Catalogue of the Teratological Series in the Museum of the Royal College of Surgeons of England.* By B. Thompson Lowne, M.R.C.S.E., &c. London, 1872. Pp. 110.

and appendages; of the osseous and muscular systems; of the sensory organs; of the heart and vessels; of blood-glands; of the digestive organs; of the urinary and generative organs; of the oviduct and ova of birds.

Mr. Lowne has well fulfilled his not easy task, and has rendered this section of the magnificent museum of the College of Surgeons devoted to abnormal forms available to the genuine student of morphology and of animal variations. He speaks of the apparent absence of laws regulating malformations; but with the aid of the collection he has catalogued, and of the descriptive notes he has supplied of the several preparations, we trust that the research of some ardent student will some day culminate in the establishment of some definite principles determining all departures from normal structure.

Journal of Anatomy and Physiology.[1]—This excellent journal continues, we are glad to see, to appear with great regularity, and to contain a series of valuable essays and papers. Foremost amongst these are the notes on myology, by Professor Humphry, who really deserves high credit for his scientific work, when his labours as a consulting physician and his numerous and onerous duties as a professor of and teacher in the University of Cambridge are considered. The notes include descriptions of the muscles of the *Lepidosiren annectens*, of the smooth dogfish (*Mustelus levis*), of the Ceratodus, and of the glass snake (*Pseudopus Pallasii*), and also some general statements in regard to the disposition of the muscles in vertebrata.

The other editor, Professor Turner, has not been idle, for we find him contributing a paper on the sternum of the sperm whale (*Physeter macrocephalus*), another on the dentition of the narwhal (*Monodon monoceros*), and a third on the structure of the human placenta, besides two long reports on the progress of anatomy. The structure of the placenta is also the subject of a paper by Dr. Braxton Hicks. Professor Struthers gives a long paper on the cervical vertebræ and their articulations in fin whales, and Dr. Watson one on the anatomy of the Indian elephant, comprising the description of the urinary and generative organs of that animal. The other principal anatomical papers are—Dr. R. Traquair on the so-called tailless trout of Islay, Mr. Frank Champneys on a communication between the external iliac and portal veins, Mr. S. Messenger's notes of myological peculiarities, Mr. Stirling on the presence of *Trichina spiralis* in the muscles of the rat, Dr. Watson on a case of termination of the thoracic duct on the right side, Mr. Galton on a case of abnormality of the teeth in man, Dr. Handyside

[1] *The Journal of Anatomy and Physiology.* Nos. X and XI, May and November, 1872.

on two cases of quadruple mammæ in brothers, Dr. Wagstaffe on a peculiar formation of the leg and foot, and Mr. Walter Rivington on valves in the renal veins.

The principal physiological papers are by Dr. Ainslie Holles on tissue metabolism, or the artificial induction of structural changes in living organisms; one of considerable interest by Mr. A. H. Garrod on sphygmography; Dr. Clifford Allbutt on the effect of exercise on the bodily temperature; Dr. Lauder Brunton on the action of digitalis on the blood-vessels; Dr. Fraser on the Kombé arrow poison of Africa, derived from the *Strophanthus hispidus;* and by Mr. Butler Stoney on the effect of stimuli on the secretion of the parotid gland.

The reports by Drs. Turner, Brunton, Ferrier, and Frazer, on anatomy, physiology, and the physiological action of medicinal and poisonous substances, are, as usual, very complete and accurate.

Ringer's Handbook of Therapeutics.[1]—The rapid sale of Dr. Ringer's treatise on therapeutics speaks well for the ready appreciation by British practitioners of new views of practice when recommended to them on sufficient authority and with sufficient precision. Indeed, a considerable portion of the therapeutical teachings of this book is very much at variance with the prevailing dogmas twenty years since. The doctrine of the efficacy of frequently repeated small doses is an innovation, and that of the antagonism in action of many drugs is a development of positive science from what were only crude guesses previously obtained.

The present edition has undergone revision throughout, and numerous additions have been made in it. Of the latter we may note a new chapter on nitrite of amyl; a digest of the experiments with cold baths in fever; a fuller account of the Turkish bath as a therapeutical agent, also of the action and uses of veratria, of aconite, and of digitalis; in the case of the substance last named the researches of Fothergill, Foster, and Da Costa being very fully represented. Other new matter is introduced in the account of the Calabar bean, and Dr. Fraser's valuable experiments and conclusions respecting the antagonism of belladonna and physostigma are pretty fully detailed. The cutaneous rash from the alkaline bromides comes in for notice, as does also the recent use of large doses of iodide of potassium in the treatment of aneurism.

On the other hand, we should have looked for a fuller history than is given of nitrous oxide as an anæsthetic, considering the great extension of its use even since the date of the previous edition. Moreover, we fail to see reference to the death that ensued not long since from its administration, although this unfortunate case pre-

[1] *A Handbook of Therapeutics.* By SYDNEY RINGER, M.D. Third Edition, 1873. Pp. 576.

sented points of interest lacking in the history of the one fatal instance the author was able to quote when he put forward the last edition. We are likewise disappointed at the little new matter introduced in the account of alcohol, and note with regret that the excellent experiments by Dr. Clouston on the use of bromide of potassium in mania have escaped notice, as also have the various recorded examples of the value of chloral in puerperal convulsions.

This third edition contains nearly one hundred pages more than the one preceding it; but it is right to add that a considerable proportion of this increase is due to the employment of a larger type, and consequently to the extension of the same amount of matter over a wider surface. When another new edition is called for we shall be glad to see a chapter on general therapeutical doctrines and on the modes of action of medicines. The treatise has, as heretofore in earlier editions, our hearty approval.

Wilson Fox on Stomach Diseases.[1]—This new edition is in some degree a new work, the author having extended his plan so as to make the work comprehend all the well-recognised lesions of the stomach. In this form we believe the treatise will be more highly valued than before. The principal additions referred to in the preface as made to this new issue are the chapters on ulcer and cancer of the stomach, besides some minor contributions; but besides these special articles much other new matter, particularly in the way of notices of opinions and researches by the best-known modern pathologists, has been also added. These notices, which are largely in the form of quotations, attract attention on almost every page, and attest the industry of Dr. Fox in seeking to make himself familiar with all that has been said and done in connection with the subject-matters he deals with.

This zeal and conscientiousness in noting what other authors on stomach disorders have written, and in crediting them with their opinions, may, in the estimation of some, be regarded as a fault in the treatise, in so far as it makes Dr. Fox the recorder of the works and views of others, to the detriment of his own character as an original observer and an experienced physician. However, granting that such an opinion may obtrude itself upon any readers, there is ample matter to prove Dr. Wilson Fox to write from personal research and experience, and to possess views of his own both in pathology and treatment. He has evidently aimed at completeness in the information presented; and, as he surmises, the collection of opinions and authorities on stomach diseases will save future writers much trouble in discovering the sources of opinions and statements

[1] *The Diseases of the Stomach; being the Third Edition of the 'Diagnosis and Treatment of the Varieties of Dyspepsia.'* Revised and enlarged. By Wilson Fox, M.D., F.R.C.P., &c. London, 1872. Pp. 236.

now generally received or still open to consideration and discussion.

The work is divided in two parts, of which the former is occupied with the symptomatology of the stomach, the latter with the special diseases of that organ. In treating of the causes of dyspepsia, as manifested by the various symptoms belonging to it, Dr. Fox has shown much brevity in the consideration of food and drink, desiring, we apprehend, to avoid being betrayed into writing a work on diet as well as on stomach maladies. Nevertheless we should have been pleased to have had a more full account of foods as sources of disease and as means of cure, and might, certainly, have looked for something more than the very meagre notice of alcohol he has presented, both in its character as a cause of disease and as a remedy. As might be supposed from the well-known bent of the author's mind, pathological details are very fully entered into, and the reader who masters them will gain a very clear insight into the symptomatology of gastric disease, and gain some steps in advance in his search after means to effectively treat them. The therapeutical directions are rather too miscellaneous and general, and the author seems timid of propounding as his own any special modes of treatment. He retreats, as it were, when he discusses treatment, under the mantle of those who have advocated some one or other particular course. The reader is consequently left very much to his choice amid a greater or less variety of plans set before him, his monitor too frequently only giving an uncertain sound. Still, our opinion remains that this work by Dr. Wilson Fox is a highly valuable one, representing very fully the most recent views relative to the pathology and symptomatology of diseases of the stomach, and offering an excellent digest of the principles and details of treatment advocated by the most eminent practitioners of the day.

Billroth's Surgery.[1]—"The study of surgery, which you begin with this lecture, is now, in most countries, justly regarded as a necessity for the practising physician."

This is the first sentence with which Professor Billroth commences a course of fifty lectures, delivered, we suppose, to his class in Vienna; but whether this be so or not, it is the first sentence we find in the introductory chapter to what may, with complete propriety, be called his great work on surgical pathology and therapeutics; if permitted, we should be disposed to venture something like a paraphrase of the above, and say, The study of *medicine*, which you begin with this lecture, is now, in most countries, justly regarded as a necessity for the practising *surgeon*.

In point of fact, the division between medicine and surgery

[1] *General Surgical Pathology and Therapeutics*. By THEODORE BILLROTH, Professor of Surgery in Vienna, &c. Translated from the Fourth German Edition, by CHARLES HACKLEY, A.M., M.D. New York, 1871.

which was so long and so pertinaciously maintained no longer exists; we no longer speak in an arbitrary and, in a certain sense, disparaging manner, of the art of surgery, as though it were distinct from, and inferior to, the science of medicine; the simple chirurgeon, the mere χειρ and ἔργον, is heard of no more, indeed, is not to be met with; and although it is true we do find, in consequence of the prejudices or predilections of individual members of the profession, that the separation between medicine and surgery is *apparently* fostered for the mere convenience of practice, it is, nevertheless, beyond all question that the most enlightened members of the brotherhood of physic give no countenance, in this our day, to any such unwarranted and unwarrantable division.

"The unity of medicine" may be said to be at this period everywhere accepted and firmly established, whilst most, if not all, of our most distinguished surgeons are so distinguished by the circumstance of their being in the van of the profession as regards their attainments in "*scientific medicine.*"

But what, it may be asked, about specialism and specialists? Is there no ground for lamentation here? Are these divisions indicative of the "unity of physic"? Does "*united medicine*" foster the multiplication of such things? Is it necessary, can it be desirable, that there should be hospitals for stone, for cancer, for consumption, for diseases of the throat, for fistula, for paralysis, for deformities, &c. &c.? Does the circumstance of this one being an oculist, that one a lithotomist, some other one a lithrotitist, and others dermatologists, ovariotomists, liver doctors, kidney doctors, fever doctors, &c. &c., seem likely to promote the true interests and advancement of the profession? Sown broadcast as the "specialities" now are, one would almost be inclined to think the public will infer that each organ and part has an independent and separate existence, and that the interdependence of each upon the whole is a mere figment of the imagination. Well may some inquire whether there can possibly be anything like "unity" in a science which is so obviously, and, at the same time, so preposterously practised, in such a fragmentary manner.

The book we have under notice certainly lends no countenance to such unseemly and ridiculous disruptions of practice; based upon principles which comprehend the very latest views and discoveries both as to physiology and pathology, it affords an admirable example of what a work on surgical pathology and therapeutics should be.

It carves out no particular segment of the human frame as being something of itself distinct from, and to be treated without reference to, that aggregate of functions which alone constitutes and maintains the life of the individual; but whether dealing with descriptive pathology or practical therapeutics, it never loses sight of the fact that those aberrations of function which (whether caused by

mechanical violence or constitutional dyscrasia) we characterise as disease, are still under the influence of what may be fitly designated "biological force," and that such aberrations of function, occurring in *any part* of the human body, are most likely to be corrected by such an application of therapeutics as shall be most likely to promote and restore physiological action.

The fifty lectures are spread over six hundred and sixty-four pages, and are illustrated by one hundred and fifty-two woodcuts, many of these being admirably executed, and all of them exceedingly well adapted to make clear the meaning of the author. Whether looked at as a text-book for students or as a work of reference for the hard-worked and busy practitioner, it deserves to be spoken of in high terms of commendation. We must not omit saying that, in our judgment, the translator has performed his task in an exceedingly creditable manner.

Sydenham Society's Publications.—We have to note the publication of the fifth volume of 'Trousseau's Clinical Medicine,' the third of 'Stricker's Human and Comparative Histology,' and the second of 'Rindfleisch's Pathological Histology,' under the auspices of the Sydenham Society. The members of that society may be congratulated on the addition of such valuable treatises to their libraries; and without making odious comparisons, and with full recognition of the great value of the other volumes, we feel persuaded that no one of the many works issued has secured to itself so general appreciation as the work of Trousseau on medicine, or has surpassed it in its influence for good.

The third volume of 'Stricker's Histology,' ably translated and edited by Mr. Henry Power, is occupied chiefly with the structure of the special organs of sense; but it contains, besides, the histology of the uterus and appendages and a history of the development of the simple tissues, and two articles, in an appendix, the one on synovial membranes, the other on the non-pedunculated hydatid.

The second volume of 'Rindfleisch's Pathological Histology' comprehends the morbid anatomy of the lungs, liver, and kidneys, of the osseous, nervous, and muscular systems, and of the ovaries, testicle, and other organs. It concludes with a bibliographical and an alphabetical index.

The society ought to flourish whilst it caters so well for its members. They have every reason to be content, both with the quantity and quality of the matter supplied.

Elam on Cerebria.[1]—Two orders of facts, as the author very justly

[1] *On Cerebria, and other Diseases of the Brain.* By CHARLES ELAM, M.D. London, 1872.

observes, are "conspicuous by their absence." These are minute morbid anatomy, and the revelations and evidence of the ophthalmoscope. To have pursued the former Dr. Elam thinks would have led him away from his special object, the *clinical history* of certain forms of disease. To have pursued the latter would, if we understand our author aright, have demanded more practical skill in the use of the ophthalmoscope than he professes to possess.

Possibly the absence of these trifles may account for the fact that the author finds that inflammatory affections of the brain occupy so small a portion of works devoted to diseases of that organ. Reynolds's 'System of Medicine,' and Dr. Aitken's 'Science and Practice of Medicine' being cited in support of this general statement. It is probable that had the author included the microscope and the ophthalmoscope among his means of investigating the "clinical history" of diseases of the brain, he would have arrived at a conclusion that has been reached by most pathologists of the present time, viz. that the brain tissue is specifically indisposed to take on the simple, non-suppurative form of inflammation. That inflammation of the brain implies inflammation of the membranes and connective tissues of that organ is familiar to pathologists, and is amply shown by the references of the author to the writings of the host of investigators to whom he refers, or from whom he quotes.

Dr. Elam has recorded three cases of what he regards as pure and simple inflammation of the substance of the brain, as contrasted with meningitis, and upon these he founds his new word "Cerebria," in favour of which he urges "that it is short; that it is not *very* uncouth, although not strictly correct; that it suggests its own meaning, by its terminal relation to other inflammations, as pneumonia, metria, ophthalmia, &c.; and, finally, that as names are only 'to know things by,' and as he has vainly sought for one more classically appropriate, this will answer the purpose, and avoid constant periphrases, as well as any other" (p. 33).

For our own part, having had the advantage of studying many of the treatises named by Dr. Elam, we are not able to follow him in his arguments for his new nomenclature.

With reference to "other diseases of the brain" treated of by the author, we fail to discover any actual novelty in observation or treatment to which to draw the attention of our readers.

Proceedings of the Dublin Obstetrical Society.[1]—This report reached us too late to include it with the notice of the transactions of the London and Edinburgh Obstetrical Societies, printed in our last number. Had it been otherwise, we should have been disposed to

[1] *Proceedings of the Dublin Obstetrical Society for Session* 1871-72. Dublin. Pp. 164.

have taken the three to serve as the basis of an analytical review of the progress of midwifery during the year. This we hope to accomplish on another occasion, should the needful volumes come to hand.

The Dublin school of midwifery is second to none in this empire; this may well be so, seeing that it possesses a lying-in institution exceeding in magnitude every other elsewhere, and that its practice has been thrown more fully open to learners, and turned to better account in the advancement of obstetrical knowledge, than any like institution we possess. The report of the practice of the Rotunda Hospital, by the Master, Dr. Johnston, forms one of the articles included in the volume before us, respecting which it is incumbent on us to state that it is the first issued by the Dublin Society, although this society has been thirty-four years in existence. We congratulate the members on their resolution no longer to hide their light under a bushel, but to hold it forth that it may be seen of all.

The volume opens with the address delivered by the President, Dr. Kidd, who takes occasion to review the doctrines and practice of the present day in the management of difficult and tedious labours. Transfusion in post-partum hæmorrhage is the subject of two dissertations by Drs. A. H. and John Ringland, in which the apparatus invented by Dr. Robert M'Donnell is commended. The last-named physician, in the discussion that ensued, gave his own account of the instrument, and of his experience with transfusion, an operation he advocates more frequent recourse to. Dr. J. More Madden contributes an essay on spurious, feigned, and concealed pregnancy; Dr. A. H. M'Clintock a description of the "use of a hemp-saw for the excision of polypoid growths;" and Dr. John Ringland the history of a case of sudden death, seventeen days after delivery, from embolism of the pulmonary artery. The use of ergot of rye was examined in a lengthy paper by Dr. Denham, the reading of which called forth an instructive discussion, as fully recorded in these pages. The report of private obstetrical practice for thirty-nine years by the veteran writer, Dr. Fleetwood Churchill, is one that cannot fail to awaken the interest of other obstetricians. The same may be said of Dr. Kidd's practical contribution on fibrous tumours of the uterus, which is accompanied with illustrations of their growth in certain of the cases spoken of.

This very practical volume concludes with some brief communications, detailing cases of uterine tumours, by Drs. Darby and Cranny.

Wood's Epistles of Horace.[1]—As the production of a learned and estimable physician, we would direct our readers' attention to this metrical translation of the 'Epistles' and 'Art of Poetry' of Horace, by

[1] *The 'Epistles' and 'Art of Poetry' of Horace, translated into English metre.* By Andrew Wood, M.D. Edinburgh, 1872. Pp. 140.

Dr. Andrew Wood. It is a sequel to the translation of the 'Satires' of Horace by the same author, which appeared in 1870, and was reviewed in our July number of 1871. As such purely literary labour as is represented by the works of the classic writers of Greece and Rome, their commentators and translators, is beyond the scope of a strictly medical review, we shall enter here (and the less so as space forbids) on no critical examination of the manner in which Dr. Wood has performed his difficult though loving task, but repeat the concluding remarks of the previous notice referred to, that "we see in every page proofs of loving industry and intelligent appreciation of his author, which justify us in giving a hearty welcome to this latest attempt to introduce English readers" to the wit and wisdom of this great Latin poet.

The translator's modesty is worthy of all praise. He writes, as he tells us, for recreation and relaxation from the labours and anxieties of an arduous profession, and publishes his translations "in the diffident hope that haply a few readers may experience some degree of that pleasure which I myself have derived from writing them." In the "Introduction" he takes the opportunity of giving a brief sketch of the life of Horace. It is rather too short to satisfy the desire of any reader who has it alone to inform him; but this brevity is the less regrettable as those who want to know more of Horace, his life and works, can so readily avail themselves of the excellent little volume by Mr. Theodore Martin in the popular series of classics for English readers now in course of publication. Another acceptable portion of the "Introduction" is that occupied by those passages most frequently quoted by writers and speakers as examples of concentrated thought and wit, and most pithy and apposite to their purpose. It will be seen that a considerable proportion of these are borrowed from the 'Ars Poetica.'

Waring's Hospital Prayer Book.—It is enough in this notice to call attention to the publication of this book of prayers and to recommend it to those for whose use it is specially designed. We can highly commend it for the truly devotional character of the prayers, in the composition of which the influence of the Prayer Book of the English Church is everywhere perceptible, although more particularly so in the suggested form of public service. This fact in itself will speak in favour of Dr. Waring's composition.

Original Communications.

I.—Memoir on Leprosy in Syria. By John Wortabet, M.D., Beyrout.[1]

Introduction.

Leprosy has recently attracted a considerable degree of attention among medical men. An awful scourge which afflicts the human body, highly ancient in its history, unknown in the largest part of Europe at the present time, and thus removed from the field of modern investigation and discovery, it has become a subject of great interest to scientific labourers. To the private contributions of various writers, chiefly the valuable work of Danielssen and Boeck of Norway, the British Government has been the means of adding a new and important book. At the request of Her Majesty's Secretary of State for the Colonies, the Royal College of Physicians issued a series of interrogatories which was sent to medical men in different parts of the world. The result was the accumulation of a vast amount of information with which was incorporated a number of highly valuable documents, such as the conclusions drawn by the committee of the College from an examination of the entire evidence submitted to them on the subject-matter of each interrogatory, the account of post-mortem examinations made by Dr. Carter of Bombay, and the notes of Professor Erasmus Wilson on nineteen cases of leprosy which had fallen under his observation. The Report of the College was printed at the expense of the British government, and will long be a rich treasure for reference on this disease.

[1] This instructive paper was sent by the author to me a few months ago, with the expressed desire to have it made known to the British medical public. Containing the results of a lengthened personal experience of the disease in different Asiatic countries, it will be found a valuable contribution to its general history in that region of the world. Dr. Wortabet, in reply to the interrogatories of the College of Physicians in 1863, communicated some interesting remarks on leprosy in Aleppo; these will be found at page 57 of the report of the College. The reader will now be enabled to compare the conclusions which a highly intelligent observer in the East has formed respecting various points about this obscure and momentous malady with those at which I have arrived from my recent opportunities of studying it in the West, and which are recorded in my report recently issued by the Colonial Office and presented to Parliament.—Gavin Milroy, M.D.

Having paid some attention to the study of leprosy in Syria, I venture to lay the results before the profession with the hope that they may be found useful in illustrating the conclusions arrived at by more competent observers, and, perhaps, in adding new information on some of the topics involved in the subject. My own study and personal observation of the disease occupies a good number of years, and I have rarely allowed a case of leprosy which came before me to pass without careful examination, and noting down its symptoms and peculiarities. I have thus collected a good number of cases which fell under my care either in private or dispensary practice. The rest of my cases are derived from the leper-houses of Damascus which I visited in the spring of 1868, and which were visited again for me by a medical friend in the spring of 1872.

The order which I have adopted in the following pages is—1. The symptoms and pathology of leprosy, with a few typical cases. 2. An account of the leper-houses of Damascus, with tabulated cases of leprosy. 3. Notes on the following topics:—Sex, age, comparative frequency of the tuberculated and non-tuberculated forms, duration of the disease to date of observation, topography, causes, hereditary taint, contagion, segregation, specific character of leprosy, Jewish leprosy, and treatment.

Photographs were taken from cases in the leper-houses of Damascus.

Section I.—*Symptoms and Pathology of Leprosy with Typical Cases.*

General description.—The most prominent features of leprosy which form its peculiar characteristics are these:—*Anæsthesia* of the extremities, often involving the face and rarely the trunk; the *skin* is generally thickened, and its colour is changed into a red, dusky, glossy hue; the *hair* of the face falls, sometimes completely; *tubercules* appear on the face and extremities, which break from time to time, or large *bullæ* are formed on the extremities, rarely extending higher than the elbows and knees, and often converted into deep corroding fetid ulcers; the *joints of the fingers and toes* are almost ivariably involved during the course of the disease, some of the phalanges are destroyed, and the hands and feet, chiefly the former, become crippled; the *voice* becomes husky or is wholly lost; and the respiration becomes difficult. While these changes are going on, the *general health* is more or less impaired; and after a few years, the period being variable, the patient sinks from exhaustion, or from some intercurrent disease.

Nomenclature.—The Greeks called this disease *elephantiasis*, evidently from the hypertrophied condition of the legs which is often a concomitant of leprosy; and it still passes under this name in

the classification of the older nosologists, *E. Græcorum*.[1] They must, therefore, have embraced the disease known now as "Barbadoes" or "Cochin" leg and leprosy under one common name. The Arabs made a careful distinction between them, and called the former *Da'el Fil* (*morbus elephantis*), from whence we have now its classical name *E. Araborum*. Nor did they adopt the equally objectionable name of "leprosy," a word derived from the *lepra* of the Greeks and indicating an innocuous cutaneous disease of a scaly character, with which the disease under consideration has hardly anything in common. They substituted for it an Arabic word, *el Judham*, derived from a root which indicates the mutilating effect of the disease on the limbs. In the ordinary parlance of the people, it is often called *ed Da' el Kebir*, the Great Disease.

Symptoms and progress.—One of the earliest symptoms, and of which the patient takes special notice, is *altered sensation*, commonly in one or both of the extremities, sometimes on the face, and more rarely on the trunk. When the disease is situated in the extremities, the most distal parts are generally first affected, the circulation becoming feeble, the colour dusky, and general sensation diminished. The patient describes it as one of numbness, sometimes he calls it stinging or pricking pain (formication); probably in most cases, there is both objective anæsthesia and subjective hyperæsthesia at the same time. The skin of the affected parts is at first puffy, and ultimately permanently thickened and somewhat scaly; and I have sometimes observed hard, corded, knotty lines running up the forearms, as if some of the superficial lymphatic vessels were enlarged. As the disease advances, the fingers and toes lose their usual sensibility, and the patient finds that his prehensile powers are considerably impaired, and that in walking he may cast off a loose slipper without his being aware of it. In trying the extremities with a pin, I have sometimes found that the extensor surfaces were somewhat more anæsthetic than the flexor. About the same time, or very soon after, the face becomes similarly affected, the change of the complexion is more obvious, and the subsequent alterations produce that disfigured appearance of the features which is so peculiar to this disease. The colour becomes red, dusky and shining, and the skin thickened and knotted; the hair of the head and eyebrows, and the cilia drop gradually until they often disappear entirely; the eyes are red and watery with venules running across the conjunctivæ. When it attacks the trunk, the skin becomes affected in patches, though the hair is not so apt to drop off. I believe the face and trunk are rarely affected, unless the extremities are previously at-

[1] The language of Aretæus, however, when he says of it that "it is most powerful in dragging men to death, disgusting to the sight, and in all respects terrible, like the beast of the same name," would seem to indicate other points besides the hypertrophied condition of the legs.

tacked. These changes are attended by fetid perspiration, chiefly of the hands and feet.

It is difficult to believe that these earlier symptoms of the disease[1] can usher it in without a considerable degree of constitutional disturbance. In his great concern as to the fearful consequences of his malady, the patient rarely speaks of it; but there is always more or less of general malaise, febrile flushes, disturbed dreams, a peculiar heaviness affecting the whole body, and depression of spirits. This latter phenomenon becomes ultimately a settled state of the mind; all active cheerfulness is lost, and the leper assumes a sad subdued mien through life. This state of mind is produced probably by moral as well as physical causes. The victim of an incurable and loathsome disease, shunned by his nearest relations, disabled from work and reduced to dependence if not absolute privation, his life becomes a burden heavy to be borne. I have known a case where this state of feeling became intolerable, and the poor man (who was in good circumstances) was so affected by the bearing of his wife and children that he put an untimely end to his life.

Soon after the invasion of the disease, an *eruption* appears on the skin, limited to the forearms and hands, the legs and feet, and the face; very rarely, if ever, appearing elsewhere. Of this eruption we have two distinct forms, viz. *tubercules* and *bullæ;* the former appearing on the face, forearms, legs and back of hands and feet; the latter on the extremities, chiefly their palmar and plantar surfaces. The *tubercules* are generally numerous, and rarely smaller than a pea. They rise from the skin, and like it are red and dusky in colour, and shining. During the course of the disease they become inflamed, crack, and discharge a thin sanious fluid; after which they heal and contract, others taking their place in different spots. The *bullæ* appear in the form of large vesicles or vesicular blebs, are generally ovoid in form, and measure from three quarters of an inch to one or two inches in their diameter. They resemble so closely the blister of a scald that the patient often describes them as burns from too near an exposure to the fire, while the anæsthesia of the parts failed to give him the needful warning. They soon break and discharge at first a thin *lymphy* fluid, and leave an ulcer which penetrates deeply into the integument, and often exposes the tendons of the muscles. The vesicle is thus converted into a deep, red ulcer which discharges fetid pus. When the ulcers are many, or the patient is not cleanly in his habits, the odour becomes intolerable. They generally heal, leaving often a well marked white cicatrix. The tubercules and

[1] The earliest symptoms of the disease are stated by El Kamory, an ancient Arabian author, as follows:—"Congested appearance of the eyes, duskiness of the skin, huskiness of the voice, fetid perspiration, puffiness of the face, with ulceration and increased redness, and gradual loss of the hair of the eyebrows."

bullæ characterise, in a great measure, the two forms of leprosy called the tuberculated and the non-tuberculated, though these frequently appear in the same subject, but not to the same extent.

The *nose* begins early in the course of the disease to discharge a fetid purulent fluid, arising from sores formed within the nasal passages. Breathing through this organ becomes difficult; the triangular cartilage is gradually eaten away; and the healing process begins only after the nose has been broken down and disfigured. About the same time, the glottis becomes affected with the same morbid infiltration; the voice assuming at first a hoarse unnatural sound, and subsequently reduced to a whisper. I have seen, however, well advanced cases where neither the nose nor the voice was completely destroyed. There is always more or less dyspnœa, but auscultation reveals nothing, when the air passages are not attacked, beyond feeble respiration.

The smaller *joints* of the hands and feet are almost invariably involved in due time. Ulceration sets in, and the distal and the middle phalanges, generally, are loosened and come off. After the loss of the bones the ulceration heals, leaving the fingers and toes shortened, clubbed and sometimes bent by the contraction of the flexor tendons. Occasionally the destructive process stops at this stage, and the whole disease appears to be arrested. Under these circumstances, the disease is called by the natives "Kurtum." The larger joints are but very rarely attacked. The nails often crack, and a fetid discharge takes place beneath them. The extremities (forearms, hands, legs and feet) are generally more or less thickened, specially the latter. The hypertrophy is uniform, and the skin becomes dark, covered by scales of *peeling* cuticle. The lower extremity assumes then the appearance of the "Barbadoes" or "Cochin" leg (elephantiasis Araborum).

The *reproductive functions* are always more or less affected. In males the sexual passion is at first weakened, and, as the disease advances, is often entirely lost. In females the catamenia are scanty, irregular or cease altogether. Fecundation is quite possible in the earlier stages of leprosy, whether we regard the agency of the male or female. When the disease begins before puberty, it is extremely doubtful whether the reproductive functions are ever properly developed; and it is worthy of remark in this place that these conditions are highly favorable to the limitation of the disease among leprous families.

The *general appearance* of a leper is so peculiar that, when the disease has fairly declared itself, or even earlier, a single glance at the patient is sufficient to identify it. The changes in the faces are quite characteristic and not easily to be mistaken. To these may be added, later on in the history of the disease, the swollen, ulcerous, maimed or crippled state of the hands and feet, and

the shortened, clubbed, contracted or bent condition of the fingers.[1]

Duration and termination.—The symptoms mentioned above continue an indefinite length of time, and it is impossible to say how long the patient may live after the first invasion of the disease. It may be stated however, in general, that leprosy is an essentially chronic disease, and that its progress and the duration of life under it commonly depend on the previous state of the constitution of the patient and the régime which he follows. When the constitution is strong, and the habits of life in relation to food, cleanliness of body, and purity of air are favorable, the leper usually lives long; whereas in opposite circumstances the chances of lengthened existence are of course much diminished. The shortest duration which has fallen under my observation was the case of a woman who was carried off by anasarca about six months after the commencement of the disease; the longest is that of a man who is still alive, now fifteen years from the date of the invasion. Danielssen and Boeck state the average duration of the tuberculated form to be between nine and ten years; and of the non-tuberculated from eighteen to twenty years. These numbers were taken from the cases in the Bergen Hospital, and appear to be too high for Syria where lepers do not enjoy the advantages of an hospital life.

The intercurrent diseases, which are generally the immediate causes of death, are anasarca, inflammation of the air-passages, diarrhœa, dysentery, low fever and exhaustion.

Morbid anatomy.—I have not yet succeeded in obtaining a leprous subject for dissection. The morbid changes which have been observed by Dr. Danielssen and Boeck of Norway and Dr. Carter of Bombay, in post-mortem examinations, are stated in the 'Report on Leprosy' by the Royal College of Physicians.[2] The main points are the following:—1. The nerves were found to be enlarged, their

[1] Avicenna gives the symptoms of leprosy in the following words:—"El Judham commences with a change of the complexion to a dusky red; the eyes are also red; the breathing is difficult; the voice hoarse and nasal; sneezing and obstruction or stopping up of the nasal passages; perspiration on the chest and about the face, its odour and that of the whole body and the breath is fetid; sleep is disturbed by disagreeable dreams and a sense of heavy weight on the body; the hair of the face drops; the nails split; the features are altered; the face becomes puffed, and its colour dark; the blood coagulates and stagnates in the joints; the breathing becomes more laboured until it amounts to dyspnœa; the voice is reduced to a whisper; the lips are thickened; glandular bodies appear on the skin, which gives the patient the appearance of an animal called by the Greeks a satyr; ulceration of the body takes place if the disease be of the progressive form; the cartilage of the nose is eaten off, and the nose and joints drop, and pus is discharged; finally, the voice and hair are completely lost, and the complexion becomes very dark. The pulse of a leper is weak" (Canon B. iv. 3). It is remarkable that neither Avicenna nor any Arabian author mentions the anæsthesia which forms so prominent a feature of leprosy.

[2] 'Report,' pp. lxxii and lxxiii.

terminal branches, however, atrophied and pearly in aspect (Carter). 2. The nervous centres in the cranial and vertebral cavities were diseased only in the anæsthetic (non-tuberculated) form (Danielssen and Boeck). 3. The skin (cutis vera) of the affected parts in the tuberculated form was thickened, and on squeezing it between the fingers, a yellowish-white viscid or gruelly fluid exuded; in the non-tuberculated form, it was thin, and the muscles were atrophied. 4. The mucous membrane of the nares, fauces and larynx was swollen, occupied with tubercules, soft and often ulcerated. The rima glottidis was sometimes nearly closed up. 5. The viscera were more or less affected; the mesenteric glands were generally enlarged; isolated rounded ulcers were occasionally found on the inner coat of the intestines; the liver was sometimes the seat of tuberculated deposit, and the kidneys were almost always found in the advanced stage of the disease more or less seriously affected, the morbid changes being usually those characteristic of albuminous nephritis. The condition of the blood of leprous patients was found by Danielssen and Boeck to vary considerably from the healthy standard, the most marked changes consisting apparently in the excessive quantity it contained of albumen and fibrin; the former being the chief constituent of the morbid exudation in the skin, tubercules, and sheaths of nerves in leprosy.

The morbid lesions mentioned above appear to establish clearly the view which has been held in relation to the nature of this disease both in ancient and modern times, viz. that it is a specific poison which, once developed in the system, diffuses itself and involves in a great measure the whole body. This is the position which leprosy has taken in the classification of the Royal College of Physicians; and with the definition of the disease given by recent authors it may be interesting to compare that of Avicenna, which is as follows:—"El Judham is a malignant affection produced by the diffusion of black bile throughout the whole body, and vitiating the structural condition, form and appearance of the organs; often attacking their continuity, so that they are corroded by ulceration and fall off. It is very like a general cancer of the whole body, and commonly terminates in ulceration."

Forms of leprosy.—Two forms of this disease have been of late years recognised, and called respectively the *tuberculous* and the *anæsthetic*. The former is characterised by the presence of tubercules on the face and extremities; the latter by their absence, and the presence of bullæ; these, though almost confined to it, do appear sometimes in the tuberculous form; and I have occasionally observed a few scattered tubercules in the anæsthetic variety. Anæsthesia is common equally to both forms, and the nomenclature is, therefore, evidently wrong and misleading. To make a clearer and more accurate distinction between them, the College of Physicians proposes the terms

tuberculated and *non-tuberculated*; and these are, so far, preferable to the old ones, and appear to answer the purpose so well that I have adopted them. The College calls attention also to the difference between the words "tuberculated" and "tuberculous" in order to avoid any confusion between leprosy and tuberculosis. It may be useful to add a third form to the two proposed by the College of Physicians, which may be called the *mixed*. It would indicate a condition of the disease which partakes equally in the characteristics of both forms, a condition which I have often observed, and which has sometimes puzzled me in classifying my cases.

Avicenna's division of the disease into two forms, the *stationary* and the *progressive*, appears to be a correct one, and might be usefully adopted. For there are certainly cases in which the disease appears to be arrested for a long time; and though it may not have entirely left the system and may break out anew, yet this condition in so grave a malady as leprosy deserves to be carefully noted, and may form the basis of a nominal, if not actual, variety.

Leontiasis, a word which occurs frequently in the works of old authors, is not so much a distinct form of leprosy as a very peculiar appearance of the face, which is occasionally met with in some cases of the tuberculated disease, giving a striking *leonine* expression to the countenance. In such cases, the skin of the face is thick, dark, tuberculated and nearly hairless; the eyebrows project forwards, are knotty and overhang the orbit; the forehead is drawn into deep vertical lines; the mouth is deep set and its corners drawn downwards; the eyes are red, hairless and vacant, and the general expression is stolid and unchangeable.

I have endeavoured, hitherto, to give a full and accurate picture of the disease as I have observed and studied it in this country. To make the picture still more faithful, I shall now give the notes of some of the cases which I have met with. I have chosen them from a large number and the selection was made in reference to their being closely typical of the different forms of leprosy.

Case 1.—H. M—, male, aged 40, native of Mount Lebanon, tuberculated form. The birthplace of this man is in one of the villages on the western slope of the Lebanon. He has two maternal aunts, and the son of one of them, lepers.

The disease began six years before, and he first noticed it as a feeling of numbness in the left leg. Four years afterwards, the right leg was similarly affected, and both legs became swollen with squamous patches. About that time, he noticed a tubercule on the right leg. Other tubercules have appeared since, and now they are scattered over the face, forearms and legs. They itch, but are not painful, and their size varies from a small pea to a filbert. Sensation is considerably impaired. The face is dusky red and slightly tuber-

culated and anæsthetic; the ears are thickened, especially the lobules, tragus and antitragus, but have sensation; the hair of the eyebrows is beginning to fall, but the beard continues to be unaffected; the eyes are red with venules running across the conjunctivæ; the lower lip is slightly tuberculated; the nose discharges occasionally; the voice is nasal; on the hard and soft palate are elevated patches where the mucous membrane is soft and ulcerated; the legs are anæsthetic, more on the anterior than posterior surface; the forearms are similarly affected, but the hands are hyperæsthetic, the slightest tap being painful; there are a few small tubercules on the chest and thighs.[1]

The patient is a well-formed man, and is temperate and cleanly in his habits. His general health is good, but feels occasionally weak; he can carry a weight more easily than he can walk a long distance.

He thinks that the disease advances somewhat every winter, and stops during the rest of the year.[2]

About six weeks ago, he tried a strong decoction of common centaury and feels much better after having used it. He says that the formication has almost ceased, and that the colour of his face is clearer.

I advised him to continue the centaury from time to time, and use cold bathing every morning. I saw him about nine months ago and have not seen him since.

Case 2.—H. G.—, female, aged 28, native of Mount Hermon, tuberculated form. Her sister was a leper, and died some years ago.

The disease commenced about three years since (when she was nursing a child) with two tubercules which appeared on the back. One of the tubercules suppurated about a year afterwards, and, at that time, a good many others came out on the extremities. Less than a year ago they appeared on the face, and since that time many of them have suppurated and healed. Her voice became changed six months ago, and the hair of her eyebrows began to fall.

A large number of tubercules exist at present on the face, hands, forearms, legs and feet; they vary in size from a small pea to a filbert; none are ulcerated. Their colour is dusky and they itch. Dorsum of left foot is thickened with deposit. Voice hoarse and nasal. Sensation appears to be unimpaired.[3] General health good.

I saw this patient in 1868. She died in 1871, as I was told, of fever. The child, whom she was nursing when the disease first

[1] Tubercules and bullæ on the trunk and upper division of the extremities are rarely met with. They occur, however, occasionally to a limited extent.

[2] Lepers living in hot plains complain most in summer. This patient lives on the Lebanon where the cold of winter is comparatively severe. It would thus appear that extreme degrees of temperature are alike unfavorable to leprosy.

[3] This is not a solitary instance of unimpaired sensation. Such cases are occasionally met with, and they are perhaps limited to the tuberculated form.

commenced, is still alive, and remains hitherto quite free from any symptoms of leprosy. During the earlier course of the disease, she had another child who died in infancy.

Case 3.—M. G—, male, aged 22, native of Mount Hermon, non-tuberculated form. His great maternal aunt was a leper.

The disease began nine years ago (1863) in the form of dark red patches on the sides of the trunk and slight swelling in the feet, accompanied by a febrile state which continued fifteen days. Under the use of tonics he appeared to get over these symptoms; he was sent soon after to a boarding school where he remained a year. While in school he began to feel numbness in the extremities, more in the lower than in the upper. The soles of the feet degenerated. In May 1865, he had a return of the patches on the trunk, the numbness of the extremities increased, and there was puffiness of the skin on the patches, the hands and the face. The eyes became red, the ears were thickened, and the swollen parts were red, glossy and insensible to the pricking of a pin. He was then treated with arsenic, diuretics and sulphuretted baths: at the end of five months, the patches and swelling desquamated and disappeared. The anæsthesia of the extremities however continued, and for this, electricity was now employed with some temporary benefit. In May, 1867, all the former symptoms reappeared, but in a milder degree compared with 1865. He was again treated with arsenic and sulphuretted baths; and after three months treatment the symptoms gave way.

I saw the patient first in October of that year, and his condition then was as follows:—The face was somewhat red and dark; the eyes were red with venules; the extremities, especially the lower, were œdematous and had a bluish colour without coldness. There was a number of large reddish discolorations on the chest and back, with a few smaller ones on the thighs. The skin on these patches was considerably thickened, covered with fine scales of cuticle, and somewhat anæsthetic;[1] the impaired sensation of the extremities was greater in the lower than the upper; the nose was somewhat stopped up, and he complained of difficulty in breathing through it. I put him on the use of quassia twice a day.

A fortnight afterwards I saw him again. He said he felt better and stronger. The discoloration appeared to be fading; the hands were less œdematous and bluish; no change in the anæsthesia; the patches presented a cleaner surface.

He continued the quassia for some time longer, and then began to take the iodide of arsenic, beginning with one twelfth of a grain and gradually increasing it to one fifth three times a day. At the

[1] The anæsthetic patches on the trunk are an unusual occurrence in leprosy. I have seen, however, another case of this kind, the notes of which I shall presently give.

end of three months, the discolorations had considerably subsided, his general health was better, the face clearer, and the hands and feet more normal; but the anæsthesia was the same in every respect as before. It was, however, doubtful whether the improvement was the result of the treatment, or of the cooler season of winter. In his previous attacks he had always got better in winter.

Since that date the affection of the skin of the trunk and thighs has passed away, and the disease has been confined to the extremities and face. His beard has become well developed, but the face is still red and darkish. The insensibility of the hands and feet has rather increased than otherwise, and bullæ occasionally appear on them. He has lost one of his great toes by ulceration, and the ring and small finger of one hand are contracted and bent. He left a few days ago to try the hot baths of Tiberias.

Case 4.—M. S—, male, aged 25, non-tuberculated, (anæsthetic patches [1]). The disease began, three years before, in the form of a patch on the external aspect of the thigh, which has continued to increase in size until it measures now ten inches in length, and six in breadth. It is hairless; colour somewhat dusky; and wholly insensible to the prick of a pin. There are two other patches on the face, one on the left cheek hairless, the other on the right side of the chin having a few hairs; both are anæsthetic. The patient has a sallow look, and his general health is feeble.

Case 5.—K. E—, male, aged 72, native of Mount Lebanon; mixed form. The patient states that the disease commenced twelve years ago with swelling of hands and feet, which subsided four months afterwards. The forehead was also swollen, and there was much hyperæsthesia in both ears. Numbness of the extremities set in from the commencement, and bullæ broke out on them. A year ago, tubercules appeared on the forearms. Up to sixty years of age, he had enjoyed remarkably good health.

Face now somewhat dusky and thickened, but sensitive; lobules of the ears anæsthetic; eyebrows and beard thinned, while most of the cilia are gone; eyes red with venules; has *arcus senilis;* nose running, the Schneiderian membrane ulcerated, and that part of the triangular cartilage which enters into the formation of the septum of the nose completely eaten off; the inferior turbinated bones appear to be whole; the vomer is gone; the discharge from the nose is thin fetid pus; the soft palate and glottis (examined with the laryngoscope) are red, but not ulcerated.

[1] This is evidently the black Baras (lepra nigricans) of the Arabs. Dr. H. V. Carter, of Bombay, makes it a third and distinct form of leprosy. Judging, however, from the history of the previous case, I am disposed to think that it will ultimately terminate in the usual non-tuberculated form. Avicenna says distinctly that the black Baras is one of the phenomena which precede el judham.

The muscles of the forearms are atrophied; the skin is dark, scaly, and nodulated; some of the tubercules discrete, others agminated; the skin of the hands whole with white cicatrices of old bullæ; there is a recent one on the back of one of the first phalangeal articulations just drying up; another is in the vesicular stage; there is a visible depression on the back of the hand between the thumb and the forefinger (atrophy of the *abductor indicis*);[1] the palmar surface is more sensitive than the dorsal; and the forearms are anæsthetic up to the elbows.

The feet are swollen, and the skin of the legs is thickened, dark and somewhat scaly with cicatrices of old bullæ; the second right toe is shortened by the loss of the middle phalanx; the skin of the anterior surface of the leg is more thickened and less sensitive than the posterior. The anæsthesia is limited upwards by the knee-joint.

The trunk is entirely free from disease. His breathing becomes difficult on going to bed. He says he feels better in the warm season, is apt to feel cold, and seeks the sun for warmth.

He has been treated from the commencement of the disease, and has taken arsenic, tonics, and diuretics. He appears to have obtained most benefit from diuretics for the swelling, and from vegetable tonics.

Case 6.—N. M—, male, æt. 40, "stationary" leprosy of Avicenna. The patient was born and brought up in Beyrout, but his parents came from the Lebanon. He married seventeen years ago, and has three living children. He has been a leper for twelve years; nine years ago he separated himself from his wife and children, and lives apart from them.

He gives the following history of his case:—The first thing he noticed was a tubercule on the ulnar border of the left forearm, which inflamed and suppurated. About the same time, the complexion of his face was altered to a dusky red, and the hair of his beard began to drop. A few months afterwards, the skin of his face, forearms and legs became thickened. Bullæ broke out and discharged for a longer or shorter period, and then healed. During the earlier stages of the disease, he was much troubled with formication in the extremities. Some years ago, his nose began to run on account of a sore within it. Soon after his voice became husky, and since that time he has felt considerable difficulty in breathing, but without cough, and independently of atmospheric changes.

His face is now very red and dusky; the skin and lips are thickened and insensible to the prick of a pin; the hair of the beard

[1] I have rarely seen a case of leprosy which did not present this remarkable appearance, so that, slight as its pathological meaning may be, it is highly pathognomic of the disease.

is thin, and that of the eyebrows is completely lost. The thickness of the skin is uneven. The voice is reduced to a whisper.

The hands have a dark appearance, are thickened, and look deformed. The *abductor indicis* is atrophied. The fingers are thick, shining and somewhat contracted. The anæsthesia extends from the elbows downwards.

The legs are somewhat thickened, and are insensible from the knees downwards; their colour is dark. The integuments are without sores on either the lower or upper extremities, but have white glistening cicatrices of old ones. The toes are whole and normal; the nails have fallen off, and new ones have taken their place.

He enjoys good health, has not entirely lost his sexual appetite. He denies any such causes as contagion or family taint. He lived like other people, and knows of no cause whatever. Though a professional beggar on the wayside, his clothes and habits appear to be quite clean, and this may be the secret of the "stationary" form of his disease. With the exception of the depredations committed by the leprosy, he is in pretty fair health, his body is and has for some time been free from sores, and his articulations have not been attacked, though the disease is of twelve years' standing.

A careful examination of this patient's nose, mouth, and larynx showed the following points:—1. The Schneiderian membrane of the nose had at one time been extensively ulcerated, and the results have been the destruction of the triangular cartilage in great part, and softening of the lateral cartilages, an adhesion of the external walls to the septum, producing a marked contraction of the nasal passages, reducing them to very narrow canals, so that the respiration of the patient has now to be carried on chiefly through the mouth. 2. There is a long cicatrix, entending on the middle line throughout the whole length of the hard palate; the uvula has been lost completely by ulceration, and its site is indicated by a small fleshy tubercle; the pillars of the soft palate are hardened by cicatricial tissue, the anterior being drawn outwards, and the posterior inwards, narrowing the isthmus of the fauces. 3. The mirror of the laryngoscope was thus introduced with considerable difficulty, and the two posterior pillars, being drawn downwards and inwards in the shape of tense curtains, precluded a clear examination of the rima glottidis. During swallowing, however, a short glimpse could be had of the glottis, which appeared gaping and glossy, as if it had been the seat of ulceration. The vocal cords are not seen at all. Auscultation revealed nothing abnormal, farther than feeble respiration.

Section II.—*Leper-Houses of Damascus.*

The object of these houses is evidently the simple one of giving a home to lepers. They are not now, nor do they seem

to have ever been, medical hospitals for the treatment of leprosy. They were built by benevolent persons for an unfortunate class of persons, who, similarly affected, do not shun each other, and thus avoid the miseries of a lonely life. There is also a prevalent idea that there is something peculiarly sanative in the climate of Damascus, which arrests the progress of leprosy, though it may not cure it. Others suppose that the climate destroys the communicability of the disease by contagion. The largest number of lepers come to these houses from different parts of the country, however, with the design of separating themselves from their families, and of obtaining the advantages of public charity. There are two leper-houses in Damascus—one for Moslems, the other for Christians.

The Moslem leper-house is situated out of the city, near one of its ancient walls. It is a miserable building—if it, indeed, deserves that name—consisting of some twenty mud cells, irregularly distributed, windowless, dark, and filthy. There are no indications of great antiquity about the place, though the current tradition is that it was built by Naaman the Syrian, after he had been cured of leprosy by the prophet Elisha, and that from the ancient Syrians it passed into the hands of the Christians, and from them to the Moslems. The establishment has an endowed property of one garden and two houses, the united proceeds of which do not exceed £15 per annum. Besides this small sum, which is distributed among the lepers, their friends, when they are in good circumstances, help them, and they are always considered as worthy objects of charity.

The Christian leper-house is altogether a better building. It is situated in the Christian quarter within the city, consisting of about twenty rooms, each room having a door and window, which open into a large uncovered court. It was built in 1864, the old one having been destroyed, with the rest of the Christian quarter, in 1860. It has some endowed property, but the proceeds are far too scanty to support the inmates, and they have to depend mainly on charity. Like the Moslem, each leper occupies a room separately, and looks after his own food, no general provision being made for the whole.

As far as I could observe, and as I am assured by the lepers themselves, they appear to live in great unanimity among themselves, their hard and peculiar lot exercising apparently a mollifying influence on their general character. Afflicted by a common disease, shunned by the public, and considered to be the special objects of the visitations of Providence, they are looked upon by all classes with unmingled pity, and called "brethren." In both, the Christian and Moslem establishments, the familiar epithet of "brother" and "sister" is used in speaking to and of each other. The Moslems

believe that the disease of Job was the leprosy, and that all lepers share with him the favoured standing which he had with God; and they are, therefore, called in official documents the "Leper-Lords" (el-gadat el-jathma).

I visited the leper-houses of Damascus in 1868; and, after having examined each case separately, reduced them to tabular lists. Table II was taken this year (1872) for me by a medical friend. Table III was made from the notes of cases which I have taken in dispensary and private practice. (Abstracts of these tables will be found subsequently.)

Section III.—*Notes and Observations on Leprosy.*

Sex.—Of the 49 cases recorded in the tables mentioned, 39 were males and 10 females; the proportion thus being nearly one fifth of females to the whole number. It would appear, therefore, that leprosy is much more frequent among males than females, and this accords with the observation of the majority of medical men who have had an opportunity of studying the disease. In the conclusions drawn by the Committee of the College of Physicians from the evidence submitted to them, it is stated that this difference may arise from the fact that, in most countries where the disease prevails, the women are secluded, and not so apt to fall under the observation of medical men as males.[1] The force of this suggestion, however, would apply more to hospitals and leper-houses than to private and dispensary practice; and, in looking at the latter class of cases in my own experience, I find that out of 18 cases, 5 only were females; which gives no less than one fourth. On the other hand, if we look at the other tables, and test the matter as to the sex of tainted relatives of lepers, as given by themselves, the difference becomes very considerably diminished; for of the whole number given of the lepers, 16 were females and 19 males. The proportion of males thus continues to appear to be somewhat higher than that of females. On the whole it may be safely stated, I think, that in Syria the disease is more frequent among males than females, and that the difference probably arises from the greater exposure of the former sex to the vicissitudes of temperature, fatigue, and other depressing causes.

Age.—I have not seen or heard of a leper who was attacked earlier than five years of age, though the single case in the tables, where the disease is stated to have begun at the age of five, may mean any period in infancy or childhood. Probably, the product of conception would show the symptoms of the disease early when either of the parents was a leper at the time of its formation. A large number are attacked about the age of puberty.[2] The most

[1] Report, p. lxii.

[2] "Children of leprous parents are either born leprous, or will have the disease

usual period is below 30; it is less frequent between 30 and 40; and rarely appears after the latter age. In reference to this point, the tabulated cases show the following results:

Age at declaration of disease.	Number of cases.
From 5 to 15	13
„ 15 to 20	9
„ 20 to 30	14
„ 30 to 40	7
„ 40 to 50	2
„ 50 to 60	2
	47
Unknown	3

Comparative frequency of the Tuberculated and Non-tuberculated Forms.—Sometimes the two forms run into each other, and become so mixed that it is difficult to make a distinction between them. Such cases may be, perhaps, properly classified under the "mixed" form. Classified thus, we have of—

The tuberculated form	22
Non-tuberculated	21
Mixed	5
	48

Duration of the Disease.—It is impossible, in this country, to follow a leper's case to the period of death; and, even in leper-houses, the patients are not stationary. I can, therefore, only give here the duration of the disease up to date of observation:

Duration.	Number of cases.
From 1 to 5 years	21
„ 5 to 10 „	16
„ 10 to 15 „	8
„ 15 to 20 „	1
„ 20 to 25 „	1
	47

As regards the relative duration of the tuberculated and non-tuberculated forms, Danielssen and Boeck state that the former runs a shorter course. The result of my cases, as to the general average of duration, is as follows:

Form.	No. of cases.	Average of duration to date of observation.
Tuberculated	22	6¼ years.
Non-tuberculated	20	7⅓ „
Mixed	5	10⅔ „

The case of longest duration of the tuberculated form that I have seen was fifteen years, of the non-tuberculated twenty-five years, and of the mixed twelve years.

at puberty" (Ibu Zekaria). From an examination of my tables it will be fouud that, whenever the father or mother was a leper, the disease appeared before or about the age of puberty.

Topography.—With a single exception, all the cases mentioned in my tables came from villages or towns situated in mountainous districts or high table lands. So far as I know, none of those places are marshy or malarious. The exceptional case is that of a leper who was born and has always lived in Beyrout, but his parents came from the Lebanon. During a long residence in Aleppo and Beyrout I have not seen a single case among the *boná fide* natives. Damascus is said to be free from the disease, though I saw there one case and heard of another. Jerusalem and Nablous have a good number of lepers; but I believe most of them come from the adjacent villages. On the whole it may be safely said that the cities and large towns of Syria, when the inhabitants are well fed and are generally clean in their habits, are comparatively free from leprosy. The limitation of the disease to villages, whose position is often high and healthy, would strongly point to the poor living, uncleanly habits of the inhabitants, and the unsanitary condition of the houses, as some of the most active causes of leprosy.

Causes.—Most of the lepers I have seen belong to the lower classes of society, and come, as has just been mentioned, from villages, where to poverty, uncleanliness of person and clothing, filthy, over-crowded, ill-ventilated houses are to be added. An hereditary taint is doubtless the principal cause in many instances; but it fails to account for every case. Over-fatigue and exposure to cold and damp weather, may serve to excite the disease. Certain kinds of diet, as fish, salted meat, grease, oil, and some lentils, are usually enumerated among the exciting causes of leprosy.[1]

After the disease has declared itself, there can be no doubt that the manner of living has great influence in hastening or retarding the course of the malady. I have known instances where good living, pure air, scrupulous attention to ablution and cleanliness, and the general observation of the laws of hygiene have kept the disease at bay for a long number of years.

Hereditary Taint.—The Arabian physicians, modern authors, and all those who have had an opportunity of seeing much of leprosy, are agreed that it is often a transmitted disease. Sometimes, however, it certainly arises spontaneously from other causes. Tabulating my cases from this point of view, we have, of forty-eight cases—

Tainted—25. Non-tainted—17. Doubtful—3. Unknown—3.

Laying aside the doubtful and unknown cases, the proportion of the tainted to the untainted is that of twenty-five to seventeen—a large ratio as to the hereditariness of leprosy. It should be also taken into consideration that it is sometimes extremely difficult to get an admission from the leper that any of his relatives are affected with

[1] Avicenna mentions the following causes of leprosy:—Contagion, hereditary taint, impure air, and certain articles of diet, as fish, salted meats, asses' meat, and lentils.

the disease, and occasionally I have had recourse to a very searching cross-examination before I could find out the truth of the matter. When the patient was positive in his denial of any knowledge of family taint, I have put him down as a non-hereditary case. Besides wilfulness or ignorance, it is quite possible that the disease may have passed two or three generations, thus making a thorough examination among ignorant people almost impossible.[1]

As regards the degree of tainted relationship in the ascending and collateral lines, the tables show:

Mother	4	Father	1
Maternal uncle	4	Paternal uncle	3
Maternal aunt	2	Paternal aunt	1
Great maternal aunt	1	Great paternal uncle	2
Maternal cousin	2	Paternal cousin	1
Taint on mother's side	13	Taint on father's side	8

It will thus be seen that the law of inheritance was nearly twice as operative in the maternal as in the paternal line, and that it was more than twice in the indirect than in the direct maternal line, while it was 8 to 1 in the paternal line. For, of 21 cases of inheritance, we have 13 on the mother's side, and only 8 on the father's; and of 13 on the maternal side 4 only were direct from the mother, and of 8 on the paternal side 1 only was direct from the father.

It may possibly be a true explanation of this remarkable difference in the agency of hereditariness between the direct and indirect lines, that lepers lose early the power of reproduction, and thus give a wider range to the family taint in the collateral line. It may also be true that, in the primary stages of the disease, males lose their reproductive functions earlier than females, thus giving a wider scope for

[1] "Drs. Danielssen and Boeck have given two tables to show the relative frequency of the influence of hereditariness in the cases of leprosy, treated in the hospital at Bergen. Of 145 cases of the tubercular form, hereditariness could be traced in 127 instances; and in 68 cases of the anæsthetic form, it could be traced in 58 instances. 'From these tables,' they remark, 'it will be seen that out of 213 leprous patients the disease was hereditary in 185, and that in 28 cases only it was of spontaneous development. Moreover, it will be seen that the hereditariness was more frequent (plus répandue) in the collateral line than in the direct line. What deserves particular notice is the mode of its propagation in passing through successive generations. The singular result is remarked that the disease not only passes over some generations, but that it manifests itself in the second and fourth generations with much greater intensity than in the first and third generations. If it has spared the first generation, it, as a general rule, appears in all the individuals of the second, who transmit the germ of the disease to succeeding generations. Tolerably often it seemed to pass over the second and third generations, and to reappear in the fourth generation, and then to spread in all directions, so to speak, with a new energy.' . . . 'We have already said that leprosy may also be acquired. We speak of those cases where the malady declares itself in persons born of healthy parents, in whose families the disease has never been seen, but who have resided, for a longer or shorter period, in countries where it is endemic, and who have lived under conditions liable to occasion its development.'" ('Report on Leprosy,' p. lxviii.)

diseased progenitors in the case of the latter than in the former. Let me repeat a remark I have made before, namely, that children born of leprous parents, prior to the declaration of the disease in them, are not so liable to inherit it as when either the father or mother was fully tainted at the time of conception. I have now in my mind a number of children of leprous parents, born before the disease had manifested itself, of different ages from 9 to 30, who are still free from any visible taint.

From the preceding numbers it will have been noticed that the agency of inheritance does not account for all the cases given, leaving at least a third to be accounted for by other causes. Even if we should add to the hereditary cases a few more on the ground of defective knowledge, we should still have a certain proportion which can be only traced to spontaneous development under favorable conditions.

Contagion.—The idea is almost universally entertained in Syria, that leprosy is a contagious disease. "Flee from a leper as you would from a snake," is one of the traditional sayings of the Arabian prophet, and well known among his followers. David el Basir, an ancient Arab physician and author, commenting on these words, says that they clearly indicate the highly contagious nature of this disease. Another tradition, however, attributing all things in nature to the direct agency of God, denies any such thing as contagion, and condemns the man who would seek to elude disease on this ground as guilty of impiety. In times of plague and cholera, and other epidemic diseases, but few Moslems leave their homes, and they are far more attentive then to the victims who fall than their Jewish or Christian compatriots. Still, they believe in contagion, however they may explain it theologically, or act practically in view of its actual presence. Avicenna and all the Arabian physicians are unanimously of opinion that leprosy is a highly contagious disease. The Greek and Latin physicians seem to have been of the same opinion. Aretæus, who wrote towards the close of the first century, says it is dangerous to have any intercourse with persons labouring under leprosy, no less than in the case of the plague, as both are readily communicated by respiration.

On the other hand, all modern observers are nearly agreed as to its non-contagious character. The College of Physicians, who have had an abundant opportunity for collecting information on this subject from different parts of the world, say—"The all but unanimous conviction of the most experienced observers in different parts of the world is quite opposed to the belief that leprosy is contagious or communicable by proximity or contact with the diseased."[1]

I have put the question to many lepers, whether there was any reason to believe that they had contracted the disease by contagion

[1] "Drs. Danielssen and Boeck state that, 'Among the hundreds of lepers whom we have seen daily, not a single instance has occurred of the disease spreading by

of any kind. They were all positive in denying it. One of them expressed himself in this way:—"I saw but one leper in my life, and I never came near him." I know instances of lepers' wives living with their husbands a number of years, who never gave it to them; and *vice versâ*. I saw a leper Jew in Damascus who lived four years with his first wife. After the disease had fully declared itself, she obtained a divorce and left him. He then took a second wife, whom I saw. She had been living with him about four years also, and was *quite* free from the disease.

And yet, considering the nature and external phenomena of leprosy, and the apparently universal opinion of ancient medical writers, it is somewhat difficult to pronounce positively that it is wholly, and under all circumstances, non-contagious. There is one instance narrated on the authority of Dr. Stangenwald, of Homolulu, physician of the King of the Sandwich Islands, in which the disease was apparently transmitted by inoculation.[1]

Segregation.—There does not appear to be a definite law in Mohammedan books of jurisprudence in relation to lepers, though the idea is a prevalent one that it provides for compulsory exclusion when the friends of the patient demand it, but not otherwise. Such cases are never, however, brought before a judge. The usual way is that lepers, after the disease is well declared, are excluded from all near intercourse with their friends, as a precautionary measure against contagion, as well as from a natural feeling of repulsion. When their friends are in good circumstances, and are kind to them, they are generally, if not always, segregated in some separate apartment, and their wants are attended to. In other circumstances they become professional beggars. In Jerusalem they have a separate quarter near the Zion gate. When they become inmates of such leper-houses as those in Damascus, it is altogether a voluntary act on their part, and they are always at liberty to leave when they choose. On the whole, the lot of lepers is a peculiarly hard one, and their sufferings, from this point of view alone, are extremely severe. The provision made for them in leper-houses is altogether inadequate to their condition and wants.

---

contagion. We know many married persons, one of whom is leprous, cohabiting for years without the other becoming affected. At St. George's Hospital many of the attendants on the inmates have lived there for more than thirty years, and are quite free from any trace of disease.' . . . 'As the result of our observations we have only to deny the contagiousness of leprosy.'" (Report on Leprosy by the Royal College of Physicians, p. lxix.)

[1] Leprosy seems to abound in the Sandwich Islands, and Dr. Stangenwald was sent on a mission to study the disease in different parts of the world. The instance mentioned above was related by the doctor to a friend of a mine, from whom I heard it. Two boys in India were playing together, one was a native and leprous, the other an English boy; the native plunged a needle in his thigh, and said to the English boy, "See what I have done, can you do like me?" His high-spirited companion took the same needle and plunged it in his own flesh, and soon after was attacked with leprosy.

Specific Character of Leprosy.—Leprosy has been confounded with *Elephantiasis Arabum* (Barbadoes leg), from which, however, it is entirely distinct. The latter is a local affection, non-hereditary, rarely attacking any other part except the leg, not involving the integrity of the toes, leaving the general health unimpaired, and having no influence on the duration of life; while the former is a constitutional affection, which taints the whole system, having its own specific causes, symptoms, and course. The confusion has arisen from the two diseases being called by the general name of elephantiasis; and although medical men have long recognised the difference, and made a clear distinction between the elephantiasis of the Arabs and that of the Greeks, still this nosology has occasionally led to the idea that they were two forms of one disease. In leprosy we have often considerable hypertrophy of one or both of the lower extremities, but the diagnosis can always be easily and clearly made out by the history of the case and the concomitant symptoms. I see, therefore, no proof whatever for the conjecture of some writers that the two diseases are allied affections.[1]

The constitutional character of leprosy, breaking out in the form of eruption and ulceration, has led to the serious question whether it may not be of *syphilitic* origin. That it has no such origin is, I think, sufficiently clear from the following considerations:—Syphilis is a modern disease, in this country at least. I have examined most carefully the medical works of Arabian authors, and it is certain that they did not know it, though they expatiate at great length on the diseases of the genital organs. Salih Effendi, who wrote his work ('Ghait él Itkan') about the early part of the last century, does not seem to have ever seen it, and evidently obtained his information about it from European sources. Dr. Russel, who lived some years in Aleppo about the middle of the last century, writes that it existed there in his time, that it was probably imported from Europe, and that it was called the Frank disease[2]—a name which it continues to have to this date. If it did not originate, it was much increased at the time of the French occupation under the first Napoleon in Egypt, from whence it is now yearly imported into Syria. I have put the question pointedly to many lepers, and they all denied having ever been the subjects of syphilis; nor have I ever seen a single case of leprosy which could in any way be traced to a syphilitic origin. In the leper-houses of Damascus I saw a man with tertiary syphilis, but he had got there by mistake, and was evidently in the wrong place.[3]

---

[1] 'Report on Leprosy,' p. lxix.

[2] 'Natural History of Aleppo,' by Alex. Russel, M.D., p. 143 (1st edition). The name he gives, "Frank Zahmedy," is of Turkish origin, and seems to indicate that the disease was brought to Aleppo from Turkey by a northern route.

[3] "We hope," remark Drs. Danielssen and Boeck, "that our description of the

Some affinity between *scrofula* and leprosy may, at first sight, appear quite likely; but the diseases which are commonly connected with the scrofulous diathesis are so very different from leprosy, that it is difficult to believe that they have really anything in common. I have never seen a phthisical leper; and though the cervical lymphatic glands are occasionally swollen in leprosy, neither the character of the swelling nor the subsequent history indicates a scrofulous origin. I believe also that the morbid anatomy of the scrofulous tubercle has been found to be quite different from that of the tubercules of leprosy. It is on the ground of such a wide distinction, and to avoid any confusion between the two diseases, that the College of Physicians calls the two forms of leprosy the tuberculated (not *tuberculous*) and non-tuberculated.

Jewish leprosy.—The description of this disease is given in Lev. xiii; and according to the Mosaic law, any "swelling or scab or bright spot"[1] may be leprosy. In the case of a *swelling* or *rising*, if "it be white in the skin, and it have turned the hair white, and there be quick raw flesh in the rising," it was leprosy (v. 10). In the case of a *scab*, its spreading was sufficient to indicate its leprous character (v. 36). A *bright spot* was leprous "if the hair in the bright spot be turned white, and it be in sight deeper than the skin" (v. 25).

Rabbi Levi gives the following summary of the symptoms and pathognomic signs of the Jewish leprosy:

"There are three signs of leprous uncleanness, viz. whiteness of hair, rawness of flesh, and diffusiveness, which show themselves in this way. If any one has a tumour, or abscess, or whiteness (*leuce*), with white hair or raw flesh, the priest on seeing him shall immediately pronounce him unclean. If, however, there be neither whitish hair nor raw flesh, he shall shut him up seven days. If the spot should then have spread, he shall again without delay pronounce him unclean, because this spreading also is a sign of uncleanness, as we have said. But if no one of these three signs of uncleanness shall have appeared during the seven days in which he was shut up, he shall shut him up a second week. If then one of these three signs of uncleanness shall have shown itself, he shall absolutely pronounce

leprosy has shown that it is a peculiar disease, which, when fully developed, cannot be confounded with any other. The spots, indicative of the tuberculous form, have been in the early stages regarded as *pityriasis;* but this confusion will speedily be cleared up. On the other hand, we have seen cases of this form when such spots existed, as well as at a later stage when distinct tubercles had appeared, mistaken for a syphilitic affection, and treated accordingly. This error is very serious; for, besides the loss of time incurred in the use of inappropriate treatment, the administration of anti-syphilitic medicines is apt to occasion very hurtful consequences, which may lead to the death of the patient. An exact knowledge of the two diseases will prevent such a blunder." ('Report on Leprosy,' p. lxix.)

[1] The words given above are from the English version, but they are differently rendered by others. Gesenius gives—1. Eminence or spot; 2. Scurf or scab; 3. A white spot. The Rabbins appear to differ: Ebn Ezra gives for the two first, burning and gathering, while R. Levi mentions tumour, abscess, and whiteness.

him unclean; if not, he shall declare him clean, because a segregation of two weeks only is appointed for the leprosy which exists in the skin of the flesh; it being understood that, if one of these three signs of uncleanness should appear in him after he had been pronounced clean, the priest shall at once pronounce him unclean, because these three signs of uncleanness are always decisive, as well in the beginning as after segregation. If the whiteness of the leprous spot approaches that of white wool or snow, but afterwards becomes darker (or contracted) and reduced to the appearance of an egg-shell, this is not yet a sign of cleanness, but quite a sign of uncleanness, as in the beginning, until it becomes darker than an egg-shell, when, because it is a pustule, he is held to be clean."[1]

Miriam's and Gehazi's leprosy is described as having transformed the natural colour of the skin into that of snow.[2] The same thing appears to be hinted in the case of Naaman the Syrian,[3] so that abnormal whiteness of the skin in patches, with a corresponding change in that of the hair, seems to have been the main sign, or at least, form of Jewish leprosy.[4] The other cases described are those of raw flesh, or of a spreading scall, both of which were considered to be decisive signs of leprosy.

A careful study of the whole subject would, perhaps, result in the following conclusions:

1. Jewish leprosy seems to have prevailed among the Hebrews in ancient times to a remarkable extent. For not only did the Levitical law recognise it and make an ample provision for its legal treatment, but the Jews were judged by contemporary nations to have been a leprous people.[5]

2. The Mosaic account can not be identified with the present disease known as leprosy. The chief and distinguishing signs of the Jewish leprosy either do not apply to the modern disease, or include many skin diseases, while the main symptoms of modern leprosy are not alluded to at all in the Levitical description.

---

[1] Rabbi Levi Barzelonita's 'Laws of the Jews.' Edited by J. H. Hottingen. Lect. clxix.

[2] Numb. xii, 10; and 2 Kings v, 27.

[3] 2 Kings v, 14.

[4] R. Levi gives the following definition:—"Lepra hæc est, cum albus fuerit locus unus aut plures in cute carnis hominis; ita ut locorum illorum albedo accedat ad albidenem testæ ovi, aut etiam superat eam. Quamdiu verò subobscura, infra albedinem testæ ovi, fuerit, non est lepra; sed leuce vel vitiligo, id est, alius morbus, qui ex lepræ genere non est, sed scabiei." ('Juris Hebræorum,' Hottinger, clxviii.

[5] Thus, Lysimachus, quoted in Josephus:—"The people of the Jews being leprous and scabby, and subject to certain other kinds of distempers, in the days of Bocchoris, King of Egypt, they fled to the temples, and got their food there by begging; and as the numbers were very great that were fallen under these diseases, there arose a scarcity in Egypt." He then goes on to speak of a law having been passed by the king to have them drowned and expelled into the desert, which resulted in the establishment of the Jewish kingdom. ('Josephus against Apion,' B. i, § 34.)

3. In the Septuagint, the Hebrew name of the disease (*tsora'ath*, a stroke) was translated λεπρα (a scale), from which comes the English word leprosy. This translation is wholly unjustifiable on philological grounds, and appears to have been the means of confounding the Jewish with the modern leprosy. Had it not been for this version of the Hebrew word, it is not at all probable that the two diseases would have been joined together under the same name and classification.

4. The principal form of Jewish leprosy seems to correspond most to the *leuce* of the Greeks, the *boras* of the Arabs, and the *vitiligo* of modern writers on diseases of the skin. It is needless to add that this has nothing in common with the disease known now as leprosy, except that the cicatrices of ulceration in the latter are white, and that it sometimes begins in the form of darkish patches, the black *boras* of the Arabs.

5. Maimonides, a rabbi, philosopher and physician of the middle ages, evidently includes the modern leprosy as one of the forms of the Jewish; for he mentions dropping of the hair as one of its characteristics,[1] and seems to consider Job's disease to have been that of leprosy, which is certainly very probable.[2]

6. The leprosy of houses and garments, mentioned in Leviticus, is supposed to have been nitrous incrustations or mildew, a supposition which cannot well be replaced by another, and which seems sufficiently probable.

Treatment.—Is leprosy ever capable of a cure spontaneous or by remedial agents? To this important and highly practical question, the answer from every quarter is extremely guarded. The collected evidence of both ancient and modern writers would indicate that it is occasionally curable, and that it is generally more or less benefited by proper treatment.

Dr. Danielssen and Boeck, whose special and extensive study of

---

[1] "De variis lepræ speciebus videatur Maymon in Hilchoth Tummeat Tzor. Inter alia ibi hæc habemus. *Tsora'ath* est nomen quod *δμονύμως* dicitur, et complectitur species multas, quorum nulla similis est alteri. Nam 1. Albedo cutis vocatur *tsora'ath*. 2. Casus pilorum quorundam capitis aut barbæ vocatur *tsora'ath*. 3. Mutationem in vestibus et ædibus lex etiam *tsora'ath* vocat; illa autem non fuit res ordinaria in mundo, sed miraculum et portentum in Israele." (J. H. Othonis, 'Lexicon Rabbinico-Philologicum,' Genevæ, 1675, pp. 324, 325, Leprosus.)

[2] "Addamus hic illud quod in Jalkut in Jobum 28, 25 legitur. Homo constat ex dimidia aqua et dimidio sanguine. Quamdiu justè vivit, non plus est aquæ in homine quam sanguinis. Quando peccat tunc vel aqua exuberat et fit hydropicus, vel sanguis superat aquam, et fit leprosus" (Id.). The disease of Job is generally considered to have been leprosy. Mr. Carey, in his commentary on Job (chap. ii, v. 7), has collected many incidental allusions and notices of the disease that occur in the book, and certainly their correspondence to the symptoms of modern leprosy is very striking. He has also given much valuable information on notice of this disease by ancient and modern writers.

this disease entitles their opinion to the highest consideration, express their views on this subject thus :—" From our experience and knowledge of the malady we can declare that the more the disease is developed, the more unfavorable must be the prognosis ; nevertheless, far be it from us to say that it is incurable, even in its advanced stage, for we have seen that nature had brought about a cure in several instances where the patients were grievously affected."[1]

Arsenic has long enjoyed a high reputation in India and other parts of the world[2] as an extremely valuable therapeutical agent in this disease. The College of Physicians speak favorably of iodine. I have tried in a few instances the iodide of arsenic, and others in this country have used the same agent, and the general opinion appears to be that, while it is no specific, it has considerable power in arresting the further progress of the disease. Mercury has been found to be highly disastrous, and I have never tried it.

The general evidence for the use of tonics, both mineral and vegetable, leaves no doubt as to their great utility in improving the health, and thus contributing to the arrest of leprosy. The same thing may be said of the systematic use of baths, cold, tepid, saline, and sulphureted.

For the anæsthesia of the extremities both myself and others have frequently tried electricity, with generally useful but temporary results.

The leprous ulcerations lose their fetor and unhealthy character, I have found, under the use of carbolic-acid dressings, and are frequently healed by them.

While all these means are useful, and should never be neglected, the main dependence should be on those general hygienic rules which are of universal application in all chronic complaints—nutritious food, pure air, frequent and daily ablution, moderate exercise, suitable clothing, and regular habits.

It is certain that, so far, we have no specific against leprosy, nor is it likely that we shall discover one. The hope of stamping out the disease by the general amelioration of the physical and moral condition of the poor seems to be the only one held out ; and it is reasonable to suppose that, as its extermination in Great Britain and other parts of the world was probably due to this cause, it may be

---

[1] 'Report on Leprosy,' p. lxxii.

[2] Mr. Palgrave says that leprosy abounds in Arabia, and that the natives use for it an unsuccessful specific, the sulphate of arsenic (tersulphide ? orpiment), which they call "yellow arsenic," and which occasionally cures the disease, and sometimes kills by an overdose, or even by external application ('Central and Eastern Arabia,' vol. ii, p. 33). Dr. Aitkin gives the formula of the famous Tanjore Pill (arsenious acid) extensively used in India ('Science and Practice of Medicine,' 6th edition, p. 941).

found again useful in other countries where the disease is still prevalent.

A good hospital for the lepers of Syria would not only be a great boon to a class of miserable sufferers who are generally neglected, and often thrown into circumstances of great destitution and want, but would afford an excellent opportunity for the further study of this terrible disease.

II.—On the Morbid Histology of the Brain and Spinal Cord, as observed in the Insane.

By J. Batty Tuke, M.D., F.R.C.P.E., Visiting Physician, Saughton Hall Institution for the Insane; late Medical Superintendent of the Fife and Kinross District Asylum.

(*Continued from vol.* lii, *p.* 460.)

Part II.

Every observer must have noticed differences of consistence in brains recently removed from the skull, some being softer and others harder than normal, some being œdematous and others abnormally dry. These peculiarities have a considerable influence on the behaviour of specimens when undergoing the hardening process, and increase or diminish the difficulty of preparing thin sections. In very old standing cases, and in subjects of advanced age where there is much general atrophy, considerable difficulty is experienced in obtaining the proper degree of hardness; such specimens will not stand the ordinary strength of solution or too prolonged immersion without becoming brittle and breaking down under the knife. The same is true of œdematous brains, whilst sclerosed, healthy and comparatively healthy brain-matter does not suffer so much from prolonged immersion; but, as has been already said, it is preferable to cut all specimens as soon as they are hard enough to stand the knife.

In using the knife a manifest difference is experienced in the feel of a healthy and an unhealthy brain; in the former the section glides pleasantly over the surface of the blade, and can be made of almost any size within that of the calibre of the section machine, whilst, in the latter, friability prevents the obtaining of more than limited specimens for the microscope, and even these have, in extreme cases, a tendency to curl up and become fissured.

The process of staining with carmine also indicates changes of structure. It may be taken for granted that a section which refuses to take on the stain of carmine acts so in consequence of its structure being composed mainly of morbid plasma. There is only one morbid product which is readily amenable to carmine, viz. amyloid bodies.

After trial of many clearing agents I have found nothing so satisfactory as glycerine; the results it gives are not nearly so brilliant as turpentine, oil of cloves, &c., but it does not destroy healthy structure or morbid appearances, and its use lessens the number of chemical agents brought into play. Specimens cleared in glycerine should be set up in a mixture of equal parts of glycerine and gelatine.

Changes in the neuroglia.—The changes in the neuroglia which have been observed in this series of autopsies are—

1. General sclerosis, or hypertrophy.
2. Disseminated sclerosis (gray degeneration).
3. Miliary sclerosis.
4. Atrophy.
5. Colloid degeneration,

1. *General sclerosis.*—Although there is reason to believe that this morbid condition is by no means rare, it has been demonstrated only once in this series of cases. The full particulars of the autopsy in which it was noted are published in the 'Journal of Anatomy and Physiology,' for May 1873. The subject was an epileptic idiot, thirty-seven years of age, the right hemisphere of whose cerebrum and right lobes of whose cerebellum were found hypertrophied, coexistent with which was atrophy of the left side of the body. Ten days after birth fits occurred, and subsequently hydrocephalus appears to have supervened. His mind never became developed: the left side of his body was always smaller than the right. At nine months of age his fits ceased and he remained free from them till he was ten years old; they then recurred and continued till his death. In September, 1871, he is described in the case-book of the Fife and Kinross District Asylum, as "a man of middle height; the whole of the left side of the body is considerably atrophied whereas the right appears normal. The face is drawn to the left; the left side of the face seems on a lower level than that of the right, which is fuller. The mouth is drawn to the left. The left arm is hardly half the circumference of the right. The left forearm rests at right angles to the arm, and the hand to the forearm. The whole arm is kept applied to the trunk. No anchylosis exists in either joint, passive movements can be made though there is no power of voluntary movement. The left leg is atrophied to a half, the knee is kept in an inverted position, and the foot is both slightly extended and inverted. The knee-joint is capable of slight passive movement, but is not so as regards voluntary action. The ankle-joint is incapable of either. Progression is solely performed by the right leg, the left being lifted and carried after by the pelvic muscles. The atrophy of the trunk is best seen in the thorax; the thorax is flat, its antero-posterior diameter being $6\frac{3}{16}$ inches; its left side is lower than the right. The circumference is $31\frac{1}{4}$ inches, the right half being $17\frac{1}{8}$ inches, the left $15\frac{1}{8}$. The forehead is low and retreating; the right side is higher than the left, the vertex being formed by the right parietal bone. The patient died of phthisis in May, 1872. Post-mortem examination showed the calvarium to be very thin, dense and hard; only two limited points of diploë were found in the whole circumference. The right side of the calvarium was one half the thickness of the left. Skull large, dome high,

ridged at the summit, to the right of which the parietal bone was round and full, the left being flattened. Capacity of skull 110½ cubic inches. Sutures faintly marked, ossification almost complete. The right side of the facial portion was uniformly on a lower level than the left. The bones on either side were mere shells immediately posterior to the condyloid foramina. On the internal surface the sutures were perfectly ossified. The area of the right middle fossa, and the right cerebral fossa of the occipital bone were much more extensive than on the left side. There were other abnormalities of the skull, but as they have no bearing on the present subject they need not be detailed. The brain weighed sixty ounces, was of firm consistence, tough and resilient, the left occipital lobe was manifestly larger than the right; the relative weights of the two hemispheres were, right 30¼ ozs., left 23½ ozs., the difference being 6¾ ozs. The specific gravity of the two hemispheres as proved *en masse* was exactly 1036, and separate portions taken from either hemisphere were of the same specific gravity. On section there was a marked difference between the gray matter of the two hemispheres as to colour and thickness. The gray matter of the left hemisphere was markedly darker than that of the opposite side; the colours were on the left side dark gray, on the right light pink gray in the outer layers, the inner ones being of a darkish brown, a white band separating them. The gray matter of the left side, as measured by Dr. Major's tephrylometer varied from 11-100ths of an inch in the frontal lobe to 4-50ths of an inch in the occipital. On the right side it was much thicker and much more even over the whole hemisphere. In the frontal and anterior convolutions of the parietal lobe the thickness was 7-50ths of an inch; in the posterior convolutions of the parietal (more especially the annectant convolutions) and the occipital lobe, the thickness was 15-100ths of an inch. The cerebellum was much deformed. The medulla oblongata was markedly larger on the right side above the point of decussation. The pons Varolii and the spinal cord were symmetrical. Microscopic examination revealed in the lobes of the left side only a degree of atrophy of the cells of the outer layers; on the right side there was observed:—1, an increase of the granular material of the outer layer of gray matter; 2, normality of the cells of the two inner layers; 3, irregularity of the cells of the intervening layers as regards their number, shape, size and distribution; 4, thickening and displacement of the nerve fibres of the white matter; 5, an increase of the neuroglia; 6, proliferation of the nuclei of the neuroglia and blood-vessels.[1] The last three abnormalities claim attention at present. The nerve-fibres lay in fasciculi consisting of four, five or six strands, separated from one

[1] The above description is abridged from the original paper in the 'Journal of Anatomy and Physiology,' May, 1873.

another by very clear plasm in which were more than normally numerous nuclei, and a fine fibrillar structure.[1] The plasm was in no way opaque, and contained no morbid products other than those mentioned. This condition was best marked in the occipital lobe, less so in the parietal and in a still minor degree in the frontal lobe; in fact it was coexistent with the degrees of hypertrophy of the several lobes. Sections of almost all the convolutions were cut and the disease was found distributed over the hemisphere increasing from before backwards.

The differences between this condition and disseminated sclerosis (*sclerose en plaques disseminées* are, that although the nuclei are considerably proliferated, they are not so much increased in number or in size, the opacity which characterises the latter disease is entirely absent, and no definite morbid products, such as amyloid, and colloid bodies, or atrophised fibres exist. It appears to be simply an increase of neuroglia which separates the fibres widely and pushes them into bundles. It differs from Virchow's Interstitial Encephalitis in that there is no increase in the size of the nuclei. That it is not caused by œdema is shown by the more than normally dense consistence of the recent brain.

The reason for the existence in this case of the increase of packing material appears to me to be as follows:—Ossification of the sutures having become complete in a unilaterally distended calvarium before the hydrocephalic fluid was quite absorbed, the rigid skull demanded a compensatory substance whilst the process of absorption was going on to completion. This not being met by an increase of the diplöe, as sometimes happens, the most rapidly produced structure of the encephalon was called on for the production of a compensatory substance, and the neuroglia, *pari passu* with the process of absorption of the fluid, exerted itself to supply the demand. This theory appears to me to receive support from the fact of the increase of neuroglia being only present in the right, or hypertrophied hemisphere. That this half of the brain was the seat of lesion is apparent from the trophesial influence it exercised on the left side of the body.

Weighty brains are not unfrequently met with in idiots who in infancy had been the subjects of hydrocephalus; and such persons are often ill-formed, rachitic, dwarfed, or atrophied. I would suggest the probability, that in them the trophesial impairment has arisen from a bilateral general sclerosis of a nature identical with that which occurred unilaterally in my patient; and that the bulging brain occasionally met with in epileptics is possibly due to the same histological condition.

It is worthy of notice, that unilateral atrophy and hypertrophy of

[1] See woodcut, loc. cit.

the brain seem both to exercise an influence on the trophesis of the opposite side of the body. Van der Kolk's famous case is one in point.[1]

2. *Disseminated sclerosis, or gray degeneration.*—The term Disseminated Sclerosis is employed to discriminate between the scattered patches of gray degeneration (*la sclerose en plaques disseminées*), and general sclerosis.

This disease, as it appears in the brains of the insane, was pretty fully discussed in the original paper by Dr. Rutherford and myself; subsequent observations have, however, tended to somewhat modify my views, and to induce me to not to adhere so closely to Rokitansky's theory of its origin. The characteristics of this disease have been so elaborately and frequently described that it is unnecessary here to enter upon them in detail. Amongst the chronic insane it is most frequently met with in the white matter of the corpora striata and optic thalami in scattered patches of various size. Less commonly is it to be found in the white matter of the cerebral hemispheres; but when it does exist in this position it occurs in more widely extended tracts. In the pons Varolii, medulla oblongata, and spinal cord of the general paralytic and epileptic it is by no means of rare occurence, and in such cases it frequently presents itself in its extreme form.

I have seldom failed to demonstrate its contiguity to a vessel or vessels where nuclei are very much proliferated, and around which the nuclei of the neuroglia are in a similar condition. In brains in which considerable proliferation of the nuclei is found, careful search will, in the great majority of cases, enable the observer to discover opaque tracts, which refuse to become amenable to any clearing agent; these, being submitted to the microscope, after immersion in glycerine, will be found not to present the normal histological appearances; the nerve-fibres are atrophied partially or completely, according to the stage of the disease; in transverse sections the axis cylinders and sheaths are destroyed, and the field is occupied by a finely molecular and fibrillated material imbedded in a cloudy homogeneous plasm. In this matrix the proliferated nuclei exist, somewhat enlarged, sometimes slightly granular in appearance; but around the implicated spot they are to be seen in much greater quantity and not actively diseased. The atrophied nerve-fibres occasionally project raggedly into the gray tract where they are lost. I have failed to observe any appearance approaching to a fatty degeneration in patches of disseminated sclerosis. In rare instances spots of miliary sclerosis may be seen in their neighbourhood.

[1] "Case of Atrophy of the Left Hemisphere of the Brain, with Co-existent Atrophy of the Right Side of the Body." By J. L. C. Schroeder van der Kolk, translated by the New Sydenham Society, 1861.

It appears to me, that the first stage of this disease consists in proliferation of the nuclei of the walls of the vessels, which is followed by increase of the nuclei of neuroglia and the development of a morbid plasm, which is, in all probability, modified neuroglia. So far my observations coincide with the views of Rindfleisch; but in the numerous cases examined nothing like nucleated cells, such as he describes, has been detected in the diseased tracts.

Miliary Sclerosis.—This disease was first described and figured by Dr. Rutherford and myself in the 'Edinburgh Medical Journal' for September, 1868. In the following year Dr. Kesteven described and figured it as "a peculiar morbid deposit in the medulla oblongata." ('Brit. and For. Med.-Chir. Review,' April, 1869).

Miliary sclerosis differs from other forms of sclerosis, in that it is not necessarily preceded, attended, or followed by any proliferation of the nuclei, that it is a circumscribed lesion not involving surrounding tissues, except so far as it displaces nerve-fibres, that no morbid plasm is diffused beyond its own area, and that it is in no way connected with blood-vessels. It is a disease which has presented itself very constantly in this series of cases in all parts of the brain and spinal cord; in the latter organ, and in the pons Varolii and medulla oblongata it has been pretty constantly noted in cases of epileptic insanity and general paresis; and in one instance it was found in the spinal cord of a man whose insanity was complicated with chorea. I reserve, however, consideration of its *locale* and its bearings on the pathology of insanities, complicated or uncomplicated with physical symptoms, until they, along with those of other lesions, come to be commented upon.

Miliary sclerosis is a disease of the nuclei of the neuroglia, and its progress is marked by three stages. In the first, a nucleus becomes enlarged and throws out a homogeneous plasm of a milky colour, and apparently of a highly viscid consistence, for the long axis of the spot is almost always in the direction of the fibres, which are displaced by its presence instead of being involved by it; thus indicating that its density is considerably greater than that of the cerebral matrix. In the centre of these semi-opaque spots a cell-like body is generally discernible, possessing a nucleus; this is the original dilated nucleus of neuroglia. In the largest patches more than one cell can be seen; whether these arise from division of the first nucleus involved, or from the original implication of more than one nucleus has not been determined; but from the fact that multiple cells are only seen in the largest spots it is most likely that the latter hypothesis is correct. Occasionally several neighbouring nuclei become diseased simultaneously, coalesce, and form a multilocular patch of considerable extent; the largest noticed is the one fortieth of an inch in its longest diameter. During the second stage of development the morbid plasm becomes distinctly molecular in

character and permeated by fibrils. It is probable that at this period a further displacement of the contiguous tissues takes place, as a degree of induration of the compressed fibres and blood-vessels which curve round the diseased tract is indicated by the increased amount of colouring material which they then absorb. At this stage the morbid tracts present the following appearances. As a rule the spots are unilocular, occasionally bilocular, and in rare instances multilocular; but whatever their condition in this respect is, they possess the same internal characteristics. A thin section prepared in chromic acid viewed by the naked eye shows a number of opaque spots irregularly distributed over the surface of the white matter; they are best seen in a tinted section, as they are not colorable by carmine. When magnified by a low power they have a somewhat luminous pearly lustre, and when magnified 250 and 800 diameters linear they are seen to consist of molecular material, with a stroma of exceedingly delicate colourless fibrils. They possess a well-defined outline, and the neighbouring nerve-fibres and blood-vessels are pushed aside and curve round them. In well advanced cases the plasm seems denser at the circumference of the spots than at their centre, and a degree of absorption of the contiguous nerve-fibres is evident; this solution of continuity is only noticeable at the point where the lateral expansion is greatest. The spots are generally colourless, but in some instances they are of a yellowish-green tint, which may be attributable to chromic acid. They vary much in size; unilocular patches are 1-50th of an inch to 1-100th of an inch in diameter, the multilocular ones from 1-200th to 1-800th of an inch. As many as eleven locules have been noticed in one patch, separated one from the other by fine trabeculæ of nervous tissue.

The nervous tissue of a section containing spots of miliary sclerosis in the second stage, when removed from spirit and allowed to dry, shrinks from the diseased patches and leaves them elevated and distinctly separated from it, so much so that they can be picked out with the point of a knife. The following is the result of a micro-chemical examination of such matter extracted from the first specimen met with. The same processes have been employed by myself in two subsequent cases with exactly similar results; but from its completeness I prefer quoting at length the result of the original investigation which was conducted by Dr. Rutherford. "They were not in the least degree gritty. Hydrochloric acid caused no effervescence. Acetic acid, boiling alcohol, and ether, produced no alteration of the molecular material. When, however, strong pure nitric acid was added to a section containing the spots, the whole section, *molecular matter* as well as *nerve-tissue*, became more transparent. This was the more remarkable, seeing that spirit of turpentine, while it rendered transparent the nerve-tissue, did not affect the molecular matter. As the molecular material became

transparent under the action of the nitric acid, a number of colourless rounded bodies, about the size of a blood-corpuscle, made their appearance in its midst. At first glance it appeared as if the nitric acid had simply revealed these bodies, just as acetic acid reveals the nuclei of most cells by rendering the surrounding matter transparent; and the fact that these rounded bodies resembled the so-called amyloid bodies, favoured this notion; but when the action of the acid was carefully watched, these rounded bodies could be seen forming by the coalescence of minute transparent drops, apparently the molecular matter rendered fluid by the acid. After the acid had acted for five minutes or so, bubbles of peroxide of nitrogen began to be freely evolved from all parts of the nerve-tissue, and from the molecular matter of the spots as well. The successive shocks consequent on the evolution of the bubbles of gas especially brought about the coalescence of the drops inside the spot. When the covering-glass was gently pressed upon, these globular bodies formed by the acid were seen to elongate, and from spots cut through at the edge of the section they could easily be squeezed out into the surrounding fluid, where they re-formed somewhat globular bodies. Many of them preserved their elongated form after the removal of the pressure; this showed that the matter of which they were composed was viscous. By repeated pressure on the covering-glass and prolonged action of the acid, the viscous matter could be entirely removed from the spots, and nothing but the delicate fibrous stroma left behind. It was a network of very delicate connective tissue—the neuroglia, in fact, from which, however, the nuclei and nerve-tubes had disappeared. The disappearance of the nuclei and nerve-tubes could not have been due to the acid, for this did not cause them to disappear from the healthy tissue, the molecular material must therefore have replaced these two elements. Around the spots so emptied of their molecular contents, the abrupt terminations of nerve-tubes could be distinctly seen, showing that a destruction of nerve element had taken place.

"The molecular material, however, did not seem to be of a composition altogether differing from that of the nerve element, for though, unlike the latter, it was not rendered transparent by turpentine, nitric acid caused the appearance, from the nerve element, of viscous matter closely resembling, if not really identical with, that formed from the molecular material. This was particularly well seen in the gray matter of the cerebellum; under the action of the acid, colourless bodies could be seen to form, which closely resembled the so-called amyloid bodies; when pressure was exerted upon them, they were seen to conduct themselves exactly like the viscous globules formed from the molecular material of the spots.

"When dilute nitric acid (one part of acid to two parts of water) was used, the disappearance of the molecular matter was more rapid.

In this case, however, the matter formed by the acid was not nearly so viscous; indeed, it formed quite fluid colourless drops, which, however, did not mix with the acid fluid, but floated about as detached globules.

"Although the viscous matter produced by the nitric acid could be pressed so as to form permanently elongated bodies, these prolongations did not form spontaneously, as is the case when protagon (myeline) is placed in water or nitric acid. Strong sulphuric acid also rendered the nerve-tissue and the spots transparent, and brought about the conversion of the molecular matter into transparent viscous globules. Caustic potash had no effect; the absence of grittiness, and the fact that it did not effervesce on the addition of strong hydrochloric acid, was sufficient to show that the molecular matter was not calcareous. Nor did it seem to be of a fatty nature, as boiling ether did not dissolve it. Had it been protagon, it would have been soluble in boiling alcohol. Alkaline solution of iodine did not tinge the spots more than the surrounding nerve-tissue, and the farther addition of sulphuric acid did not produce a blue colour.

"Let it be remembered, that the preceding account of the action of re-agents applies to a tissue that had been steeped in alcohol, in a solution of chromic acid, and in an ammoniacal solution of carmine."

It is still doubtful whether this lesion can be detected in recent specimens. In two cases, in which, by the chromic acid process, miliary sclerosis in the second stage was demonstrated, the recent white matter, when squeezed out under a covering-glass, exhibited spaces containing a clear material in which some rounded nucleated cells were visible. It is, however, only by prepared sections that its presence can be definitely ascertained.

In the third stage of miliary sclerosis the molecular matter becomes more opaque and contracts on itself, the boundaries become puckered and irregular in outline, and the material often falls out of the section, leaving ragged holes, These holes cannot be mistaken for empty perivascular canals, which, whatever their size are smoothly rounded or oval. When in this condition the morbid products of miliary sclerosis are distinctly gritty, effervesce immediately on being subjected to the action of nitric acid, which produces no such appearances as are evolved by its application in the second stage.

Atrophy.—The wasted condition of the senile brain consequent on the impaired nutritive powers of atheromatous vessels is a well-known pathological condition which is evidenced to the naked eye by wide sulci, sharp and thin convolutions, and a concomitant amount of compensatory serum. When prepared sections of such brains are viewed under the microscope all the tissues are seen to have undergone more or less modification—the cells of the grey matter having suffered from the changes hereafter to be spoken of,

the fibres having become thickened, and the neuroglia, the element now under consideration, being much atrophied. One result of this atrophy of the packing material is to render the brain substance rapidly brittle and stringy when subjected to chromic acid, and to cause sections to split in the direction of the fibre under very slight pressure, like over-dried wood. In spinal cords where atrophy exists these splits are well seen, resembling the divergent cracks of the stump of a tree after exposure to the air and sun. The fibres in such sections are very considerably thickened, as if endeavouring to supply the place of the lost neuroglia. Nuclei are rarely seen, and when present are larger than normal and irregular in shape. Where this condition exists colouring material is very readily absorbed by sections, and so generally as to but slightly differentiate between white and grey matter. Atrophy of neuroglia is only recognisable where the general brain wasting has been extreme.

Colloid degeneration.—Colloid degeneration is one of the most important and interesting forms of brain lesion, being, I believe, the primary pathological change in certain of the most prominent and well-defined varieties of insanity. As its relations to symptoms must be considered at length in the concluding portion of this paper, I will merely say now that good reason exists for believing that on colloid degeneration depend those cases in which the vague term "brain disease" is employed to indicate that a primary affection of that organ is the exciting cause of the mental symptoms.

In its earliest stages this abnormal condition shows itself in circumscribed semi-translucent spots scattered irregularly over the surface of the section, varying in size from the 1-4000th to the 1-2000th of an inch in diameter; they have well-defined irregular edges, and their contents are molecular in appearance. In fresh specimens, however, this molecular appearance is not observable, and colloid bodies appear as round or oval in form, having a distinct wall containing a clear homogeneous, transparent, colourless plasm, and occasionally showing a small nucleus, but no nucleolus. Colloid bodies are not colourable by carmine, however strong the solution and however long they are submitted to it. They appear first in the white matter immediately contiguous to the cortical substance, but as the disease advances they become diffused outwards and inwards. In extreme cases the appearance of sections containing them may best be compared to a slice of sago pudding, for they exist in such large numbers as almost completely to fill the field of the microscope, separated slightly from each other by a fine granular material. Although readily recognisable when set up in Canada balsam or turpentine, the characteristics of colloid degeneration are best brought out by glycerine.

I feel strongly inclined to regard this as a form of degeneration of

the nuclei of the neuroglia; it is first seen and is best marked in the white matter, but in certain specimens in which it occurs in the grey matter cells have been seen which are undergoing, or have undergone, changes in many respects resembling those noticed in the nuclei of neuroglia. It is not associated with proliferation of nuclei. Careful study of a large number of specimens leads to the conclusion that the nuclei are the original seats of the disease, for in all cases in which colloid degeneration shows itself they are to be seen more or less departing from normality; in fact, it may be safely stated that they are always unhealthy, and appear to merge gradually into the colloid condition.

There are points of resemblance (on paper) between colloid degeneration and miliary sclerosis, but there are many differences manifest to the observer which entitle them to be regarded as distinct pathological conditions; of these the chief are that colloid bodies do not undergo the same gradations of development as miliary sclerosis, that they do not push aside the fibres, and that they never can be removed as a separate substance from dried sections in which they exist. It is subject for regret that the several appearances cannot be figured in this place.

Dr. McKendrick, Assistant to the Professor of Physiology in the University of Edinburgh, when conducting experiments on the functions of the corpora striata, found in sections of pigeons' brains which had been traumatically injured certain bodies in the neighbourhood of the healing cerebral substance. These were recognised as colloid bodies. Dr. McKendrick kindly communicated this fact to me, and with his permission I followed up the observation to some extent. The brains of birds were injured by being stirred up or incised with tenotomy knives, and time allowed for the process of healing. At various periods after the infliction of the injury the birds were killed and sections of the wounded cerebra prepared by the chromic acid process. In every case in which the injury was of ten days' standing colloid bodies were found, and they increased in number up to the seventeenth day; they were seen in the neighbourhood of the cicatrix in clusters, their size being much smaller than in the human subject, never exceeding the 1-6000th of an inch in diameter, but otherwise they presented identical characteristics. Further remarks on this line of experimentation are reserved until the series of observations is more thoroughly worked out.

III.—Original Notes of Dr. W. Hunter's Lectures on Anatomy, Physiology and Surgery.

Introduction.—I bought some months since, at a sale in London, the copy from which the following extracts are taken. It is in four quarto volumes, paged consecutively from 1 to 1161; towards the end (pp. 1163—1335) are J. Hunter's lectures. It is fairly written; unfortunately, pp. 1007—1012 are wanting. The title-page of the first volume is dated January 18, 1781, and on the first page is the signature, Thomas Denison. The first fifteen lectures contain the introduction, the general anatomy and physiology of the blood, the vascular system, the absorbing system, the glandular system, &c.; pathological and other subjects are occasionally discussed, *e.g.* the diseases of the arteries, phlebotomy. Special anatomy follows, and is treated in a similar way. The operations of surgery begin at p. 971, and the course concludes with the art of embalming.

Notes of lectures, taken by a student, must always fail to give any correct idea of the lectures themselves; yet imperfect as the present manuscripts evidently are, they furnish at least an outline of the subjects treated, and we find in them, scattered here and there, little portions interesting in themselves, but far more as coming from one so celebrated in his day as William Hunter.

The author of an excellent sketch of W. Hunter[1] states that "in the library of the Royal Medico-Chirurgical Society there are several volumes of manuscript lectures of Hunter. These are of great interest, as indicating the method of his teaching. On reading portions of the manuscript we find, however, that they are not notes by the lecturer, but by students who were attending the courses. One volume, written by a careful hand throughout, bears on the cover the following:—'Anatomical, Physiological, and Chirurgical Lectures, by Dr. Hunter, 1759.' Three other small books in manuscript are entitled, 'A Course of Anatomical Lectures by Doctor Hunter, January 20th, 1773.' A third volume of manuscript, very carefully written out, gives, again, the 'Anatomical Lectures,' without date, but with writer's name, John Ashley; and a fourth volume in manuscript, including the lectures on the 'Gravid Uterus,' is by Dr. Skeete. The care with which these manuscripts have been copied indicates the value attached to them by the writers. In the library of the Medico-Chirurgical Society there will also be found, bound up with the last two introductory lectures of Hunter, some papers relating to his intended plan for

[1] 'Med. Times and Gazette,' 1859, i, 502.

establishing a museum in London for the improvement of Anatomy, Surgery, and Physic."

This writer describes Hunter[1] as "a gentleman and a scholar. He can talk about everything, and knows all that is going on. The antiquary brings him an ancient coin, and finds a colleague who can discuss the points of date and of value. The naturalist shows a fossil bone, and straightway the two are at home and comfortable. The historian discourses with him as a brother; and with the classic he crosses excellent Latin. With the ladies his conversation is charming, for his voice is musical, his expression faultless, and his manner gallant." Hunter began to lecture in the winter of 1746, succeeding Sharpe in his course on the operations of surgery. "He is said to have experienced much solicitude when he began to speak in public, but the applause he met with soon inspired him with courage; and by degrees he became so fond of teaching, that for many years before his death he was never happier than when employed in delivering a lecture."[2] He continued to lecture till 1783; "at length, on Saturday the 15th of March 1783, after having for several days experienced a return of wandering gout, he complained of great headache and nausea. In this state he went to bed, and for several days felt more pain than usual both in his stomach and limbs. On the Thursday following he found himself so much recovered that he determined to give the introductory lecture to the operations of surgery. It was to no purpose that his friends urged to him the impropriety of such an atttempt. He was determined to make the experiment, and accordingly delivered the lecture, but towards the conclusion his strength was so exhausted that he fainted away, and was obliged to be carried to bed by two servants."[3] He died on the 30th of the same month.

"As a teacher of anatomy he has been long and deservedly celebrated. He was a good orator, and having a clear and accurate conception of what he taught, he knew how to place in distinct and intelligible points of view the most abstruse subjects of anatomy and physiology. Among other methods of explaining and illustrating his doctrines, he used frequently to introduce some apposite story or case that had occurred to him in his practice, and few men had acquired a more interesting fund of anecdotes of this kind, or related them in a more agreeable manner. He had the talent of infusing much of his ardour into his pupils, and if anatomical knowledge is more diffused in this country than formerly, we are indebted for this, in a great measure, to his exertions."[4]—Thomas Windsor.

---

[1] Loc. cit., p. 391.

[2] S. F. Simmons, 'An Account of the Life and Writings of the late William Hunter.' London, 1783, p. 11.

[3] Simmons, loc. cit., p. 63.

[4] Ibid., loc. cit., p. 68.

Circulation of the Blood (p. 70).—Some have said that the action of the arteries contributes nothing to the circulation of the blood; but I am of opinion that they contribute to it greatly and independently of their elasticity; which power, I think, may be called muscular. For, endeavouring to inject milk into the mesenteric veins by the mesenteric arteries in a dead subject, I met with great difficulty, but in a living animal it readily passed from the artery to the vein without any further force than filling the artery with a syringe, and then stopping the orifice with the finger it passed of itself.[1]

Rapid pulse in the healthy (p. 74).—We sometimes find a pulse always beating in health 100 or more; Dr. Askew, in common health, had it beat 150 in a minute for many years.

Parts tinged with bile (p. 101).—By the parts surrounding the gall-bladder being tinged, it would appear that the bile itself would transude; but Mr. J. Hunter says that this does not take place till after death. He is convinced of this from having opened several live animals and others just dead, in order to examine those parts.

The lymphatics absorb (p. 111).—I received the first hint of the lymphatics being absorbent vessels from a friend's asking my opinion of Dr. Freke's chirurgical publication about the year 1748; to which I answered that I had not read it. My friend then mentioned Mr. Freke's advising a surgeon in venereal cases to cut out a bubo from the groin, for he said he would by that means eradicate the poison, which, he supposed, was conveyed in some way or other from the genitals to the inguinal gland and detained there. This struck me immediately, so that it came into my imagination that the lymphatic vessels were the conveyors of the poison. Taking up the subject upon this supposition, I was by numerous experiments and observations soon convinced that he was right in his conjecture.

Cause of difficulty in reducing dislocations (p. 257).—It was the general doctrine that, when a bone was luxated, the capsular ligament stretched and dilated before the head of the bone. But upon examining this ligament in a dead subject, one would conclude *à priori* that it is too strong and tight to admit of luxation from external force, without being ruptured. Dr. Hunter says that he believes that the capsular ligament is always torn, for he has several times attempted to luxate the humerus and femur in a dead subject, but never could effect it without tearing through the capsular ligament. These trials, he thinks, are conclusive, because mechanical force acts exactly the same on the dead as on the living subject. He says the phenomena of luxations prove this theory. Sometimes a dislocated bone is reduced with great ease, at other times not with-

[1] I have corrected here and in the following extracts the punctuation and some verbal errors.—T. W.

out great difficulty. This difficulty was accounted for by supposing that in the first case the muscles made little or no resistance to the reduction, but that in the second they contracted strongly, so as to require great force to overcome their action. It is clear, however, that the muscles do not contract so much as to counteract us in making extension, when the patient gives himself up entirely to our directions. The difficulty in reducing a luxated bone consists then in the smallness of the rupture in the capsular ligament, and the direction in which it is ruptured, so that sometimes by making a strong extension, we shall be so far from reducing the luxation, as even to render it impossible to be performed in that manner. After having used all manner of extension and means of reduction to no purpose, it sometimes happens that the bone goes into its place voluntarily as it were. Dr. Hunter says that the torn ligament embraces the head of the bone, as a button-hole does a button, and prevents its return into the joint, and that by turning the limb to and fro there is a much better chance of reducing the luxation by wriggling the bone through again, than by making strong extension, which often tightens the stricture and increases the difficulty. It may sometimes happen that a disease of the joint may so relax the capsular ligament as to allow of luxation without its being torn, but when the luxation happens from external force, I believe the ligament is always torn. The reason why in luxations the tearing of the capsular ligament does not occasion great mischief, as a wound penetrating the joint does, is because no air is admitted to it.

Irishmen with tails (p. 309).—Carpus, or Berengarius Carpensis, as he is called, wrote in the year 1520 a comment upon the book of Mundinus, which was held in the greatest veneration by himself and others, as mentioned in the introduction. He says that in far-distant western islands (he lived in Italy) there is a people called Hibernii, with long tails, two of whom he had seen, but as he had not handled them he could not tell whether the substance of the tail was cartilaginous, fleshy, or otherwise. Linnæus at this present time thinks that some of the human race have tails, for instance, the ouran outang.

Fracture of the patella (p. 358).—In a transverse fracture of the patella, the base was thought to be separated from the apex by a jerk of the muscles in the same manner as the tendo Achillis is ruptured. A man was passing through Holborn with a burthen on his head and fell backwards; his patella was fractured in the fall, though the knee did not touch the ground. This was supposed to be caused entirely by the action of the muscles; the true reason is this, viz. when the leg is in the middle state between flexion and extension, the patella is snapped by any sudden jerk in the same manner as we break a stick across the knee. When the patella is

fractured, the cavity between the fractured ends communicates with that of the joint.

Painting by Hogarth (p. 525).—Mr. Hawkins has a painting done by Mr. Hogarth of a cancer of the coat of a testicle, where the substance was perfectly free from it.

Child born through perinæum (p. 694.)—I once had a patient, in whom, while I was supporting the fore-part of the perinæum and expecting the child would make its way by degrees naturally, the head forced itself through the perinæum, between my hand and the anus, making a transverse laceration through there. The child was born through this laceration without passing through the vulva. The woman soon recovered and had no inconvenience remaining after it, for the sphincter was not torn, only the perinæum suffered, the rectum was not injured, and the woman is still alive and well.

Illness of Dr. Hunter (p. 746).—Dr. Hunter had a complaint in his right kidney, attended with bloody urine, pain sometimes severe, sometimes dull. It was supposed by his medical friends to be occasioned by calculi, but he could not think so, for when the pain was dull, if he rode out in his chariot, the pain was increased by the jolting over the pavement. It turned out to be the rheumatism, for he was seized with it in his shoulder, and from that time his nephritic complaint ceased wholly. This rheumatism was attended with bursting of vessels, for when it attacked his head, blood was extravasated in the tunica conjunctiva and about the eye.

Removal of the spleen (p. 758).—A man at the battle of Dettingen was wounded by a sabre in the left side of the abdomen. The spleen was protruded and mortified so that it was judged necessary to cut it off, except a small piece of it. The wound healed and the man did well, and continues well without any difference in his constitution to this day. Mr. Wilson, apothecary in Henrietta Street, Covent Garden, can affirm the truth of this fact, for he was surgeon of the troop to which the man belonged, and he cut the spleen.

Rupture of the œsophagus (p. 773).—A man was carried to St. George's Hospital with a fractured skull; he was quite senseless from the moment of the accident, of which he died. He was wrestling, and was thrown by his antagonist on a marble floor. Dr. Hunter, whilst examining the thoracic viscera, accidentally discovered a longitudinal rupture of the œsophagus. He supposes it was done by the man's antagonist at the time of the fall, having one arm round the neck so as to compress the œsophagus, and the other arm round his belly so as to compress the contents of the stomach into the œsophagus and burst it.

The uterine sound (p. 854). A woman after delivery had such a fulness and hardness at the bottom of her abdomen, that she was thought to have another child; but by examining the uterus with a probe, it was found contracted, so that there could be no other

child remaining. The woman died, and upon opening the body, the spleen was found greatly enlarged, and had changed its situation so as to give the appearance of a child in utero when the abdomen was examined.

Offensive breath (p. 950) may be owing to rotten teeth, or to animal substances sticking and putrefying about them; but the worst kind of offensive breath is caused by neither of these. Though the mouth and teeth be ever so clean, yet it continues and is supposed to be a mark of bad health, which is, however, a false supposition. Its origin is owing to a disorder of the tonsils. For the cavities, if examined, are found to contain a pretty firm mucus, which when picked out smells very offensively. It may be remedied by rubbing and sponging the tonsils every now and then with a piece of sponge fixed to the end of a stick, to get at the mucus. If this does not cure it, that part of the gland, where the mucus accumulates, may be cut out without any detriment to the patient.

A patient operates for hydrocele (p. 1026).—A scissors-grinder had this complaint, and I often let the water out; at length he performed the operation himself with a sharp-pointed knife, and did not return to me for help.

Early operation for harelip (p. 1093).—If the case is favorable, the operation should be performed as soon as possible after the child is born, because in the first days the child takes but little nourishment, and in the meantime the wound is cured. In all the cases, where I have performed the operation soon after birth, it has always succeeded. I would advise it to be done two or three hours after birth.

Derivation of trephine (p. 1112).—We generally use the trephine since Woodall's time. He was its inventor, and called it so from its having three ends (tres fines).

Compression of subclavian in amputation at the shoulder (p. 1140).—I was the first to recommend pressure to be made on the artery where it passes over the first rib, to answer the purpose of the tourniquet in other amputations.

Cases of embalming (p. 1148).—Two or three years ago a lady died in London, whom I much respected. I was desired by the family to enbalm her. We tried and it seems to be successful, as it is now in a leaden box and in fine condition. In January 1774, a gentleman requested that his wife should be preserved in the same manner; she died gradually. March 1, 1776, I saw her, and found all moisture going off. I think what remains will be entirely deprived of the power of putrefaction.

Chronicle of Medical Science.

REPORT ON SCANDINAVIAN MEDICINE.[1]

By J. W. Moore, M.D., M.Ch., Dubl.,

Diplomate in State Medicine and Ex-Schol. Trin. Coll., Dublin; Honorary Fellow of the Swedish Society of Physicians.

1. Following the precedent laid down in last year's "Report on Scandinavian Medicine," we take up the 'Northern Medical Archives' first in order of the journals and works under review.

[1] *List of Journals, Papers, and Works, analysed in the above Report.*

1. 'Nordiskt Medicinskt Arkiv.' Redigeradt af Dr. Axel Key. Fjärde Bandet. Med 14 Taflor och flere Träsnitt. 1872. Stockholm. 'Northern Archives of Medicine.' Edited by Dr. Axel Key. Fourth Volume. With 14 Plates and several Woodcuts. 8vo.

2. 'Hygiea: Medicinsk och Farmaceutisk Månads-skrift.' Trettiofjerde Bandet. No. 1—12. Januari—December, 1872. Redigerad af Dr. A. Jäderholm. Stockholm. 'Hygiea: a Monthly Journal of Medicine and Pharmacy.' Thirty-fourth Volume. Nos. 1—12. January to December (inclusive), 1872. Edited by Dr. A. Jäderholm. Stockholm.

3. Förhandlingar vid Svenska Läkare-Sällskapets Sammankomster, år 1872. Protokollsförande Dr. Edholm. 'Proceedings of the Meetings of the Swedish Society of Physicians in 1872.' Edited by Dr. Edholm, Secretary.

4. 'Svenska Läkare-Sällskapets Nya Handlingar.' Serien II, Delen V. 1. 'New Transactions of the Swedish Society of Physicians.' Second Series. Part V, 1. 8vo, pp. 126.

5. 'Upsala Läkareförenings Förhandlingar.' Sjunde Bandet. Häftet 1—7. Upsala, 1871 och 1872. 'Proceedings of the Upsala Medical Association.' Vol. VII. Parts 1—7 inclusive. Upsala, 1871 and 1872.

6. 'Norsk Magazin for Lægevidenskaben.' Udgivet af det Medicinske Selskab i Christiania. Tredie Række, andet Bind. Christiania, 1872. 'Norwegian Magazine of Medical Science.' Published by the Medical Society of Christiana. Third Series. Vol. II. 8vo, pp. 750.

7. 'Forhandlinger i det Norske medicinske Selskab i 1872.' Christiania, 1872. 'Proceedings of the Norwegian Medical Society in 1872.' 8vo.

8. 'Det mekaniske Misforhold under Foedslen og dets Behandling.' Af Dr. A. Stadfeldt. Andet og tredje Hefter. Kjöbenhavn, 1873. 'Mechanical Disproportion during Parturition, and its Treatment.' By Dr. A. Stadfeldt. Parts II and III. Copenhagen, 1873.

9. 'Beretning om den Kongl. Foedsels-og Pleie-Stiftelse i Kjöbenhavn for Aaret fra 1ste April 1871 til 31te Marts 1872.' Kjöbenhavn, 1872. 'Report of the Royal Lying-in Hospital and Orphan House of Copenhagen for the year ending March 31st, 1872. Copenhagen, 1872.

10. 'Hospitals-Tidende: Optegnelser af praktisk Lægekunst fra Ind-og Udlandet.' 1872. 15de Aargang. Kjöbenhavn. 'Hospital Gazette: Notes on Practical Medical Science, Home and Foreign.' 15th Annual Volume. Copenhagen. Folio, pp. 208.

11. 'Ugeskrift for Læger.' 3dje Række XV. Nr. 1—22. D. 4. Januar 1873 —D. 3. Maj 1873. Redigeret af Dr. F. Trier. Kjöbenhavn. 'Weekly Medical

"From the Hospitals in Germany and France" is the title of the first paper in the present, the fourth, volume of this valuable periodical. The author is Dr. Jacob Heiberg of Christiania, who, on receiving a "stipend from the public funds for the purpose of studying general and ophthalmic surgery," proceeded to Berlin in the year 1870 with the intention of seeking duty in the Prussian army hospitals. An interesting account is given of the magnificent Barrack Hospital which was constructed on the parade ground at Templehof in the outskirts of Berlin. In this Dr. Heiberg served as a volunteer from the end of August, 1870, to the middle of March, 1871. As a full description of this establishment was published in the English medical journals of the time, it will suffice to state here that it consisted of fifty barrack hospitals, each containing from twenty-seven to thirty beds, with 27,000 to 30,000 cubic feet of air, or 1000 cubic feet per patient. Fifteen of the buildings were constructed at the expense of the state (the so-called "Fiscal" group), fifteen at that of the Berlin Aid Society (the "Hülfs-Verein" group), and twenty by the City of Berlin (the "Municipal" group). The different barracks lay échelon-fashion to each other in the form of an enormous W, with their long diameters from east to west, so that the prevailing wind might sweep over all the buildings at once. In the autumn, and when the severity of the winter was past, many of the patients used, immediately after the morning visit of the surgeons, to be carried out into the open air, where they lay, ate, smoked, read novels, and were often even chloroformed and operated on. The author describes how the hurtful influence of hospital air told on the physicians themselves in the winter season, when they were obliged to work within the wards. They decreased in weight, lost appetite, became subject to catarrh, sore throat, epistaxis and so on, and were all inclined to be drowsy.

Rather an amusing account is given of the extent to which disinfection by carbolic acid was carried in the treatment of the wounded. "In the war of 1864 the favourite disinfectant was liquor chlori, in 1866 permanganate of potash, and in the present war carbolic acid, and carbolic acid to a degree of which one can form no conception.

Journal.' Third Series. Vol. XV. Nos. 1 to 22 inclusive. January 4th to May 3rd, 1873. Edited by Dr. F. Trier. Copenhagen.

12. 'Oversigt over Köbenhavns Sygdomsforhold, navnlig de epidemiske, i 1870.' Aftryk af 'Ugeskrift f. Læger,' 3 R., XII, Nr. 5, 1871. Meddelt af Stadslæge, Dr. Med. P. A. Schleisner. 'Oversigt over Köbenhavns fornemlig epidemiske Sygdomsforhold i 1871.' Aftryk af 'Ugeskrift f. Læger,' 3 R., XIV, Nr. 7, 8, 1872. Meddelt af Stadslæge, Dr. Med. P. A. Schleisner. 'Review of the Morbility of Copenhagen, especially as regards Epidemic Diseases, in 1870.' Reprint from the 'Ugeskrift for Læger,' 3rd Series, Vol. XII, No. 5, 1871, By Dr. P. A. Schleisner, Medical Officer of Health to the City. 'Review of the Chief Epidemic Affections observed at Copenhagen in 1871.' Reprint from the 'Ugeskrift for Læger,' 3rd Series, Vol. XIV, Nos. 7 and 8, 1872. By Dr. P. A. Schleisner, Medical Officer of Health.

13. 'Om Börns Anvendelse i Fabriker, særlig med Hensyn til vore Forhold.' Af Dr. E. Hornemann. Kjöbenhavn, 1872.

'On the Employment of Children in Factories, with Special Reference to our Arrangements.' By Dr. E. Hornemann. Copenhagen, 1872. Reprint, pp. 44.

The walls were drenched with carbolic acid, beds and floors were sprinkled with it, wounds were dressed with carbolic oil, and bullet tracks were injected with carbolic acid solutions. Physicians washed their hands in carbolic acid, we lived in an atmosphere of carbolic acid, so that on entering a café or a theatre the audience began to cough and to wonder whether 'the walls had been newly painted.' I had patients who had passed through half a dozen hospitals, and had in every instance been treated with carbolic acid. Apothecaries began to judge of the ability of physicians by the quantity of carbolic acid they used, and in Berlin it was adduced as a proof of the ignorance of Frenchmen, that in the whole town of Sedan not so much as a trace of carbolic acid could be found." Dr. Heiberg speaks of the great use of the *thermometer* in the surgical practice of the late war. In his experience, when the temperature rose suddenly without the occurrence of shivering, this rise might be attributed to one of two causes—either a deficient exit for pus, or an attack of erysipelas. So important to the surgeon were the indications of the instrument both for diagnosis and for prognosis, that the author is led to say with Jörgensen, "*Sine thermometro nulla therapia.*" From an American, Dr. Trueheart, of Galveston, he learned to use a short thermometer, and to place it in the axillary line *along the patient's side.* It was left *in sitû* for half an hour, and never for less than a quarter of an hour. In the *clinique* at Rostock, from which place the author writes, the temperature of all the patients was taken every third hour. Passing on to speak of the wounds which came under treatment, Dr. Heiberg expresses himself strongly as to the superiority of the ordinary stone-cutter's chisel over the trepan in raising depressed fractures of the skull. "The chisel," says he, "is to the hard parts what the knife is to the soft parts." Fractures of the thigh by ball were the most common class of wounds; the methods of treatment were three—plaster of Paris bandaging, extension, and splints. Stress is laid on the bad consequences which may follow the first, and how to secure rest is shown to be the great problem in all lesions of the extremities. This valuable paper concludes with an account of the Barrack Hospital at Leipzig and of the *Sanitary Transport.*

Next in order Dr. Christian Lovén, of Stockholm, contributes a physiological paper on the "Vital Mean Position of the Lungs," in the course of which he describes an improved double spirometer with which he carried out numerous experiments.

The third communication in the first number is one by Dr. A. Goldschmidt, of Copenhagen, on "Total Fœtal Movements and their etiological significance in Head Presentations." By the term "total fœtal movements" the author means such movements of the entire fœtus as either actively or passively alter its topographical relations to the uterus, by a rotation of the fœtus either on its long axis or on its transverse axis. It would be impossible to do justice within narrow limits to this long and learned paper. "Some further remarks on Ulcerative Catarrh of the Cervix Uteri" are from the pen of the late Dr. Sven Sköldberg, of Stockholm. The lamented

author supposed that the morbid process which is commonly termed "ulcerations round the *os externum*" has its origin and cause in an identical process in the cervical canal. In all the cases where direct inspection of this canal is possible, ulcerations are not wanting in it when they are found around the *os externum*. Even if ulcerations are wanting in the vaginal portion, they are nevertheless generally likely to be found in the cervical canal. In order to inspect the cervical canal Dr. Sköldberg availed himself of Sims' speculum, and placed his patients in the position recommended by that physician. After fixing the bill of the speculum, he applied gentle traction to the os by means of a small hook, a manœuvre which distended the canal most perfectly.

Dr. Adam Öwre of Christiania, in the fifth article, answers the question "From whom is syphilis inherited?" He says: "Three years since I expressed myself on this question as follows: 'From my investigations and experiences I would be most inclined with Cullerier to answer without reserve—*from the mother alone;* and yet I would not venture at present to be so dogmatic.' The time that has intervened has not afforded me occasion to deviate from this opinion, rather the contrary; for what I have since seen has only confirmed my confidence in the teaching of Cullerier." He adds: "It appears to me that we shall best deduce our experiences on this point from private practice; we should be syphilo-therapeutists and in preference work in small, not too densely populated, localities, so as to have an opportunity of observing our patients and their offspring through a very long period of time. This I have done, and that with great accuracy, *without having so far met with a single undoubted example of hereditary syphilis after direct contagion from the father.*"

In the fourth number of the Journal, however, Dr. Öwre's views are called in question by Professor Abelin, of Stockholm, already well known to our readers as a man of distinguished talents and wide reputation. He supports his opinions by the clinical history of seven cases of hereditary syphilis transmitted from the father.

Through the second and third parts of the volume runs a treatise by Professor Faye, of Christiania, on "Epidemic Diseases, and the best means of combating puerperal infection." A concise description of the system as adopted at the Christiania Lying-in-Hospital, both in a hygienic point of view generally and as regards prophylactic and curative measures, is contained in this paper. The results of this system the author has every reason to be satisfied with. He says it is well known that the hospital has for several years been free from epidemics which had previously been of rather frequent occurrence within its precincts. The principal building of the hospital, two stories high, is divided by a centre corridor in the second story, and by two contiguous open apartments transversely in the first story. Ventilation in these passages, on which the lying-in apartments open, is so free that the air is about as fresh and also of about the same temperature as the outer air, which streams in without intermission. No doubt the hospital staff is in

consequence of this very free ventilation somewhat more exposed to colds; but in order to prevent a possibly injurious interchange of foul air in the lying-in rooms, this draught is steadily maintained day and night. In a more recently constructed wing of the building the arrangement is the same. The apartments vary in size from about 1400 up to 2400 cubic feet, and there are only two rooms which contain about 5000 cubic feet. For many years back, air-pipes have been laid beneath the floors, and through these fresh air is conveyed into ovens, from which, when warmed, it flows into the apartments. Besides these, Arnott's ventilators are used in the partition walls, being made to open into the flues from the ovens. Although the ventilation thus induced in winter is considerable, the windows and, in the second story, the doors, are kept more or less open as soon as the intense cold is past, except that care is taken to moderate the in-flowing current and to make it continuous by means of a guard or network suspended before the window. In summer the air is given still freer access, and each apartment is in turn cleansed and painted. Except the ceiling, and the highest part of the internal walls, which are whitewashed, the interior is painted in oils; in some apartments the floors are also similarly painted. This plan Professor Faye approves of, for the floors can be frequently mopped with soft-soap and warm water, and oil-painting also bears concentrated fumigation with sulphurous acid well—a process to which each apartment is subjected about every three weeks as it becomes vacant. The bedsteads are old and of wood, furnished with an under mattress of straw and an upper one of horsehair, an inclined bolster and a smaller pillow, besides blankets and sheets. The children's beds are similarly furnished. The hair mattresses after use are systematically cleansed by exposure to the air and in a hot-air chamber, and lastly, by thorough disinfection with sulphurous acid. Blankets and linen are similarly treated after they have been well washed in the first instance. Each lying-in woman passes through her labour in an isolated apartment, in which she remains alone for the first five or six days. If at the end of this time she is quite well, she is placed with one or two other patients who have been treated as convalescent for a week or more. The regular period of treatment is fourteen days.

The hospital staff consists of a chief physician, an assistant physician, two candidate pupils, a head nurse, an under nurse, and two hospital nurses. The day and night duty in the lying-in wards is carried out by pupils of the midwifery school, generally eighteen or twenty in number, two of whom in turn reside at the hospital. Professor Faye regrets that through want of room the other students have to reside in the city, from which they might possibly carry infection to the hospital. From four to six of the senior students are allowed access to the lying-in hospital for three months, where they practise themselves in the physical examination of pregnant and lying-in women, keep notes, attend the physicians on their morning visit, and undergo a series of clinical examinations. The students at the midwifery school are in the habit of taking vapour-baths and

having their clothes fumigated; but if one or more of the pupils is in attendance on a dangerous case of illness in a puerperal woman, complete isolation from the other students both within and without the hospital is insisted on. When it is desirable to observe more than ordinary caution, new cases are examined only by one, or at most two, pupils. All use, while on duty, a white, perfectly washed blouse. "This then," adds the professor, "is the system we have now followed for many years with favorable results (the mortality *from all causes* has in the last four years been one in ninety-one), "and although it must be taken into account that the establishment is small, receiving some 200 patients a year, yet I believe that this system is essential, for in a former period I have frequently met with an epidemic outbreak, sometimes of a very malignant character. It should not be forgotten, however, that in this death-rate from all causes, some of the cases which ended fatally could not be considered as essentially due to puerperal infection. And even where actual puerperal fever has become developed, this has repeatedly been due to antecedent evils which are not unfrequently peculiar to the class of women who constitute the majority of the hospital patients, namely, the unmarried, wretched unfortunates of the city and its neighbourhood—a class which is always the most prone to the diseases of childbed. Renal affections belong to the more common maladies of the present day—even with secretion and excretion of pus through the urinary tract; and when dropsy or violent eclampsia supervenes, death may occur rapidly. If we isolate these and other causes of death, such as contracted pelvis with very difficult delivery (cephalotripsy, and so on), large pelvic tumours with rapid death, the death-rate in the period mentioned will not exceed 1 in 180."

Professor Faye concludes his paper with a capital dissertation on the remedies used in, and methods of treatment of, puerperal fever.

In the second number of the journal we meet with a paper on "Otomycosis," or "Parasitic Fungi of the Ear," by Dr. A. Liljenroth, of Stockholm. The author describes and remarks upon six cases of *aspergillus flavescens* which had come under his notice. We quote the first of these cases, almost in the author's own words. "In June, 1869, a farmer came under my care suffering from an acute catarrh in the nose and throat, with obstruction of the Eustachian tube, and diminished power of hearing, which was subsequently improved by catheterisation of the tube. On examination of the left ear two loose epidermal scabs, as it seemed, were observed close to the membrana tympani. On removing these with a forceps, and subjecting them to microscopical examination, they were found to contain a few specimens of *aspergillus flavescens*." The sixth case was that of Mrs. L—, of Stockholm, æt. 56, who ten years before had had a running from the right ear. This disappeared four years ago; but in February, 1870, it recommenced simultaneously with an attack of inflammation in the left ear. She complained on the right side of "a noise and a second voice in her head," which distressed her much. For a year back she had suffered some pain in

the right ear. When examined on October 12th, 1871, a perforation was found in the right membrana tympani, and white membranous structures were seen in the external meatus. This, on the 16th, was filled with a white mass, which, on removal, was shown to consist of aggregated membranes; and in its lowest part, situated nearest the membrana tympani, was everywhere of a dusky, yellowish-green colour. This colour depended on the presence of innumerable spores and sporangi of *aspergillus flavescens*. The aspergillus is described as belonging to that class of fungi which especially demand a somewhat dry place of growth. The position in the ear which is most frequently the seat of fungous growths is the external surface of the membrana tympani; the reason for this is partly to be found in the fact that such a structure may here grow most undisturbed, since the fungus is here least liable to be disturbed by external influences, partly, perhaps, also in the fact that the epithelial layer of the membrana tympani, in consequence of its softness, is admirably suited for the development of the fungus. The aspergillus grows outwards and inwards, penetrating into the epidermoid layer until the irritation of this by the matured sporangi becomes so violent that all the coherent membrane is thrust off. The objective symptoms of the presence of the parasite are, according to Wreden, at first a persistent redness along the handle of the malleus, when the fungus is situated on the membrana tympani. This redness gradually becomes more diffuse, and spreads over the whole membrana tympani, the phenomenon corresponding to the growth of the spores, and lasting from one to two or even four weeks. The membrana tympani by degrees loses its lustre, and becomes dull like glass which has been breathed on. This is due to the development of the mycelial threads, a process which takes some two weeks. A white lardaceous membrane shortly becomes developed, which denotes the completed formation of the fungus, and which in about a week is ready to be thrust off. Two illustrations of the sporangi of *aspergillus flavescens*, one showing them magnified 300 diameters, are given. The indications for treatment are stated to be threefold: first, the separation and expulsion of the newly formed false membranes; secondly, the destruction of every remaining particle of fungus possessed of germinative power; and thirdly, the subduing of the secondary affections induced by the presence of the fungus. Further, the possibility of relapse is to be guarded against. To hasten the separation of the membranes, the ear should be kept clean by syringing with lukewarm water, besides which an alkaline solution (one part of bicarbonate of soda in 100 parts of water) should be dropped into the ear a few times daily. The membranes usually come away with the syringing, or may be removed by a forceps. For the destruction of the fungus Wreden recommends, as the result of experiment, *calx chlorata* (a solution so weak as 2 to 4 in 1000 parts), *carbolic acid* (1 part in 100), *Fowler's arsenical solution*, *nitrate of silver* and *tannin*, dissolved in 50 per cent. spirit (1 part in 4), as the most efficacious remedies. In addition to these remedies, the author has used a solution of subacetate of

lead (1 part in 10) with advantage. The secondary affections set up by the presence of the fungus are, lastly, to be treated as though they were of primary origin during and after the removal of their exciting cause.

In the third number of the present volume Dr. G. Berghman, of Stockholm, reports a case of extensive destruction of the anterior cerebral lobes, unaccompanied by aphasia. A dragoon, æt. 29, was admitted to the Surgical Department of the Royal General Garrison Hospital on June 28th, 1872, having been kicked in the forehead by a horse two hours before. The frontal bone was broken into several pieces, the brain substance protruded from the wound, and some of it was found scattered about the scene of the accident. The patient was able to sit up in bed while the wound was being dressed, and showed no paralytic symptoms, with the exception of a right facial palsy. He was perfectly conscious, and answered all questions put to him clearly. Pulse 112, full and regular. Next day convulsive movements were observed in both upper and lower extremities, especially on the left side. The patient lay apparently quite unconscious, but yet gave clear answers to every question. Pulse 116, temp. 99·7°. The patient made water voluntarily at 8.30 a.m.; it contained no albumen. On the 30th his state was much the same. No lesion of speech, No paresis except the facial palsy. No lesion of sensation. The tongue, freely movable, did not deviate towards either side. The urine had now to be drawn off by catheter; the power of voluntary micturition, however, returned, and remained until the day of his death, the 5th of July. On this day he became comatose, with stertorous breathing, floccitatio, and convulsive movement of the extremities. The pulse, difficult to count, was about 160, and extremely small. The cerebral substance at the bottom of the wound continued inflamed and stinking. Attempts to swallow were induced by the washing of the wound. The patient died at 3.30 p.m. A very full account of the autopsy is given. It is mentioned that on the right side of the cerebrum only those parts of the cortical substance lying close to the middle line were damaged, *i. e.* the olfactory convolution, internal orbital convolution, and the posterior portion of the posterior orbital convolution as far as the fissure of Sylvius. But on the left the whole under surface was involved, and the inflammatory action had spread, not merely outwards and upwards to the third frontal convolution, which was altogether destroyed, softened, and discoloured, but also to the *island of Reil* on this side, which, however, did not present the same degree of discoloration and breaking up. The lateral ventricles, dilated, contained a very large quantity of clear fluid. With these and other extensive injuries of the brain-substance, especially on the left side, there was, as already stated, no aphasia. Two days before his death, indeed, the patient, when asked to what squadron he belonged, answered, "To the Sigtuna squadron," whereas he really belonged to the Upsala squadron. It is to be remarked, however, that another squadron, called the "Sigtuna squadron," actually was attached to his regiment.

The third paper in the fourth number of the journal, on "Subcutaneous Injections of Corrosive Sublimate in Syphilis," is from the pen of Professor Ernst Oedmansson, of Stockholm. In the course of the year 1870-71 this method of treatment was followed at the Stockholm (Lock) Hospital in 123 instances. There were 2694 injections in all, or as nearly as possible an average of 22 per case. In 28 cases, or about 23 per cent., the treatment had for one cause or another to be prematurely interrupted. In 37 out of the remaining 95 cases (*i. e.* in about 39 per cent.) relapse took place. In all the cases treated by means of injections warm baths or hot-air baths were employed, as a rule, twice a week; and, as circumstances required, local remedies of various kinds and tonics were also had recourse to. As regards the strength of the injection fluid, the author states that in a series of 50 cases the weakest of Lewin's solutions (18 centigrammes of corrosive sublimate to 30 grammes of water) was used, and in a second series of 73 cases a solution of medium strength, or 24 centigrammes in 30 grammes. The injections, with few exceptions, were made on the back between the inferior angle of the scapula and the crest of the ilium, and when it proved necessary, on the lateral aspect of the body, over the upper part of the hips or in the space between the shoulder-blade and the spine. They were performed once a day; only exceptionally and towards the close of the treatment, every second day. The frequent occurrence of shooting and of local pains some three or four hours after the operation is noticed, and is shown to depend largely on the point selected for operation. In his concluding remarks Prof. Oedmansson says that, from his experience of injections of corrosive sublimate, they appear to be about equal in power and efficacy to green iodide of mercury, calomel and corrosive sublimate taken internally, but to be far inferior to the method by inunction. They are most efficacious in cases where considerable constitutional but trifling local symptoms are present—cases which are not rarely met with.

The last article we shall notice at any length is one entitled "A Contribution to our Knowledge of the Properties of the Syphilitic Poison," by Prof. Dr. W. Boeck, the well-known syphilographer, and Reserve Physician Axel Scheel, of Christiania. These gentlemen performed a very interesting series of experiments with a view of obtaining an answer to the following questions:—(1.) *How long does the syphilitic virus preserve its power of inoculation?* (2.) *What influence do heat and cold exert on this poison?* (3.) *Is it inoculable after being dried into crusts?* (4.) *How far does it preserve its inoculability when mixed with various substances?* We shall briefly enumerate the results of the experiment with reference to each of these inquiries.

(1.) Twelve experiments were made with syphilitic virus kept, like vaccine lymph, in hermetically sealed glass tubes. Positive results followed all the inoculations with virus kept thus for one to five days; but negative results with virus kept for six to eight days. Virus kept on hollow glass slides for eight and eleven days re-

spectively gave negative results, its inoculability being generally lost after three days when kept in this way.

(2.) Eight experiments were made with *frozen* syphilitic virus kept in a vaccine-tube (after exposure to a cold of from 18° to —4° Fahr.) and all gave positive results. An experiment with virus kept twelve days in an ice-block failed, but inoculation with virus kept thus for five days was successful. Seven experiments were made with *warmed* virus, enclosed in a vaccine-tube. Virus warmed to 99·5° and 111° Fahr. gave positive results, but some when warmed to 122° Fahr. and upwards gave negative results. However, virus when enclosed and dipped into water of the temperature of 122° took readily. In two cases individuals were inoculated on the arms, which they then immediately dipped into water heated to 122°, and kept there as long as they could bear it (not longer than half a minute). In both cases all the inoculations took.

(3.) Twenty-five experiments were made with crusts taken from dried syphilitic pustules. Crusts kept up to six days were invariably inoculable; after six days the results became doubtful, yet in two cases positive results followed the inoculation of crusts kept twelve days. Various other experiments were made with virus which was allowed to dry on a lancet with very similar results. In eight instances virus allowed to dry on a piece of linen and again moistened proved to be incapable of being inoculated, even when kept for only two days.

(4.) Thirty-five experiments were made with virus mixed with water. Positive results universally followed the inoculations made with a mixture of 1 part virus in 100 of water. With virus diluted 300 times the results were doubtful; and virus diluted 600 times lost all its power. Seven experiments were made with syphilitic pus mixed with a solution of *carbonate of potash* (1 part carbonate to 2 parts of water). In two cases the virus was mixed with an equal quantity of the solution and the results were negative. On the other hand, a mixture of two parts virus and one part solution, as also a mixture of 3 parts virus and 2 parts solution, gave positive results. Five experiments were made with syphilitic pus mixed with *olive oil*, all with more or less positive results, even where the mixture was in the proportion of 1 part virus to 50 parts oil.

Among the other papers in this volume of the 'Northern Medical Archives' we must mention an article by Dr. C. Lange on the "Pathology of Tabes Dorsalis;" "Some Investigations on Epithelial Regeneration in the Cornea of Dogs," by Sophus Fenger, of Copenhagen; "A Contribution to our Knowledge of Cystoma Testiculi" by Carl Wettergren, of Stockholm; "Studies on the Anatomy of the Nervous System," by Prof. Axel Key and Dr. Gust. Retzius (beautifully illustrated by a series of coloured lithographs); "On the officinal 'Rhizoma Chinæ," by Dr. Oskar Th. Sandahl, of Stockholm; "On an open *Saft-canal* (juice-canal) system in Mucous Membranes" by Prof. Hjalmar Heiberg, of Christiania; and "A Case of Congenital Absence of the Quadriceps Cruris Muscle," reported by Prof. A. G. Drachmann, of Copenhagen. Besides

these and other papers, the usual quarterly report on Scandinavian Medicine is included in each number.

2. As briefly noticed in last year's Report, a new and improved series of the "Hygiea" was commenced in January, 1872. In the first number Dr. W. Netzel communicates a case of "Intra-uterine Hæmorrhage before Delivery" occurring in a patient, married, and aged between 20 and 30, who at short intervals had passed through four normal confinements. In his subsequent remarks on antepartum hæmorrhage, Dr. Netzel notices the influence of too frequent pregnancies in causing relaxation of the uterine walls, and consequently a tendency to hæmorrhage. To this same number Dr. Oskar Th. Sandahl contributes an article on "Cortex Condurango,' and in the second number Dr. Alrik Lindh reports six cases in which skin-transplantation was generally most successfully practised at the Seraphim Hospital in the clinical year 1871. In the March number are two short, but most practical papers by Dr. H. Nordenström; the first contains the clinical histories of three cases of empyema treated with antiseptic injections. The author expresses his opinion that the chief indications for treatment in such cases are persevering evacuation of the contained fluid, washing out of the pleural cavity, and, where the effusion is fœtid, the employment of an antiseptic solution; that recommended by him is Gahn's *amykos aseptin*, a saturated solution of boracic acid in decoction of cloves. The second paper is a report of three cases of favus, cured by treatment with carbolic acid (one part in six of olive-oil or lard). The patients, all of whom had been the subject of the disease for several years, were aged 16, 17, and 8 respectively. Dr. A. Kjellberg writes, in the May number, on the treatment of congenital atelectasis by means of inhalations of warm water. He adopts the following method of keeping the infant in an equably warm and moist atmosphere. The child is laid in a so-called "steam-press," a sufficiently roomy chamber or tent made of felt. In this, steam is developed from a vessel of warm water, so as to keep up the temperature to about 80°, ventilation meanwhile being sufficiently provided for. The temperature in the "steam-press" should for the first few days not decline below 77°, and should scarcely ever exceed 86°. The treatment may take from eight to fourteen days, and the child should be gradually made accustomed to the air and temperature of the dwelling-room. When an infant is kept in such an atmosphere as that above described, the respiration becomes freer and deeper, the collection of mucus in the bronchial tubes is prevented, the warmth of the body is maintained, a desire for food is manifested, and so on—in a word, the infant spends the first few days of its life under the most favorable circumstances possible. In the July number Prof. C. Santesson gives an account of two cases of myeloid sarcoma. In the first case, a tumour, the size of an ordinary apple, grew on the right side from the zygoma and superior maxillary bone of a woman aged 38. It was removed by an operation involving resection of almost all the zygoma, of some of the superior maxilla, and of the zygomatic process

of the temporal bone. The growth proved to be an example of a myeloid spindle-celled sarcoma with multi-nucleated giant cells. The patient recovered perfectly. The second case was that of a girl aged 24, who suffered from a swelling at the lower end of the right thigh-bone, diffuse, firm, and somewhat elastic. The limb was amputated, but the patient sank on the fourteenth day after the operation. Microscopical examination showed that the tumour was a myeloid round-celled sarcoma with multi-nucleated giant cells. Professor Santesson remarks on the obscurity of the origin and the rapidity of growth of both tumours (three and six months respectively), on their appearance in individuals otherwise healthy, and of youthful age, and on the necessity for *early* operation in these cases.

Under the heading, "On the State of Hospitals in Sweden during 1870," Dr. O. F. Hallin gives a great deal of practical and statistical information. At the close of 1870 the number of hospitals and *Kurhusen*, or "Houses of Recovery" (the latter title corresponding, apparently, to our term "*Lock Hospital*") in the kingdom amounted to 59, 5 having been opened during the year; and in them there were beds available for 3293 patients—an increase of 320 beds on the number in the preceding year. In the institutions mentioned 21,604 patients passed under treatment in the course of 1870; 18,148 persons were discharged as "well," "relieved," or "uncured." The deaths numbered 1250; 1182 in the hospitals and 68 in the *Kurhusen*. Of *venereal diseases* 7441 cases were treated at the civil and military hospitals throughout the kingdom. The number of cases in 1868 had been 8400. Of all the cases 6573 cases were treated at the *Kurhusen*, 90 in the other civil hospitals, and 778 in the military hospitals.

The September number contains a record of 10 cases of ovariotomy, performed by the late Dr. Sven Sköldberg, in addition to 20 cases already reported by that distinguished gynæcologist. The patients recovered in nine instances. Dr. M. Malmberg communicates and remarks on a case of central placenta prævia, in which after delivery by manual interference the child revived, although at first it was apparently stillborn.

In concluding this notice of the "Hygiea" for 1872 it is but just to allude to the admirable manner in which the second part of the Journal, that devoted to *Translations*, *Reports*, and *Reviews*, is edited. Here are to be met with admirably arranged digests of the most important foreign papers on modern medicine and surgery, which are thus brought under the notice of every member of the profession in Sweden.

3. At a meeting of the Swedish Society of Physicians, held on January 16th, 1872, Hr. Jäderholm read a paper on the *latest development of cholera in Europe*. The author considers that the history of cholera in 1870 and 1871 fully justifies the suggestion of the Cholera Conference at Constantinople that posts of sanitary observation should be established at fixed points on the Red Sea, besides quarantine stations at El Wesch on the Arabian coast, for

pilgrim vessels bound for Egypt, and at Tor, at the lower end of the Gulf of Suez, for other vessels.

On the 4th of June, Dr. Curman, a member of the Society, who had lately returned from a sojourn at Amsterdam, where he had been successfully treated by Dr. Mezger for an affection of the ankle and knee-joints, of many years' standing, at the request of the President gave a short account of Mezger's method of treatment by *massage* or shampooing. Of this a detailed description will be given further on in this report.

During the year many important clinical cases were brought under the notice of the Society by individual members. Among these were a case of cerebral embolism, occurring in a woman æt. 48, the subject of chronic endarteriitis with right pneumonia, and in whose spleen and kidneys emboli were also found (communicated by Hr. Malmsten); a case of left perinephritis, with perforation of the diaphragm and effusion into the left pleura, causing secondary suppurative pleuritis with compression of the lungs (Hr. Malmsten); a case of renal and vesical calculi, temporarily relieved by lithotrity, the patient subsequently becoming the subject of stricture of the sigmoid flexure of the colon and chronic peritonitis, caused by primary cancer of the liver and secondary miliary carcinoma in the peritoneum (Hrr. Santesson and Wettergren); a case of enterotomy for cancer of the rectum, in a girl æt. 19, the patient living for six or seven months after the operation (Hr. Leijer); a case of extra-uterine fœtation (Hr. Netzel); a case of death from chloroform, which happened on June 24th, at the General Garrison Hospital, Stockholm. The patient, 28 years of age, was chloroformed preparatory to the performance of forcible dilatation of the *sphincter ani*, which was fissured. In about seven minutes he became cyanotic, and respiration ceased. The heart and lungs were found perfectly healthy after death, and there was nothing to contraindicate the use of chloroform. It was ascertained several days afterwards that the patient had spent the day before the operation in a state of incessant dread of it—a circumstance which Dr. Berghman (who reports the case) thinks may have tended to depress him.

In the course of the discussion which followed the reading of the notes of this case, Hr. Santesson observed that during his period of service two cases of death from chloroform had happened at the Seraphim Hospital, but that he had never met with a similar accident in his private practice.

At subsequent meetings of the Society Hr. Strandberg communicated a case of aneurism of the abdominal aorta of a traumatic nature; Hr. Malmsten, one of carcinoma of the liver and stomach; Hr. Berghman, the following interesting surgical case:—A man, cut on the head with a knife, remained almost quite well for two days; he was then admitted to hospital in a comatose state. After his death, from purulent meningitis, a piece of the blade of a knife was found *sticking in the brain*. Hr. Malmsten further reported two cases of thrombosis of the pulmonary artery, the lesion occurring, in one instance, in connection with a chronic double pneumonia, in the

other, secondarily to a thrombosis of the right femoral vein, and in connection with a pleuro-pneumonia of the right side.

4. Two articles make up the fifth part of the 'New Transactions of the Swedish Medical Society' (second series). The first is a farewell address to that Society, delivered on October 1st, 1872, by the retiring President, Dr. C. J. Lilljebörn, who chose for his subject "The Means of Protection against Smallpox." The second article is a prize treatise by Dr. Anders Fredrik Kullberg, on "Prostitution and the most Efficacious Means of Controlling Venereal Diseases, with especial reference to the question as it concerns Stockholm." The author gives a very good *résumé* of the chief hygienic and moral measures which should be resorted to with a view of lessening the prevalence of the scourge of syphilis.

5. The 'Upsala Proceedings' are more than ever replete with new and valuable material. In the first number, Fred. Björnström makes some remarks on three cases of arsenical poisoning from a green room-paper. The clinical history is, in one instance, graphically given by the patient himself. An obstinate fever, with frequent relapses, was at last found to depend on the patient's sojourn in a room which he had covered with a green paper. C. B. Mesterton reports four cases of neurectomy for facial neuralgia. In No. 1 resection of the inferior dental nerve of the right side was performed, just above its entrance into the inferior dental canal. In Nos. 2, 3, and 4, simple resection of the infra-orbital nerve in its passage through the infra-orbital canal was performed. In all the cases the operation was a *dernier ressort*, and in three the cure was complete in degree and duration. In a paper on "Therapeutic Novelties," Fr. Björnström describes a nose dilator (*rhineurynther*, as it is called by Küchenmeister), analogous to the vaginal dilator, and devised for stopping epistaxis. It consists of an oval, thin-walled caoutchouc bladder, from $1\frac{1}{4}$ to $1\frac{3}{4}$ centimètres in length and 1 to scarcely $1\frac{1}{2}$ centimètres in breadth, attached at one end to an elastic tube, and capable of being introduced by means of a suitable sound through the bleeding nostril as far as the naso-pharyngeal opening. When *in sitû*, it is filled with air or iced water, and is then drawn somewhat backwards into the nasal cavity, so as to completely close it up. In the second number are several physiological papers by Frithiof Holmgren, namely, on "Förster's Perimeter and the Topography of the Perception of Colour," on "Blood-vessels in the Hyaloid Membrane of the Frog's Eye," and on an "Ophthalmometer." Olof Hammersten writes at length on the Chemistry of Respiration, and Fred. Sundewall reports a case of trichophyton treated by camphor inunctions. The preparation used was a pommade containing prepared lard 50 parts, camphorated oil (the camphor liniment of the Brit. Pharm.), 20 parts, and oil of cloves 1 part. It was rubbed in twice a day. After four or five weeks the spores had all disappeared from the hair. The case was watched for nine weeks altogether, but within that time no fresh growth of hair worth speaking of had made its appearance. J. A. Waldenström reports on the "Policlinik" of Upsala in the Medical Session 1870-71.

In an important note the author says that to the word *policlinik* a somewhat different meaning is attached in Sweden to that conveyed by the term in other countries. Derived from πόδις = a city, and κλίνη = a bed, *policlinik* is properly a system of medical clinical instruction, the subjects of which are *patients lying ill in bed at their homes throughout the city.* The system is thus distinct from the hospital clinical instruction which is termed *nosocomial-clinik.*

To the fourth number Carl Nyström contributes the first part of a paper on *aseptin* (boracic acid). He performed a series of experiments on (1) meat macerated in water, (2) urine, and (3) infusion of malt. The results are thus summarised:—I. Boracic acid prevents the immigration of bacteria into fluids, which otherwise offer favorable conditions to the life of these organisms, and by means of which decomposition is caused. II. The agent, by killing the bacteria or preventing their further increase, essentially checks the process of decomposition, even when it has already commenced. III. Boracic acid rapidly kills active *infusoria.* IV. It in many cases seems to act as a deadly poison on *articulata* and their larvæ. V. It in no way counteracts the formation of mould, which is, however, prevented by decoction of cloves, as pointed out by Gahn.

The fifth number contains the first part of a long paper on *Tachycardia exophthalmia strumosa,* under which name (proposed by H. Lebert) our readers may have some difficulty in identifying the well-known 'maladie de Graves,' or 'morbus Basedowii.' The author, Hr. O. Glas, has mastered his subject, and illustrates it by eight very full clinical histories. In the sixth number P. Söderbaum reports a case of iliac luxation of the right femur, which he reduced after Professor Mesterton's method, without chloroform. The account of the operation is given somewhat as follows:—

"The patient was laid on a low sofa having a wooden back, with his right leg to its far side; a strong farm-servant placed one of his hands on the anterior superior spine of each ilium, and was ordered not to let these two points be disturbed from their position. The patient's leg was then flexed almost to a right angle; I placed my right knee in his popliteal space, with my right foot resting against the back of the sofa and my right hand grasping the injured limb immediately above the ankle-joint. I now raised myself slowly, but firmly, on my toe, when a loud scraping announced that the head of the bone was moving. It then became possible to rotate the foot outwards so much that the toes pointed directly forward, and the faulty position of the limb generally was improved. It was now supposed that the head of the bone lay on the brim of the acetabulum. Having again placed my right leg so that the entire sole of the foot rested against the back of the sofa, I once more raised myself on my toe, bringing my knee at the same time beyond the edge of the sofa. By this manœuvre abduction was caused, during which, with a violent jerk, the head of the bone shot into the acetabulum, the position of the limb becoming quite natural, and the foot sinking

down of its own accord on its outer side." It is to be remarked that the patient was a man 56 years of age, and the accident had occurred eight days before reduction was effected in the manner detailed. In this same number P. Hedenius describes a colossal aneurism of the aorta, engaging the three stages of the thoracic aorta, sacciform; its length from above downwards, 16 centimètres; its breadth from right to left, 15 centimètres; and its diameter from behind forwards, 12 centimètres. Want of space necessitates the omission of any mention of many other papers in this volume.

6. The eighth number of the present volume of the 'Norsk Magazin' contains a paper by Cand. Med. A. Daae on an epidemic of Acute Muscular Rheumatism, which prevailed at a place called Drangedal in the summer of 1872, and which seemed to spread by contagion. The affection appeared to be an acute rheumatism attacking the muscles of the chest, back, and to some extent of the abdomen. It commenced sometimes suddenly, sometimes after general malaise of a day or two in duration. As a rule it was ushered in without rigor, but was attended with some feverishness. There was much constitutional disturbance; but little appetite; tongue thickly coated with a white fur. The illness lasted from two to fourteen days. Relapses were of frequent occurrence. No case terminated fatally. Those attacked were from 1 to 30 years of age; few were more than 30, and none were over 40. The period of incubation was very short. The disease spread from a wedding-guest to several families, some of whose members had also been guests at the same marriage.

In this same number H. Vogt gives somes interesting statistics as to the childbed mortality in Norway in the decenniad 1859—68. In the first five years the deaths numbered 2001, and the mortality for the whole country was 1 in 135·6; in the second five years the deaths were 1801, and the mortality was 1 in 144·3. According to Faye's and Schönberg's statistics (published in the *Norsk Magazin* for 1866), twins were met with in the ten years 1853—62 once in 81·6 births. H. Vogt found twins once in 81·1 births in the ten years 1859—68, and triplets once in 5969 births, as compared with Faye's and Schönberg's estimate of once in 6000 births. The much greater mortality of lying-in women in the winter six months is attributed by the author, and justly it seems, to the overcrowding and defective ventilation of the patients' dwellings in the cold season of the year. On this point a reference is made to page 24 of Miss Nightingale's *Introductory Notes on Lying-in Institutions.*

Considerable interest has of late been shown in medical circles in Norway with respect to the method of treatment recently revived by Dr. Mezger, of Amsterdam, under the name of *massage*, or *shampooing*. Accordingly, the subject was brought before the Norwegian Medical Society at the meeting of the 12th of June, 1872, by Dr. Conradi, who described the method in all its details, and papers bearing on the matter appeared in the eleventh number of the *Norsk Magazin* from the pen of Cand. Med. L. Faye, and of Chief-Physician P. Winge.

Hr. Faye commences his paper with a definition of *massage*, by which term we are to understand *rubbing*, *beating*, *rolling*, and so on—in a word, a mechanical treatment by hand of a limb for therapeutical ends. *Massage* may thus be reckoned as a form of "passive gymnastics." The author gives an admirable historical sketch of the method, which dates from Hippocrates, and numbers among its advocates Galen, Celsus, and many other worthies of old. Three centuries ago Ambroise Paré recommended *massage*, and in 1698 one Paullini published a book entitled *Flagellum Salutis*, in which he mentions that by "chafing and beating" certain parts of the body with the hands it was possible to cure many sicknesses, such as lameness, deafness, melancholy, stomach-affections, obstinate hiccough, &c.! Sydenham also strongly recommended bodily and manly exercises. The next question considered is the mode of action of *massage* from a physiological point of view. The author writes:—"The theory is that by movements and rubbings we intend to excite a greater degree of vascular activity. The distended and hyperæmic vessels consequently contract, a stronger blood-circulation arises, and this infuses new life into the affected parts. Resorption of the effused fluids is thus promoted by means of the absorbent vessels." The third portion of the paper is devoted to the investigation of the therapeutical importance of *massage*. It is indicated in two great groups of diseases—*first*, in chronic and subacute joint-affections of various kinds, such as old effusions into joints, chronic synovitis, swellings after sprains, relaxation of capsular ligaments, some forms of rheumatic affections, white swelling, and the like. *Secondly*, in nervous affections, such as neuralgia of the sciatic, obturator, or intercostal nerves; other pains of different kinds, migraine, &c. In cases of pressure on nerves by exostosis and similar lesions, the treatment is, of course, of but little value.

Dr. Winge's article deals chiefly with the various modes of carrying out the treatment by *massage*, as carried out by Mezger, whose practice he attended for three weeks at Bonn last summer. The leading principle of the method is in general to promote and increase the absorption of morbid effused products. This author draws a distinction between *massage*, which is more properly friction with the fingers, and *effleurage*, a stronger rubbing with the whole palm of the hand. The hip-joint alone, from its deep position and thick covering, is almost inaccessible to *massage*. Dr. Mezger proceeds to operate as follows:—When the affected part is covered with hair, he begins by shaving it (round the roots of the hair acne pustules readily develop under friction, and these sometimes become so large as to necessitate the temporary suspension of the treatment). The operator, for the sake of convenience, sits opposite to, and at a somewhat lower level than, the patient. The part having been rubbed with some fatty substance (Mezger uses hog's-lard, to which he adds some etherial oil to perfume it), he rubs firmly, and with all his fingers, the soft parts where induration, infiltration, or fluid is felt. He executes the rubbing so that, as far as possible, the power should act especially along the course of the lymphatic

vessels, that is, along a limb from below upwards. In the case of the knee he works the fingers of one hand rather across the joint, and to both sides below the patella, at the same time pressing in towards the joint, while with the fingers of the other hand he works in the same way upwards along both sides of the patella, over the capsular ligament or any other ligaments felt to be swollen. Having rubbed the part in this way for from three to five minutes, he catches the knee in his right hand, and with a firm pressure brings his hand from below upwards over the patella to the superior insertion of the capsular ligament. This manipulation (*effleurage*) is then several times repeated. An acute serous synovitic may be removed after this treatment has been continued for two or three weeks. The cure of hyperplastic synovitis takes a longer time, from one and a half to two or three months, or even longer. Dr. Winge gives the following example of the use of *massage* in sprains:—"One day a young Dutchman presented himself, who some days previously had met with a severe sprain of one ankle-joint, which was considerably swelled and painful on slight pressure, or when he walked on the foot, with extensive dark discoloration from blood extravasated under the skin about the joint. This last faded to a great degree under the first application of *massage*, and soon disappeared. After six manipulations, extending over three days, the case was cured." In the treatment of neuralgic affections Dr. Mezger employs various manœuvres, such as blows with his *percussion-hammer*, *tapotement* or slapping, and *pincement* or pinching. To the list of affections in which *massage* may be used, already given, Prof. Winge adds *exudations in the cornea*, *maculæ*, *leucoma* (after Donders), *acute mastitis*, *chronic ulcers of the leg*, *lead-palsy*, and *various tumours* except sarcomata (after Mezger).

In the present volume of the 'Norsk Magazine,' we also meet with reports of the Royal Hospital of Christiania (medical, surgical, and 'cutaneous diseases' divisions) for 1870 and 1871, and of the head-division of the Christiania Municipal Hospital for the five years ending 1871, besides several clinical communications of considerable interest and value. The latter are observations of vaso-motor paralysis, by E. Bull; a case of absence of the uterus and vagina in a girl otherwise well-developed and of most womanly presence, by Aug. Koren; a case of molluscum contagiosum in a patient (a man), æt. 22 years, by Cæsar Boeck; aphasia, the result of a fall, in a girl, æt. 8¾ years, by H. Vogt; a case of necrosis from phosphorous poisoning by A. Smith; and removal of a fibroid tumour of the uterus by laparotomy, death ensuing nine days afterwards—but not from peritonitis, of which *no trace* was found at the autopsy, by A. Vogt.

7. The Medical Society of Christiania (Norwegian Medical Society) held twenty ordinary meetings, and one extraordinary meeting in 1872, communications were made at these meetings on the influence of fungi as a cause of disease; on Mezger's treatment of various forms of disease by *massage;* on a peculiar epidemic affection in the neighbourhood of Krage island; several cases of accouchement in

which the cranioclast was employed; on several cases of retino-choroiditis in the same family. Among the preparations laid before the society were a piece of intestine which sloughed away, the patient recovering a case of communication between the bladder and intestines; and a specimen of mycosis pericardii.

8. Our readers may remember that in last year's report on Scandinavian medicine a short review was given of the first part of a work by Professor Stadseldt of Copenhagen entitled 'Lecture on Midwifery.' The subject first treated of in the second part of the professor's admirably arranged manual, which has just issued from the press, is *artificial, premature delivery*. The operation is stated to be called for when there is apprehension of difficulty during childbirth caused by considerable mechanical disproportion, or where complications such as may endanger the life of mother or child, occur during the latter period of pregnancy. The author enumerates the various methods proposed for causing premature delivery. For his own part he recommends that Braun's vaginal dilator and the vaginal douche should be generally used at first; that then Cohen's method of injecting an antiseptic fluid between the ovum and the uterus, or Krause's method of inserting an elastic bougie between the ovum and uterus, should be followed. Lastly, rupture of the membranes is to be resorted to when the os uteri is five to six centimètres in diameter. *Induced abortion* is the topic next taken up, and after disposing of it, the author proceeds to treat of the second class of disproportions in roominess of the parts concerned in childbirth, namely, *narrowness in the soft parts of the genital tract*. The *os* and *cervix uteri* may be abnormally rigid or œdematous, the seat of the so-called *allongement hypertrophique de col de l'uterus* (Huguier) they may be deficiently dilated, affected with cancer, or the seat of fibrous tumours, polypi, hæmatomata and abscesses. The *vagina* may be abnormally narrow or closed by cicatricial membrane or a muscular hymen. The *vulva* may be narrowed by rigidity of the labia and perinæum, by œdematous infiltration of the labia majora; by hæmatomata, abscess in the glans of Bartholini; lepomata; agglomerated condylomata; mucous papillæ, cicatricial membranes after large losses of substance; and cancerous tumours. In addition to these, rupture of the fourchette and perineum in primiparæ is mentioned as a cause of narrowing. A description of the operation of *episiotomy*, or pubic section (*ἐπίσειον* = *pubes*) concludes this part of the book.

The third branch of the subject to which this course of lectures is devoted, is the mechanical disproportions during childbirth induced by abnormalities in the *size or development of the fœtus*, (1) of a healthy character, (2) from emphysema, (3) from hydrocephalus, (4) from hydrothorax, (5) from ascites, cystic kidney, distension of the urinary bladder, enlargement of the liver, (6) from hernia of the umbilical cord, spina bifida, perinæal tumours, &c. The presence of twins, or of monsters in the uterus, may also in some instances give rise to disproportions in roominess.

In the fourth division of the book Dr. Stadfeldt treats of mechanical disproportions arising from abnormalities in the *position* of the

fœtus, such as hand or foot beside head, face, brow, and vertex presentations and cross-birth. In the seven years from April 1st, 1865, to March 31st, 1872, an *extremity beside head* presentation was met with in 26 out of 8068 deliveries at the Royal Lying-in Hospital of Copenhagen, that is, once in 310 deliveries; a face presentation in 28 cases, that is, in 0·34 per cent. of all the cases, and a cross-birth presentation in 55 cases, that is, in 0·68 per cent. of all the cases.

This second part of the work concludes with a description of the operation of turning.

9. From Professor Stadfeldt's "Report of the Royal Lying-in Hospital and Orphan House of Copenhagen," it appears that in the year ending March 31st, 1872, 1084 women were admitted to the Lying-in Hospital and its branches, of whom 20 left without being confined. The number of those confined was therefore 1064, and the number of children born was 1077 (there were 13 cases of twins). Of those confined, 259 were described as "married," and 805 as "unmarried;" 547 were primiparæ, and 517 pluriparæ. The number of abortions was 19. Of the children 563 were girls and only 512 were boys—in two cases the sex could not be determined. The still-births numbered 72, while 1005 children were born alive. Among the still-births are included thirty cases where the fœtus was putrid. Excluding these, the still-births amounted to 42, or to 4 per cent. of all the births (1047). Various operations became necessary in 100 cases—including *gastrotomy* in a case of ruptured uterus, and *turning* on 18 occasions. The puerperal mortality was only 1·1 per cent. (1 in 89 against 1 in 70 in 1870-71), and the morbility 7·3 per cent. (1 in 14 against 1 in 13 in 1870-71). The mortality among the *married* was 1·1 per cent. (3 fatal cases out of 259); among the *unmarried*, 1·1 per cent. (9 out of 805); among *primiparæ* 1·5 per cent. (8 out of 547), and among *pluriparæ* 0·8 per cent. (4 out of 517). Of the children born alive (1005) 17 died within 24 hours after birth. The *malformations* noted were:—Harelip, two cases; spina bifida, two cases; diaphragmatic hernia, one case; hemicephalus, one case; paraphimosis, one case; supernumerary and deformed fingers, three cases; varus, three cases. The report is illustrated by several tables, and includes a list of the cases (111 in number) treated in the gynæcological "poliklinik" during the year.

10. In the numbers of the 'Hospitals Tidende' for January 10th and 17th, 1872, Dr. J. P. Poulsen makes a very remarkable clinical communication. A healthy girl, aged nine years, was seized with a continued fever, accompanied by pains in the joints and a train of gastric symptoms. Early in the third week a secondary endocarditis set in, the physical signs of mitral valve disease becoming developed. Four days later convulsive movements of the limbs and facial muscles, lividity and coldness of the face and left arm, with delirium, appeared. After another six days, complete *right* hemiplegia was ushered in by a transitory loss of consciousness, and the lips and extremities became cyanotic. The child died in six weeks from the commencement of her illness, having for the last day of her life

suffered from incessant dyspnœa. The inferior curtain of the mitral valve was covered with numerous fibrinous vegetations. The lower lobes of both lungs were the seat of hæmorrhagic infarctions, one of which was already softening. In the spleen were two large breaking down infarctions, and several of more recent date. There was embolism of the left brachial artery. In the brain, the outer half of the left corpus striatum, and the neighbouring white substance, were softened to an extent equal in size to a hen's egg; and one of the most considerable branches of the *middle cerebral artery* was occluded by a fibrinous plug, having all the characters of the vegetations found on the mitral valve. The cerebral ventricles were much distended with fluid, and both cerebral hemispheres were generally œdematous and hyperæmic. In No. 5 of the journal, Dr. Angelo Petersen reports a case of trichina-poisoning in a girl, aged twenty, who in December, 1871, had dressed pork sausages, and had several times tasted the raw meat. A few days afterwards she became ill, having a general feeling of malaise in connection with a swelling of the face and hands, and tenderness in the arms, legs, and loins. She was also heavy and sleepless. The patient completely recovered, the treatment enjoined being rest in bed, quinine and morphia, and local fomentations with elder and juniper tea. Dr. Petersen examined the meat microscopically, and found a mass of trichinæ, partly free and partly encapsuled.

A paper, by Professor A. Brünniche, in No. 17, on "Reversed Daily Typical Range of the Temperature of the Body, a symptom of Miliary Tuberculosis," deserves special notice. The author, knowing well how the temperature both in health and under almost all morbid conditions rises towards the afternoon, was struck by the frequent deviation from this rule observed in cavernous phthisis. He accordingly proceeded to investigate the matter more closely in 52 selected cases of this disease, which came under observation in his hospital *clinique* during 1871. In 33 cases (*i.e.* 63·5 per cent) he found the morning temperature to be either once or repeatedly higher than the evening temperature. Of the patients, 33 were men, in 18 of whom (54·5 per cent.) the typical daily range of temperature was reversed; and 19 were women, in 15 of whom (79 per cent.) it was reversed. Of 14 cases of phthisis *without miliary tuberculosis*, which were included in the total number (52), 6 were men, of whom 5 showed the ordinary typical range and 1 the reversed range, while 8 were women, of whom 1 showed the ordinary and 7 the reversed range. Among the 38 individuals, in whom miliary tuberculosis existed beyond all doubt, the reversed daily range of temperature was observed in 25 cases (65·5 per cent). The author shows that frequently on the days when the abnormal temperature range was observed, symptoms of suffering of organs considered as the seat of the deposition of miliary tubercle showed themselves. In 6 out of 7 cases of undoubted acute miliary tuberculosis without phthisis, the reversed range was found. As the result of his investigations Prof. Brünniche considers that we have here a very important aid to diagnosis in the disease known as miliary tuberculosis.

To Nos. 20, 21, and 22, Reserve-Physician V. Schepelern contributes an article on *Det Cheyne-Stokes'ske Respirations-Fænomen*, as our Danish brethren complimentarily term the peculiar form of respiration known here as *Ascending and Descending Respiration*. The article is based on five clinical observations of the phenomenon.

A case of extra-uterine gestation running on to the *twelfth* month, and in which the fœtus was removed by operation, is reported in No. 18 by Dr. P. V. Heiberg. An incision was carried from the cavity of the uterus through the uterine wall into the sac. Cephalotripsy was then practised. The placenta was removed in three pieces, and there was considerable hæmorrhage. In six and a half weeks the patient was dismissed well.

A second paper from the pen of Professor A. Brünniche also calls for notice. It is on the operative treatment of empyema, and is based on the results of twelve cases which came under the author's care at the Copenhagen Municipal Hospital between March, 1870, and the end of 1871. Eleven of the patients were males, and one was a girl, æt. 6½. In 6 the effusion was on the right side (2 recoveries and 4 deaths); in 6 it was on the left side (4 recoveries and 2 deaths). In 2 the indications for operative interference were of a vital character, suffocation being imminent. *Subcutaneous aspiration* was resorted to as the only treatment in 3 patients, as an initiatory step in the treatment in 7 others. The limited pneumo-thorax which occasionally resulted from the operation disappeared after a few days, but, on the other hand, an entrance of air into the pleura might make it necessary to secure a free exit for the fluid contents of that cavity. In 3 patients the method by incision, with subsequent irrigation with carbolised water and carbolic acid dressing, was tried. In one of these cases 100 cubic centimètres of pus were evacuated on the fifteenth day after an attack of croupous pneumonia, and the patient was discharged well sixty-nine days afterwards. In another case of right empyema after nephritis considerable hæmorrhage from a large muscular branch took place, blood entered the pleura where it coagulated in large masses, causing septicæmia, erratic erysipelas, and death in six days. With a view of obviating the risks attendant on hœmorrhage and the entrance of blood into the pleura, the author is in the habit of evacuating the pus by means of a syringe, a thick trocar being inserted at the base of the thorax. The end of the drainage tube attached to the canula is carried under the surface of carbolised water in a vessel beside the bed. The wound is syringed several times a day with carbolised water. The advantages of this plan are that (1) it is easy, (2) it combines the antiseptic and subcutaneous methods, (3) it gives (even in the adult) the lung freedom to dilate, and (4) it is less painful, less dangerous, and allows the patient to leave his bed sooner.

In No. 26, Dr. P. V. Heiberg commuuicates a case of Cæsarian section, followed by recovery. The patient had a cyphotic, obliquely contracted pelvis, with a conjugate diameter of about eight centimètres, but had notwithstanding passed through three confinements, although with difficulty. The operation was performed on April

16th, 1871, and on May 18th, the abdominal wound was completely healed.

Dr. Boyson reports three cases of herniotomy in the journal for September 25th (No. 39). In two instances the patients were women, aged fifty and forty years recpectively; and the third patient was a man, aged about seventy. All suffered from femoral hernia, in all the operation included opening into and partial removal of the sac, and all the patients made good recoveries.

A paper on intermittent neuralgic vesicular keratitis depending on raumatic causes by Dr. Edward Hausen (who also contributes to this volume of the 'Hospitals-Tidende' a very full and interesting account of the meeting of the Ophthalmological Congress in London last year) appeared in No. 51. The form of this affection described is one which, according to Dr. Hausen, has not hitherto been noticed. It takes its origin unquestionably in a direct traumatic action on the terminal nerve-fibres, probably those of the corneal epithelium which, doubtless, exercise an important control over the vitality of the epithelial cell. So pronounced are the neuralgic phenomena attending this affection that it might, perhaps, with propriety be called "neuralgia of the cornea" rathrr than "keratitis." The origin of the malady is always a wound in the shape of a scraping of the epithelium caused either by a neeedle or a twig, or some such thing. In from eight to fourteen days suddenly and generally by night, violent pains set in in the eye, shooting outwards in all directions from it, and accompanied by profuse lacrymation and photophobia. Treatment consists in dropping in a solution of atropine, warm fomentations, and, perhaps, quinine and bromide of potassium, internally. Eight illustrative cases are given. In No. 51, C. Lange communicates two cases of aneurism of the basilar artery, one that of a woman, forty-nine years of age, who for a year and a half had suffered from local palsies (ushered in by an apoplectiform attack) and epileptiform convulsions.

11. A valuable paper by Dr. V. Budde is met with in the fifth number of the first volume of the 'Ugeskrift for Læger,' for the present year. It is entitled "A Contribution to our Knowledge of the Treatment of Diabetes Mellitus." The clinical history of two cases of this malady which were recently under treatment in the second division of the Microscopical Hospital, Copenhagen, is given. The first case terminated fatally from an intercurrent attack of pleuro-pneumonia. The patient was son of a deranged father; he was intemperate, and four years previously had suffered from an epileptiform attack. The second patieut had several years before the disease manifested itself received a blow on the left side of the top of his head. Thus a neurotic origin for the malady seemed to be established in both cases. Glycerine was used with advantage in the second case. It was ordered in the following form:— Citric acid five grammes, pure glycerine twenty grammes, water 500 grammes, to be taken in the course of the day. The daily dose of glycerine was shortly raised to thirty-five and finally to fifty grammes. Dr. Budde alludes to the form proposed by Professor

Schultzen, of Dorpat, for the administration of this remedy, namely, twenty to fifty grammes of glycerine, 1000 grammes of water, five grammes of acetic or citric acid, to be taken in the course of the day. This preparation Dr. Budde thinks would be improved by diminishing the quantity of water to one half or even one fourth. If colicky pains or diarrhœa should set in during the use of glycerine, Dr. Budde adds to the above mixture, fifteen to twenty-five drops of Vinum Opii or of laudanum with great advantage.

To the number of the journal for February 8th, 1873, Dr. J. Zachariæ, Quarantine Medical Officer to the port of Copenhagen, contributes a report of the action taken last year to prevent, if possible, the entrance of cholera into Copenhagen by sea. On July 25th, the regulations, in accordance with the quarantine law of the 1st of May, 1368,[1] and with the decree of the Minister of Justice of October 19th, 1870, were put in force. The total number of ships which coming from cholera-infected ports anchored in she roads and sought communication with the shore from July 25th to December 31st was 345, namely, 102 steamers and 243 sailing vessels. The number of cholera cases carried into the roads on board these vessels was three, besides the corpse of one cholera patient who had died on the voyage. Of other epidemic diseases several cases were found, including from Cronstadt and Petesrburgh five cases of small-pox, three cases of exanthematous typhus, and one of dysentery. All the patients were at once removed to hospital.

At the meeting of the Medical Society of Copenhagen of the 6th of March last, a discussion took place on the subject of a very sad accident which on February 20th had caused the death of a veteran Danish physician, Professor Thomas Christopher Mürer, Knight of the Dannebrog. The attending physician, Dr. John Brodersen, of Lyngby, described what occurred. Dr. Mürer, who was in his 80th year, had for many years suffered from a severe chronic bronchitis with pulmonary emphysema, and asthmatic attacks of such a kind as repeatedly to place his life in great danger. On Thursday, February 20th, while at dinner, a pigeon bone stuck in his throat. He attempted to push it down with a probang belonging to himself, and on failing to do so sent for Dr. Brodersen. The latter, at the sufferer's request, used the same instrument. Just as the sponge touched the posterior wall of the pharynx, and was gliding down into the œsophagus, violent attempts at vomiting necessitated the withdrawal of the instrument. On withdrawing it, the sponge was found to have disappeared. Almost instantly, Dr. Mürer began to suffocate, respiration ceased, and the face became deeply cyanotic. A few seconds later respiration recommenced, but was manifestly insufficient. Additional medical assistance was now obtained, one medical man suggesting that the sponge had passed through the larynx into one of the bronchi. Auscultation at once revealed a great want of respiratory murmur in the left lung, with exaggerated murmur in the right side. The diagnosis was now clear. The suf-

[1] Vide 'Brit. and Foreign Med.-Chir. Review,' vol. xlviii, p. 475.

ferer refused to have tracheotomy performed, and death terminated the painful scene within an hour. At the autopsy the laryngeal opening was found very large, the epiglottis being ossified, and standing stiffly forward. Dr. Brodersen expressed his opinion that at the moment when the sponge had passed over the entrance to the larynx, it had broken off and fallen down upon the opening, that in the violent effort at inspiration the vocal cords had opened and the sponge had then been drawn downwards into the trachea and so had fallen into the left bronchus. The story is, indeed, a sad one, and excites a feeling of sympathy for the physician whose lot it was to take a part, though an innocent part, in so melancholy a scene, and of regret that a veteran brother should have ended a useful and honorable life after so pitiful a fashion,

To later numbers of the journal Dr. Chr. Tryde contributes a treatise on "Nervous Pulse," based on clinical sphygmographical investigations. Want of space forbids a detailed notice of this and of many other important papers. A few words are, however, necessary respecting a sharp epidemic of measles which has recently prevailed at Copenhagen. The outbreak seems to have commenced early in December last. It rapidly assumed epidemic proportions, no less than 865 new cases being reported in the week ending December 31st. The acme of the epidemic was reached in the following week (1350 cases), after which a much more gradual decline then set in. In the last week of April only 16 new cases were reported. In November no deaths were attributed to measles in Copenhagen (population about 200,000); in December there were 41 deaths, in January 154, and in February 51. The mortality in January was 4·1 per cent., and in February 5 per cent. of those attacked, by far the largest number of whom were aged between one and five years. The accompanying table gives the weekly number of cases reported. When shall we follow the example of Denmark, and institute a simple and efficacious system of *disease*-registration?

Table giving the Weekly Number of Cases of Measles occurring in Copenhagen during the Recent Epidemic.

Week ending		Week ending		Week ending	
1872 . Dec. 31	. 865	1873 . Feb. 11	. 303	1873 . Mar. 25	. 62
1873 . Jan. 7	. 1350	„ „ 18	. 183	„ . April 1	. 34
„ „ 14	. 1071	„ „ 25	. 126	„ . „ 8	. 36
„ „ 21	. 725	„ . Mar. 4	. 101	„ . „ 15	. 23
„ „ 28	. 611	„ „ 11	. 59	„ . „ 22	. 22
„ Feb. 4	. 400	„ „ 18	. 62	„ . „ 29	. 16

The *hyperbolic* tendency displayed by the curve of the morbility in the decline of this epidemic fully bears out the Rev. Professor Haughton's wave-theory of the spread of epidemics by contagion ("Brit. Med. Journal," May 10th, 1873, p. 544).

12. The courtesy of the distinguished Medical Officer of Health for the City of Copenhagen, Dr. P. A. Schleisner, has placed in our hands the reports of the health of that city in 1870 and 1871.

In 1870 the state of health was good. From 118 physicians

returns of 32,162 cases of sickness were sent in. Of *measles* but 151 cases were reported, of *scarlatina* 1661 cases (with 174 deaths). *Smallpox* occurred in a sporadic form (116 cases with 4 deaths). At Christianshavn, a suburb of Copenhagen, a fatal case of *cholerine* took place, and was regarded by the attending physician as one of Asiatic cholera. The most sickly week in the year was that ending February 22nd (836 cases of sickness reported), and the least sickly week was that ending July 12th (442 cases). With respect to scarlatina it is interesting to note that it followed its usual course, becoming gradually more severe towards the late autumn, and reaching its maximal degree of prevalence and fatality in December, with 320 cases and 47 deaths. *Venereal diseases* declined from 5506 cases in 1869 to 4875 cases. The deaths in 1870 numbered 4193, giving a death-rate of 23·1 per 1000 of the population annually (population of Copenhagen by the census of February, 1870, was 181,291). August was the most fatal month (408 deaths), and July the least fatal (298 deaths), Among the causes of death, *pulmonary consumption* heads the list with 563 deaths, *pneumonia* comes next (496 deaths), then in order *atrophia infantum* (261), *inflammation of the brain* (201), *scarlatina* (174), *cancer* (170), and *organic disease of the heart* (158).

In 1871 the state of health was not quite so favorable, and this was attributable to two causes—first, to a greater epidemic development of scarlatina, hooping-cough, typhus, and smallpox; secondly, to the meteorological conditions of the year, namely, intense cold in January and February, and a very cold spring with night frost far on into the month of May. From 118 physicians, returns of 34,776 cases of sickness were received. The maximal morbility fell in the week ending April 18th (948 cases), the minimal in that ending December 26th (431 cases). Of *scarlatina* 1654 cases, with 217 deaths, were reported. The epidemic, which, as already mentioned, began in August, 1870, reached its highest point in January, from which it gradually declined. In the first quarter of the year the number of cases was 793; in the second quarter, 488; in the third quarter, 212; and in the fourth quarter, 161. An epidemic of *smallpox* commenced early in November. The number of cases of this disease reported during the year amounted to 331 (with 23 deaths), of which 228 occurred in the last two months. The further progress of this epidemic was described in last year's 'Report on Scandinavian Medicine.' Six cases of suspicious *cholerine* occurred in August and September, and all rapidly ended in death. To the port of Copenhagen 8 suspicious cases were carried during the year from cholera-infected ports. Of these 1 died on board ship, 3 were treated at the special cholera hospital, and the rest were treated on board. Five cholera corpses were also carried into the roads on board ship. Of *venereal diseases* 4597 cases were reported. The epidemic disease, which deserves special notice as having prevailed in 1871, is exanthematous typhus. Dr. Schleisner shows how the disease has been lurking throughout Northern Europe since the wars of 1866 and 1870. In the latter year several cases were

imported into Copenhagen by sea, and at the close of the year the disease showed signs of assuming epidemic proportions. In 1871, 347 cases in all were reported, with 48 deaths. July had the largest number of cases (83). The cases occurred in 164 different localities, of which 124 were closely inspected by the sanitary police. Of the holdings visited by the disease 74 were found to be in a state of more or less disrepair, and suffering from various sanitary defects, and 43 were in addition found to be overcrowded. The deaths in Copenhagen in 1871 numbered 4875, including 829 deaths from epidemic diseases and 480 from pneumonia. The annual rate of mortality per 1000 of the population was 26·9, a figure considerably higher than that of the year before, but one which contrasts favorably with the death-rates of most of our own cities in 1871—the year of the smallpox epidemic in England.

Since writing the above the *Norsk Magazin* for May, 1873, has reached us. From the Tables of Mortality in Christiania for April contained in it, that city also appears to have been lately visited by a considerable epidemic of measles. In January only 7 cases of this disease; in February, 100 cases; in March, 1305 cases, with 27 deaths; and in April, 2132 cases, with 124 deaths, were returned as having occurred. In all 3544 cases have been reported within four months, the total number of deaths having been 151 up to April 30th. These numbers give a death-rate of something over 4·2 per cent. of the registered cases. The epidemic appears to have been very general throughout the city, but, on the whole, it has not been so far of a very unfavorable type. A discussion on the epidemic took place at the meeting of the Norwegian Medical Society on April 9th, when Dr. Larsen raised the interesting question as to the prevalence of the disease among children at the breast. The experience of the majority of the speakers went to show that very young infants did in many cases contract the disease, but they did not suffer from it more than older children. Dr. Larsen, judging by an analysis he had made of the epidemics of 1861 and 1867, thought that the present outbreak would possibly follow the course pursued by the former ones mentioned, namely, attain a maximum in the second and third months, and die away in about five months from its commenrement.

13. Dr. E. Hornemann has, at the request of the first Northern Industry Meeting (Industrimöde) compiled a most valuable paper on the laws respecting the employment of children in factories which are in force in various European countries. This paper has been reprinted from the 'Hygieiniske Meddelelser,'—'Hygienic Communications'—(Vol. 7, Part III.), and has been placed in our hands by Dr. Schleisner, who has lately visited this country. Until very recently no special legal enactments were in force in Denmark for controlling the employment of the young in factories. But a bill, based chiefly on the information collected by and on the suggestions of Dr. Hornemann, was introduced early this session into the Danish Parliament, and became the law of the land on the 23rd of May of the present year. One suggestion made by the author is of

cosmopolitan value, namely, that every factory or industrial establishment which employs more than ten artisans of different ages and sexes should have a physician attached to it, and that this physician should yearly furnish an accurate report as to the children and women employed in the factory, and as to the general state of health prevailing therein. It is to be hoped that a clause to this effect is embodied in the new Danish "Factories Act."—*June 16th*, 1873.

REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,
Member of the Royal College of Physicians, Physician to the Great Northern Hospital.

On Microscopic Appearances in Inflammation, and the Origin of Pus-Cells.—The controversy between Stricker and Cohnheim, and their followers, on the question whether, as Cohnheim maintains, pus-cells are emigrated white blood-corpuscles, or whether all living cells of tissues can change by division into pus-cells, has occupied much attention, and produced a number of observations both in Europe and America. Cohnheim's view is founded upon his observation that in the mesentery of the living frog, when drawn out of the abdominal cavity and prepared for microscopical examination, large numbers of white corpuscles are to be seen emigrating from the vessels and wandering through the tissues. Virchow maintained that connective-tissue-corpuscles undergo division during the inflammatory process, in order to be transformed into pus-cells. This is denied by Cohnheim, who maintains that the cornea-corpuscles do not change during inflammation of the cornea. He injected anilin into the vessels, and afterwards exciting the cornea, found pus-cells in the inflamed cornea bearing anilin. From his observations he maintains (1) that pus-cells and white blood-cells are not to be distinguished from each other; (2) that the formation of pus-cells from cornea-corpuscles has never been observed; (3) that pus-cells emigrate; (4) they wander into irritated tissue. Stricker, on the other hand, maintains that it is not true that the cornea-corpuscles do not change during the inflammatory process. He states that the stellate cornea-corpuscles, after the cornea has been excited to inflammation, draw back their processes, multiply their nuclei by division, and become amœboid. Stricker observed the division of such larger cells into smaller ones. Pus-cells and white blood-corpuscles are young cells, and young cells are not to be distinguished from each other, whether they are born of a cornea-cell by division or whether they wander from a vessel. Anilin injected into the blood-vessels has been seen by Stricker and Norris of Philadelphia in the cornea (stellate) cells, therefore the pigment-bearing pus-cells may as well be daughters of the stellate cells as emigrated white blood-corpuscles. Dr. S. Talma ('Archiv für Ophthalmogie, xviii vol., 2nd part, Berlin, 1872) has made some

experiments in this difficult subject. He finds that on irritating the centre of the cornea the opacity which supervenes shows itself at first at the circumference, and afterwards extends towards the centre. The cornea so irritated is detached and placed in a concentrated solution of sugar. After treatment by this reagent two forms of cells, differing clearly from each other, may be seen by the microscope. One is the pus-globule, become spherical, and strongly refracting light; the other is the fixed cornea-cell, flattened, pale, stellate. Between these two elements he does not find any having an intermediate character, or which may be considered as a transition from the one to the other. Talma acknowledges that in an inflamed cornea treated by chloride of gold transitory forms between the fixed corpuscle of the cornea and the pus-globule are found, but he does not consider that this invalidates the former experiment. On the other hand, R. v. Pfungen ('Stricker's Medizin Jahrbucher,' 1873, pp. 80—95), in some experiments carried on under Stricker's direction, arrives at different results. He produced inflammation of the cornea by passing a thread of silk through it. Preparations of the inflamed cornea treated with chloride of gold constantly exhibit an active proliferation of the fixed cells of the cornea. Six hours after the injury the fixed cells of the cornea exhibit club-shaped prolongations, which anastomose with like prolongations from neighbouring cells. Many of these prolongations are only connected with the cell by a very fine pedicle—by a sort of filament of protoplasm. The body of the cell exhibits the characteristic nucleus of the cell of the cornea. At the end of about twelve hours the body of the cell becomes constricted and divided into segments, as well as the nucleus, which is replaced by several elements answering completely to the form of pus-globules, except in size, which is variable. They are not, therefore, white globules which have penetrated into the fixed cells of the cornea. Von Pfungen concludes, therefore, that in inflammation of the cornea the fixed cells of that tissue proliferate and give birth to nuclear corpuscles identical with pus. Similar phenomena occur in corneas inflamed after section of the fifth pair, with or without destruction of the Gasserian ganglion. Similar observations were recorded by Straus and Duval in the 'Gaz. Méd. de Strasburg,' 1870. Dr. S. H. Chapman, of New York, has published some researches on artificially produced pericarditis which are strongly confirmatory of Stricker's views. His observations were made in the frog and toad. Pericarditis was induced by opening the pericardium and applying nitrate of silver to the pericardial sac. On microscopical examination it was found that at first all cell elements of the tissue became hypertrophied; the endothelia and their nuclei, retaining their contours, grew in area and in thickness, the connective-tissue-cells enlarged, the vessels and nerves were more readily found than in normal tissue, and the cilia upon the outer endothelia increased in length and calibre. On the second day the changes were more marked; the endothelia of either surface had begun to develope towards the formation of a new membrane; nuclei of outer endothelia split up; cells show a tendency to divide; nuclei of internal endothelia proliferate by a more

uniform division, enlarge and separate from one another, at the same time the cells have been changing their forms by amœboid movements. The connective-tissue-cells elongate and their nuclei divide. The vessels and nerves are at least increased in size, if not in number. On the sixth day, in the ground substance, which was multiplied to many times its normal thickness, were scattered free cells of different shapes, resembling pus, the transformed nuclei of connective-tissue-cells and stellate and elongate cells. The appearances of the nerves supported the belief that a new growth was taking place. The endothelia of the outer surface no longer possessed their normal outlines, but were transformed into elongate or spindle-shaped cells. Upon the inner surface the change was even more marked. The amœboid-like endothelia had divided into innumerable cells of various forms and sizes, varying from that of the pus-cell to several times the size of normal internal endothelia. In a more advanced stage the spindle-shaped cells of the external surface unite with one another, arrange themselves in rows, and form a compact membrane. The cells of the inner surface throw out delicate filaments, which unite with like filaments from neighbouring cells to form a new network of fibres and a new connective tissue. The author thus sums up his investigations:—1. All cell elements of a tissue during inflammation multiply. 2. New formations take place—first, of cells; second, of connective tissue (false membrane); and third, most probably of nerves.—*G. Hayem's Revue des Sciences Méd.*, Avril, 1873.—*The American Journ. Med. Sciences*, Oct., 1872.

Septicæmia.—In December, 1872, M. Davaine brought before the Académie de Médecine the results of some new experiments:—1. One drop of sanious fluid from gangrenous lung, taken from a man who died from pulmonary gangrene in the Hôpital Saint Antoine, was inoculated on a rabbit; there was no result. The blood taken from the heart was inoculated on three rabbits in the dose of one drop, one thousandth of a drop, and one millionth of a drop. The three rabbits died in from one to two days. 2. To a sheep, aged three years, 100 grammes of pork brine were administered daily. It died on the tenth day, having taken one litre. Three rabbits were inoculated with blood from the heart in doses of one tenth of a drop for one, and one millionth for the two others; they died ten and thirteen days after inoculation. 3. On the 28th October blood of a patient suffering from typhoid fever was obtained from the median basilic vein by means of a Pravaz syringe. One thousandth of a drop was inoculated in a rabbit. The rabbit died on November 28th; the patient recovered. Another patient, suffering from typhoid fever in its decline, furnished blood from a pricked finger. Two rabbits inoculated with a thousandth and a millionth of a drop of the blood respectively died in thirteen days. A third patient, suffering from severe typhoid, likewise furnished blood from the finger. Inoculations with a thousandth of a drop of the blood thus obtained, performed after several days' interval, killed several rabbits. Similar experiments with the blood of a fourth patient with typhoid were followed by similar results. A fifth patient, in

the fifteenth day of very grave typhoid, furnished some drops of blood obtained from a small vein. A rabbit, inoculated with a millionth of a drop of this blood, died in about fourteen hours.—*Arch. Gén. de Méd.*, Feb., 1873.

M. Onimus in March, 1873, reported the following experiment to the Société de Biologie:—Having made a sort of filter with dialysis paper, he filled it with putrid blood, or blood taken from a septicæmic rabbit; he then placed it in water. In a very short time the water contained considerable quantities of bacteria—of vibrios; this water injected in different doses into rabbits did not kill them. M. Onimus, therefore, concludes that in septicæmia bacteria are not the cause of the propagation of the putrefaction or of the septic poison. The septicæmic poison is not a dialysable substance. The bacteria formed in the water were not derived from the putrefied blood; they did not traverse the dialysing paper. This, however, does not prove that bacteria have nothing to do with septicæmia. It only establishes the fact that bacteria formed in the neighbourhood of a putrid liquid do not convey the septicæmic virus.—*Gaz. Hebdom. de Médecine et de Chirurgie*, March 7th and 14th, 1873.

The following is a summary of Davaine's experiments:—Putrefied blood injected in guinea-pigs and rabbits does not kill more than half of the animals operated on when the quantity injected is less than one drop. But by the injection of a minute portion of the blood of animals that have died of septicæmia the toxic power increases, and this in such a degree that blood taken from animals of the twenty-fifth series of experiments is sufficient to kill in the injected dose of one trillionth of a drop. The septicæmic poison acquires wonderful power by passing through living organisms. The septicæmic virus is gradually destroyed during the putrefaction of the animal that it has killed. Hence the greater danger of a dissecting wound when putrefaction of the body has not set in. Davaine's experiments have been confirmed by Vulpian and Professor H. Bouley. Vulpian finds there is no infarctus in animals that have died of septicæmia, and that septicæmic blood shows an immense number of bacteria. Stricker has recently performed experiments which confirm, on the whole, Davaine's observations. He found that the malignity of the septic poison is increased with transmission; ·0008 cubic centimètre of the blood of the twelfth animal injected was sufficient to kill the thirteenth. Stricker does not confirm the above observation of Onimus that septic matter is not diffusible through membrane. He found that fatally poisonous results were produced by fluid obtained from the blood by dialysis.—*Bullet. de l'Acad. de Méd.*, Sept., Dec., 1872, Jan., 1873; and *Brown-Séquard's Arch. of Scientific and Practical Med.*, Feb., 1873; *Wiener Allg. Mediz. Zeit.*, 1873, No. 20.

On the Presence of a very minute Filament, endowed with motion, in the Blood of Patients suffering from Relapsing Fever.—Dr. Otto Obermeier, in 1868, remarked the existence of a parasite in the blood of persons attacked with relapsing fever. The last epidemic, which lasted a year in Berlin, afforded him an opportunity of again

examining the subject. The parasite is in the form of an extremely firm filament, of the thickness of a filament of fibrine, and of the length of from one and a half to six red corpuscles. In the blood of a patient attacked with relapsing fever several were seen in the field of one preparation. As long as it remains fresh very lively movements of two kinds are observed in the filaments. There is first a movement of undulation of the filament itself, and, secondly, there is a movement of locomotion, which is tortuous. The latter movement was observed from one to two hours; the movements of undulation may go on for eight hours. They resemble those of spermatozoa. Hitherto the author has only discovered the filaments during the febrile state, during the crisis or a little time before, never during the remission. Their presence seems peculiar to relapsing fever, but Obermeier does not adventure an opinion as to their signification or their nature.—*Centralblatt*, No. 10, 1873, and *G. Hayem's Revue des Sciences Méd. en France et à l'étranger.*

On Fatty Heart.—Cases of fatty degeneration of the heart may be referred to two great classes; the first comprehends those in which the muscular structure is previously diseased, and becomes attacked with fatty degeneration, the cause being some obstacle to the circulation, and it remains localised. In the second class the fatty change is generalised, the muscular structure having been previously healthy and the valves intact. This class comprehends cases of infectious diseases; those from steatogenous poisons; and to this group must be referred those cases of so-called idiopathic fatty degeneration which have generally served as the type in classical descriptions. This latter is distinguished from the two preceding groups by its slower progress, and by the occurrence of portions of tissue in which the disease has advanced farther in the midst of a general uniform degeneration. The papillary muscles are the seat of these more advanced changes. Dr. Ponfick seeks to show that this last-named group itself is not a uniform one, and that under the designation "pure idiopathic fatty degeneration," are comprehended two completely distinct states, which have nothing in common except their final stage—they both terminate in adipose of the myocardium. The first is the senile and plethoric type (the type almost exclusively described), which, side by side with various alterations proper to old age, presents a complex lesion of the whole aortic system—a deforming endarteritis. Cerebral apoplexy by embolism, which is the proximate cause of death, is but a consequence of the disease of the arterial vessels. 2. The anæmic type, in which after death, beyond adipose change, we find no notable lesions except an extreme anæmia of all the organs. This decoloration of the viscera Ponfick believes to be diagnostic. It may be recognised during life by a pallor of the integuments which is only comparable to that produced by great hæmorrhages. This particular form of degeneration is met with principally in persons from twenty to forty years of age, and frequently at the autopsy it is impossible to attribute death to any particular lesion. The little blood contained in the cardiac cavities is liquid or recently coagulated; it is

of a dull red; the white globules appear very numerous, but their increase is only apparent, and depends on the considerable diminution of the number of the red globules, a fact which explains the frequent co-existence of dropsical effusions. The anæmic type is met with in women after difficult labour, in persons who have had severe acute maladies especially typhoid fever), after chronic diarrhœas dependent on affections of stomach and intestines, and after hæmorrhages.—*Berlin. Klin. Wochens.*, 1873, and *Hayem's. Revue des Sciences Médicales en France et à l'Étranger*, No. 2, Avril, 1873.

On a Sphygmographic Character of certain cases of Mitral Narrowing.—M. Lépine, in April, called the attention of the Société de Biologie to three well-marked cases of mitral contraction, in each of which the sphygmographic tracing presented a well-marked dicrotism, as well marked as in typhoid fever. The temperature of the patients taken by the thermometer was absolutely normal. This peculiarity, which the author believes has been unnoticed by any observer, is explained by the diminution of the arterial tension. Lowering of arterial tension resulting from diminution of the volume of the blood, the force of the ventricle not being weakened, is one of the conditions favorable to the production of dicrotism.—*Gaz. Méd. de Paris*, Mai, 1873.

On kinds of Hemiplegia hitherto unknown or very little known.—Dr. Brown-Séquard, as the result of the collection of cases in which hemiplegia occurred on the same side of the body as the lesion of the nervous centre, has come to the conclusion that unilateral paralysis so located may be due to organic lesions, not only in the spinal cord and medulla oblongata, but also in almost any part of the brain or cerebellum. According to the part of the nervous centres which is the seat of the lesion the symptoms allied with paralysis vary, so that for each part there is a special kind of hemiplegia. If we admit the received opinion that one half of the brain is the seat of the will for the muscles of the opposite half of the body, we cannot explain many of the cases on record. We must admit that either half of the brain alone can will and regulate the movements of the limbs on the two sides, and that where disease exists in any part of the brain proper, the cerebellum, the pons Varolii, or the medulla oblongata, the lesion can produce paralysis either in the corresponding side or in the opposite side, and also that if paralysis is produced (if—because paralysis is not constant) it is not by the destruction of either the organ of the will or that of conductors between it and the muscles, but by an inhibiting or arresting or suspensory influence exerted on distant parts by the irritation of the diseased fibres or cells.—*Brown-Séquard's Archives of Scientific and Practical Medicine*, Feb., 1873.

Peculiar Paraplegiform Affection, under the name of Tetanoid Pseudo-paraplegia.—Dr. E. C. Séguin describes a form of false paraplegia characterised by impairment of the functions of the lower extremities when the patient is in the erect posture, without any loss of power in these parts. The seeming paraplegia is dependent upon

tonic spasm of the muscles of the lower limbs. There is absence of ataxia, and often preservation of sensibility. The pathological condition on which the symptoms depended appeared to be compression of the anterior part of the spinal cord in the dorsal or cervical region. In three out of five cases observed this could hardly be questioned, since cyphosis existed. Tetanoid pseudo-paraplegia the author looks on as a symptom of moderate compression of the spinal cord at some point above the lumbar enlargement. He believes, however, that an analogous state may be observed in hysteria. It seems probable, however, that in cases of increased spinal excitability, without lesion, the spasms would be more clonic (saltatory) than tonic (tetanoid) in character.—*Ibid.*

Aneurisms of the Arteries at the Base of the Brain.—Dr. R. Bartholow, in this paper, discusses the symptomatology, diagnosis, and treatment of aneurisms of the larger intra-cranial arteries, which, by interfering with the functions of different parts of the brain, give rise to symptoms common to new formations. A tumour of any sort, slowly growing in the cranial cavity, will alter or abolish functions by direct pressure, by setting up irritative action and consequent structural change, and will affect the cerebral functions generally by pressure on the whole contents of the cranium. The general symptoms of intra-cranial aneurism are vertigo—one of the earliest symptoms. The author thinks that the early occurrence of vertigo is of value as a diagnostic mark, for in other tumours of the brain vertigo is a symptom coming on at a much more advanced period in the history of the case. There may also be a localised throbbing or "snapping" experienced in the head, as was noted in a case of aneurism of the basilar artery recorded by the author. The throbbing sensation is generally referred to the neighbourhood of the aneurism. Headache is a constant symptom, the site of greatest intensity of pain indicates the position of the aneurism. Epileptic convulsions of various intensity occur in about one half of the cases. Besides convulsions there are local spasms, as twitching of the facial or cervical muscles, of the trapezius, &c. "The occurrence of epilepsy appears to be influenced by the vicinity of the aneurism to the 'spasm centre' of Nothnagel; hence this symptom is to be expected in aneurism of the vertebro-basilar system, and to be considered exceptional in cases of aneurism involving the anterior and middle cerebrals." The author does not agree with Jaccoud that in cases of intra-cranial aneurism the mental functions are little or not at all affected. This is not in accordance with the facts of a number of reported cases. With regard to special symptoms, an aneurism of the internal carotid will affect the sense of smell by compression of the olfactory nerve, cause ptosis, convergent strabismus and dilated pupil by pressure on the motor oculi, produce amaurosis by interference with the optic, congestion of the eye and facial venous congestion by compression of the cavernous sinus, and tic douloureux by irritation of the ophthalmic division of the fifth. There will be pulsating noises, impaired hearing, and deafness. There may be paralysis of the face from the facial becoming in-

volved. Softening of the neighbouring middle lobe or pressure on the crus cerebri will be followed by crossed hemiplegia. An aneurism of the middle cerebral may attain considerable size without producing any other symptom than those common to adventitious products in the brain. When the motor oculi is impinged on there will be ptosis, divergent strabismus, &c. When the aneurism occurs on the posterior communicating artery disturbances of vision, due to pressure on the optic and motor oculi, are likely to be present. As the tumour enlarges, pressure on the crus cerebri would give rise to disorder of locomotion or hemiplegia. The basilar is more frequently affected with aneurism than any other vessel of the brain. Headache, commonly occipital and sometimes limited, is one of the earliest symptoms. Tic douloureux is sometimes present, dependent on irritation of the trunk of the fifth. Convergent strabismus will be produced by paralysis of the sixth from pressure; and ptosis, dilated pupil, and divergent strabismus when the third nerve is destroyed. When the third, fourth, and sixth nerves are involved the eye will be motionless, the pupil dilated and fixed. Pressure on the cavernous sinus will cause optic neuritis, "choked disks," and swelling of eyelids and face from venous obstruction. Vertigo, disorder of locomotion, anæsthesia, or hyperæsthesia, may be present. Bilateral numbness, alternating hemiplegia, auditory paraplegia, or general paralysis, may ensue, and dysphagia, aphonia, alteration of speech, mental symptoms. In aneurism of the vertebrals we find only dysphagia, aphonia, vomiting, obstinate constipation, disorders of motility and sensibility, epileptiform convulsions, and mental symptoms common to aneurisms of the other arteries of the base.—*Amer. Jour. Med. Sci.*, October, 1872.

On Hysterical Oliguria and Anuria, and the Vomitings which accompany them.—The condition described by M. Ch. Fernet, under the terms hysterical oliguria and anuria, has already been noticed by M. Charcot as hysterical ischuria; but the term ischuria would point rather to difficulties of micturition dependent on vesical troubles than to the condition the author is describing, in which there is a diminution in the quantity of urine reaching the bladder, or complete suppression. For several days in succession the quantity of urine passed by the patients will be either reduced to a few drops or will be wanting, the bladder being proved empty by catheterism. After a time repeated vomitings occur, which continue as long as the suppression or diminution of urine lasts. There is a reciprocal compensation between the vomitings and the oliguria; as the one diminishes the other increases, and inversely. Chemical analysis shows the connection between the vomitings and the suppression of urine, for urea is found in the vomited matters. The condition is analogous to that of an animal in which the kidneys have been removed or the ureters tied, in which case the supplementary elimination of urea or of carbonate of ammonia by the intestines has been established. The author records a very interesting case, in which a daily examination and analysis was made of the amount of aliment taken, urine passed, and vomited matters. From

a table of daily analyses, extending from September 14th to 30th, it appears that urea was always present in a notable quantity (from 0·55 to 1·87 grammes) in the vomited matters. When the urinary secretion was suppressed the quantity of urea in the vomited matters gradually increased, and when a normal amount of urine was passed into the bladder the urea in the gastric secretion immediately diminished and disappeared with the vomiting. The question arises why these patients escape the grave accidents of uræmia. M. Charcot attributes their immunity from these accidents, together with the diminution of the urea excreted, to the same cause which enables hysterical patients to exist for a length of time upon an insufficient or on almost no diet—a slowing or enfeeblement of the process of denutrition. Frerichs has found carbonate of ammonia in fluids from the stomach, and Hepp, of Strasbourg, has discovered urea in vomited and intestinal excreta in cases of disease. The author found, in the case of a woman suffering from that form of Bright's disease which we know as contracted kidney, with constant vomiting, that the vomited matters contained from one to two grammes of urea daily. The author concludes—1. That hysterical oliguria exists and deserves special description. 2. It is characterised by a diminution or temporary suppression of the secretion of urine, and, secondarily, by vomitings, in which urea may be found. Hysterical vomitings have often this origin. 3. It may last a long time without inducing the accidents which arise, under other circumstances, from insufficient urinary depuration. — *L'Union Médicale*, Avril, 1873.

Febris Diphtheroides.—The author proposes this name for a hitherto undescribed disease, which he believes to be peculiar to the Chinese rivers. He has met with it up the Yang-Tse-Kiang river, at Shanghai, and at Ningpo. Diphtheroid fever presents some of the features of diphtheria and some of intermittent fever. It seems to be peculiar to the fresh-water rivers. The features of the disease are fever of greater or less intensity, inflammation of the mucous membranes with exudation, which has a tendency to become organised. The fauces are first the seat of exudation, sometimes also the gums and mucous membrane lining the cheeks; the œsophagus and stomach may be affected and the small intestine is so in the majority of cases. Exudation in the larynx sufficient to interfere with respiration is rare. Exudation is at first thin and transparent, then becomes opaque and thick. Exfoliation takes place within a few hours, and is generally followed by a second and third formation, each new formation being accompanied in some cases by a chill. The lymphatic glands of the neck enlarge, but do not suppurate. The symptoms are lassitude, anorexia, constipation, chill followed by fever, high temperature, transient roseolar eruption over abdomen, or, in other cases, an appearance as if the body had been rubbed with oil, tongue resembling in colour an unbaked brick with red raw-beef-like edges, variable pulse, pain and tenderness over abdomen, especially over the sigmoid flexure of the colon, tympanitis; the throat trouble is rarely complained of, the sensibility of the inflamed

mucous surface appears to be wanting, no discomfort is produced by pricking or tickling; there is temporary paralysis, although deglution is performed except in bad cases. The exudation can be stripped off; it is ashy white; it occurs on the second day. The patient is rapidly prostrate and exhausted, but the gravity of the case does not depend upon the local condition. In the worst cases coldness of surface occurred on the second day, and death on the fourth day. In grave cases only there is delirium. In the majority there is albumen in the urine, but death occurring early with delirium and coma is probably due to urræmic poisoning. The duration of the disease is from one to two weeks. There may be paralysis of the bladder, and the author thinks the constipation due in some extent to paralysis of the rectum. The paper contains no record of a post-mortem examination. The author thinks that the tympanites and abdominal pain and tenderness over the colon distinguish this disease from diphtheria. He looks upon it as an endemic disease of miasmatic origin.—*Amer. Journ. Med. Sciences*, Jan., 1873.

Ancylostomum Disease, Tropical Chlorosis, Tropical Hyperæmia, Intertropical Anæmia.—O. Wucherer describes that special form of anæmia which has been known by the names of "cachexia Africana, intestinal anæmia, Egyptian chlorosis, mal d'estomac des nègres, geophagia, cachexia aqueuse, &c. This disease, which occurs only in hot climates, was referred by Griesinger to the presence in the small intestine of a large quantity of ancylostoma, worms of the nematoid order. This view has been confirmed by the author. Wucherer, whilst residing in Bahia, met with a number of cases. The disease principally attacks negroes, but Europeans are not always safe from its attacks; it is rare in great towns; less frequent in the female than in the male; agricultural pursuits favour its development. Its attack is slow and insidious; strength gradually diminishes, the skin becomes discoloured, digestion painful, the tongue pallid; the expression is languid, temperature falls, serous effusions make their appearance, at first slight, about the eyelids and ankles, afterwards more extensive. Sometimes there is increasing dyspnœa; the pulse becomes rapid; slight efforts produce palpitation, præcordial pains, noises in the ears, vertigo; there are characteristic anæmic bruits in the heart and vessels; the patients seek solitude, their character and taste change; they will eat wool stuffs, charcoal, earth, plaster; hence the name geophagia. Menstruation generally ceases in the female; the urine pale abundant, without albumen, of normal density; there are pains in head or extremities; large collections in the serous cavities; metrorismus; in the last stage constipation gives place to colliquative diarrhœa; the patients die comatose or asphyxiated. After death there is found decoloration of the tissues; the intestinal mucous membrane is thickened and softened; the small intestine is in some parts narrowed, so as to admit with difficulty a finger; its internal surface presents a large number of patches covered with sanguinolent mucus, and also a great quantity of small worms (ancylostomes), having the head strongly attached to the mucous

membrane between the valvulæ conniventes. The mesenteric glands are swollen. Wucherer supposes that the larvæ of the worms are introduced into the system by drinking water. He has never found the acylostomes or their eggs in the patients' dejections. The medicines of most use are the juices of anthelmintic plants, especially that of *ficus doliaria*.—*Deutsches Archiv für Klinische Medicin*, vol. x, Sept., 1872; and *Revue des Sciences Médicales en France et à l'étranger*, Janvier, 1873, tome i.

On the Variations of Temperature in the course of Tuberculosis.—Lebert, from observation of the temperature in 153 cases of tuberculosis has formulated the following conclusions:—1. The course of the temperature in tuberculosis is such as we observe in inflammatory diseases of long duration; it does not exhibit specific characters. 2. Although the fever of the tuberculosis rarely attains a high degree of intensity, it powerfully contributes by its long duration and frequent irregularities to induce the exhaustion of consumption. 3. Neither age nor sex, at least after puberty, exercise an appreciable influence over the intensity of the fever. 4. In the typhoid form, which especially appertains to acute tuberculosis, but which may also be observed in the commencement of chronic cases, the course is quite other than that of typhoid properly so called. There is not the progressive rise during the first days; the mean temperature is less high; the remissions are less marked, they do not present any regularity, and it is not rare to see them come on in the evening. 5. The course of fever in phthisis often presents peculiarities for which neither the form nor the evolutions of the disease account. It is probable that morbid products are developed in phthisis which exercise a pyrogenetic action, and which perhaps contain a more or less poisonous element. 6. The information, therefore, furnished by the thermometer cannot throw light sufficiently on disturbances which nutritive changes undergo in phthisis. 7. In the morning the temperature is often normal at the beginning and at the end of the disease. 8. In the evening the temperature is generally febrile, rarely very high; generally from 39° to 39·15°; 40° is often reached, 42° very rarely. In the last stage the evening temperature falls, generally not lower than 38°; occasionally only it rises temporarily to a high number. 9. In acute tuberculosis high temperatures are somewhat more frequently observed. 10. The long duration and irregular course of the fever in tuberculosis are to be referred rather to the increasing weakness of the patient than to the intensity of pyrogenetic processes. 11. The decrease of fever in the ulcerative stage of the disease is to be attributed to collapse. The temperature is affected by two sets of causes. Pyrogenetic processes tend to raise it, whilst other causes tend to lower it, and so produce collapse. The latter predominate at first in the morning, after that later in the evening. 12. In most cases temperature attains suddenly a high number, without any circumstance to account for it. 13. In the last weeks or last days of the disease the temperature suddenly falls enormously. It may go down to 35°, 34°, and even to 32·5°; generally there is a consecutive rise almost as great. It is a sign of the end, nevertheless death rarely takes place during this

period. 14. The chart may show considerable oscillations in the temperature of one day; this is a peculiarity characteristic of the temperature in tuberculosis. 15. The morning temperature generally influences the type of the fever. 16. In certain cases the inverse type predominates. 17. When the disease undergoes a temporary arrest it is not a bad sign if the morning temperature remains below the normal. 18. When death approaches, the temperature may fall progressively, or it may remain normal, or it may rise at times to a considerable height, even 42°. In acute tuberculosis especially, the final temperature becomes high. 19. In the first stage of chronic disseminated pneumonia the temperature is at first normal; gradually there is a slight evening rise, which does not much exceed 38°. 20. When this disseminated tuberculisation is accompanied by fever the temperature may take a different course. 21. When the tubercular foci multiply it may be accompanied by fever, with evening exacerbations. Fever is also more intense when the morbid products undergo retrogressive metamorphoses. 22. After abundant hæmoptysis, fever may notably diminish or entirely fall. 23. Intense febrile changes may be developed in the course of chronic tuberculosis without any complication or occasional cause. In favorable cases they gradually recede. 24. Febrile exacerbations observed in phthisis are not due to the resorption of the products of decomposition; they occur where there is no softening; and, on the other hand, in broncho-pneumonia with caverns the fever may remain constantly moderate. The thermometric range in caseous pneumonia is not characteristic. 25. When in the last period of chronic tuberculosis there is a development of miliary granulations, fever rarely undergoes a notable increase; it generally retains its preceding character. 26. Acute or subacute miliary tuberculosis of the lungs, whether primitive or consecutive, presents almost as many varieties as chronic tuberculosis. Fever may be slight, or of moderate intensity, with evening exacerbations. At other times intense and continuous at the beginning, it soon becomes irregular. In some cases it presents a remittent type. 27. The same may be said of miliary tuberculosis of the peritoneum and meninges. 28. The occurrence of pneumothorax causes a rapid rise of temperature, which is not long maintained. 29. Intercurrent inflammations cause the temperature to rise. 30. Perforation in peritonitis, when it kills rapidly, produces a sudden fall of temperature—as low as 34°.—*Deutsches Archiv für Klin. Med.*, Nov., 1872; and *G. Hayem's Revue des Sciences Médicales en France et à l'étranger*, Avril, 1873.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIÈNE.

By Benjamin W. Richardson, M.D., F.R.S.

Chronic Poisoning by Fumes of Zinc Oxide. — Dr. Leo Popoff reports the case of a man thirty-six years of age who had been a bronze founder for twelve years, and had lived, consequently, in an

atmosphere charged with fumes of oxide of zinc. At first he suffered from pyrosis, from gaseous eructations; then, every day, when he returned home, he perceived upon his lips a whitish efflorescence, arising from a deposit of oxide of zinc, at the same time experiencing a very decided metallic taste in his mouth. In the winter, when the doors of the workshops could not be kept open, the intensity of the occurrences increased; he often had violent choleraic attacks (intense headache, severe shiverings, cramp in the extremities and especially in the calves of the legs, nausea, vomiting, and considerable action of the bowels). At this time he was placed under the care of Professor Botkin; the vomitings had become almost continuous, returning regularly three or four hours after a meal; he had always pyrosis and acid eructations, and complained of a dull pain in the epigastrium and on the right of the umbilicus: a general weakness, more marked on the right side, and habitual constipation continued during six, eight, and ten days. He slept badly, and was often disturbed by violent headache and painful cramps in the calf of the legs; had extreme sensitiveness in the forehead, and an appetite small, capricious, and at times even altogether failing. There was paleness of the mucous surfaces, with disappearance of the subcutaneous cellular tissue, and atrophy of the muscular system, much more marked on the right side, in the face as well as in the limbs. Decrease of sensibility to touch; tickling, on the contrary, was more quickly responded to than in the normal state. The temperature in the morning was 36°, in the evening 36·3°; weight of the patient, 45 kilogr. His answers were slow, and he spoke as if in a bad temper. The daily quantity of urine was 550 c.c.; it was neutral, or even alkaline, containing neither sugar nor albumen; nevertheless, appropriate reagents revealed in it the presence of zinc two and a half months after the patient had left off work. The first cardiac sound was hoarse and rather prolonged; the liver was voluminous. The stomach was considerably dilated as far down as the navel, its peristaltic movements at times being very appreciable through the abdominal wall; the pylorus hypertrophied, sunken, and movable, formed a tumour sensible to the touch, sometimes to the right, sometimes to the left of the umbilicus. At last, under the right false ribs another hard tumour was found, slightly indented, not well defined, but painful when pressed, and which gave rise to the idea that a new growth depended from the liver; but repeated examinations showed that its size was very variable, that prolonged pressure of the fingers caused its gradual disappearance, and that it was most evident during the violent gastric crises from which the patient suffered; it was caused purely by the contractions of hypertrophied muscle, on the right side, resting upon a solid surface formed by the increased mass of the liver.

The matters vomited, the daily quantity of which varied from 600 to 2000 grammes, gave an acid reaction and a smell of rancid oil. Besides the remains of the food the presence of products of fermentation was proved, such as butyric acid and lactic acid, the existence of a large number of sarcinæ and cryptogams. The gas from the

eructations burned in daylight with a pale flame, scarcely visible, indicating the presence of hydrogen.

The patient had chlorine water administered to him for the stomachic fermentation, rhubarb for the chronic catarrh of the stomach, and his diet was composed exclusively of animal food. He left the hospital cured.—*Berlin. klin. Wochens.*, 1873, No. 5; and *Revue des Sciences Médicales*, Tome I, No. 2.

Toxicological Studies on Hydrocotarnin.—Dr. F. A. Falk has made with this alkaloid of opium two series of experiments, one on rabbits, the other on frogs. He employed the chlorhydrate of hydrocotarnin, administering it by subcutaneous injection.

The results at which he arrived were:—For killing a rabbit, two decigrammes of the salt were required for every kilogramme of the animal's weight. This new alkaloid then takes its place as an active agent of the third rank, after thébaine and codéine, but before morphine.

The lesion most constantly found after death in rabbits poisoned by the salt was the dark colour of the blood contained in the left ventricle. But it is known that this transformation of the arterial blood into venous blood is to be found in numerous kinds of intoxications.

As with codéine, two symptomatic forms are distinguishable in poisoning with hydrocotarnin—a tetanic form and a narcotic form. In both cases the symptoms commence with respiratory disturbance and agitation, and there is always a final identical stage which is adynamic. In the tetanic form there is elevation of temperature, in the narcotic form the temperature decreases.

To poison a frog a dose ten times the quantity is required—two grammes, or two and a half grammes, of hydrocotarning per kilogramme of frog. The phenomena of intoxication are the same as with rabbits—tetanus, apparent death, then actual death. During apparent death the heart is the only organ still active.—*Vierteljahrsschrift f. gerict. Med.*, Janvier, 1873, p. 49; *and Revue des Sciences Méd.*, Tome I, No. 2.

Substances Antagonistic to Phosphorus as a Poison.—Professor Gubler states, in reference to substances antagonistic to phosphorus, that arsenic, which much resembles it in a chemical point of view, seems to be the antagonist of phosphorus as the bromide of potassium is in relation to the alkaline iodide. The gas arising from the combustion of carbon, sulphuretted hydrogen, the anæsthetics, the cyanides, and sulphide of carbon, are the dynamic antagonists of phosphorus.

The chemical counter-poisons of phosphoric preparations are sufficiently numerous and tolerably efficacious. To neutralise acids derived by oxidation of the phosphorus the Professor advises the use of lime-water or of hydrate of magnesia (Brullé, Poggiale, C. Paul). By means of carbon, which he first recommended, he says it is possible not only to absorb the disengaged vapours of phosphorus in the alimentary canal, but also the metalloid in suspension in the aqueous liquids. The ulterior experiments of Eulenberg and Vöhl

upon animals also established and confirmed his previsions. But the most certain method of treatment, or, at least, that which has the most confirmed experience, is the use of the essence of turpentine, an agent employed for a long time in a manufactory at Stafford to protect the workmen during the steeping of the chemical matches. The first clinical experiment elsewhere and crowned with success was made by Dr. Andant, of Dax, in 1868. Other favorable facts were observed by the same gentleman, as well as by Dr. Sorbets, and, abroad, by Köhler. Personne also has made some interesting experiments on dogs, which place beyond doubt the efficacy of this method. But if all investigators agree in proclaiming the virtues of the essence of turpentine, and the same could doubtless be said of all *hydro-carbonated essences*, which, like those of bergamot, citron, lavender, copaiva, mint, mace, mustard, rosemary, and thyme, prevent the phosphorescence of the metalloid, they are far from unanimous in agreeing upon the mode of operation of this kind of chemical counter-poison. Personne attributes to the essence of turpentine the power of opposing itself to the oxidation of phosphorus, with which it penetrates into the circulation, and so prevents the production of asphyxia by possessing itself of the oxygen of the blood. This theory has been doubted by Currie and Vigier, who, taking advantage of their negative results, have denied the experimental proof and the theory of the original investigators. It seems, however, difficult to contest the good effects of the essence of turpentine in phosphorous poisonings. It might even be asked, as a German experimentalist did, whether the innocuous property of the phosphorus is not attained by means of a rapid combustion, effected by the ozone present in the hydro-carbonated essence, or whether the essence of turpentine, by surrounding the molecules of phosphorus and opposing combustion, does not at the same time suppress the production of ozone, cause rapid waste of the blood-globules, and of all the organic tissues, to which Gubler attributes the physiological excitement arising from small doses of phosphorus as well as the more serious results from large quantities.—*Bulletin Général de Thérapeutique Méd. et Chir.*, Tome lxxxiv, 10e livraison, 30th May, 1873.

II.—Forensic Medicine.

Relation of Uræmic and Alcoholic Poisoning to Testamentary Capacity.—Dr. Stephen Rogers relates the following history bearing upon the above-named subject. Near the close of the year 1869, E. T. C— was quietly, if not clandestinely, married to a man whom report says, her education, religious and secular, would, in the ordinary course of events, have led her to shun rather than to love.

A key to the secret of this untoward occurrence is furnished by accumulated evidence, to the effect that she was an incorrigible and hopeless inebriate. Though trained and fairly educated in one of the best schools for girls, this vice of drunkenness seized her soon after her return home from school, and as a consequence she soon became the cause of constant and harassing care to her friends, and

her habits were the bane of the last days of her aged and widowed mother, who died about the date above given, leaving this only child sole heir to a considerable property. About the time of the mother's death this rich and dissolute girl was induced to marry, under circumstances already stated. Rumour attributes the marriage to a necessity arising from antecedent illicit familiarity between the parties, favoured by the intemperate habits of the girl. However this may be, there is no question that they were married, and the following unusual history commenced. Not long after her marriage it appears in evidence she became the victim of attacks which, from the general description given of them by various witnesses, would appear to have been of an hysterical character, more or less modified by her intemperate habits, for these she continued to practise after her marriage even with less restraint than before.

During these attacks she screamed, talked incoherently, and threw herself about so violently as to require control, and she is said to have recovered from them after a few minutes, or at most a few hours, with no perceptible impairment of her intelligence. It also became apparent, not long after her marriage, that she was pregnant, to which fact we may justly attribute a portion of the general and violent perturbation which she at this time suffered. This perturbation was manifested, as before stated, in an especial manner by the attacks, or spells, or fits, or paroxysms, as they are variously named by the witnesses, whose principal features I have already mentioned, and except these no alarming symptoms occurred to her until about eight months after marriage, though the evidence is conclusive that she had before this date suffered many of the afore-mentioned attacks and had more than once been on the verge of true delirium tremens. Either from the fear that some of the paroxysms might terminate fatally, or that her uncontrolled intemperance would induce disease that would suddenly terminate her life, or from some other fear or motive, her husband and her father-in-law, who were associated in business, went, unknown to her, some months after the marriage, to an attorney, and requested him to draw up a will for her, at the same time giving him instructions as to its provisions. The will was accordingly prepared, and in such terms as to be almost exclusively in the interest of the two parties who had ordered it drawn. They took it, carried it home and laid it away in a trunk, as the evidence shows. There is no evidence furnished to prove that the alleged testatrix ever read or heard this paper read, except the declarations of the parties who caused the will to be drawn. It is from them alone that we hear that she even knew of the existence of this paper until almost the hour of her death. Her husband, who, by its terms, was sole heir to her estate, alone says she approved it after reading it, and still months passed and it was allowed to lay away in his trunk, unexecuted and unwitnessed. Thus stood the affairs of this case when, on a Monday, this alleged testatrix exhibited signs of the approach of one of her accustomed ill turns, which, as already said, the evidence all goes to prove were the result of alcoholic excesses on a nervous system rendered more susceptible by the pregnant state.

On the following day, Tuesday, she was unusually ill, suffered a number of the paroxysms mentioned, and drank a considerably large amount of alcoholic liquor. She was still worse on the following day, Wednesday, but apparently not too ill in her husband's opinion, to render him incompetent to prescribe for her, though a physician had visited her both Tuesday and Wednesday. On Thursday morning she was so much worse that a second physician was called, who reached her about noon, and found her so seriously ill as to give him great solicitude. He found her suffering nausea and frequent vomiting, insatiable thirst, and a sense of burning at the stomach, great restlessness, getting out of and into bed, muttering and often noisy delirium, with hallucinations of sight and hearing, though she would obey orders and reply to simple questions, apparently rationally. Even up to a few hours before her death, which took place some eighteen hours after the above visit, when spoken to she would respond, and, so far as could be seen, rationally, but in a moment she would again relapse into wandering delirium. It also appears that, up to within a short time before her death, she was able to and did rise from her bed and walk about the room, a tendency her physicians say they tried to restrain, knowing that such efforts, in her exhausted condition, might suddenly destroy her life. There is no evidence that she suffered any paroxysm to which the name of convulsion could be properly applied during the last two or more days of her life. A consultation, attended by three physicians, was had at eight o'clock on Thursday evening, and resulted in the conclusion that, among the complications of the case, there was *uræmia*, and death soon afterwards took place. Against the theory of uræmia as a cause of death, Dr. Rogers raises many objections; he urges that the post-mortem examination in no way supports the conclusions drawn or that the symptoms preceding death warrant them. He infers from the previous hysterical attacks that the lady had suffered from the excessive use of alcohol, and that these attacks had been treated by laudanum. He urges thereupon that this alleged testatrix had no conscious appreciation of what she did or said for at least twenty and probably seventy hours before death. The court before whom the case was heard, nevertheless decided that the decedent at the time she signed the paper was of a sound and disposing mind.—*New York Medical Journal*, April, 1873.

On Muscular Irritability after Death.—We have ourselves recently recorded some experimental facts and experiences bearing on forensic inquiries, in relation to muscular irritability after death. Our researches have had reference to the action of cold, of the motor forces, of the supply and the abstraction of blood, and of the influence of chemical agencies. We will condense here what relates to the action of cold and of certain chemical agents.

Previous to the time of John Hunter it was supposed that cold was the most effective agent for destroying muscular irritability, and to this day the same impresssion is commonly maintained, so that the sensation of cold in a motionless animal is accepted as the surest evidence of death. Hunter was the first to show by direct experi-

ment that this was an error, and that cold suspends irritability without destroying it.

In our experiments on the muscular irritability of fish, reptiles, and frogs, we have shown that cold can be made to suspend without destroying the muscular irritability for a long period of time, and that in fish (carp, on which we have made the greatest number of experiments) the restoration of irritability could be perfected to the extent of the restoration of the living function.

Passing to warm-blooded animals, we find that, in the process of cooling, in every animal that has been suddenly deprived of life without mechanical injury there is a period in the process of cooling when general muscular irritability may be made manifest. We demonstrate this fact by the simple experiment of throwing a current of water heated to 120° Fahr. over the arterial system of the recently dead animal. If the surrounding temperature be high at the time of this experiment the operation should be performed within a few minutes after death, but if the temperature be below freezing-point it may be delayed for a long period. In one experiment we reproduced active muscular contraction in an animal that had been dead and exposed to cold, six degrees below freezing-point, for a period of three hours. In this case the muscles generally remained irritable for seven minutes after the injection of the heated water, while in the muscles of the limbs, by repeating the injection at intervals, the irritability was maintained for two hours.

We are here led to a comparison between these experimental results and the phenomena of muscular irritability that have been observed in the human subject after death by cholera. These movements are not conscious nor are they promoted by electrical excitation; but the flexors and extensors belonging to each part in which there is movement are alternately contracted and relaxed as if from some internal influence. The same observations apply to the phenomena of contraction and relaxation in the muscles of animals that have been held in abeyance by cold, and have been recalled into action by the injection of heated fluids.

The influence of cold in suspending without destroying muscular irritability is further evidenced by the experiment of subjecting some young animals to death by the process of drowning them in ice-cold water. We have shown that in the kitten the muscular irritability may be restored to the complete re-establishment of life after a period of two hours of apparent systemic death, and although the muscles when the animal is first removed from the water give no response to the galvanic current.

This same continuance of irritability after apparent systemic death by drowning in ice-cold water has been observed in the human subject, not in so determinate, but in an approximated degree. Dr. Belgrave has supplied us with an example in which a youth who had been deeply immersed for twelve minutes in ice-cold water retained muscular irritability so perfectly that he recovered, regained consciousness, and lived for a period of seven hours.

In the process of restoration of muscular power we find that a certain period of time is required before the irritability is raised

from a mere passive condition, in which it responds only to external stimuli, into the condition necessary for independent active contractility. The change of condition from the passive to the active, when it does occur, is so sudden as to seem instantaneous at first, then it is slowly repeated. This rule holds good in respect to voluntary muscles and involuntary. It is specially true in regard to the heart, which organ may perform its office under two distinct degrees of tension or pressure. A low tension, in which the organ itself is reduced in size, and moves almost insensibly, and a full tension in which it is of larger size and moves with a sufficient power to impel the blood so as to overcome the arterial elasticity and the capillary resistance.

Another fact bearing on this subject is, that in rapid decline of muscular irritability the muscles most concerned in the support of the organic functions, namely, the heart and the muscles of respiration, are the last to yield up their spontaneous power; but that when they have lost their power they are the last to regain it. To this rule there is one exception, viz. in the muscular fibres of the right auricle of the heart.

The degree of cold required to retain the irritability is fixed within certain measures of degree; from 38° to 28° Fahr. being the most favorable degrees of exposure. Above a temperature of 38° Fahr. the muscles in a little time pass into permanent rigidity, *rigor mortis*. Below 28° the muscles, if the effect of the cold be extended to their whole structure, pass into some new molecular condition from which they do not return into active life; at least they do not by means of any process of recovery of which we are at present conversant.

In respect to organic chemical agents, we have found in experiments with nitrite of amyl on cold-blooded animals (frogs) that the muscular irritability may be suspended for a period of nine days, and may then be restored to the extent even of restoration of life. In one instance this restoration took place after the commencement of decomposition in the web of the foot of the animal. In warm-blooded animals a series of supensions have been effected by nitrites and by cyanogens, not for so long a period, but for periods of hours, in one instance extending to ten hours.

In the whole series of our inquiries no fact has impressed us more forcibly than this, that the muscular irritability, in so far as it belongs to the muscle, may be sustained for hours after the nervous excitation which calls it into spontaneous action has ceased. Hence we infer that after death the nervous matter undergoes a change of condition which *in result* is identical with that change in muscle which is called rigor. There is evidence, moreover, from some rare cases, that the final inertia of nervous matter may be suspended and revived so that all the muscles may be reanimated. This point is well elucidated by certain phenomena that have recently been observed by Mr. Wadsdale Watson, of Newport, Monmouthshire, on a double monster. In this instance two children were born so attached that the separation of them was impossible. Both lived

equally for three hours after birth, and then one died, and remained dead three hours, while the other lived. At the end of the time named the dead child recommenced to breathe, and showed other signs of restored muscular power; then it sank into a second death, but at intervals of about four hours moved again. At length, twenty-three hours after its first apparent death, during a fit of crying of the living child, it recovered sufficient power to move, and even to cry, and manifested evidence of life in all its muscles, except the heart, for twenty minutes, when it had a severe convulsion which closed all further motion.

In this instance, we believe, the retention of muscular irritablity depended upon the retention in the nervous organism of the conditions necessary for motion.—*Croonian Lecture, delivered before the Royal Society* on May 29th, 1873.

III.—Hygiene.

The Sanitary Influence of Light.—Dr. William A. Hammond contributes a most valuable paper on the influence of light on health, in which he corrects, as we believe, with perfect reason, the common and false practice of shutting up the sick in chambers from which light is excluded. Referring to the experiments of Edwards, who showed that tadpoles, when deprived of light, did not develop into frogs, Hammond states that in repeating these experiments he has been led to the same results. On one occasion he prevented, for 125 days, the development of a tadpole by confining it in in a vessel to which the rays of light had no access. On placing it in a receptacle open to the light the process of transformation was at once commenced, and was completed in fifteen days.

Upon another occasion he took two kittens of the same litter, and when they were twenty days old weighed them accurately. One weighed eighteen ounces and the other eighteen ounces and a half. The first was placed in a box to which the light of the sun had free access, the second in a similar box to the interior of which no light could reach. Both were fed alike, and in all other respects, save as regarded the light, were kept in the same condition. At the end of the first five days number one weighed twenty-two and a half ounces, while number two weighed but twenty and three quarter ounces; at the end of a second period of five days number one had attained a weight of twenty-four ounces, while number two scarcely weighed twenty-two ounces. The two animals were now placed together in the box which was exposed to the light, and at the end of the third period of five days each weighed within a fraction of twenty-five ounces.

A lady now under Dr. Hammond's charge is subject to severe neuralgic headaches, which are promptly relieved when she sits in the direct rays of the sun, and are as promptly reinduced should she retire into a closet, or even a dark corner of the room. Another can always cut short an attack of sciatica by allowing the light of the sun to fall directly on the affected limb.

The effects of deficient light upon the inmates of hospital wards

and sick chambers, have frequently come under Dr. Hammond's personal observation. Most physicians know how carefully the attendants upon the sick endeavour to exclude every ray of light from the apartment, and it must be admitted that the members of the profession are often fully as assiduous in this respect. That the practice, except in some cases of actual disorder of the brain and other parts of the nervous system, is pernicious, Dr. Hammond is well satisfied. During the early years of the late war he visited the camp and hospital of a regiment stationed in West Virginia. Reports had reached General Rosecranz's head-quarters that the sickness and mortality were something frightful, and Dr. Hammond was ordered to examine minutely into all the circumstances connected with the situation of the camp, the food of the men, &c. Among other things, he found the sick crowded into a small room from which the light was carefully excluded by blinds of india-rubber cloth. They were as effectually bleached as is celery by the earth being heaped up around it. Pale, bloodless, ghost-like-looking forms, they seemed to be scarcely mortal. Convalescence, under such circumstances, was almost impossible, and doubtless many had died who, had they been subjected to the operation of the simplest laws of nature, would have recovered.

The condition known as etiolation is mainly the result of insufficient light, and is similar in general features to the state induced by deprivation of light in vegetables. The blood becomes thin, the fibrin, the albumen, and the red corpuscles are diminished in quantity, while the water is increased. The face is discoloured, and, losing its red hue, acquires a tint analogous to that of yellow wax. The veins of the skin are no longer to be perceived even in those parts where they are naturally largest and most numerous. The pulse is very frequent, beating at the rate of from 90 to 100 per minute, without increasing the heat of the skin, and always being small and weak. Palpitation of the heart occurs in paroxysms, and the least muscular exertion renders the action of the heart still more frequent. The prostration of the forces of life is extreme, and it is distressing to see the miserable beings thus affected scarcely capable of sustaining their lean and prematurely decrepit bodies. They are extremely subject to dropsy, petechiæ, and passive hæmorrhages, and when attacked by any disease, however slight, show little recuperative power.

The most frequent subjects of this condition are miners, whose occupation shuts them off to a great extent from the full influence of the sun's rays, and the denizens of our crowded courts and cellars. There are many of these latter places in this city to which the direct light of the sun never reaches, and many of the inhabitants of which never see a ray of sunlight during their infancy and early childhood. A very cursory inspection of these children reveals the fact that they are weak and puny, and the frequent subjects of deformities. Indeed, there is no doubt that the scrofulous diathesis is often induced by the mere deprivation of light.

Many of our schoolhouses are so situated that the direct rays of

the sun never enter some of the rooms. Into these apartments children are often excessively crowded, and as, generally, there is little or no attention given to proper heating or ventilation, it is not a singular circumstance to find the pupils suffering, to a greater or lesser extent, from etiolation.

As has already been intimated, the management of the light in the sick chamber is rarely the subject of intelligent and scientific action. In anæmia, chlorosis, phthisis, and in general all diseases characterised by deficiency of vital power, light should not be debarred. In convalescence from almost all diseases it acts, unless too intense or too long continued, as a most healthful stimulant, both to the mental and physical systems. The evil effects of keeping such patients in obscurity are frequently very decidedly shown, and cannot be too carefully guarded against by physicians. The delirium and weakness which are by no means seldom met with in convalescents kept in darkness, disappear like magic when the rays of the sun are allowed to enter the chamber. Dr. Hammond thinks he has noticed that wounds heal with greater rapidity when the solar rays are occasionally allowed to reach them, and when they are as far as possible exposed to diffused daylight, than when they are kept continually covered. A similar statement has been made by other authorities. Epileptic attacks have been known to be excited by darkness, and Laennec has frequently seen paroxysms of dyspnœa measurably abate as the light of day began to appear. Asthma, as is well known, is more severe in its manifestations at night than during the day.

In this country it is rarely the case that disease or injury is induced by excessive light. Occasionally, however, we meet with eye affections due to excessive light, either coming directly from the sun or reflected from water, snow, or sand, or resulting from the intense light of a flash of electricity passing near the individual. Bright artificial light may also cause derangement of the visual organs. A child of Dr. Hammond's acquaintance was rendered permanently amaurotic by looking intently at a bright object while her photograph was being taken.

The practical application of these remarks is this, that care should be taken, both in health and disease, to ensure a sufficient amount of light to the inmates of houses, and that it is impossible to rear well-formed, strong, and robust children unless attention is paid to this requirement. Sun baths, or apartments in which the solar rays can fall upon the naked body, are doubtless highly advantageous to health, and rooms for this purpose could probably easily be constructed in or on most of our city houses. At present a chief object of city families seems to be to devise means for keeping the sunlight out of their houses. That this is contrary to nature needs no argument. The world is said to be under-fed; it is certainly under-lit as we manage it. Let us, then, to use the dying words of Humboldt, have "Mehr Licht."—*The Sanitarian*, May, 1873.

Influence of Vaccination on the Mortality of Smallpox.—The following abstract of a paper by Dr. Grieve, late Medical Superintendent of Hampstead Smallpox Hospital, was arranged for publica-

tion in our last report, but was withheld from want of space. We insert it this time because of its sterling value as a chapter of statistical evidence on a subject now more than ever demanding full elucidation. Of 6221 patients admitted, 1248 were without marks of vaccination, and of these 638, or 51·12 per cent., died; whilst among the 4973 who showed proofs of being vaccinated, in only 567 instances did the disease prove fatal, giving a percentage of mortality of 11·40. From these numbers it is seen that, although the number of patients received into the hospital of the vaccinated class exceeded the number in the unvaccinated, a fact of which the Anti-vaccination League has made vigorous use, the larger number of deaths occurred amongst the unvaccinated.

The general percentage of mortality is 19·36, which is above the average of late epidemics. This has been ascribed by Mr. Marson, who has noticed the same circumstance at the Smallpox Hospital, and whose lengthened experience entitles him to speak on this point with authority, not only to the form of the disease generally being more severe, but also to the large proportion of cases of the malignant and hæmorrhagic type which have come under treatment. This great prevalence of hæmorrhagic smallpox is characteristic of the present outbreak, and seems to depend upon a peculiar development of the epidemic influence.

It is possible for individuals to be vaccinated, and yet not to obtain all the protection they might do, as the vaccination may be inefficiently performed. It may be inefficient either in quality or quantity. As to quality, where there are different observers with possibly different standards, it is very difficult to reduce the results to figures.

With quantity we have not the same difficulty; there are the marks, and they have but to be looked for and counted.

In the 3555 cases in which the number of marks was noted it was found that the percentage of mortality in those showing one mark is 17·39, showing two 12·17, showing three 10·58, showing four 8·38, showing five and more 6·43—a scale in which the mortality is in inverse ratio to the number of marks seen. The practical deduction to be made from these numbers is that the larger number of places in which we vaccinate the more protection is given. To obtain even a fair average of protection at least three marks are required, but something is gained by exceeding that number.

An interesting study in connection with the influence of cow-pox over smallpox is to watch the diseases when concurrent. A considerable number of children have come as patients into the hospital suffering from both vaccinia and variola, and the author's experience is, that if the cow-pock reach the eighth day before the smallpox eruption makes its appearance the latter disease is modified. As the result of the investigations he has been able to make into the question of the period of incubation of smallpox, he has come to the conclusion that, in the great majority of cases, the smallpox eruption shows itself on the fourteenth day after the reception into the system of the variolous influence; vaccination during the first three or four days of the period of incubation will accordingly be useful—a strong

reason for vaccinating persons in a house where smallpox has already shown itself.

Of smallpox after revaccination Dr. Grieve has not seen much, owing to the rarity of its occurrence. Out of the 6221 cases above mentioned, in only three could any satisfactory proof of previous revaccination be discovered. A good many of the patients said, on their admission, that they had been revaccinated; but, on pressing the inquiries, it was found that while the operation had been performed no after effects were produced, and that thereupon the doctor had assured them that as they were not susceptible to the vaccination there was no fear of their taking small-pox. Their presence in the hospital was sufficient proof of the fallibility of this doctrine, one which is inculcated even yet by many members of the profession.

The nurses and servants in constant and close attendance on smallpox, when protected by revaccination, do not take the disease; and in this respect the experience at the Hampstead Hospital coincides with that of the older institution at Highgate. Dr. Grieve wishes it were possible to bring home to the minds and belief of the general public his conviction regarding revaccination, namely, *that it is a sure protection against smallpox.* To ensure this protection, revaccination producing some local effect must have been performed after the individual had reached fifteen years of age. Cases of variola subsequent to revaccination are merely the exceptions that prove the rule; they are more uncommon than second smallpox, and differ also in this way, that whereas the latter are frequently severe and sometimes fatal, the former are very mild indeed.—*Essay read before the Epidemiological Society*, 1872.

BOOKS, PAMPHLETS, &c., RECEIVED FOR REVIEW.

Manual of Chemical Analysis, as applied to the Examination of Medicinal Chemicals: a Guide for the Determination of their Identity and Quality. By Frederick Hoffmann, Ph.D. New York, Appleton and Co. 1873. pp. 393.

Manual for Medical Officers of Health. By Edward Smith, M.D., F.R.S., &c. London, Knight and Co. 1873. pp. 346.

The Liverpool and Manchester Medical and Surgical Reports. Manchester, Cornish. 1873. pp. 216.

Mechanism of the Ossicles of the Ear and Membrana Tympani. By H. Helmholtz. Translated by A. H. Buck and N. Smith. New York. 1873. pp. 69.

Report on Quarantine on the Southern and Gulf Coasts of the United States. By H. E. Brown, Assistant-Surgeon, United States Army. New York, Wood and Co. 1873. pp. 117.

St. Thomas's Hospital Reports. New Series. Edited by Dr. Bristowe, Dr. Stone, and Mr. Croft. Vol. III. London, Churchills. 1873. pp. 381.

Guy's Hospital Reports. Edited by C. Hilton Fagge, M.D., and Arthur E. Durham. Third Series. Vol. XVIII. London, Churchills. 1873. pp. 498.

Report on Leprosy and Yaws in the West Indies, addressed to Her Majesty's Secretary of State for the Colonies. By Gavin Milroy, M.D., &c. 1873.

Family Thermometry: a Manual of Thermometry for Mothers, Nurses, &c. By Edward Seguin, M.D. New York, Putnam and Sons. 1873. pp. 72.

Outlines of Physiological Chemistry. By C. H. Ralfe, M.B., &c. London, Lewis. 1873. pp. 236.

The Causes and Treatment of Deafness; being a Manual of Aural Surgery for Students and Practitioners. By James Keene, F.R.C.S.E., &c. London, Hardwicke. 1873. pp. 180.

A Treatise on Apoplexy, Cerebral Hæmorrhage, &c. By John A. Lidell, M.D., &c. New York, Wood and Co. London, Sampson Low and Co. 1873. pp. 395.

Manual of Human and Comparative Histology. Edited by S. Stricker. Vol. III. Translated by Henry Power, M.B. Sydenham Society. 1873.

Principles of Animal Mechanics. By the Rev. S. Haughton, F.R.S., &c. London, Longmans. 1873. pp. 495.

The Preventive Treatment of Calculous Disease and the Use of Solvent Remedies. By Sir Henry Thompson, F.R.C.S., &c. London, Churchills. 1873. pp. 72.

Medical Hints for the Districts and Companion to the District Medicine Chest. By G. H. Hunter, Surgeon, Bombay Army. Second Edition. Bombay. 1872. pp. 156.

Notes on the Pharmacopœial Preparations (B. P. 1867), especially arranged for the Use of Students. By W. H. Griffiths, Ph.D., &c. London, Baillière and Co. 1873. pp. 110.

A Manual of Dental Mechanics. By Oakley Coles. With 140 illustrations. London, Churchills. 1873. pp. 283.

An Introduction to the Study of Clinical Medicine; being a Guide to the Investigation of Disease, for the Use of Students. By O. Sturges, M.D., &c. London, Smith, Elder, and Co. 1873. pp. 131.

Health in India: Medical Hints as to Who should go there, and how to Retain Health, &c. By G. J. Hunter, Surgeon, Bombay Army. Calcutta. 1873. pp. 98.

Experimental Researches on the Causes and Nature of Catarrhus Æstivus (Hay Fever or Hay Asthma). By Charles H. Blackley, M.R.C.S. London, Baillière and Co. 1873. pp. 202.

Notes of a Course of Lectures on Natural Philosophy, delivered at Guy's Hospital. By G. F. Rodwell, F.R.A.S., &c. London, Churchills. 1873. pp. 166.

Consumption and its Treatment in all its Forms. By Dr. Carl Both. Boston, A. Moore. London, Trübner and Co. 1873. pp. 157.

An Introduction to Pathology and Morbid Anatomy. By T. Henry Green, M.D., &c. Second Edition, revised, enlarged, and illustrated. London, H. Renshaw. 1873. pp. 366.

Fourth Report of the State Board of Health of Massachusetts, January, 1873. Boston. pp. 473.

Second Annual Report of the Board of Health of New York, 1871-72. New York. 1872. pp. 408.

The Royal London Ophthalmic Hospital Reports. Vol. VII, Part IV. London, Churchills. 1873. pp. 266.

A Manual of Pathological Histology, to serve as an Introduction to the Study of Morbid Anatomy. By Dr. E. Rindfleisch. Vol. II. With Index. Translated by E. Buchanan Baxter, M.D., &c., for New Sydenham Society. London. 1873. pp. 410.

The Physical Basis of Mental Life. A Popular Essay. By R. R. Noel. London, Longmans. 1873. pp. 74.

Digest of the English Census of 1871. By James Lewis. London, E. Stanford. 1873. pp. 204.

Lectures on Madness, in its Medical, Legal and Social Aspects. By Edgar Sheppard, M.D. London, Churchills. 1873. pp. 186.

A Practical Manual of the Diseases of Children. With a Formulary. By Edward Ellis, M.D. Second Edition. London, Churchills. 1873. pp. 340.

Tablets of Anatomy and Physiology. By Thomas Cooke, F.R.C.S. Two Parts. London, Longmans. 1873.

Memoir of Sir James Y. Simpkson, Bart., M.D., &c. By J. Duns, D.D., F.R.S.E. Edinburgh, Edmonston and Douglas. 1873. pp. 544.

The Medical and Surgical History of the War of the Rebellion, 1861-65. Prepared under the Direction of Surgeon-General Joseph K. Barnes, United States Army. Washington, Government Printing Office. Part I, Vol. I, Medical History; PartI, Vol. II, Surgical History. 4to.

Les Ambulances de la Presse pendant la Siège 1870-71. Paris, J. B. Baillière et fils. 1873. pp. 367.

Leçons Cliniques sur les Maladies des Femmes. Par T. Gallard, Médecin de l'Hôpital de la Pitié. Paris, J. B. Baillière et fils. 1873. pp. 792.

Principes d'Electrothérapie. Par le Dr. E. Cyon, Professeur de Physiologie à l'Université de St. Petersburg. Paris, J. B. Baillière et fils. 1873. pp. 277.

Revue des Sciences Médicales en France et a l'Etranger. Recueil Trimestriel Analytique, Critique et Bibliographique. Dirigé par G. Hayem. Tome I. No. 2. Paris, G. Masson. 1873. pp. 591.

Parallèle de l'Hystérie et des Maladies du Col de l'Utérus, &c. Par le Dr. Dechaux. Paris, J. B. Baillière et fils. 1873. pp. 444.

Etude sur l'Angiome simple sous-cutané circonscrit. Par le Dr. Charles Monod. Paris, Baillière et fils. 1873. pp. 86.

Etude sur les Fistules de l'Espace Pelvi-rectal supérieur, ou Fistules Pelvi-rectales supérieures. Par le Dr. S. Pozzi. Paris, Masson. 1873. pp. 69.

Du Traitement des Plaies en général et en particulier par le Coal-tar et le Carbon. Par le Dr. Louis Beau. Paris, J. B. Baillière et fils. 1873. pp. 131.

Traité théorique et pratique des Maladies de l'Oreille et des Organes de l'Audition. Par le Dr. Bonnafont. Deuxième édition. Paris, J. B. Baillière et fils. 1873. pp. 699.

Clinique Chirurgicale de l'Hôpital de la Charité. Par L. Gosselin. Tome II. Paris, J. B. Baillière et fils. 1873. pp. 738.

Die Ohrenheilkunde der letzten 50 Jahre. Von Dr. W. Kramer. Berlin, 1873. pp. 77.

Dei Solfiti ed Iposolfiti nella cura delle Febbri Intermittenti. Memoria del Dott. Giovanni Faralli. Milano. 1872. pp. 128.

Pamphlets.

Syllabus of Materia Medica for the Use of Teachers and Students. By A. Harvey, M.D., and A. D. Davidson, M.D. London, Lewis. 1873.

A Report on the Sanitary Condition of Leicester in 1872. By J. W. Crane, M.D., &c.

On the Constitutional Character and Treatment of the Diseases of Women connected with Chronic Inflammation of the Uterus. By T. More Madden, M.D., &c. Dublin. 1873.

Journal of the Scottish Meteorological Society. No. XXXVI.

Fifty-second Annual Report of Charing Cross Hospital. 1873.

On the Structure and Functions of the Rods of the Cochlea in Man and other Mammals. By Urban Pritchard, M.D. (Reprint.)

On the Relationship of Abscess of the Liver to Gastro-Intestinal Ulceration. By James Finlayson, M.D. (Reprint.)

The Induction of Sleep and Insensibility to Pain by the Self-Administration of Anæsthetics. By John M. Crombie, M.D. London. 1873.

Observations on the Surgical Treatment of Ingrowing Toe-Nail. By George Stilwell. London, Churchills. 1873.

On the Results of Thyrotomy for the Removal of Growths from the Larynx. By Morell Mackenzie, M.D. London, Churchills. 1873. (Reprint.)

Twenty-fourth and Twenty-ninth Reports relating to the Registry and Return of Births, Marriages, and Deaths in Massachusetts for the Years ending December 31, 1865, and December 30, 1870. Boston, 1872.

Reports of Births, Deaths, and Marriages, from the Registrar-General.

Reports of Asylums.—The Royal Edinburgh, Aberdeen, Cumberland and Westmoreland, Sussex, Nottingham, Richmond District (Dublin), Fife and Kinross, Halifax (Nova Scotia), Hants.

De l'Ingestion des Eaux Marécageuses comme cause de la Dysenterie et des Fièvres intermittents. Par L. Colin. Paris, J. B. Baillière et fils. 1872.

Studi sulla Difteria. Rapporto del Dott. G. Faralli. Milan. 1872.

Journals.

The Dublin Journal of Medical Science. April to June, 1873.

The Edinburgh Medical Journal. (Monthly.) April to June, 1873.

The Journal of Mental Science. April, 1873.

The London Medical Record. April and May, 1873.

The Medical Press and Circular, the Medical Times and Gazette, and the Lancet.

The Indian Medical Gazette; a Monthly Record. Calcutta. March, April, and May, 1873.

Canada Medical and Surgical Journal. Edited by G. E. Fenwick, M.D. March, April, and May, 1873. Montreal.

The American Journal of the Medical Sciences. April, 1873. (Quarterly.)

The Western Lancet. San Francisco. (Monthly.)

Charleston Medical Journal and Review. Edited by F. P. Porcher, M.D., and R. A. Kinloch, M.D. New Series. Vol. I, No. I, April, 1873. (Quarterly.)

New York Medical Journal. February to May, 1873.

Archives of Scientific and Practical Medicine. No. 2. February and March, 1873.

The Sanitarian: a Monthly Journal. A. N. Bell, M.D., Editor. Vol. I, Nos. I and II. New York. April and May, 1873.

Archives of Ophthalmology and Otology. Edited by Professors H. Knapp, M.D., and S. Moos, M.D. Vol. III, No. I. New York. 1873.

Schmidt's Jahrbücher der In- und Auslandischen Medicin. March and April, 1873.

Archiv für Pathologische Anatomie und Physiologie. Von R. Virchow. April, 1873.

Deutsches Archiv für Klinische Medicin. Band XI.

Monatsschrift für Ohrenheilkunde. March and April, 1873.

Allgemeine Wiener Medizinische Zeitung. March and April, 1873.

Archiv für Gynaekologie. Band v, Heft 1. Berlin. 1873.

Archives Générales de Médecine. March to June, 1873.

Gazette Hebdomadaire. March to June, 1873.

L'Union Médicale. April and May, 1873.

Le Mouvement Médicale. April and May, 1873.

Bulletin Générale de Thérapeutique. March and April, 1873.

Revue Photographique des Hôpitaux de Paris.

El Anfiteatro Anatomico Español. March and April, 1873.

O Correio Medico de Lisboa. March and April, 1873.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1873.

Analytical and Critical Reviews.

I.—Causes of Epidemics.[1]

THOSE who have paid attention to the physical etiology of diseases either always or sometimes epidemic may be classified according to the prominence which they assign to one or other of the following kinds of causes:

1. General factors, that is to say, the working, separately or combined, or insufficiently balanced, of earthly forces, more or

---

[1] 1. *Die Seuchen, ihre Ursachen, Geseze und Bekämpfung.* Von Dr. FR. OESTERLEN, Tübingen, 1873.

2. *Epidemiology; or the Remote Cause of Epidemic Disease in the Animal and Vegetable Creation.* By JOHN PARKIN, M.D., F.R.C.S. London, 1873.

3. *What is Malaria?* By C. F. OLDHAM, M.R.C.P.E., Assistant-Surgeon, H.M. Indian Forces. London, 1871.

4. *On Relapsing or Famine Fever.* By R. T. LYONS, Assistant-Surgeon, Bengal Army. London, 1872.

5. *Fever and Cholera from a New Point of View.* By ALEXANDER SMITH, M.D. Edin., Staff Surgeon-Major, British Forces in India, 1873.

6. *Disease Germs, their Supposed Nature.* By L. S. BEALE, M.B., F.R.S. London, 1870.

7. *Disease Germs, their Real Nature.* By L. S. BEALE, M.B., F.R.S. London, 1870.

8. *Remarks on the Prevalence and Distribution of Fever in Dublin.* By THOS. W. GRIMSHAW, M.D., Physician to Cork Street Fever Hospital, &c. Dublin, 1872.

9. *Ueber die Diagnose des Flecktyphus.* Von C. WUNDERLICH. Leipsig, 1871.

10. *On the Period of Incubation of Typhus, Relapsing Fever, and Enteric Fever.* By CHARLES MURCHISON, M.D., F.R.S., 'St. Thomas's Hospital Reports,' vol. ii.

11. *Fourth Annual Report of the State Board of Health of Massachusetts,* January, 1873.

12. *Ueber die Ætiologie des Typhus. Vorträge gehalten in den Sitzungen des ärztlichen Vereins zu München.* Von BUHL, FRIEDRICH, v. PETTENKOFER, &c. München, 1872.

less present, and varied by circumstances. Such are excesses of heat, cold, damp, decomposition of organic matter, famine, crowding.

2. The generation of a noxious substance or influence by an extraordinary conjunction of these general factors, which influence does not pass away when the conjunction ceases—a diffusible poison, indefinitely permanent or transitory.

3. Direct contagion, or communication from person to person.

4. Indirect contagion, or the poisoning, by the agency of an influence elaborated in the human body, of media presented by houses, soils, water, and the like, which thus become foci of disease.

As to the nature of the infecting noxious influence, the adherents of the last two classes may be divided into such as favour (1) the theory of ferments, (2) that of organic germs, and (3) the theory of bioplasm.

To each of these etiological explanations of the historical phenomena of epidemics there are weighty objections. These we will look at first, for to say the truth they are the first part of the subject that strikes the mind, and afterwards we will, in the necessarily short manner of a review, sketch out the arguments in their favour, and indicate the possible ways of adding to these arguments.

Against the view which attributes epidemics to general agencies may be urged the fact that, in spite of the more or less universal distribution and constant presence of these cosmical forces, epidemics break forth only at irregular intervals and in particular places. And again, that the diseases cease to spread, in spite of the persistence, nay, in spite of the increase, of the unfavorable conditions of the population which the prevalence of illness has given rise to. So that it would seem essential that there should exist for them some temporary cause over and above the permanent obnoxious conditions. Moreover, it is hard to conceive that the few injurious physical agents which we recognise should present such a variety of combinations as to generate the many forms of sickness that are occasionally epidemic. We can count on the fingers of one hand the elementary phenomena which produce on the human body symptoms at all resembling the symptoms of definite fevers; whereas these definite fevers, although they have got only thirty-three names amongst them in the nomenclature of the College of Physicians, yet, when they become epidemic, present varieties and apparent combinations which terribly puzzle the collectors of synonyms.

If it be urged that common factors also give rise to various

morbid processes, that, for example, too long-continued cold may give to one man rheumatism, to another chronic bronchitis, to another Bright's disease, it may be answered that the variation in the effect depends upon the diathesis of the patient, whereas the diathesis is of small moment as to the risk from epidemics.

It is true, indeed, there may be cosmical forces of whose movements man's senses are imperfectly cognizant, even with the aid of science—such as terrestrial magnetism, for example; but the fact of their being imperceptible is an evidence that their direct physiological effect on the human body is slight, however great their meteorological significance. Excess of heat, of cold, starvation, and overcrowding, have been known as morbific agents from the earliest times, and it is unlikely that any equally general and equally powerful should have been overlooked. The only way in which they could escape notice would be by their being rare, intermittent, special forces, whose mere presence, not their excess, should cause disease—in fact, by their being morbid poisons.

It may be observed that one or other of these common factors is often pointed out, by both historians and medical practitioners too, as the prominent cause of most various pandemics. There seems very little in common between the Athenian plague which Thucydides (quoting the punning proverb λοιμος ἐπι λιμῶ), with great justice, traces to famine, the "relapsing fever" of Dublin, the "three days' fever" of Cavell, the "jungle fever" of Eyre and others, the "epidemic ague" of Eteson in 1869, the "yellow fever" of Bryden in 1870, the "red fever" of Chevers in 1871, the "*febris e fame*" of Udoy Chund Dhut, the "dengue" or "*scarlatina rheumatica;*" yet all these, with two-and-twenty other epidemics, Mr. Lyons attributes to want of food.[1] The mind requires something more than varieties of degree of famine to account for the peculiar characteristics by which several of these epidemics are distinguished. And, considering that exposure to the sudden impression of cold in ordinary places is apt to cause rarely anything more than catarrh, or pneumonia, or rheumatism, and those only in persons predisposed to each of those complaints, it seems not sufficient to attribute to abstraction of heat the distinctive dysentery, as well as the intermittent and the remittent fevers, which, in malarious districts, attacks whole regiments at once. It is difficult to agree with Oldham that malaria is only "chill."[2]

Again, the known products of the decomposition of organic matter are few, much fewer and simpler than the organic

[1] 'On Relapsing or Famine Fever," section ii.
[2] 'What is Malaria?' p. 185.

matters, and growing continually fewer and simpler as they are disorganized into their elements. And the quantitative proportions they bear to one another are pretty constant in all places and climates, however copious or scanty the whole resultants of decay may be. So that one cannot imagine how "the vapours of the retributive poison" which Mr. Simon forcibly tells us "steam up incessantly, though unseen, from the polluted bosom of the river,"[1] should alone, and without the addition of specific agencies, produce such a variety of epidemics.

The phenomena of latency, also, and the cyclical course run by fevers, is very different from what follows on the action of excess of common factors. If a sewer-man inhales more sulphuretted hydrogen than is good for him he becomes comatose forthwith; carried off straight to the hospital, he either dies or begins to revive, and at the physician's visit next day very seldom requires any prescription. So is it with excesses of temperature and their effects on the animal frame. Put a patient in a cold bath, and he begins immediately to cool down; take him out, and soon his bodily heat returns. But if you found him to retain his previous warmth during and after the bath, and, twelve days being past, then first to grow colder and colder, till after a definite period he began to warm up, irrespective of all treatment, you would suspect some other agency than the bath to be at work.

The immediate action of common factors is shown first and most prominently on the weakly, sickly, least perfect members of the community, and if the action is not powerful, it is shown on them alone. But that is not the case with the diseases that are liable to assume an epidemic character. Very often the robust and well-to-do, in spite of being in a minority, are the first to suffer, and when they are attacked they run the most danger. It can be predicted with considerable certainty what persons are most likely to have their lives shortened by the ordinary cosmic influences, and the profits of life insurance offices depend on the exercise of such prediction; but no medical foresight can guard the funds from losses by cholera and typhoid, and other epidemics, against which there is no protection.

The foregoing and the like considerations have led to the hypothesis that, from extraordinary combinations of common factors, a new matter, or at least a new influence, is generated, which survives its parents for an indefinite period, and is the immediate cause of the disease. If the common factors are powerful and generate it in large quantities, its consequences are a broadcast epidemic; if they be weak and in small quanti-

[1] 'Fifth Report to the Privy Council,' p. 23.

ties, sporadic cases only occur. The varieties of circumstances and places, of organic and inorganic constituents, will sufficiently account for the variety of epidemics, for the disappearance of old and the birth of new diseases. This is the hypothesis of Morbid Poisons, and the worst that can be said of it is that it *is* an hypothesis.

Where the laboratory of these morbid poisons is situated has somewhat divided speculators, and, indeed, is of prime importance. Sydenham and others have followed the middle ages in fancying its birthplace deep down in the interior bowels of the globe (*interiora terræ viscera*[1]). The objection to this is that nothing abnormal of a mineral nature can be found distributed during epidemics, while we can hardly imagine anything mineral escaping detection. Moreover, places and seasons in which the bowels of the earth are most distinctly opened are not peculiarly the nurseries of this class of diseases. Volcanic eruptions and earthquakes were never so accurately watched as now, and we have never heard so little importance attached to their noxious vapours as in recent reports. Comets' tails, the aurora borealis, and other meteors, have in all ages been looked upon with suspicion if they happen to accompany disease, but, as Coleridge said of ghosts, we have seen them too often to believe in them. So that of late speculators have rather inclined to the ideas of the Greeks,[2] and sought for the workshop of Febris in the more superficial strata of our planet, among substances which have not yet resolved themselves into their inorganic elements. The main difficulty in the acceptance of this theory for all epidemics lies in the history of the diffusion of the poison. If it be spread by the air, how is it that the diseases travel sometimes against the wind? If by the water, how do they mount up-stream to a higher level? How do they spread from the coast to the interior?

Add to this the confession that the researches of chemists and microscopists have hitherto failed to detect the presence of any peculiar noxious substance during epidemics. Yet the number of individuals attacked would lead to the supposition of its existence in enormous quantity.

These difficulties lead to the pretty general adoption of the doctrines of the "Contagionists." They believe that the most important element in the spread of epidemics is the living body

---

[1] 'Obs. Med.,' 1, 2, 4, v. 4, 16; 'Tractatus,' I, § 35; and Schedula Monit., § 3.

[2] Notably Hippocrates in his 'Airs, Waters, and Soils,' and Empedocles, the Dorian engineer, who cured Selinunte, near Palermo, of its epidemics by turning a running stream through the neighbouring marshes. An earthquake, by destroying the trunk-drainage, has again made the place uninhabitable.

itself. Here the poison is multiplied, and incorporation with the body is, if not essential, at least the most usual mode of the poison's increase. Indirect contagion is by some called infection, to avoid using a word whose derivation implies "touching" (contingere) for cases in which the contact is not immediate. We are not careful about the matter ourselves, for the practical point of distinction between the third and fourth classes of epidemiologists which have been named lies not in whether they think the poison is conveyed from person to person in the shape of dust, of liquid, of solid, of an anonymous force, by touch of skin, by stray waft of epithelium, or by all of these modes and others too, as in whether multiplication of it or its completion goes on outside the body, or whether it does not.

The feature in the history of the communication of disease which has led to the idea of the elaboration of the communicating medium outside the body is the marked resistance in some cases to what a *primâ facie* judgment would pronounce a very powerful contagion. All know the story, versified by Darwin with more real feeling than generally characterises the Botanic Garden,[1] of the young Dutch lady, who, when attacked by carbuncular plague, is turned adrift into a garden, where her lover seeks her out, tends her, "and clasps the bright infection in his arms." She "braves the chill night, caressing and caressed, and folds her hero-lover to her breast." Yet he remained uninjured, and of course legally married his patient. Similar tales are told, minus the poetical incidents, of every epidemic. Now, Thiersch, Pettenkofer, and others, not quite content with the explanation that "Love round their couch effused his rosy breath, and with his keener arrows conquered Death," have suggested that the poison, as it leaves the body of the patient, is in (what may be called) a larval or imperfect state, and that it multiplies and becomes virulent only after a certain time of separation. Thiersch convinced himself of this by poisoning mice with cholera secretions. When fresh they were innocuous, but caused morbid symptoms when they had been kept a certain time, and again lost their virulence when decomposed. And Pettenkofer thinks he can, in the medical annals of British India, trace that period of innocuousness in choleraic evacuations till such time as they get mixed with the surface water of the locality.[2]

[1] 'Canto iv, l. 87. The incident took place in 1636, and is related by Vinc. Fabricius, in 'Misc. Cur. Ann. II, Observ.,' 188.

[2] That is to say, to Pettenkofer is due a rationale of the connexion between the seeming innocuousness of recent contact and the virulence of indirect communication in certain cases. The importance of subsoil water in the etiology of epidemics was long ago (in 1744) remarked by Sir John Pringle, who says that, in Dutch Brabant, it was possible to form a judgment of the healthiness of a village by looking into the wells, and that in proportion to the distance of the

The main objections to the hypothesis of contagion are—

1. The difficulty in obtaining positive evidence of it.

2. The fact that at the beginning of epidemics so many are attacked at once.

3. The suddenness of the attacks, and sometimes of the fatal event, seems opposed to the idea of multiplication, which requires time.

4. It has been argued that if poisons multiplied in the body at the expense of its substance, the process must needs go on as long as life lasts, or as long as any of the substance remains to afford a pabulum. (Parkin, p. 41.)

5. The fact that there are many who are apparently exposed to the supposed contagion yet escape, at the same time that others who avoid contact are attacked.

6. Bad consequences of social and financial character have ensued on a belief in contagion.

7. The measures founded on the belief have been often unsuccessful.

8. It is illogical to assume the existence of that which cannot be made evident to the senses.

Stimulated especially by this last objection, which points out a defect in the evidence, very glaring, and yet capable of being supplied by human labour, the chemists and microscopists have separately and jointly exerted themselves with vigour in this matter. The directions in which they have speculated may be called (1) the Ferment theory, (2) the Organic Germ theory.

That in the class of cyclical diseases molecular changes take place, analogous to those which ensue on the introduction of yeast to sugar, is, indeed, at first sight, a justifiable inference from several of the phenomena. In healthy living bodies there is no decay or decomposition, in the ordinary sense of those words, among the solid substances of the organs, in the molecules of the tissues, or of the blood, spite of the presence of ferments along with moisture and warmth. The excrements in the digestive canal alone show signs of putrefaction. Yet is this living substance highly susceptible of chemical change by reason of the exceeding instability of its constituents. And if it be exposed to highly destructive influences, such as the introduction of foreign substances (call them poisons or ferments) —if by such means its innervation and vital powers of resistance be lowered—then its constituents may be expected to enter on a new series of molecular chemical changes. Moreover, in the class of diseases spoken of there is shown an intensity and quickness of oxidation of tissues, a proneness to resolution and

water from the surface, so were the people more or less subject to fevers. ('Diseases of the Army,' p. 2.) A useful hint for military surgeons.

destruction, very akin to what we call decomposition. Furthermore, the rise in temperature in fevers cannot but suggest to a chemical mind an increase of chemical inorganic action; and this idea is strengthened by seeing the new morbid compounds which are formed become rotten more hastily than in health—*e. g.* the stools are abnormally fetid and changeable in colour, the urine soon becomes ammoniacal, the minute anatomical forms in tissues and secretions melt down and swarm with new movement, in the shape of bacteria, &c., with extreme rapidity. All this is so highly suggestive of what is seen in dead organic matter, in consequence of the introduction of other organic matter in a state of molecular change, *i. e.* of a "ferment," that one cannot but suspect the presence of a "ferment" also as the cause of the spread of spreading plagues.

The most weighty arguments against this "Zymotic" theory are—

1. That the analogy is too vague, and that there are too many gaps in the sketch left to be filled up by a staunch faith or an active fancy.

2. Nobody has found either the ferment itself or the materials whose decomposition is induced by the ferment.

3. There is no proof of increase of quantity in the infecting agent, since many poisons act in extremely small quantities.

4. In some "zymotic" diseases, as, for example, in ague, there is evidently no multiplication of the infecting agent in the patient's body.

5. The action on the blood of animal poisons whose action can be traced, as, for example, that from a dead body or putrilage, is not fermentative or self-multiplying, but simply chemical.

6. If there be really no fermentation, no practical good can come of provisionally supposing one (Griesinger, 'Infectionskrankheiten,' p. 2), for the hypothesis does not explain the phenomena of fever (Œsterlen, p. 51, &c,).

7. Even should it appear that a molecular change, somewhat analogous to fermentation, takes place in the body affected with a "zymotic" fever, still there is no evidence that such change is an essential part of the fever. It may have, like decomposing matter, a secondary promoting influence, without any specific action, that is to say, it may generally promote disease, but not one disease more than another (Œsterlen).

8. Living substance is so different from dead substance, physiological forces from chemical processes, that it cannot be expected that the laws of one should be explained by the laws of the other (Œsterlen, p. 53).

9. What, however, more than anything else, indisposes

people to rest upon the ferment theory is, that it contributes so little to our stock of ideas on the subject. If the word "fermentation" is to be made so wide as to take in all interstitial processes such as those described, it becomes a mere synonym for disease, if indeed it would not include health itself. It adds no more knowledge to call fever a "fermentation!" than to call the formation of alcohol from grape-sugar a "fever." The simplest fermentation, the saccharine, is still a mystery, so that the comparison of disease to it throws no light on the subject.

In approaching the subject of the "Organic Germ" theory, it is needful to say that it is not (as Dr. Parkin and Dr. Œsterlen suppose) a lineal descendant of the idea of epidemics spreading by the aid of insects endowed with voluntary motion; which idea, originally started by Athanasius Kircher in 1659, gradually died out as microscopes became objects of faith. It has less connexion with that than it has with Shakespeare's attribution of vitality to "the gay motes which *people* the sunbeam," the popularity of which expression has at least habituated Englishmen to the notion. The "organic germ" theory is historically the result of the observations which led to the attributing fermentation to the simple, yet seemingly independent and living, forms of matter which accompany its development. As the conversion of sugar into black damp and alcohol was carried from one vessel to another by the transfer of a minute mould, so might morbid processes be carried from one body to another. And microscopists sought diligently for special moulds suited to convey special diseases.

The boldest of these investigators of the causes of disease has been Dr. Salisbury, of Cleveland, Ohio, who in 1865 pronounced that malarious fevers were traceable to the absorption into the system of the spores of a freshwater mould, which one sees tinging with a burnt-sienna line the edges of marshy pools[1] (the Gemiasma). He attributes the phenomenon of intermittence to the ripening of successive crops of the vegetable, which, however, in ordinary ague are retained within the body of the patient. In a subsequent work[2] he attributes variola, vaccinia, and enteric fever to organisms of a similar nature, whose fruitful spores might be distributed broadcast, and so account for any contagiousness which might be proved to exist in disease.

Plenty of simply formed matter, endowed apparently with independent movement and growth, and more or less specific shape, has been found in morbid tissues and fluids. The

[1] 'American Journal of Medical Science,' Jan., 1866.

[2] 'Microscopic Examinations of the Blood and Vegetations found in Variola, Vaccinia, and Typhoid Fever.' New York, 1868.

difficulty is that none of these parasitic encumbrances have been found to affect the patient with anything like cyclical or constitutional symptoms. Their action is purely mechanical—severe, it may be, and even fatal, but still mechanical in its nature. Such is, without doubt, the fact as regards the fifty-five human parasites enumerated in the authorised nomenclature. It is true there are other forms, of the nature of infusoria, bacteria, and vibriones, which are found more commonly and more numerously in dead or dying morbid tissues than in dead or dying healthy tissues. Their presence implies more or less rapid disorganization of their habitat, but they have no specific differences in accordance with the specific form of the patient's malady.

In lesions indubitably dependent on extraneous vegetable growths (such as Pebrine, Madura foot, &c.), the structure of the parasite, at various stages of its independent life, can be easily demonstrated, and its definite characteristics laid down; and the way in which its peculiar mode or direction of growth causes a peculiar lesion is obvious to the pathologist. This cannot be done in the case of fevers. Foreign substances of a vegetable nature, moulds, zymes, slimes, cells, spores, and threads, are found in abundance among the worn-out tissues of both the sick and the healthy; our fæces and the coatings of our tongues are full of them. But they do not seem to do any harm or to give rise to fermentative or febrile action. There is probably room in the world for us all, as St. Francis said to the fleas in his frock.

The "Bioplastic" theory, instead of making the material cause of specific disease a parasite, as foreign to as it is inimical to the host in which it settles, derives its origin from flesh of similar nature to that which it afflicts. Dr. Darwin attributed contagion to a transfer of "organic mucus" endowed with "unusual motions," which, introduced into a healthy body, produce similar actions and effects; but his language is antiquated, and we hope it is not going too far to formulate a modern statement of the hypothesis thus:—Growth is a molecular change; a particle of animal matter, growing or capable of growth, may be separated from its connexions and continue to grow elsewhere; it will grow normally if it be normal, abnormally if abnormal; hence particles of diseased bodies may carry on, in new bodies to which they are introduced, the diseased processes which they had taken part in previously.

The bioplastic theory may fairly claim to chime in very well, and to elucidate rationally much that is mysterious in the history of disease. One difficulty has always been the few that are attacked. If yeast is scattered broadcast it ferments all the

grape-sugar it meets, and any common poisons borne on the wings of the wind would affect everybody more or less. External circumstances must be very exceptional to modify their nature gravely. But it is not so with living matter, which is very easily disorganized and rendered inert. When a surgeon places snips of healthy germinating skin on a sluggish ulcer, he does not despair if some of them fail to take root; still less would he be surprised if the same failure followed the throwing them in at the bedroom window, or scattering them on the Serpentine, which is a mild way of expressing the wide irregular dispersion of the particles given off from a sick person's body. The good luck of the multitudes who escape epidemics is readily thus accounted for.

Again, the introduction of morbid organic matter into an animal body is shown by the experiments of Messrs. Lewis and Cunningham to be very uncertain in the results which follow. When putrid fluids, swarming with organisms, were injected into the veins of dogs, some of the animals died right off, some after an illness, some recovered, and some seemed quite unaffected.[1] This behaviour of morbid organic matter is not like the behaviour of most deleterious agents, but it is like the behaviour of epidemic disease, to which certain persons appear constitutionally impervious; while others, apparently strong, suffer very severely, and others, apparently weak, very slightly.

The bioplastic theory may be considered as an offshoot of the ferment theory, in so far as it represents the morbid process in its new homes as directly continuous with that developed in its original birthplace. The life which is borne away by each separate particle of plastic animal matter may have the substance in which it is situated renewed, repeated, multiplied, modified, perhaps through several generations; but it is still the perpetuation of a life which has formed part of an individual animal.

The uprising of new diseases in the world, the change of type in old ones (an exaggerated but real fact), their sudden extinction, apparently through rational interference, seem more intelligible by the light of this than of any other theory. It is hard to imagine the common cosmic forces to be at any time newly combined so as to produce a sudden new effect, such as syphilis, or smallpox, or cattle plague; anything these powers do surely they must have been always doing. But the bodies of animals, especially of domestic animals, and especially of

[1] 'A Report of Microscopical and Physiological Researches into the Nature of the Agent or Agents producing Cholera. By T. R. Lewis, M.B., and D. D. Cunningham, M.B. Calcutta, 1872.

man, are widely modified by voluntary action as well as by involuntary circumstances in successive generations, and we can readily suppose that their morbid productions may be placed in new circumstances and take on new qualities. The half life of these self-multiplying substances may, when separated from the original body by several generations, acquire virulent powers which it had not at first starting, or lose them by variation. Or, again, it may be altogether extinguished by art or accident. But if left alone or placed in fostering conditions it is much apter to grow more and more virulent; just as the products of inflammation, experimented upon by Dr. Sanderson and others, which produce an intenser inflammation when introduced into the peritoneum of A, are found still more active when transferred thence to the peritoneum of B, and acquire still further activity when passed on to the peritoneum of C.

In all inflammatory diseases the loss of vitality, which constitutes the departure from health, travels from above downwards; that is to say, it is the higher functions of a tissue's life which are lost first and the lower which are lost last. Thus the plasma, incapable of ranging itself into normal form, yet retains the power of assimilating external material, increasing, and multiplying. Just as in the animal kingdom the creatures which have fewest characteristics to give them rank in the scale of being are those which most quickly propagate their kind. Now, the ardent fever of epidemics exhibits more than any other fever the phenomenon of a rapid augmentation of this germinating matter, useless for formative growth, active only in perpetuating its own obnoxious existence, making masses of granular *débris*, of pus, laudable and the reverse, putrilage, false membranes, loose fibrin, eruptions, &c. And it is naturally amongst the germinating matter that one will first look for the contagium. But as the germinating matter, in the form of pus-globules, is also rapidly developed in lesions certainly not contagious, wounds and sores, to wit, it cannot itself be the specific poison. So in his researches into the pathology of cattle plague Dr. Beale has fixed his attention, not upon the familiar products of inflammation, whose forms are seen with moderate microscopic power, but upon certain minute particles hitherto classed as "*débris*" or "granular matter," which he considers the moving agents of contagion.

The most weighty of the objections to the bioplastic theory is that stated by Dr. Beale ('Disease Germs,' p. 162), namely—

"That the highest magnifying powers hitherto placed at our disposal serve but to convince us that a minute particle of the bioplasm

of the most malignant tumour, or the most rapidly growing pus-corpuscle, resembles in every particular that we can ascertain by observation and experiment a minute particle of healthy living bioplasm from the blood or from any tissue; and it is proved beyond a doubt by the same means of inquiry that the living particles of bioplasm in vaccine lymph cannot be distinguished from those present in normal lymph or chyle. In short, no differences exist in colour, form, density, chemical composition or movements, between living particles which we suppose capable of producing the most diverse results, and giving rise to the production of the most different substances. I think we shall find ourselves compelled by the necessities of the case to refer the properties of these different substances to what must be termed *a difference in vital power*."

Now, specific diseases present most marked points of difference one from another, and we may fairly anticipate that agencies which produce them should, either in the microscope or in the laboratory, exhibit features of distinction corresponding to their several qualities. As long as all animal bioplasm looks alike, one expects it to act alike: but that is not the case with the causes of fevers, which behave in many fashions.

Morbid bioplasm differs, indeed, in quantity and in the proportion of its constituents from healthy bioplasm. But if it be self-multiplying, quantity cannot be an important distinguishing character.

It has not been shown that the apparent motions and changes of outline which pus-globules exhibit under the microscope, when subjected to artificial light and heat, are evidences of life; nor that, when these motions have ceased from cold and dryness, they can be renewed. The examples of seeds and eggs prove nothing, inasmuch as "germs" are not seeds or eggs.

Dr. Parkin objects that if these particles could reproduce themselves, when present in the blood, in the tissues, and in the secretions, there can be nothing to prevent this process going on for ever, at least nothing short of their destruction or that of the patient; they would go on increasing as long as any pabulum, the nutrient portion of the blood, remained. When that were destroyed the patient must die, and contagious diseases would be invariably fatal. Whereas, as he further objects, their tendency is to get well without the aid of medicine; and, he adds, this recovery is aided by change of place, which seems as if the patient did not carry the seeds of disease within his own body.

The rapid spread of epidemics is also urged by Dr. Parkin as a reason against their being diffused by living entities; and, again, the quickness with which they destroy life in certain

cases seems to militate against a self-multiplying virus, which must be powerful in proportion to its age, and take its time to become strong. A few patriarchs who went down into Egypt became a mighty army, but they took four hundred years to do it in.

The arguments against the various classes of theories, professing to account for the spread of epidemics, have here been gone through before those which may be held to favour them. Such seems to be the general practice of modern writers on the subject. Dr. Beale published in June, 1870, a volume against the "supposed nature" of the seeds of disease, and not till November the one which explains his views of their "real nature." Dr. Parkin has issued as yet only the first part of his work, consisting entirely of an analysis of doctrines to which he objects. In Professor Œsterlen's 564 closely printed pages his own opinions are so drowned in a flood of monotonously reiterated arguments against other people's that it is exceedingly difficult to find them. The arrangement, then, appears to be found a convenient one; but there are dangers attending it, dangers which, even in this article, and with the warning of the mote in our brother's eye before us, it may be difficult to escape. In the first place there is the pitfall of party spirit: a hard and fast line is drawn between those who hold one opinion and those who hold another, and no concession must be made to opponents lest their doctrines should have to be accepted in a lump. Thus Dr. Bryden, as is remarked by Professor Pettenkofer, "holds so little to the influence of intercourse, and so firmly to the diffusion of the earth-born cholera through the air, *on this account only*, because he dreads lest he should fall into the hands of the raw contagionists, regarding whom he has the conviction that they have no conception of the nature of cholera." And Dr. Œsterlen actually taunts Simon and others with being retrograde reformers, because after they have given due weight to the action of common factors, they incline again to the camp of contagionists. Surely this is to unite all the vices of conservatism to a radicalism stripped of its virtues. Another danger is a most unreasoning scepticism and despair of truth ever being atttained by the methods whose failures have been so exclusively dwelt upon. Thus the Cattle Plague Commissioners allow themselves to express the opinion that the contagious matter is "of a kind which is, *and always will be*, undiscoverable by the microscope." This "supposition of the impossible" is a great impediment to the progress of knowledge, as Bacon truly remarks. And Dr. Parkin (p. 174) designates the hopes of "sanitary reform," that is, the hopes that we shall be healthier than our forefathers, and our children healthier

than us through our exertions, as "hopes that never will be, never can be, realised." A melancholy prophecy! refuted, thank God, by all man's experience on earth, which has taught him that hopes of improving his condition not only always have been realised, but have brought about their own realisation. Whatever we do let us keep clear of the paralysing miasmata of Doubting Castle.

Probably it is a result of the evil spirits above alluded to that writers on this subject are apt to employ modes of argument which they cannot but know to be dishonest. Professor Œsterlen is the author of a 'Handbook of Materia Medica,' which has gone through seven editions, and has therefore won the confidence of students; he has also published a work on 'Medical Logic,' so that he may be presumed to recognise a fallacy when he sees it; yet a more elaborate collection of inconclusive reasonings than his volume placed at the head of this article it is difficult to imagine. It is based throughout on the assumption that, until a physical agent has been made evident to the senses, it is illogical to maintain its existence, ignoring the fact that the greatest advances in mathematics, astronomy, and general physics have been made by working out the results of unknown quantities and principles, and that in chemistry a bold *anticipatio naturæ* has often opened out more truth than the vastest collection of observations. Worse than that, he piles up an immense mass of documentary evidence without references to enable the reader to verify it. Worse than all, when the passage cited is with infinite labour identified, it is found to be misquoted and perverted. As one example (life is not long enough to waste in detecting many), he states that Hirsch is driven to consider rheumatism and stiffneck infectious disorders (page 200). Now what Hirsch really suggests is the inquiry whether, in addition to the acknowledged sporadic rheumatism, there is not also "an acute specific infectious disorder, bearing to it the same relation as influenza does to catarrh"[1]—a very different thing.

Dr. Œsterlen is not even superior to the old-fashioned advocate's dodge, now happily limited to police courts, of sneering at the plaintiff's attorney when you have no case. For example, he calls Kircher a "Jesuit" (page 31), not that his belonging to that corporation has the slightest bearing on the matter, but because members of it are at a discount now in Prussia. An Irish lady, who caught scarlet fever from a letter, is described as "eine Miss in Coleraine," though her implied missishness, however funny, could not weaken the testimony, which is not

[1] 'Hirsch. Historisch-geographische Pathologie,' p. 598.

hers, but that of her medical attendant (page 559). Again, he adopts the pernicious method which so conspicuously spoils such works as Buckle's "History of Civilization," of heaping together all the authorities which tell one way, without any attempt to weigh their comparative value, or test their integrity, if only they are on the right side. Lists of surnames, without titular or Christian appendages, some of them obscure, some so common as to defy identification, some (at least one and perhaps more) misspelt, seem to Dr. Œsterlen to make a show of authority. Sometimes the very evidence invalidates the credibility of the witness. For instance, in speaking of the transmission of variola through the air, he first mentions cursorily the names of a number of the leaders of our profession who believe in it, and then considers that he knocks them all over with a certain Mr. O'Ryan (page 556), who day after day placed some unprotected French babies round a little table covered with dirty rags from variolous sores, and states that they did not contract smallpox. We cannot imagine that any of our readers would believe such a man on his oath. King Herod was a modest saint in comparison, for at all events he did not boast of his baby farming, or expect Rachel to entrust her future innocents to his care. Yet Dr. Œsterlen quotes the experiment without shame or apology.

We wonder whether teachers of logic ever feel any responsibility in propounding fallacies. Dr. Œsterlen's favorite butt for ridicule is the idea of disease being carried by "paper" or "rags;" now in the "Report of the Massachusetts Board of Health" we read of "paper-rags," or "rags," as the medium of the transmission of smallpox to eleven towns in the State during the thirteen months ending last February.[1] It is to be hoped that when "Die Seuchen" circulates in America this fact will be added in an Appendix, in order that the author's European reputation may not impress his countrymen over the water with a contemptuous disregard of their "paper-rags."

Our author's favorite mode of reasoning is a note of admiration (!) intended to be taken ironically, one does not always see why, which is much more irritating than convincing. He would seem to have taken in earnest Shaftesbury's paradoxical argument, that ridicule is a test of truth. Surely this is not what the reader could have expected from a German University professor, a scientific teacher, and a writer on logic.

Our own countrymen cannot be entirely cleared from the blame of occasional indulgence in rather hard jeering. Dr. Beale is much more satirical than the incident warrants on a

[1] 'Fourth Report,' &c., 1873, p. 468.

popular lecturer, respecting a brilliant demonstration to a mixed audience of the quantity of organic matter contained in city air. "Don't let us laugh at one another: the public is only too ready to join," was the rebuke of an elderly physician to a priggish clinical class (before whom he had made some scientific blunder), fresh in the memory of at least one of their number, though nearly thirty years have gone by; and it may be commended to Dr. Beale's thoughtful consideration. A note seems to imply that the attack on Tyndall is retaliative; which is an aggravation of the offence in the reader's eyes, for it shows that he has been beguiled into purchasing a lot of paper and print occupied with a personal squabble in which he has no concern. 'Disease-Germs' would be improved by the omission in a second edition of this dozen of pages. The work in it is too good to be smudged with useless matter out of place.

Dr. Parkin also permits to himself a sneery style about men of undoubted honesty and ability, which we must not excuse on the ground of its giving us a hearty laugh. In the matter of the "Thames Pestilence" of 1859, for example, it is most unjust to ignore the fact that the "Woe! Woe!" of the pest-prophets was followed by active measures to arrest the pace at which London appeared to be nearing the precipice. Their predictions were better than accomplished, being effectual in preventing the evil, and to turn them into ridicule shows an utter misconception of the true function of prophecy.

British India has shown herself an excellent school of manners in this particular, for the writings of her adopted sons that we have had occasion to read lately combine with a manly independence of tone an equally manly tolerance and respect for the views of others. And we may also quote as an example of dignified controversy the discourses delivered before the Medical Association at Munich, by Drs. Wolfsteiner, Buhl, von Pettenkofer, and others, on the etiology of typhus.

It may be observed that each group of ideas on the etiology of epidemics, into which the speculations on the subject are here divided, is a development of that which precedes; so that far from contradicting what had been previously advanced, it in some measure implies its value. For this reason especially we regret the iconoclastic tendencies of authors, and the cold water they would throw on all research beside their own.

To treat lightly the influence of common factors is certainly to fly in the face of many crucial experiments, which show one or more of these to be abnormally prevalent at some period of every well observed epidemic. The most obviously active agent in the production of disease is the rapid variation of temperature in a moist air. The symptoms which it gives rise

to, even in the healthy body, such as chill, reaction, painful congestion of internal viscera, the collection of mucus on the epithelial surfaces, &c., closely resemble the phenomena of fevers. Every patient affected with the chronic consequences of fevers knows how frequently complete relapses are brought on by chills, whether in a hot or temperate climate. And what brings a relapse is easily understood to be a possible cause of the original malady. Again, the comparative immunity of the well-sheltered and well-clothed, and the greater frequency of first invasions during the night, observed in all epidemics, seem to point unerringly to changes of external temperature. Furthermore, by tracing them to climatic vicissitudes, is easiest explained the connexion, otherwise obscure, of zymotic fevers with pneumonia, bronchitis, hepatitis, dysentery, rheumatism, the transformation into one another of the fevers and their reputed "complications," and other points of contact so intimate and numerous, as to constitute what M. Haspel calls "a family connexion."[1] Next to variations of temperature Famine strikes the mind most forcibly as a common factor, and it certainly does occur epidemically itself, and rarely without being followed by epidemic diseases which in some of their phenomena bear a resemblance to the effects of acute starvation. Mr. Lyons has in the work cited at the head of this article put together a quantity of evidence, bearing more or less on the point, which would tend to trace to imperfect nutrition the origin of all those various epidemics which are distinguished by irregular relapses after intermissions or remissions of uncertain duration. And under the designation of "Relapsing or Famine Fever" he groups all those forms of disease usually attributed to "Malaria." Overcrowding is another common factor about whose influence in lowering the body to a morbid state there can be little doubt. The breathing and rebreathing air filled with emanations of the human body seems to supersaturate the mass of the blood with dead stuff; the nutrient fluid becomes dark, uncoagulable, easily decomposed, incapable of supplying the brain, or the capillaries, or the muscles with working power. We expect *à priori* to find epidemics of typhus, where the blood exhibits the above characteristics, among overcrowded populations. Filth, a decomposing organic matter in excess, is another almost invariable precursor of zymotic disease, either sporadically or epidemically. Its influence is less notorious than that of other common factors for several reasons; of which the most potent is that the gases which accompany it are not noxious in proportion to their offensive action on the senses.

[1] 'Maladies de l'Algerie,' i, 86.

Indeed, it is doubtful whether the really injurious element is a stink at all. The scent of the feculent matter is found by some people appetising (Parkin, p. 129); but we believe that mixed in appreciable quantity with water or food it invariably produces nausea, and in other ways shows its unsuitability for the upper part of the digestive canal. In fact, all mucous membranes, other than that of its own home, are offended by it. Great pain and inflammation announce its introduction into the bladder; and again, in cases of recto-vaginal fistulæ, air will pass through from the bowel for a long time without the cognisance of the sufferer, but the smallest quantity of solid or liquid causes infinite distress and inflammatory action. The stage also of decomposition and the circumstances under which it takes place, whether freely exposed to air or in a confined space, whether in a healthy body or in an unhealthy, and what substances are added, seem to modify the wholesomeness of excreta; and thus the apparent conflict of evidence on the subject is fully accounted for. As all organic matter must be decomposed somehow and somewhere, the process comes strictly under the notion of a common factor, as it is only the excessive localisation and accumulation which can be considered abnormal. It has been suggested that the laws of electrical action might be found to bear some relation to the distribution of epidemics; but observation has not yet made out anything to support the suggestion, except so far as electricity, ozone, &c., are dependent upon chemical processes or the other physical phenomena already mentioned. Professor Schönlein[1] tries to put himself ahead of his age by hinting that terrestrial magnetism is concerned in the matter, on account of the periodicity exhibited by it, which corresponds in some degree with the periodicity of fevers. We mention this last mainly to attach to it a protest against picking out the least known as our greatest enemy, stoning a stranger simply because he is a stranger, a savage practice which impedes scientific progress as much as social civilisation. The little we know of terrestrial magnetism seems to clear it of all charge of direct influence on the human frame.

The main strongholds of the idea of common factors acting through the intermediate generation of malarias and morbid poisons are, the specificity of fevers, the fact of their continuing after the apparent cosmic causes have ceased, and their period of latency, Doubtless the subdivision of fevers, as of all diseases, has been carried too far, and our sphere of experience has been narrowed by our observing differences rather than resem-

[1] Schönlein's 'Pathologie und Therapie,' i, 10.

blances. There is also a disposition among pathologists to confound the specificity which resides in a patient with that which resides in a disease. Still, no one, probably, will deny that there are some specific maladies or causes of malady, and evidence of specialisation seems clearer in the instance of epidemics than anywhere else; and did this special character depend entirely on the peculiar combinations of terrestrial phenomena, the fact could hardly have been overlooked so often. With regard to the period of latency, it may be remarked that it is much longer than can be accounted for by slowness in the patient's body to react on physical stimuli or by retarded absorption; there are no external agents or natural poisons whose character is that of lying dormant for a fortnight after their reception. Also natural poisons are noxious in proportion to their dose, whereas the producers of epidemics are not so—a most essential difference of nature.

The last point is, indeed, so essential, and so different from anything to be observed in other departments of matter, that it alone gives probability to a fresh speculative excursion. It can be understood only by attributing to the exciting cause of these fevers one at least of the properties of live substance, namely, the power of self-multiplication. Without this contagion becomes a mere word; for even if one were to limit the idea conveyed by it to the transmission of an unsubstantial influence, that influence must be multiplied manyfold when a single sick person infects many. It is perfectly rational to assume the existence of a "contagium," though all the aids of art should till the end of time fail in making it cognisable to our senses. The aids of art do already show us live substance fulfilling many of the conditions which have been objected to as opposed to the hypothesis of contagion. The rapid growth of low fungi (such as the stinking morell, at nightfall an invisible spore, at sunrise protruding its fetid impudicity all over our fernery)—the flashing of the yeast-mould through the gigantic beer-vat—its growth under our very eyes assisted by a microscope (Beale, D. G., Pl. I, fig. 8); and again the nature of that growth, not in successive crops or generations, but continuous—the detection in the air of solid live spores, the pace of whose march is only limited by the swiftness of the wind—all these things use us to see nothing absurd in the idea of the rapid spread of disease by means of a substance passed from one centre to multitudinous new foci at once by means of self-multiplication. Nor need we distress ourselves much at our inability to identify the substance; or when we have identified it, at a difficulty of classifying it according to the future disease it is in the process of giving rise to. It may very likely turn out to be something we have been looking

at all our lives, and calling it pus, or mucus, or extractive, or fat, or granular or adenoid matter. In the first stages of their life the germs of birds, beasts, and fishes are indistinguishable from one another; and here is the very germ of germs; can you justly expect to predict its future phase of existence by the eye alone? Indeed, the fact that they claim to make this diagnosis, to assign to one organism found in decomposing matter the causation of one disease, and to another organism the causation of another disease, shows a *primâ facie* suspicion on the deductions drawn from such praiseworthy excursions into an unknown realm of nature as those of Budd, Hillier, and Lostörfer. But this suspicion should not lead to treating lightly the observations themselves, whose very fallacies may be made gates of knowledge.

The various advocates of the fermentation and of the organic germ theories seem to differ mainly in words and degrees. The strictest sect of chemists does not deny that independent organisms may be the carriers of molecular action, and the most devoted believer in the individuality of moving particles supposes them to convey, not themselves alone, but a vital power. However, a real step in advance is made if it can be shown that the transfer of fevers is not by an independent parasite, to which man has no relation, except the supply of food, like as in the case of a lion, or a louse, or a lichen, but by a detached part of a human body, capable of becoming bone of its bone and flesh of its flesh with another similar being. And as the detached fragment of healthy skin communicates a healthy growth to the stagnant ulcer where it is grafted, so it seems reasonable enough to expect that a particle of unhealthily living bioplasm should communicate an unhealthy mode of life to the body where it settles. The exceeding minuteness of these particles, and a minuteness which seems to bear a certain proportion to their virulence, prepare us not to be astonished at the idea of their passing into the person in all directions, or leaving the infecting agent at any pore; and the ease with which they find their way out of the patient, and into the external world, receives further light from the apparent similarity of their nature to the nature of pus, one of whose most striking characteristics is its rapid destruction and perforation of epithelium, skin, and other tissues, which stand between it and freedom.

The impossibility of weakening without destroying the effects of contagion by mixture with air, water, or other diluents, is a characteristic feature. You cannot immediately lower a fatal into a remedial action, as you do when you use a diluted poison as a medicine. The only way in which you can alter it is by giving it a completely new nature, as when smallpox virus, by

passing through the body of an animal, becomes vaccinia. Now vaccinia is not a mild variola, but something quite diverse.

If a person catches a cyclical disease at all, it depends on himself, not on the amount of cause, whether he has it slightly or severely. This seems to show that the contagium, however wide apart its particles may be dispersed, is not soluble in ordinary media. Such insolubility is also a characteristic of germinal matter, as we see it in pus and mucus, which may be even swallowed and passed through the whole of the digestive canal without injury to the outlines of its particles, an ordeal which is quite a touchstone of immutability in organic matter.

It is remarkable also that in all acute complaints suspected of being communicable there is more or less rise of temperature. In the slower infections, such as syphilis, the rise is spread over a long period, and, therefore, is less perceptible. Still, feverishness can always be made out during the years of eruption, and if that amount were concentrated into a few days the thermometer would show it readily enough. Now, rise of temperature is, as far as has been observed, always associated with an excess of bioplasm in the blood. Professor Beale states that "in cases where the body heat rises several degrees in the course of a few hours the germinal matter increases with marvellous rapidity. The capillaries of a great part of the body are found to be gorged with particles of living matter, for the most part descended from the colourless blood-corpuscles, while the masses of bioplasm of the neighbouring tissues have increased to twice their normal size." The readiest explanation of this active virulence is the idea of its being the continuation of a process going on in the body whence the disease is transmitted.

The practical inferences which epidemiologists draw from the opinions advocated by them are the points at which they come into contact with the general public, and we are bound to say they are not sparing of their advice. Dr. Parkin would persuade us that these general pestilences "are regulated by natural laws *over which man has no control*," and recommends us to "frankly avow the fact," and so "be saved the mortification of having unfulfilled promises and disappointed expectations thrown in our teeth."[1] Surely this is to behave like the sheep in Lammennais' parable,[2] who saw one after another of their flock carried off by the wolf, hoped that each would be the last, and went on eating and drinking, and renewing the silly population, on the same pasture. However, the more common counsel inclines rather to the adoption of the method of the Ninevites, who jeered not at their Jonahs, but at once laid aside their lazy

[1] Page 174.

[2] 'Paroles d'un croyant,' iv.

luxury, and in the best way they knew of grappled with the threatened evil. We need not scorn to imitate such a practical, square-jawed, broad-thumbed people, as their portraits show them, not penny-wise either in public works, as the ruins prove. If the Jonahs choose to be sensitive about unfulfilled promises being thrown in their teeth, they may be advantageously referred to the history of the prophet and the rebuke he received for his touchiness.

Dr. Œsterlen has strong faith in the active carrying on of statistics by means of a complete registration of disease and health, which should be persevered in at all times, and not only in sickly seasons, enforced in all places, and not only in those smitten by the scourge. He justly argues, perhaps at needless length, that such observations carried out through a series of years, would do much towards showing the influence of common factors in making diseases epidemic. We should learn also, what now we are quite ignorant of,[1] the influence of individual constitution in determining the form of fever, its fatality, and the liability to it of different persons. It were to be wished that he had contributed more details as to the collection of these data, the means of avoiding fallacies, and of separating opinion from fact. It seems rather a platitude to prove that more would be known of the human race if better information as to its habits were obtained. What is really wanted is for a logician to make a study of the art of framing questions, so that the answers should be worth having, and for a statesman to tell us how best to shun collision with endemic prejudices, and to point out the most economical sources whence to seek knowledge; in short, we should like to see a technical volume on 'How to Work with a Register.' It is all the more called for just now, by reason of the recent appointment of some hundreds of educated men as officers of health, without formal duties enough to occupy the time which their salaries imply shall be given to the work, and with plenty of zeal to expend on anything shown to be worth doing. One of the most useful efforts in this direction is the publication of the London College of Physicians of their 'Nomenclature of Diseases,' with translations into German, French, and Italian, so that a considerable portion of the world may begin calling things by equivalent names.

Limited local investigations, accompanied by maps, such as that in Dr. Grimshaw's 'Remarks on Fever in Dublin,' are

[1] For example, at Dublin lately, there was an evenly balanced discussion as to whether a diathesis so common as scrofula did or did not predispose to such a frequent malady as enteric fever. There were no facts cited on either side.—'Brit. Med. Journal,' April 26th, 1873.

most valuable, not only for temporary purposes of administration, but as permanent records. The author has gone on the plan of marking with red dots the houses in each street where fever has appeared during two years, so that a crowd of these spots indicates to the eye a "fever-nest" much more rapidly than description in words. Maps on the same scale of the water supply, drainage, geological formation, &c., would give a useful aid to comparison.

The general registration of disease is the most important matter which medical reformers have at present on hand. The obstacles stated to impede its use are the difficulty of legal enforcement and the question of payment. Now, in Massachusetts an example has been set by taking the bull by the horns, which may be commended for imitation. In a Board of Health code, drawn up by two physicians and strictly enforced, we read, "Any physician who may be called to a case of either of the diseases specified in the foregoing regulations shall at once report such case to the board," &c. And in the General Statutes,' chap. xxvi, § 5, "The board shall make such regulations as it judges necessary for the public health and safety . . . . . Whoever violates any such regulation shall pay a fine not exceeding 100 dollars."[1]

Statistics, however, can only direct or weigh knowledge; they cannot originate it; in particular they cannot aid the physiological prescience, to which men look for guidance in their dealings with disease.

Dr. Beale very properly upholds the value of minute inquiry into individual cases by personal investigation, and especially of those microscopical researches in which he is so skilled; and to this end he gives, in the larger volume, very valuable details, accompanied by pictorial illustrations, of the methods of working; and the energy he throws into the description, and the new world of which he promises the sight to any number of new Columbuses, makes the reader long to start at once on the voyage of discovery. Dr. Maddox's germ-collecting apparatus for microscopic observation of the atmosphere has the features of simplicity and of working automatically on large quantities of air—a great advantage when we consider how few and far between disease-germs must float under ordinary circumstances, and that a considerable part of the inquiry must consist of tedious repetition of common forms. The articles for examination are collected by adhering to a plate of glass smeared with glycerine. It would be desirable to have a corresponding apparatus for searching streams of water, and we would suggest a

[1] 'Fourth Annual Report of the State Board of Health of Massachusetts,' 1873, p. 464.

trial of gelatine and soft spirit varnish, of spun glass, and powdered silica filters, as means of detecting the organic contents of large bodies of water. The search of the solid and opaque soil presents greater difficulties; it is itself a filter, and cannot be filtered; but, as a compensation the remark may be made that what it retains it retains for good—that a morbific particle once entangled in its meshes is unlikely to give any further trouble unless by being diffused into a more fluid medium, where it could be caught by other means.

Observations on the rise and fall of the water supply are generally contributed largely during epidemics, and it is assumed that its condition at those periods is a pernicious one. More continuous observations of this point are urgently required for comparison, and could be made with moderate labour.

By meteorological investigations on a gigantic scale, the spectroscope may possibly give us some information on widespread constitutions of epidemic seasons. Meteors, comets, dust, rain, blights, high specific gravity of the air, and aurora borealis, have been noticed as companions or forerunners of pestilence. And now that these irregular visitors are generally recognised as extra-planetary, it will be well to know whether they contain any ingredient directly noxious to man, or whether they only indirectly affect his nerves and his nutriment. Since bloody rain was found by Ehrenberg to consist of live monads, there has always been a suspicion that, at all events within our atmosphere, some meteorological phenomena might be due to organic germs; though few, perhaps, will go the length of the eminent Scotch professor last year, who suggested that the first spark of life possibly came to our globe on some wandering meteorite, ignoring the much greater difficulty of its getting to the little stranger than to the mighty planet.

Minute investigations of the above sort have been hitherto left almost entirely to "the doctors," whose necessary employment in practice prevents them from giving more than odds and ends of time to the work. Dr. Beale complains of this, and reckons that for the price of each monster cannon the government might purchase a first-rate scientific man body and soul, and at the cost of every discharge might carry through an important investigation into the nature of disease. We will not discuss the estimate, which certainly seems the proffer of a bargain; but we would remark that intellects are not like metal, forthcoming in quantity proportioned to the demand; and that in natural studies, as in art, an exaggerated demand brings forth a supply of an inferior article, which swamps the superior. We should be sorry to see science drowned in its own popularity, as painting in the period of the Renaissance and at the

present day. If, however, our professional readers are of opinion that the bracing tonic of poverty is not still a proper treatment for the muse of the lens and the test-tube, we would yet persuade them to abstain from looking primarily to Government for help. Private munificence has in England been always the best financial promoter of sound knowledge. Millionaires are dying every month, often with only the slightest claims on their wealth, and their most trusted friend for years has often been their medical man; why are not the needs of science urged? as were in former days those of religion and education and charity, which are now enriched to the full extent of political expedience. Perhaps these few words of ours may have the same effect as the chance letter to a periodical by a passing stranger, which founded the Missionary College of St. Augustin at Canterbury.

Professor von Pettenkofer goes farther than Dr. Beale, and says that "the eyes of practitioners are dimmed by the sweat of their brow," that they cannot observe well because there is too much for them to observe, and their feelings and affections are so engaged in the work that their reason is not a *lumen siccum*. With all due respect to special experts we would remark, that it is just such special correction by the men of heart and head that their logical labours require; bare facts must be classified and valued in proportion to their influence on human hopes and fears, or science will soon cease to have any charm for mankind.

There is no need, however, to limit the work of the active practitioner to mere administration. There is plenty to be done for scientific epidemiology which he only can accomplish. All that can be learnt by observation of symptoms must come through him, and that is much more than appears at first sight. For example, accuracy of diagnosis is by no means yet attained to. It is not necessary to be on the staff of a fever hospital to be convinced that blunders arising from ignorance in this particular must seriously invalidate statistics. Every physician who is honest to himself must see how often he is right or wrong by mere chance, the diagnostic marks to which he has trusted failing to give any light. There is hardly any acute disease for which enteric fever may not be mistaken; and if practitioners persevere in the usual habit of forming a diagnosis at the first interview, and sticking to it, the case is useless for scientific purposes. Dr. C. Wünderlich has in the small treatise which is quoted at the head of the article done good service by pointing out the various diseases which may be taken for typhus at several stages, showing the peculiarities in the course of the latter which may distinguish it from them, and excluding one

after another of the sources of error as the illness goes on. For example, he notices how the rapid rise of temperature (viz. up to 104° Fahr.) during the first three days cuts off the idea of enteric fever, where the rise is slower. But there still remains the possibility of its being a recurrent fever, a pneumonia, a meningitis, an acute exanthem; and from these a further rise in temperature during the rest of the week separates it. In meningitis this does not take place, in pneumonia only in the bilious form (whose etiology is, indeed, only doubtfully different from typhus), and in acute exanthemata extremely seldom. Moreover, other febrile affections, when their heat at all comes up to that of typhus, do not keep it up, but either prove fatal, or cool down. Recurrent fever alone of those mentioned remains as a likely source of error. This is excluded in all probability at the beginning of the second week, when a marked remission occurs, whereas in typhus, on the eighth day, there is by the evening a fresh rise in temperature. By the same sign, says Dr. Wünderlich, also a bilious pneumonia is excluded, and a final blow given to the expectation of acute exanthem or meningitis; and so on through the whole course of the fever up to health. This continuous revision of the diagnosis seems peculiarly judicious in epidemic fevers, in the interest both of the public and of the patient: of the public, because in respect of preventive measures accuracy is of more importance than promptitude; and of the patient, because the physician is forced to prescribe for him and not for disease, to discard routine and expectant medicine, and daily to administer to the daily requirements. The greater the difficulty that is found in putting a Latin name to the complaint, the safer will be the treatment. Dr. Wünderlich's method is very preferable to the sort of catch-signs by which typhus is offhandily distinguished in our current systematic treatises.

The period of incubation of contagious fevers, or the period which elapses between the entrance of the contagium[1] into the system and the first manifestation of its presence there by symptoms, has not only a practical bearing, but is a matter of scientific interest to the etiologist. It would be of the utmost importance to know if the time which has passed between the independent existence of the cause is to be reckoned as part of the period of latency, or not. For example, supposing a contagium starts from a human body or a common factor, and is some time before it reaches a patient, will the period of latency be shortened? And if it be imbibed fresh and direct, will the period

[1] In using the words "contagious" and "contagium," it is not intended to prejudge the question about communication, but simply to avoid a pedantic periphrasis.

be proportionately long? Or does the duration of latency depend on the form of fever? or the form of fever on the latency? Or does the place by which the poison enters nose, fauces, stomach, skin, lungs, influence the matter? The subject has, nevertheless, not received the attention which it deserves, and little is really known about it. Positive statements based on insufficient data have been freely copied by one writer from another; there is a want of recorded facts which prove anything as to the duration of the latency. Even with regard to smallpox, the glaring symptoms of which are so easily dated, a collection of trustworthy instances is still to be desired.

Cases throwing light on the incubation of a disease may be classed in three categories:

1. Those in which there has been a single limited exposure to the cause.

2. Those in which there has been a long exposure, both limits of which are known.

3. Those in which there has been a long exposure of which only one limit is known, Either the exposure has persisted up to the date of the seizure—in which case it can only be said that the incubation has not exceeded such or such a period—or an interval has elapsed between the end of the exposure and the seizure, and then all that we learn is that the incubation has not been less than that interval.

The first are the best, but are extremely rare, as nearly all of those whose exposure is limited, escape altogether. But the other two classes, to which most of the recorded instances of latent periods belong, are not without value; and on a large collection of them, occurring in the course of personally observing upwards of ten thousand fever patients, Dr. Murchison has constructed what may be taken as a model for the utilisation of cases bearing on the subject. The fevers of which he reckons the latency are Typhus, Relapsing, and Enteric.

Of Typhus Dr. Murchison tabulates thirty-one luciferous instances, and comes to the following conclusions:

1. The period of incubation varies in different cases.

2. In a large proportion it is about twelve days.

3. In exceptional cases it is longer, but rarely, if ever, exceeds three weeks.

4. In one third or more it is less than twelve days, and occasionally there is scarce any latency, the symptoms dating almost from the instant of exposure.

The mode of reception of the agent of morbid action probably influences the period of its maturation, for it is remarked that in all those cases where persons immediately after exposure suffered from headache, nausea, and rigors, followed by the

usual course of typhus, there was complaint made of an offensive odour proceeding from the beds or bodies of the sick (Marsh, in 'Dub. Hosp. Rep.,' vol. iv).

In Relapsing fever the facts, so far as they go, seem to indicate that the period is somewhat shorter, apparently about nine days, and that it is occasionally much longer, as in typhus, and occasionally absent altogether, the symptoms commencing immediately after exposure. But trustworthy information is wanting.

Enteric fever is longer on its road to maturation. Its period of incubation is most commonly about two weeks. Instances of longer protraction are less rare than in typhus or relapsing. But yet, as in the others, circumstances may shorten it to as little as one or two days.

In the famous instance of the school at Clapham, investigated by Drs. Chambers and Latham, where twenty out of twenty-two boys were attacked by enteric, the whole poisoning took place within four days after the contents of a cesspool were spread over an adjoining field. In an outbreak of the same at Guildford, in 1867, the latent period was in a great number exactly eleven days. The only condition generally coincident with the invasion was the high service of the town water supply, and it was found that, eleven days before, water polluted with sewage, which had been stored up for sixteen days, had *on one day* been distributed by this service to the 330 houses in which the fever appeared ('Tenth Rep. of Med. Off. of Privy Council,' p. 34).

The precise period of three hundred and twelve hours, which we have learnt since our youth as the duration of the latency of smallpox, has been decided by inoculation; and as the experiments were formerly numerous and cannot now be repeated, they may be considered as decisive in respect to that mode of communicating the disease. But according to Mr. Marson, "three or four instances have occurred, in which it seemed likely the disease had appeared between the tenth and eleventh days after receiving the infection; they were cases occurring after vaccination, under which condition other stages of the disease are often interrupted or cut short."[1] One would suggest, as a more probably source of modification of at least this part of variola, the mode of reception. Sir Henry Marsh and Dr. Murchison remark, in respect of typhus, that when it is perceived in the respiratory organs, the latent period is singularly brief, and the same may be expected to be the case with variola. The shortening of the latency may in such case be explained by the

[1] 'Reynolds' 'System of Medicine,' i, 435.

ripening of the virus in the air before its reception, instead of that stage being gone through within the body, as must happen in inoculation.

Of Scarlet fever to determine the incubation is of more importance in respect of the etiology than of any other disease, for it will help much to detect the medium of communication. The shortest period is easy enough to make out, and very short it is; but there is considerable difficulty in fixing the longest, on account of the strong suspicion that the contagium may be kept alive an almost indefinite time in suitable receptacles, such as woollen stuffs, for example, before it enters the body and commences its course of activity.

About the incubation of Diphtheria absolutely nothing appears to be known. And that is quite a sufficient reason for the mode, or even the possibility, of communication remaining an entirely open question.

Of these last, as well as of measles, and several other epidemics, sufficient instances in point can be collected only by the domestic attendants on the population; hospital staffs and consulting physicians are comparatively powerless. We would especially call the attention of district health officers to the matter. A well-framed set of queries would exclude fallacious cases, and draw observation to the true points at issue.

The symptomatology of fevers may contribute in another way to elucidate their etiology. There are certain symptoms which run through large groups of very dissimilar diseases; do these show a common point in the nature of the diseases? or in the nature of the patients? or in the mode of reception? Take, for example, inflammation of the solitary and agminated glands of the intestines—it is found not only in typhoid, in typhus, in scarlatina, in intermittent and remittent fevers, in dysentery, in cholera, but also (according to Dr. Peter Latham[1]) in scurvy, and (according to Dr. J. Harley[2]) in poisoning by toadstools, and as an effect of colchicum, and of hemlock, and of rotten German sausages. Can there be any common nature in these agencies? That the patients have any special diathesis, beyond that common to all mankind, seems denied by the impartiality of the pest. Is it possible that the affection of these organs is an indication of the road by which the poison has entered the system?

The last question opens up a further subject for enquiry most interesting to practitioners—is it true, as our forefathers thought, that the local increase of excretion in fevers is a cleansing out

[1] 'Account of a Disease lately Prevalent in the General Penitentiary.' London, 1825.

[2] In 'Reynolds' System,' i, 623.

of injurious matter? Is it better out than in, and is the excess of excretion a beneficial "effort of nature?" or is it simply melted-down tissue, the amount of which shows the violence of the destruction? Of course, the object will be to aid it, if the former; to limit it, if the latter. What is the meaning of local lesions? Does the poison, if poison there be, for example, in typhoid, inflame the intestines in going in or going out? It is obvious, that in this are involved many most delicate problems, not only of practice, but of etiology; and we see nothing impossible in the answers being given by means of a crucial series of experiments on animals. Dr. Beale urges that the arguments from physiology are against, rather than in favour of, the doctrine of "elimination" altogether. But it is to be remarked that he assumes, perhaps somewhat prematurely, that the poisons of fevers are living entities, endowed with separate powers of motion, and insoluble so long as they are living and active.[1] The believers in elimination probably all look upon them as soluble in the fluids of the body, and related to the ordinary toxic agents which the test-tube shows are got rid of by skin, kidneys, and bowels, incapable of automatic movement or self-multiplication.

Take, again, the phenomena of intermission, the different forms and degrees of which are a feature in all fevers, more or less—is the cause intermittent, or the effect? *i. e.* does the intermission depend on the excitant of the malady or on the nature of the functions which it attacks? Griesinger points out, as an argument by exclusion, that at all events it does not depend on the periodicity of common factors, inasmuch as the periods do not correspond.[2] The earth goes round in twenty-four hours, but an ague, for example, may be quartan, tertian, or quotidian; and when the latter, may lengthen or shorten its interval daily, as it grows better or worse, till its type becomes continuous or tertian.

The action of the nervous system might be tested by repeating at regular intervals certain impressions similar to those made by morbid action, and trying how far they are capable of inducing automatic repetition. Dr. Œsterlen quotes an experiment made by a Lyonais physician, who took a cold bath every night, till at length the shivering and fall of external temperature, followed by reaction, occurred without the original exciting cause. He supposes that in this way a quotidian ague might have been generated. But no evidence is given that the doctor was not previously an ague subject, and that which he experienced was simply a relapse. In fact, the experiment

[1] 'Disease Germs,' p. 129.
[2] 'Infectionskrankheiten,' p. 32.

has been tried in London on a large scale, with a different result. There is a considerable body of eccentric persons who bathe in the Serpentine daily all the year round, except when the ice is too thick. When prevented by that or any other impediment, they have no rigors, sensation of cold, or reaction; nor do they have ague, though some of them persevere in their strange ritual after sixty-five years of age, at least one that we happen to know has done so.

Dr. Alexander Smith[1] contends not only that the cause of periodicity resides in the patient, but that it is not so much morbid as the modified remnant of ordinary vital movements essential to health. And certainly it is true that periodicity is the rule, rather than the exception, in all healthy involuntary action throughout the animal organs, whether of circulation, respiration, excretion, or generation; whereas the same cannot be predicated of strictly morbid processes. And the longer the departure from health lasts the more it reduces abnormal functional action from periodicity to continuity; and the more grave and irremediable the form of tissue change is the less unevenness is exhibited in its course. It may be remarked, in passing, that Dr. Smith, like many men of action, is very much under the dominion of words. "Inhibition" and "malaria" are to him moving powers; he does not seem to feel that the first is merely a negative term, implying a deficiency of vital action, and the latter an abstraction to bring together a variety of external causes capable of existence independent of the body.

The phenomenon of protection seems to offer a scope for investigation, the results of which may be of incalculable moment. The tradition is preserved of a wonderful cow who lived among the cattle plague poison for weeks, and was even inoculated with it, but never took the disease. What has become of her? The value of the animal must be immense; for if the exemption from the plague was innate, it would for breeding purposes outweigh a thousandfold any of the points of shape or character; and if it was acquired, the knowledge of it would be still more important. Why should not the same dogged perseverance which invented vaccination succeed in discovering the laws of protection against scarlatina, typhus, &c., and invent a means of profiting by them for those who are willing?

To this end one cannot conceive a life better spent than in investigating that magnificient example of successful protection, about which the present generation seems to know scarce anything more than Jenner knew. Failure in such a cause would

[1] 'Fever and Cholera,' p. 61.

be a more agreeable retrospect than success in most others. By finding how vaccination acts, we should see our way to the invention of perhaps even more complete defences of the same sort against disease.

From the history of smallpox and vaccination, it is fair to expect that in civilised communities the most important future movements against epidemics will be in the way of personal protection by art. The removal of the sources of pestilence by united efforts and public measures cannot be carried much farther without restrictions on trade, infringements of liberty, and insults to individual dignity, which are a higher price than people can be asked to pay. Besides which, make your cordon as stringent as possible, broken it must be now and then by some trumpery oversight, and then your care is wasted, or at least seems wasted for the time. Superhuman watchfulness is needed to do more than just weaken epidemics by this means. Again, hygiene, or the art of high health, is studied and practised in England to a degree which sometimes even sacrifices interests higher than those of the flesh. We cannot look for deliverance from epidemics by its being improved, or by a wider-spread use of the pleasant remedies prescribed in the 'Counseill against the Sweatyng Sickness," by Dr. Caius, for keeping our "bodies lustye, galiard, and helthful." Such counsels were invaluable at the time, and aided by the "Book of Sports" and the example of an athletic king, contributed much to amend the frowsy habits of the previous generations and raise the national health. But they do not check epidemics. Indeed, it seems doubtful whether high health is in itself any protection against morbid poisons, and whether the escape of vigorous persons be not due to their living more than invalids in the open air, and so diluting the virus. Besides which, high health is a frail armour to trust in, lovely though it be, and priceless for more important purposes even than keeping off pestilence. An accident may rob us of it to-morrow, but only time can obliterate our vaccination.

We cannot sympathise with those who look upon an epidemic virus as a disciplinary provision of nature for enforcing the laws of purity and order. Cleanliness has no inherent merit; filth is unholy only *because* it is unwholesome; to be dirty is sinful only because it selfishly endangers and annoys our neighbour; let the health and the senses cease to be injuriously affected by decomposing animal matter or any other poison, and it ceases to be poisonous. Complete protection against the foe extinguishes it as a foe for us, and perhaps makes it a useful friend.

It is an open question whether the protection afforded by previous disease, natural or artificial, depends on the exhaustion

of some material capable of receiving the infection, or whether it be effected by the continuous presence of a quasi-foreign material. Dr. Beale suggests that the bioplastic theory brings within the scope of our understanding the latter view rather more than the former. For we may conceive bioplasts being produced which continue to give rise to others. And as long as this production of new germs by descent proceeds, the bioplasts must take up pabulum, which other disease-germs might in their absence have appropriated. In this way the fact of protection lasting only for a time can be accounted for. When these germs cease to produce descendants, or when the qualities of these descendants change, protection ends. But that matter capable of receiving a virus should be once extinguished, and then after a few years grow again, is to Dr. Beale inconceivable. The subject involves such an excursion into the unknown, that nobody should be surprised at either speculation being true, or neither.

Temporary personal protection for those who are exposed for short periods to the causes of fever is a matter which has fallen into unmerited neglect. Those who are unused to it can hardly credit that physicians to fever hospitals, who, like Dr. Murchison, have suffered several times for their boldness,[1] seem to scorn all attempts in this direction. Some proposals, doubtless, are not tried because, like mediæval armour, they are so cumbersome as to make the wearer inefficient and ridiculous; such as, for instance, the plan of visiting patients guarded by a respirator which filters the air through cotton-wool and charcoal. But there is nothing disagreeable to self or neighbours in oiling the skin (*probatum est*), or in gargling the throat with an astringent or with carbolic acid before seeing a case of scarlatina or diphtheria. And the clean-shaved upper lip and open nostrils really are not so charming as to compensate for the protection afforded by hair grown in the ancient British fashion, which, when the mouth is worn shut, must be an admirable respirator. Tobacco and camphor have long borne fame as preservatives, and tanners have been reputed by more scientific authority than the gravedigger in Hamlet to imbibe a defence against morbid decay. As the odour of these alexi-pharmics is unpleasant to many, it would be interesting to have evidence which would enable them safely to be denounced as useless, or else tolerated as disagreeable friends. There is nothing to be found in literature on the subject except opinions.

Dr. Parkin, Dr. Œsterlen, and others, use the fact of our

[1] Two of the cases reported in Dr. Murchison's essay on 'The Period of Incubation,' are reports of his own attacks of fever contracted by limited exposure.

present ignorance as an argument against continuing sundry practical precautions which the fears of our ancestors established. It is true that the precautions are troublesome, costly, and often ineffectual; still if it were merely for the sake of making observation more accurate, we think they are well worth while. Quarantine, as carried out, is pronounced a hollow ceremony. Supposing it to be so, let example be taken from the prudent housewife, who, instead of breaking up her empty bottles, refills them with good liquor. Cowards, fools, and villains are exhibited in their true colours by the prevalence of the doctirne or contagion; and one cannot but blush to read the records of this exhibition, whether collected in the pages of anti-contagionists or of naïve novelists like Boccacio and Defoe. Nevertheless, the acknowledgment that there is some ground for the idea of contagion does not create selfishness and sin; nay, rather, it drags them forth for reprobation: nor, indeed, either is virtue thereby created; but yet it is purified and braced up to a degree beyond the power of any false security. Against the brutalising effects of panic may be fairly contrasted elections in which contagionist physicians and house-surgeons struggle to be on the staff of fever hospitals; and one may point also to the impossibility of finding a nurse or a clergyman who ever shirked their duty on this ground; though it is an extreme minority that does not believe in, nay, exaggerate the danger. Man is not a timid animal, and the bolder you tell him the truth, the bolder he grows.

There are two methods of studying the subject of contagion. One is to take each separate form of disease and sift evidence as to its propagation by intercourse, direct or mediate. Another is to try and trace the physiological stages and conditions of transmission in undoubted instances, and test how far these conditions are possible in epidemics. By "undoubted instances" is meant inoculation and experimental communication. The difference between the two methods is that remarked by Hippocrates, and probably his proverbial criticism of their respective demerits cannot be bettered; for it is still as true as ever that while technical research takes a long time, and life is short in which to pursue it, yet "experience is deceptive." And it is never so deceptive as when liable to have its most conscientious collectors blinded by educational prejudices or party spirit, as is certainly the case in epidemiology. While, then, we would give every encouragement to the careful elaboration of statistics, we shall always be ready to correct them by the light of minute investigation and synthetical experiments; and by these latter we expect to count the steps of progress.

Nothing but increased knowledge of the nature of the infecting

agent can enable us to decide what are specific differences and what are accidental varieties in cyclical fevers, to anticipate a change of species under varied circumstances, and (crowning mercy) to produce that change of species by intentionally varying the circumstances. The circumstances may be varied, not only by protection of the individual, but by a change of type in the disease, even in the unprotected. The black death and the sweating sickness are known only in history; dancing manias and other infectious hysterias no longer spread like wildfire through Europe. Leprosy has apparently ceased to be communicable, and nearly died out in civilised communities. Syphilis is transferred now hardly ever through an unwounded skin, appears as an epidemic only just often enough to confirm the statement of the historians of its first outbreak,[1] and is certainly less severe in its consequences to the tissues year by year. Ague in many places, even while attacking an equal number of the population, has become less fatal, and absolutely non-contagious. While, on the other hand, we find other places where the malarial miasma seems to associate itself with a virus of human origin and become irregularly infectious, as one of the many varieties of "remittent," "relapsing," "yellow," "red" (Chevers, 1871), "gastro-hepatic" (Hunter, 1836), "bronchitic" (Adams, 1828), "jungle," "Peshawur," and "Bengal" fevers, according as the symptoms or their anatomical localisation, or the geographical distribution influence the nomenclature. Again, there is reason to believe that enteric fever becomes from time to time extraordinarily fatal, and is becoming gradually more fatal, and more frequently mixes itself up with typhus, scarlatina, diphtheria, and pneumonia; and it at times has an infectiousness which it wants at others. Now, in their action upon animals and upon savages diseases seem the same to-day as of yore; some things, therefore, that modern man has done accidentally have modified it for him. Let him find them out, and do them intentionally.

Let not the reader chafe at the many pages which have been consumed on this subject. An advance of pathological knowledge by individual labour we hold to be absolutely essential to a maintenance of the progress of civilised communities in proportion to their increase in population. In dealings with barbarous nations much, indeed, may be done by the application of what Englishmen consider truisms and almost part of an instinct. No doubt horrors such as those described in

[1] Reference is intended to the extraordinary outbreaks of syphilis at Rivalta, in 1861, and three other places in Italy, in 1814, 1841, 1856, and that recorded by M. Ricord in the 'Gazette des Hôpitaux,' 1862, in which latter, all possibility of a vaccinal origin is excluded. See Seaton in 'Reynolds' System of Medicine.'

Hunter's 'Orissa,' as occurring at the Jaganâth pilgrimages, might be abated by bringing into play very stale science. But at home we are sure that no attainable perfection of administration, no disciplinary interference of law, can check much further the ravages of epidemics. Each secret force of nature, a mythical sphinx, lion-limbed, eagle-winged, but woman-hearted, cries ceaselessly, "Read me my riddle and make me thy slave, O man, or be devoured!"

II.—Bree on Darwinism.[1]

Every student of nature acknowledges the great obligations Lamarck conferred on natural history early in this century; and those naturalists who labour in the arrangement and classification of the invertebrata constantly have to refer to his wonderful volumes.

He was essentially an observer of nature as displayed in the animal kingdom. Great series of animal forms from every part of the globe were studied and described by him. He had a marvellous amount of knowledge respecting the organization of the invertebrata, and the information he obtained of fossil forms was by no means contemptible. He had to teach and to classify. That is to say, he had to tell to the world what he believed was the truth, and to arrange animals in groups by their differences and resemblances. What Lamarck believed to be true, what he felt bound to teach in consequence of what he called his intellectual "force de choses," was not generally received as verity by his contemporaries. They admired his facts and discredited his philosophy. Posterity has misquoted his opinions, and long ere he died he was regarded as one of the dangerous French philosophers by the soberest thinkers. His positivism was decided enough, and he held that the simplest forms of living things were transparent jelly-like entities, without life in the first instance, and that physical forces operated on the structureless plasm, producing chemical action, and involving the universal structural gift of irritability. Starting with the assertion that, in natural history, arguments, observed facts and careful inductions from them can alone be employed, he insisted that animals, vegetables, and minerals, are natural productions. There was a deep meaning in this apparently self-evident proposition, for he proceeded to state that, if they are

[1] *An Exposition of the Fallacies in the Hypothesis of Mr. Darwin.* By C. R. Bree, M.D., F.Z.S., &c., pp. 418.

natural productions, they must obey and are vitalised by natural laws and forces. The only forces in nature which are appreciable are physical, and she has performed everything by the aid of groups of forces all arranged in a hierarchy of powers mutually interdependent and harmoniously combined. The term natural production and its inferences stood forth as the barrier between Lamarck's faith and that of the majority of the world, and it was his quiet protest against the doctrine of special creation and a continual overruling providence.

He believed in the instability of organic forms; and, although he has not had the credit given to him for it, he clearly stated that alterations in the physical geography of localities would of necessity produce variation in the forms of life living therein. Long and careful research and thought enabled him to offer the celebrated "laws" to the world—the laws which have led to caricatures, and yet live in the beliefs of the Darwinian school.

Life, writes Lamarck, constantly tends to increase the volume of bodies possessing it, and to extend the dimensions of their parts, up to a certain and peculiar point. "Jusqu'à un terme qu'elle amène elle-même."

The production of a new organ in an animal body is the result of a new want, which continues to make itself felt, and of a new kind of movement which this want produces and necessitates.

The development and strength and power of organs is constantly in relation to the employment of these organs. Everything which has been acquired or changed in the organization of the individual is transmitted to those who descend from it. In fact, Lamarck was the Darwin of the Napoleonic age, so to speak. He was opposed mainly on theological grounds, and the geologists of the day with few exceptions wrote against him, as they were believers in the doctrine of repeated special creations and destructions. Omalius d'Halloy, however, recognised the importance of believing in the influence of varying physical conditions upon the production of new species. Such opinions did not produce a definite influence on the philosophy of the age, for people were wearied with its irreligious tendencies, and included all speculations respecting life amongt those which ought to be deprecated. But the teaching of Lamarck bore some fruit; it had its origin in the mind of a great and truthful man, and, moreover, it was the reflexion of the thoughts of some illustrious predecessors. John Hunter, for instance, has left some fragmentary notes, which prove that he, for one, was disinclined to accept the popular belief of his day. The notion of a definite relation between altering physical circumstances and

changes in the form of animals was old enough, and hence the Lamarckian belief was just kept in existence in the minds of a select few.

The parallel of 1815 and 1873, between the days of the appearance of the introduction to Lamarck's great work, and of the last of Mr. Darwin's books is striking enough. What will be the belief in fifty years hence? Dr. Bree decides that Darwinism is going out, and labours, therefore, almost unnecessarily to hold up its theories to the obloquy of the "thoughtful." Suppose that the next revival should be in the same ratio as the present bears to that of the days of the French philosopher. This possibility is, perhaps, what has led to the publication of the work now under consideration. It is to crush the heresy in the bud, and, perhaps, it may influence future unbelievers, for who would care to bring forward a new philosophy with the prospect of being subjected to such a vigorous philippic. Whether Dr. Bree be right or not in his estimate of the decay of the theories generally embraced under the term Darwinism, it is certain that they are held by a great number of men who are admirably adapted by their education and experience to think for themselves on a point which is settled for the whole of mankind by those who are not so learned and experienced. It is quite impossible to dispose of a theory held with more or less modification by such Englishmen as Hooker, Owen, Huxley, Mivart, Parker, Carpenter, Ramsay, Flower, and Lyell, and which is taught openly in some of the most important schools by no mean naturalists, by referring to the authority of Agassiz, Flourens, and Haughton, whom Dr. Bree quotes as inveterate opponents of a "cold, unsound, unphilosophic degrading system of assumed probabilities." One authority is as good as the other. It is necessary to select that theory which is most consonant with acknowledged facts and reason, if there is to be one at all; and if any one hypothesis offers a greater credibility, it should be received for the nonce.

What are the theories of the origin of living forms, of the succession of species on the earth, and of the cause of distributional provinces? for all these matters must enter into our consideration. They may be paralleled with those of the days of Lamarck, and in the abstract may be divided into those which refer to the direct and repeated operations of the Creator, and to those which acknowledge the action of the Creator by secondary laws.

It is not right to assume the existence of a third series in which matter is admitted to evolve itself, and to proceed by virtue of self-creating forces to produce the diversity and harmony of the universe. This phantasy exists simply in the minds of

the fabricators of those men of straw who are so ably disposed of on public occasions.

Practically, the issue has to be decided between the advocates of the belief, as Dr. Bree terms it, of special creation, and the holders of the hypothesis of evolution, including so-called Darwinism.

The attempt to specify these theories is instructive enough, and sufficiently humiliating. It affords little pleasure to those who have been accustomed to induce upon facts the result of long and conscientious labours; although it appears to be exciting and inviting to those who have but an elementary knowledge of nature. The ideas involved in the one theory shock the humble student, who is impressed with the vast distance between the method and matter of his thoughts and those of the Almighty Creator of the universe; and the opinions included in the other are too often suggestive of the desire not to acknowledge the wisdom and care of the Ruler of all things.

Their consideration proves our ignorance to ourselves, although, when they are debated upon in public, the ignorant are attempted to be impressed with the vast acquaintance of the leaders in the struggle with the mysteries and facts of nature. It is inevitable that there should be these disputes, for the desire to know the whence and the whither becomes stronger as man becomes more intellectual; but it is most probable that when the rival beliefs are calmly criticised a century hence there will be no small indignation expressed at the uncharitable assumptions of the opponents of the doctrine of evolution, and at the guesses of the advanced party in natural science. We have never read a careful exposition of the belief of those savans who hold the theory of so-called special creation. Even Dr. Bree, who holds it against everything relating to evolution, does not give us a detailed account; but what is involved by it may be gleaned from the work before us, from many theological brochures, from such works as the 'Bridgewater Treatises,' Hugh Miller's writings, and the 'Transactions' of the Victoria Institute.

The belief must not simply account for the existing state of things, but for the vast past. The creature and the plant were created as they are and were by the Divine will—all perfect and admirably suited for the definite physical conditions under which they were to live. A species or kind thus brought into the world is a fixed quantity; it has no potentiality for variation in order to meet changing conditions, and all variation is artificial or is a monstrosity.

Species die out with altering physical conditions, not accidentally, but from design. Yet these units of nature are bound

together in a scheme, for they have resemblances, and their structures are often homologous. Every slight difference of anatomical arrangement is due to the direct operation of the Creator. During the successive geological periods all the species of the world have died out, and new creations have taken place. Some species have lived longer than others, and amongst these were introduced new forms to fill up gaps and to establish a new fauna and flora. Every organ was fashioned with a view to meet a particular exigency and use; it was made perfect at once, and design can be traced readily enough.

The phenomena of instinct are gifts especially conceded to each organism. During the progress of the geological ages there were constant changes of climate and of the relative position of land and sea, and when there was a state of equilibrium and *statu quo* in one locality, vast changes were in progress in others. Dr. Bree writes, "Thousands of new eventualities have to be provided for, and each of these eventualities must be an act of special creation," and this quotation refers to such changes in the organic world as the palæontologist insists there have been in consequence of altered physical geographical conditions. The geographical change occurs by law and direct interference, and new organisms are at once created "according to the necessities which the great scheme of the Creator demands."

The difficulty of applying such a theory to any part of the history of the world is immense, unless it is believed that a supernatural and miraculous state of things environed the newly created forms to preserve them from their natural foes, and to adopt their instincts properly.

On the other hand, the evolutionist of the school of Darwin, Huxley, Herbert, and Wallace, asserts that the first organisms created by the Maker of all things were endowed with certain potentialities or innate powers, which obey secondary or indirect laws. The law is the expression of the will of the Creator, who is presumed to act once and for all, so as to produce the succession of changes in the inorganic and organic kingdoms.

Häckel writes that every organism has a gift or force which enables it to reproduce others like unto itself, and a second which enables it to change its habits and construction gradually during successive generations, under the influence of new and changing external conditions.

Animals and plants vary slightly, owing to the influence of external conditions; and if these variations in structure are for the benefit of the organism, they are transmitted by hereditary descent. All things being the same no variation will occur, but changes in the physical geography of a district involve

alterations in the method of life of many of the organisms, and either they are unable to vary and therefore die out, or they vary and produce what are called races, permanent varieties or new species. The species is an abstract idea; it is not a reality; it is a conventional unit, which is necessary for scientific purposes; or it represents the sum of the variability of a group of forms. Instincts are acquired, and are transmitted.

The evolutionist regards the forces of the inorganic kingdom as allied to, representative of, and analogous to, but not identical with, those which produce the phenomena of life. He acknowledges plan and design in nature, and that every possible permanent variation is for the benefit of its possessor.

Dr. Bree considers that Darwinism is on the wane, and that Dr. Hooker was wrong in asserting that it held a position in the estimation of many great naturalists; and, certainly, if his extraordinary notions of what Darwinism and evolution are correct, he is right. It is useless to deny that a theory or hypothesis or an ill-defined series of conceptions termed evolution is credited by the majority of the rising naturalists and palæontologists of England and Germany, although it is quite clear that Mr. Herbert Spencer's writings are in advance of the thoughts of most of them.

Men who are sober-minded enough, and who have studied and described forms from every geological formation and from many parts of the world, and who are no mean students of existing nature, are to be found, like the late illustrious Pictet, gradually giving up the doctrines of special creation, and siding with Mr. Darwin with greater or less modification of his views. To assert with Dr. Bree that these men are "blinded by the gross materialistic doctrines of the new school," or that they belong to the "lesser lights, who glean their knowledge of science from the pigeon-loft," is unwarrantable, as unwarrantable as his assertion that the school of evolution—of the men who are named above —"is founded upon the operation of physical forces and secondary laws without the supervision and guidance of an exterior power." If he had written—without the constant interference—there would have been some reason; but the author of the "Fallacies" forgets the concluding passage of Darwin's 'Origin of Species':—"There is grandeur in this view of life with its several powers, having been *originally breathed by the Creator* into a few forms or into one; and that whilst this planet has gone cycling on according to the fixed laws of gravity, from so simple a beginning, endless forms, most beautiful and most wonderful, have been and are being evolved."

Throughout the work Dr. Bree sneers at the materialistic and infidel tendencies and beliefs of men who do not hold

his conception of the origin and persistence of life on the globe, although he professes to dismiss the *odium theologicum.*

Here we have, then, the days of Lamarck almost brought back again. The old weapons are brightened up, and the new theory is but slightly in advance of the older.

Dr. Bree professes to meet his opponents scientifically, and to dispose of them *secundum artem,* and whether he does this satisfactorily or not can, of course, only be decided by a critical examination of his arguments and proofs.

We were struck, on opening the book accidentally at page 316, with a sentence which refers to the changes in the egg of Ascidiæ:

"And yet what are these observations? Simply changes supposed to have been seen in a small egg, in salt water, in a watch-glass, *through a microscope*, one of the most difficult and uncertain means of observation, requiring constant attention, with every chance of error in microscopical investigation, intensified by the circumstances of the case. The microscope is open to sources of error recently unsuspected."

He refers by a note to Royston Pigott's researches.

Now, for whose benefit was this rather involved paragraph written. Certainly not for any one who is competent to give an opinion on the question at issue. He reflects on the truth of some distinguished anatomists by employing the word "supposed," and he over-estimates the difficulty of microscopic investigation in skilled hands by referring to researches which relate to the distorting effects of badly constructed lenses.

This is slipshod science, and its employment by Dr. Bree makes one doubt his knowledge on some other points and his powers of appreciating the true value or the reverse of some of the opinions he quotes against Mr. Darwin. For instance, he brings forward Agassiz very constantly, and believes in everything written by him. He quotes as follows (p. 356):

"At all times the world has been inhabited by as great a diversity of animals as exists now, and at each period they have been different from those of every other period. Thus, in the lowest of all geological fossiliferous strata, the Taconic, we find remains of animals belonging to all the four great kingdoms of nature, including the Vertebrata; and what is still more remarkable, we have representations of all the classes of the first two kingdoms, and, with the exception of insects—which, however, may yet turn up—of the third class."

This quotation from Agassiz proves at once and for ever, to any practical geologist, that Dr. Bree's knowledge of the

"lowest of all geological fossiliferous strata" is sadly defective. Emmons fancied he had discovered a grand system of rocks in the State of Maine, which underlaid the Silurian formation, and believing that what was true in one locality must be true in all, he endeavoured to establish a system—the Taconic. But the distinguished state geologist Hall, the most careful surveyor Sir W. Logan, and the author of 'Siluria' Sir Roderick Murchison, were not able to agree with Mr. Emmons, and at the present time all the American geologists believe that the Taconic schists and slates are the metamophosed representatives of the Lower Silurian group. Now, there is an immense depth of fossiliferous strata beneath the Lower Silurian, and the Taconic system contains fossils which are the remains of a marine fauna, which lived and became extinct ages after the existence of the wonderful fauna now found embedded in the Cambrian rocks below.

As to vertebrate remains being found in the Taconic rocks, it is a gross mistake, for the oldest known evidences of the vertebrata are distinguishable in the Upper Silurian formation. The oldest known fossil fauna is that which was described by a hard-working general practitioner, Dr. Hicks, from the Longmynd group of rocks of the Lower Cambrian of South Wales. The classes contained in it, and which can be clearly defined, are Pteropoda, Brachiopoda, Trilobita, and Spongida, and there are markings which are presumed to have been made by Annelida; that is to say, the Articulata, Mollusca, and Protozoa, are present. In exposing the glaring error into which Dr. Bree has been led, it is necessary to remark that it is perfectly useless to attempt to extinguish Darwinism by the employment of any other than first-class weapons, and that the doctrines of special creation are not benefited by mistakes.

Geologists are impressed at the present time with the importance of the researches of Huxley in the group of extinct reptiles, of which the huge Iguanodon, so suggestive of the memory of Dr. Mantell, forms an important species. The distinguished American palæontologists, working in the same field, arrived almost simultaneously with Huxley at the same conclusions. These were also in the mind of Phillips of Oxford, and are now received as correct, not because they are ideas simply, but because the anatomical facts upon which the classification of the Ornithoscelidæ depends can be seen by any anatomist who knows a little of the bones.

The pelvis and hind limbs of the Cassowary, of the Iguanodon and of the Crocodile can be examined readily enough, and there can be but one opinion respecting the intermediate nature of the Iguanodon and other Dinosaurian parts,

so far as those of the Struthiose birds and Crocodiles are concerned.

Considering this as an advanced fact, which is in favour of the theories of evolution, what does Dr. Bree write about it? To the surprise of those who understood that this work was to be a scientific refutation of Darwinism, Dr. Bree writes (p. 9):

"Professor Huxley writes a book to show that man has been evolved from the ape, and has latterly wasted much of his valuable time in writing and teaching the transformation of reptiles into birds."

Again, the humblest student of comparative anatomy will stand astonished at the following extraordinary description of Amphioxus:

"This creature looks like a piece of jelly, but it is found to have a rudimentary nervous system, and something which is presumed to be a vertebral column, but which is a simple cartilaginous rod with a *chord* of nervous matter above it." "This organism's place in nature has hitherto been considered doubtful, but the Darwinian and evolutionary dogmas find it a useful means to leading from the vertebrate to the invertebrate sub-kingdom. So important is it considered by Häckel that he wants to place this jelly-like creature in a separate order."

It does not require much knowledge of the subject to be able to prove that the author is not versed in the anatomy of the lower vertebrates. The employment of the expression, "a simple cartilaginous rod, with a *chord* of nervous matter above it," leads to the supposition that the author is in doubt as to the meaning of a notochord —of the structure which reaches the whole length of the body of Amphioxus. This has no vertebral centra, and no one ever said it had, or that the fish had a vertebral column. The spinal *cord* overlies the notochord, and is in its turn covered by a series of rod-like bodies; the animal is not doubtfully placed by comparative anatomists, and never was so after its anatomy was made out. Proceeding with his anatomical criticisms, Dr. Bree informs his readers—

"that the Lepidosiren is an amphibious reptile, in which is found the first-appearance of a lung. This remarkable creature is called amphibian, not because it can move about like a frog and live in water or on land, but owing to the fact that it has the power of remaining dormant, buried in dry mud for six months, and when the water is renewed, become active and lively again."

This is all that is to be gleaned from Dr. Bree about Lepidosiren—from the sharp critic of Huxley, Owen, and Mivart. What will Günther say to this terrible snub of his Dipnoi? It is usually taught that Lepidosiren is a *mudfish*, and that with an eminently piscine arrangement of the limbs and spinal

column (notochord and sheath), it has fin-rays like the Elasmobranchs, and internal as well as external gills, besides lungs. Instead of being one of the amphibia, animals now termed so, not from their habits, but from their anatomical construction, it is just one of those strange forms which link together the great groups of the amphibia and pisces. Surely Amphioxus and Lepidosiren should have received elaborate examination from the writer of a book which professes to dispose of the doctrine of evolution scientifically.

These shortcomings abound in the book, and assertion too frequently takes the place of logical reasoning in it. Thus, it is announced that in Chapter II, Professor Tyndal's views are disproved, in reference to those which met with such little countenance at Norwich in 1868.

But on carefully examining the report of what Tyndal did say, and on comparing it with Dr. Bree's quotations, it will be found that they are so disconnected as to convey the impression that the distinguished physicist believes that crystallization and vital force are identical. Dr. Bree did not quote—"The problem of the connection of body and soul is as insoluble in its modern form as it was in the pre-scientific ages." "On both sides of the zone here assigned to the materialist he is equally helpless. If you ask him whence is the *matter* of which we have been discoursing, who or what divided it into molecules, who or what impressed upon them this necessity of running into organic forms, he has no answer. Science is also mute in reply to these questions." Instead of clearly understanding that physical forces act in minerals and in the bodies of members of the organic kingdom, and that there is a something else, inexplicable as yet, which operates besides in living things, the author jumps at the conclusion that Tyndal means nothing of the kind. He writes, further on, "Without wishing to derogate from the well-earned fame of Professor Tyndal. I must take exception to his statement, that the correlation of physical and vital force is a proved fact in science; and if it be not proved, then clearly it is wrong to make it the basis of a philosophical argument."

There is no such statement by Professor Tyndal in the address criticised, but in common with every observer of the simplest phenomena of life he assumes the proposition as an axiom. What does correlation mean? Things what are correlative are those which have mutual relations. Are there any mutual relations between the forces which are manifested in organic and inorganic nature? Is there such a thing as heat being evolved from organisms, or light, or electricity, or motion, under the influence of that vital action of whose ultimate nature we know nothing?

Can an organism exist without the heat and light and chemical influence of the surrounding inorganic world? Are not the forces of both kingdoms inter-dependent, and, moreover, ruled, by laws clearly emanating from the one great first cause?

Dr. Bree knows all this, but he confounds the idea of correlation with that of identity. Instead of disproving Tyndal, he adds to the confusion of an inexplicable subject.

The following is Dr. Bree's account of the germination of a wheat-grain:

"It (science) tells us that in that grain of wheat there is a germ having life; that this living germ, if supplied with food, is in a position to grow, which a crystal never does. Science also tells us that the germ being within the seed, cannot at first obtain a supply of food from the atmosphere or soil, but that by the combination of heat, air, and moisture, the molecules of starch which the embryo *cannot* absorb are converted by chemical decomposition into molecules of sugar, which the cells of the embryo can absorb. The embryo then grows. It sends down into the earth delicate rootlets, which are tubes covered at their extremities with a porous membrane. It sends upwards a stem, which has a peculiar structure, upon which the existence of the world depends. It is necessary that that stem should be so coated with flint that its texture may not be too brittle and be broken by the wind, or too soft to stand up as it rears its ears of grain above the earth. For this purpose the cells of the stem *select* from the watery mineral solutions brought up from the earth by the roots a silicate of potash, and then the cell further decomposes the silicate, and places or posits the molecules of flint in regular workman-like order in the coats of the stem."

He then challenges Tyndal:—"Now, will Professor Tyndal contend for a moment that this vital act of selection is performed by 'tremulous motion,'" like the molecules of a crystal? Granted that the molecules in each case may be posited in a similar way, so as to act in like manner upon the rays of polarized light, why not? Is there not order in the universe? Are not like effects produced by like means?" Tyndal, like most physicists, agrees that heat is tremulous motion, and is by no means likely to contend as Dr. Bree desires, but probably, after reading the sentences just quoted some botanists may feel disposed to regret that their writings have been woefully misunderstood.

Mr. Darwin insists upon the important bearing of embryology in relation to the theory of descent with modification. He believes that the embryo is the animal in its less modified state, and that so far it reveals the structure of its progenitor. In two groups of animals, however much they may at present differ from each other in structure and habits, if they pass through the

same or embryonic stages we may feel assured that they have both descended from the same or nearly similar parents, and are, therefore, in that degree closely related. This community or embryonic structure reveals community of descent.

These opinions must be met by any opponent of the theory of evolution with careful argument, if they are to be disproved at all; and yet Dr. Bree, after referring to Von Baer, or, as he terms him, Van Baer, Herbert Spencer, and Huxley, simply writes as follows:

"These are the facts discovered by Van Baer; but Mr. Spencer adds, with truthful candour, what the student will readily infer for himself, if he will consult Mr. Huxley's figures [those of the embyro of man and the dog]." The reader must also be cautioned against accepting this generalisation as exact. The likenesses thus successively displayed are not precise, but approximate. Only leading characteristics are the same, not all details. "But" (adds Dr. Bree) "exact or not, the evolutionist and Darwinian draw much capital from these facts. They say that this similarity in an embryonal condition between the different phases of development in the animal kingdom indicates an intimate connection of structure which has resulted from an original community of origin, and that each animal has throughout countless ages been differentiated into their several now existing forms by arrested development, natural selection, and the struggle for existence. *But what real force is there in this community of origin?* Literally *none whatever* [the italics are not Dr. Bree's]. In the first place there is a real and significant difference in the embryo of man as compared with brutes, from the earliest period of structural existence."

Naturally the anatomical readers of the 'Fallacies' will linger over this fact of Dr. Bree's, and eagerly expect a demonstration of the error they had apparently fallen into from their misconceptions respecting the analogies of the mammalian and human embryo. We are told—

"The first indication of structural growth in the embryo of the mammal is the formation of a nervous axis, which has the appearance of a double chord, one portion of which is the rudiment of the future nervous system, the other that of the alimentary canal, and between these there appears the first rudiments of the skeleton in the form of a gelatinous cylinder, in a membranous sheath, called the notochord or chorda dorsalis. Now, this notochord developes two plates—one the neurad to enclose the nervous axis, and the other the hœmad to enclose the vascular axis. But the notochord of man can at this early stage be at once distinguished from that of brutes by the position of these plates, the neurad being backwards in man and upwards in brutes, the hæmad being forward in in man, downward in beasts. . . . . For obvious reasons, it is impossible to investigate this subject in man as it ought to be; but the instance I have given is sufficient to upset the whole series of

arguments and deductions which have been drawn from this supposed resemblance."

Dr. Bree proceeds—

"That there is a similarity in the external appearance of the various phases of the embryo of the highest mammal to that exhibited by other animals below him in the scale, as shown by Van Baer, is undoubted. But what of this? It merely exhibits a unity of plan, which is one of the grand characteristics of the organic world. All organisms being formed by the addition of new matter, their embryonic condition is similar as the foundations of a cottage and those of a castle are similar."

Dr. Bree's notions about the development of the embryo are on the same par as his zoological ideas. He tells us that

"The lowest (grand division) or *protozoa* and cœlenterata are formed upon the plan of radiation, that is, the different parts of the animal radiate from a common centre. The mollusca are created upon a plan totally distinct, soft bodies protected by shells and formed symmetrically. The plan of the annulosa, including the lobster and insects, is that of annulation. The body is more or less made up of annular segments or rings; hence the name. These segments in insects are perforated with holes for the purpose of respiration, while in the lobster that function is performed by gills."

In page 201 the non-geological reader is informed that the Devonian system is "at the bottom of the great secondary series of strata," although classificatory works tell geological readers that the Permian and the Carboniferous series intervene. These last readers will be surprised to find the "great researches displayed by the magnificent works of the Palæontographical Society," associated in an argument against evolution. To those who read as well as possess these volumes the so-called heretical opinions of many of the contributors should be well known. Palæontologists will be disagreeably surprised at Gandry's deductions from his elaborate investigations of the Pikermi fauna being termed rhapsodies; but when they read (p. 379) that "there is no evidence in nature of birds with imperfect wings," they will probably consider that M. Gandry has been misunderstood. Oddly enough Dr. Bree does not combat Mr. Darwin's very strong arguments, which are founded on the relation of rudimentary and useless organs to ancestral forms. He is, however, an able advocate of unity of plan and design, and, using Haughton's admirable lectures as his groundwork, urges the incompatibility of evolution with those evidences of Divine power.

But are unity of plan and design opposed to the hypothesis of

Darwin or that of evolution? When an animal is adapted for its peculiar method of life the influence of design is asserted, and when certain organs are very similar in very different kinds of organisms, unity of plan is believed in. A very able writer asserts that if a new writer desired to compile the most elaborate and convincing series of design arguments, he would have recourse to the Darwinian armoury for the most striking of recently ascertained facts. The same writer suggests that the existence of a supreme intelligence, endowed with corresponding power, logically excludes contrivance in the human sense of the word. Skill is the quality of a finite being who has to overcome difficulties, and difficulty and skill cannot be found in the operations of nature regulated by a first cause. We may look for design and for wisdom, but not for any quality which would reduce Divine operations to the level of human ones. There is a constant danger of suggesting that the Divine Creator has to meet difficulties in the inorganic kingdom, and to arrange organic nature accordingly, if the teleological argument so forcibly urged by Dr. Bree is not very carefully handled. He is apt to be measured by finite understanding. How can man estimate the mysteries of the Creator correctly? We can only judge according to our finite intellectual capacity. But there is, or rather there was, a great school which inculcated final causes; its professors could see, or fancied they saw, the reason why everything was done.[1] They collected together a great mass of information concerning special adaptations, and it was assumed that no organ or portion of an animal not deformed was without its special use to that particular creature. But plain and palpable facts did not sustain the universal application of the theory; yet there could be no exceptions if it were true. The rudimentary organs, so useless in numerous organisms, were found quite beyond the application of the teleological argument, and hence final causes have been classed by Huxley as barren virgins.

Doubtless there is a law which regulates those variations of form which are credited by Mr. Darwin. How can we assert that there is no design in the production of racial variations? Agassiz, so frequently quoted by Dr. Bree, insists upon the negro and the white being different species, yet every anthropologist of eminence credits the origin of man from one pair. There is clear design in the constitution of those black people who alone can exist and reproduce in the tropical valleys of Western Africa.

It does not appear that the teleological argument disposes of

[1] See an essay on "Darwinism and Design," 'The Student,' 1869, p. 270.

Darwinism, for it is prone to and full of error, and the modifications of form presumed to occur may still be ruled by Divine law.

The doctrines of Mr. Darwin and the more advanced hypothesis of the evolutionist are parts of that method of thought which has been noticed at the commencement of this review as flourishing for a time early in this century, and which has never quite been lost sight of. They are explanations of natural phenomena which commend themselves to many of those men who are singularly qualified to investigate them. Full of contradictions, defective in continuity, and ill-supplied with positive proofs, they depend mainly on probable evidence.

But is the belief inculcated by Dr. Bree any more satisfactory to the competent investigator? That there is a beneficent Creator and Ruler of the universe is evident, and most men can testify that a Providence has been on their side at some time or other; but although it is possible, still it is improbable, that certain organisms were especially created to meet particular modes of life, and that when the external physical conditions were altered they were systematically destroyed from off the face of nature. Special creation cannot be demonstrated, and, like the other theories which suggest the action of secondary laws, it can only be offered as a probability.

Dr. Bree thoroughly believes in what he writes, and, therefore, many excuses may be made for inaccuracies; but his book is only likely to be read by persons not conversant with natural history. Such readers will be impressed with erroneous ideas concerning the relation of modern science and revelation, and this is greatly to be deplored. Any book which is to expose the shortcomings of the hypothesis of evolution must be free from errors of detail, must be logical, charitable, and original.

III.—Duchenne on Electricity.[1]

It were impossible even amid the small army of workers now engaged upon electro-therapeutics for modern physicians to forget what they owe to the admirable labours of Dr. Duchenne. The claims of many rivals, while they cannot supersede those of Duchenne in our memories, may, nevertheless, lead us to forget how much we really owe to him as the founder of anything like applied electricity in the field of medicine. Even well-in-

[1] *De l'Electrisation Localisée.* Par le Dr. DUCHENNE (de Boulogne), Paris, 1872.

formed men may scarcely now realise the utter chaos in which the whole subject of electro-therapeutics was found when Duchenne began his labours, and may, therefore, scarcely realise the magnitude of the debt which we owe to this indefatigable discoverer. Duchenne, like many leading men, is easily criticised. He is often verbose even to tediousness; he is often narrow in his views of practice, and often, perhaps, still narrower in his philosophical conceptions, so that he presents many a vulnerable joint to the enemy; but these are faults inseparable from his character—"les defauts de son esprit"—faults which seem almost essentially to belong to his virtues. Duchenne, nevertheless, is really to be called a genius, and he has those chief notes of genius, enthusiasm, great capacity for work, and dogged industry; while, on the other hand, like all others, or nearly all others who have possessed genius and strongly marked characters, he presents what we are pleased to call faults, but which are the stronger shadows of the brighter light. Taking his character as a whole, and speaking as one of his old pupils, I know not any master for whom as a teacher I have felt more esteem and regard than for Duchenne; nor one who inspires his pupils with a truer enthusiasm for the most accurate kind of work. In the course of a quarter of a century electro-therapeutics may be said to have taken its birth and growth as an applied science. When electricity was first discovered, and electrical machines first made, great hopes of its value in medicine were naturally entertained. But the first efforts were ill-directed, and the earlier results, therefore, disappointing; the means for obtaining electric action were also cumbrous and out of the reach of most persons, so that buoyant hopes sank with a good deal of alacrity into the waste waters of indifference. Physicians and physicists, such as Humboldt, alike investigated the matter to some extent, but added little of value to our medical knowledge. Electro-therapeutics, as a branch of applied science, dates really from the time when Faraday placed the induction coil in the hands of Duchenne, and when Duchenne, shunning all vague and indefinite exploration, gradually elaborated his method of localised electrisation. This method is now, and probably will always be, the chief means of applying both the induced and the continuous currents, for it admits of far greater precision, both in the use of the means and in the estimation of its effects, than any other. At the same time we may fairly admit that general electrisation, whether galvanic or faradic, may likewise have its uses.[1]

[1] *Vide* the recent treatise of Drs. Beard and Rockwell, of New York.

Perhaps the most remarkable evidence of the refined precision both of Duchenne's method and of his manipulation is to be seen in this, that his therapeutic researches have led him by the way into anatomical discoveries of substantial importance, and into many new views of anatomical relations which are scarcely less valuable. Besides these, his electric investigations have also thrown no little light upon certain conditions of parts in various pathological states, the true understanding of which is of the first necessity both to the physician and the surgeon. That in addition to these discoveries Duchenne should have enriched our classifications by his well-known descriptions of more than one malady previously unrecognised, such as locomotor ataxy, glosso-laryngeal palsy, myosclerotic palsy, while on the one hand it proves the value of his method, on the other hand it is full of credit to him as a clinician, and elevates him above the ranks of mere specialists. As a clinician, again, Duchenne has not only described new maladies, but he has likewise brought out with much acuteness and success the obscurer features of maladies previously known. Of these labours and their well-won results the present third edition of his treatise on localised electrisation is a monument; we have only to regret that its appearance has been delayed and its accuracy endangered by the sufferings of the distinguished author, who is the subject of a tedious and painful disease. As a guide to one branch of electro-therapeutics, this volume, though more bulky, perhaps, than was really needful, is not only an adequate guide, but is the only complete and trustworthy guide. We lately noticed the first part of the excellent English edition of it which is appearing under the able editorship of Dr. Herbert Tibbits. As regards other branches of electro-therapeutics, and in respect of the uses of the continuous current in particular, the volume is almost wholly deficient. For this part of the subject the student must betake himself to other sources.

The first chapter of the present work deals with the general properties of electricity regarded in its medical aspect, and chiefly with the properties of the induced current, contrasting especially and almost too minutely, as it seems to me, the effects of the primary and secondary coils. But to this point we shall return. The second chapter introduces and describes the method called localised electrisation. This chapter is, of course, the corner stone of the whole work. The third chapter is concerned with historical and critical considerations of the chief methods of electrisation, such as electro-puncture, the reflex method, localised faradisation, galvanisation by interrupted currents and by direct or indirect continuous currents. This chapter in its latter portion leaves much to be desired, and

deals with the alleged values of galvanism by continuous currents in a prejudiced and comparatively uninformed spirit. Chapter the fourth deals with the various kinds of apparatus which are in use for medical purposes, and concerning which we have nothing to say in this place. These four chapters make up the first part of the work, which is, therefore, mainly technical. The second part of the book is that which has the chief interest for the reader of to-day, as it contains the chemical researches, the electro-pathological, electro-therapeutical, and the so-called "orthopædic." This part contains most interesting chapters on palsies consequent upon traumatic lesions of mixed nerves, their complications and consequences; on the atrophic spinal palsy of childhood; on the acute spinal palsies of the adult; on subacute spinal general paralysis; on progressive muscular atrophy in infancy and in adults; on glosso-labio-laryngeal palsy; on pseudo-hypertrophic or myo-sclerotic palsy; on progressive locomotor ataxy; on lead palsy and the so-called "vegetal palsy;" on palsies from cold; on hysterical muscular affections; on cerebral and cerebellar affections; on palsies of muscular and articular sensation; the "muscular sense" or faculty of movement independent of vision as he puts it, and electro-muscular contractility; on disorders of sensation and of the senses; on muscular contractions and palsies of the head, thoracic, abdominal and genital organs; on local palsies and contractions of the muscles of the limbs, spasms and functional palsies; on functional spasm and muscular weakness, and, finally, on what has been called "prothère musculaire physiologique;" that is, the varied and ingenious apparatus of many kinds which the author has devised, firstly, to supplement as far as possible the individual and voluntary action of palsies or atrophied muscles by re-establishing or aiding the natural movements; and, secondly, to prevent or oppose deformity in joints by balancing the "tonic forces" which preside over the normal relations of their surfaces. All these chapters are full of interesting matter, but it is impossible for us to pretend to survey even any considerable part of them.

Taking both parts of the treatise together, it will be seen that Duchenne, in his devotion to faradism and in his comparative neglect of galvanism, has nothing to say on many points, both physical and physiological, which to the modern electrician are of the highest interest. Concerning the present state of opinion on important questions, such as electrotonus, or the laws of contraction, on which such contradictory statements are recorded, Duchenne helps us but little if at all. He will say, perhaps, and say reasonably, that he has worked, in this respect at least, not as a physiologist but as a practitioner.

At the same time it is a decided loss to modern physiological medicine that the wonderful ingenuity, dexterity and patient observation of Duchenne have not been imported into discussions which are now too often marked by qualities the very opposite. Again, there are no questions which to myself seem to have more interest and importance than those which concern the relative resistances of the several tissues and states of tissue, and which seek out the paths of least electrical resistance in the body. Such researches as those of Erb, who seems to have proved that a continuous current may be passed in any direction through the head, are examples of the kind of research now much needed. What value, if any, are we to attribute to the frequent and careless language of those who talk with the easy confidence of experts of galvanizing the sympathetic in the neck, and of influencing from thence all and any part of the system however distant? These questions must be faced and carefully worked out, but Duchenne gives little aid towards their solution. He repeats for us with his usual minuteness the methods of overcoming the resistance of the dry skin, which method is the foundation of all his subsequent work; and he gives us again his somewhat too categorical, though probably true assertions, that the current thus introduced into a muscle is, or may be, limited therein, and in particular finds no reflex reinforcement by way of irritated spinal centres. Another difficulty which presses for a solution is the value of the direction of the galvanic currents. Opinions the most conflicting are held on this primary question, such men as Benedikt holding views which are denied point-blank by others, such as Brenner and those of his school. For my own part, Brenner's researches appear to have a very great importance; and whether we are or are not to follow him exclusively in his views on the direction of the currents in the human body, he has nevertheless distinctly proved a series of propositions concerning the different effects of the two poles, which, as I have said before in this review,[1] seem to command for the polar method in the future development of electro-therapeutics a place almost as great as localised faradisation has already obtained. Among the points which are thoroughly discussed by Duchenne is the well-known comparison between direct and indirect muscular faradism, a question which has provoked a good deal of disputation and rather tart writing, more especially between Duchenne and Remak. It was certainly my own impression that in some sense Remak had the better of it, but in the present volume Duchenne's farther statements seem to confirm his own positions. Shortly put, the matter lies thus—muscles can be faradized directly or immediately by the application of

moist rheophores to the moistened skin which covers them. Muscles can also be faradized indirectly or mediately by placing the moistened rheophores on the moistened skin at other points, which may be outside the muscle altogether, and which may often be far away from it. These points, which in the first instance at any rate, seem to have been empirically determined, have been mapped out by Duchenne himself, by Remak and most minutely by Ziemssen. Remak then claimed for himself the farther discovery that these select places are places where the nerve supplying the affected muscle lies near the surface, and he supported his assertions by anatomical demonstrations. Duchenne contends, in his turn, that the select places include no doubt the points of best accessibility of the nerve-trunks, but as a group are far from conterminous with these. If, indeed, says Duchenne (p. 85), I sometimes placed my rheophores on she level of the points of immersion and emergence of the muscular nerves in order to call forth the contraction of each muscle in the mass; sometimes, also, I departed from these points of immersion in order to obtain the isolated action of their several bundles, which often indeed constitute so many independent muscles.

It was not, he continues, sufficient therefore to mark, as has been done, the points of immersion of muscle nerves, but it was necessary also, as I have now illustrated, to indicate precisely the points where the rheophores ought to be placed when it is desired only to put in action one of the portions, or indeed one of the fascicles of large or fasciculated muscles; which points are not always easy to find: I have sometimes been obliged to seek them *empirically*, as Remak would say. Plates therefore upon which the points of emergence and of immersion of the muscle nerves are indicated by marks are certainly useful, but they are not sufficient from a practical point of view; in fact, it is necessary to choose some given standard points, and to define the average distances which are found to exist between such standard points and the select points. I have quoted, as an example of this, the faradisation of the extensor communis digitorum or of its bundles. So far Duchenne, and in a question of this kind no living authority can easily compete with him, either as an anatomist or as a manipulator. He quotes a passage from his edition of 1855, stating his intention of publishing plates showing the select points as Ziemssen has since done, and in a note says, that since that date he has been incessantly engaged in the publication of new electro-physiological researches. "Pourquoi donc," he adds, rather pathetically, "me reproche-t-on si amèrement de n'avoir pas encore fait ce tableau synoptique? Je ne puis cependant doubler mon existence." A glance at the por-

tentous list of the author's publications appended to the present volume gives at least a justification of his complaint. It is certainly correct that Ziemssen, whether right or wrong, has pinned his colours to the belief that the select points are always the points of immersion of the nerves, and that the marks on his plates, which I have now before me, correspond, as he admits, to these points and to these only.

Another inquiry of great importance in therapeutics is that raised by Duchenne many years ago concerning the supposed differential properties of the two coils. Duchenne contended that the two coils are endowed with properties so distinct that neither physiologically nor therapeutically can the two be used indifferently. This, he added, is no accident, nor is it any question of more or less, but a question of wholly different qualities. For instance, he stated that the current of the inner coil has a greater influence over sensibility and contraction in parts under the skin, while the current from the outer coil or secondary current, as we call it, exercises its influence more especially upon the sensibility of the skin itself and upon the retina. The kind of inference to be drawn by the therapeutist from this was of course tolerably obvious. Duchenne did not and still does not profess to give any explanation of these definite peculiarities. Now Rosenthal, in the second edition of his 'Electricitätslehre" (1869), accounts for this apparent inconsistency, not by the formulation of any essential difference between the two coils but by an accidental difference in their tension. Rosenthal points out that the secondary coil of Duchenne's instruments, and of all instruments in their degree, is composed of a wire so fine and so long that its tension, as compared with the short thick wire of the primary coil, is excessive, and that to this excessive tension it owes the variation of its results. It appears from the pages devoted by Duchenne to this matter that Becquerel had taken the same objection, and when it came under my notice in Rosenthal's pages in 1869, to which Duchenne by the way does not refer, it certainly commended itself to me as a highly probable solution of this obscure difference. Since that time, therefore, I have always taught in accordance with Rosenthal's (or Becquerel's ?) explanations. Duchenne, however, now, in his turn, will have none of it. He again asserts the essential distinction between the two coils, and holds that they are to be used as very distinct agencies. This farther declaration he reinforces by clinical examples which have much apparent force, the force of one of them having the additional value of an amusing *argumentum ad hominem* in the person of a "distinguished professor of physics," whose testicle the author secured between his rheophores and then practised

upon by means of a commutator. This question, then, still awaits its answer.

In dealing with a book of the commanding ability and power of this work of Duchenne a critic finds it difficult to know how to deal with it in any adequate way. To give anything like an idea of the work and of the wealth of clinical material and instruction therein contained is impossible within our limits, and it seems best, therefore, to turn over the pages and consult those chapters and sections wherein matter of more special interest may be contained, or wherein any matter of dispute may be found. The chapter on palsies consequent upon nerve lesions is full of valuable matter, and the subject is not only one important in itself, but, at present, is exciting great interest in relation to the supposed dependence of nutrition, in some measure, upon nerve action, that is, whether there are special "trophic" nerves or not. The excellent work of Keene and Morehead upon this subject, based upon the rich experience of the American military hospitals, has, no doubt, been in the hand of most careful readers. Duchenne's cases are of great interest, and the inestimable value of faradization in these cases is demonstrated.[1] . The author concludes that every palsy consecutive to a traumatic lesion of the nerves, in which the electro-muscular contractility is not abolished, should be submitted, at the very first opportunity, to the treatment by localised faradism. But that, when electro-muscular contractility is lost, or, at any rate, escapes detection by the usual tests (which shows that spinal influence no longer reaches the muscles), we must await the healing of the nerves. Then only will faradism have a chance of success. Each muscle must be separately faradized according to the degree of its affection, the current being the stronger and the longer applied the more the contractility and sensibility of the muscle is diminished. The more the sensibility becomes aroused the slower should be the interruptions and the more moderate the current, and the fewer the sittings, or harm may be done. The primary current is chiefly to be used. Care must be taken, even in most defective muscles, not to fatigue them by too long applications or by too rapid interruptions, a minute to each muscle being generally enough. This minute, moreover, should not be given to each muscle all at once, but the operator must run quickly on them all, returning again and again to those which most require attention (p. 378, A). With all this I cordially agree, save in respect of the

[1] Medical men, as a rule, are not unfamiliar with horses, and, unfortunately, their diseases are not much hidden from us. Yet I have never seen a notice in any essay on "trophic neuroses" of the curious degeneration of tissues which often follows the nerving of horses for navicular disease.

higher value of the primary coil, concerning which I cannot express any opinion, having, I fear, used either coil indifferently without making any careful comparison between them. But I cannot but regard the neglect of the continuous current in these cases, and in the cases like them, such as lead palsy, &c., as a great omission. The short section devoted to the mention of this agent is not only meagre in the extreme, but is almost wholly personal and polemic, the one important sentence in it being that in which the author announces that he is making farther experiments with the continuous current at the present time. Those who have made use of the invaluable aid of this current in the earlier stages of the treatment of cases of peripheral palsy, and who have, by its means, both saved much time and who have seen muscles awaken under its influence, which, without it, had fallen into hopeless atrophy, will feel that no chapter on the subject can be even tolerably perfect which omits all, or almost all, allusion to it. Duchenne, always conscientiouc and accurate, reports his cases in such a way as to produce the impression upon me that cases proved very tedious under his management which would have given way far more rapidly and satisfactorily under treatment by galvanism and faradism combined.

There is also another point which has to be worked out in connection with the present subject—I mean the similarity between the results obtained in some cases by mere acupuncture to those of faradism. Duchenne reports more than one of those cases of palsy of the deltoid and neighbouring muscles in consequence of a blow upon the part. Such cases, in which the shoulder may be almost powerless, often give way very promptly to faradism, but they also give way with a greater celerity, with a celerity quite marvellous, when treated by acupuncture. My colleague Mr. Teale is fond of this operation, and he has obtained effects by it in cases which I have sent to him which are really surprising. I have by chance the notes of such a case under my hand at this moment. A man in consequence of a blow on the right shoulder had palsy, and some wasting of the deltoid muscle chiefly, and of neighbouring muscles in various degrees. He could not raise his arm, nor could he cross it upon his breast, nor could he pass it behind his back. Mr. Teale thrust the needle once or twice into the fleshy parts of the shoulder, and the man was instantly enabled to perform a considerable range of actions before impossible. He came under my observation again in a fortnight, during which time no further punctures had been made, when he reported his arm as quite well. This is but one of several such cases in which like results have been obtained; and from the comparison of these with those cured by

faradism, we might be enabled to make some important deductions as to the *modus operandi* of the latter. On turning to Duchenne's cases of lead palsy, I have again to note that the results obtained by him by means of faradism alone seems, so far as I can tell, to be far below those which I have been accustomed to obtain by means of galvanism and faradism combined. The author seems to forget that the comparison does not lie between faradism and galvanism exclusively, but between faradism and galvanism together. With one important observation of the author I entirely agree, viz. that increased sensibility to the electric current is a valuable sign of coming improvements.

We may now skip over a few pages to reach the chapter on palsies of the seventh pair of nerves, which is full of interesting matter; such, for example, as the comparison between central affections of this nerve and those which are peripheral, between the electric reactions which take place in these cases respectively and the new light thrown upon these classes of cases by the experiments and researches of such men as Clarke and Vulpian. Space does not allow us to enter into these refined and attractive inquiries. In respect of treatment we have to make the same reservations as in the previous cases, and Duchenne lays himself open to still farther cross-examination when he volunteers the statement (p. 878) that the continuous current only has some incidental power in so far as it acts upon the local circulation—a proposition open to the gravest doubt. He also gives as a reason against the use of galvanism that it causes "etourdissements" in the patients and "une fois, le malade est tombé en syncope, après quelques secondes d'application." Now, while no one could dream of neglecting faradism in these cases, I have only to say as regards the syncope, that the accident is at least as likely to follow the use of this method as of the other. About eighteen months ago I applied a very gentle faradic current to the affected side of the face of a sufferer from Bell's palsy, and the current had not passed twenty seconds when he turned pale and sank into a large easy chair utterly unconscious. I bathed his face and faradised his precordial region, and so by degrees he recovered his consciousness, nor does he know to this day what had befallen him save that he had felt rather sick. This is perhaps the only instance in which I have produced actual syncope by faradism of the face, but my patients have complained more than once of feeling "very queer" under its careful and gentle use. It is worth noting, perhaps, that Duchenne seems not to have seen the curious response in the unaffected side of the face in Bell's palsy, which in certain cases may follow the application of the continuous current to the affected side—a phenomenon first de-

monstrated, to me at least, by Dr. Gowers, of the Hospital for the Epileptic and Paralysed.

We may now turn to the chapter on cerebral palsies, to ordinary hemiplegia following encephalic hæmorrhage for example, as no subject is perhaps of greater importance in the ordinary run of practice, nor can experience in any direction be more valuable to the general reader than in this. Duchenne's teaching in the whole of this chapter seems to me to be excellent, and to place on a firm and reasonable basis the aphoristic rules which of late have guided most practitioners. It is now generally recognised that in ordinary hemiplegia we have to do with two kinds of palsy and not with one only; and that these two palsies scarcely coexist, but pass insensibly the one into the other.[1]

The first palsy is due directly to the encephalic lesion, and as this heals gradually, so, in some like measure, does strength return into the affected limbs. During this time, as Duchenne judiciously points out, faradism, or any such interference, would be ill-timed, for two reasons—first, it would be a forced interference with a curative process already at work by natural causes; and, secondly, the painful irritation would seem likely to produce—and often does, as a fact, produce—new central irritation. When the central lesion, however, has healed we often find that there is not a proportionate degree of returning strength in the limbs. Now a new palsy has set in, one due in a measure, no doubt, to obliteration of a portion of the central nervous organ, but often due, in greater measure, to loss of conducting power in the channels of the will and to loss of tone in the muscles themselves. Now this second kind of palsy is susceptible of very great degrees of relief, if not of actual cure, by faradism, and the very interesting practical question arises—When have we to deal with this secondary palsy, and when have we no longer to deal with a bruised and healing centre? On this point Duchenne's enormous experience and weighty opinion bear with great force, and he says, speaking I presume of average cases, that the use of faradism is rarely to be justified before the expiration of six months. Thus time is left for the restoration of muscles still susceptible of relief, and the time of probable injury to the centre is past. In my own practice I have not realised the necessity of allowing so long an interval to elapse, for I have been probably in needless fear that if we deferred treatment so long we were risking a deterioration of peripheral nerve and muscle which would preclude the chance of a good result. But I willingly admit that the three and four

[1] *Vide* chapters on "Electro-therapeutics," No. II, by the present reviewer, in the 'Brit. Medical Journal,' 1871.

months, which have been my own limits, appear to be too short. Gradually I have shrunk more and more from so early an interference, and henceforth I shall preach patience and Duchenne to impatient sufferers. I have never been so unfortunate as to have set up any serious mischief by too early an interference; but I have certainly been noticing more and more of late that in faradizing the arm and leg of hemiplegic patients any improvement in these parts has been more than compensated by deterioration in other respects. Speech, for instance, has been more difficult after the commencement of the faradism, or there has been fulness or dizziness in the head, or the mental faculties have fallen back. The influence upon aphasia has seemed especially instructive, and I have often tested the toleration of earlier faradism upon such patients with the effect of increasing the aphasia if applied within the earlier weeks of the malady.

It seems, then, not only that we ought to wait six months before applying faradism in hemiplegia, but that we may safely do so without letting the limits deteriorate too far. The author also adds very needful warnings against the use of the remedy for facial or lingual palsies, as such applications may do a serious injury. He has himself, excellent manipulator as he is and admirable as are his mechanical appliances, in more than one instance set up central disturbance, which has given him much cause for regret. In my more careless days I have alarmed myself at least so much, if I have not so permanently injured my patients, by faradizing the face in cerebral palsies as to have given up the practice once for all. We have also some valuable hints as to the application of faradism to the affected limbs in such a way as least to cause irritation in the weakened brain. The chief instructions which Duchenne gives us are to use chiefly the secondary coil, to keep the rheophores near together so as to limit the excursion of the agent, and to use slowly interrupted currents. All this in respect of faradism in hemiplegia is so excellent that we regret that the use of the continuous current to the brain, or otherwise, is barely mentioned. The chapter ends, as usual, with a few paragraphs of feeble polemic anent this method, which polemic is here and always directed against Remak, as if any serious person was now a follower of that uncertain guide and incautious practitioner. The names of men like Erb and Benedikt seem to Duchenne to be almost unknown, and their statements really require, at this time, the control of a serious criticism. The chapter on infantile palsy will attract many readers, on account of the frequency of that sad and hitherto rather hopeless affection which so often brings sadness and deformity into early life. Duchenne has long been known for his close and accurate investigations into

this disease, and we find much practical instruction in his descriptions of treatment. Depreciating the use of the continuous current, as is his wont, he adds one argument against it which certainly is borne out by my own experience, namely, that it causes such pain and distress to the children as to drive away the parents from the consulting-room, and also to seriously injure the health of the little patients themselves. The reason given is the electrolytic action of the poles. Certainly, children submitted to the continuous current often howl and shriek piteously, but I have never been able to satisfy myself why this is so. Any local irritation caused by the poles seems to an adult, at any rate, to be far from distressing, unless the rheophore be kept in prolonged contact with the skin; moreover, while some children resent the application with all their might others seem to care comparatively little about it. In my own experience it has, unfortunately, seemed that those shrieked most for whom the current was acting most effectually; that is, when muscles, insusceptible to the faradic current, reacted powerfully to the weakest galvanic currents. It seems rather to me that the cause of agony lies for some reason in the unwonted and forced action of a long inactive muscle. Place the poles on other parts of the body and no such terrible pain appears to be awakened, and, imagination apart, the pain seems to be less and less as the muscles lose their abnormal irritability under the galvanism and begin to answer to the interrupted current.

Duchenne treats of recent cases and of old cases in separate chapters, and takes the lead in speaking cheerfully of the prospects of treatment in this hitherto rebellious affection. In early cases he makes a very favourable prognosis, and few cases are so advanced or so bad as to make him despair of some measure of relief. As knowledge advances on this subject cases will find their way to the electrician from the first, and by attention to the following rules success will be very frequent, as our records in the Leeds Infirmary can show. The rules, as given in Chap. IV, art. ii, may be condensed as follows: 1. Avoid faradism at the onset when fever is present, and treat the patient by other appropriate means. 2. In two or three weeks, when all febrile conditions have subsided, we must use localised muscular excitation, and in connection with this the author advises the use of certain internal medication, which seem to me, I must say, to be at least of doubtful value, such as "revulsives addressed to the skin and intestines, calomel and strychnine." 3. In order to alarm and pain the child as little as possible, the intermittences of the current are to be made as distant as possible. In this way he says that faradization may be practised for a long time, and so as to cause energetic muscular contractions without

unduly distressing the patient. Paragraphs 4, 5, and 6 relate to the degree in which muscles may be said to be in peril, or to be lost. Duchenne finds so often that a weakened and wasted muscle may be recalled to some kind of usefulness that he rarely despairs if the child is seen within the first few months of the malady. When a muscle is irreparably gone then he refers us to his means, detailed in the final chapter, for giving such mechanical aid as the case may admit of. I cannot but think that a more extended and candid use of the continuous current in the cases where electro-contractility is partially or wholly abolished would enable the author to speak even more hopefully still of success, and in any case we cannot read such chapters as the present without that lively sense of increasing power over disease, and of genuine and permanent advancement of the formularies of therapeutics, which is our one consolation—nay, indeed, our one gratification—amid the painful and weary incidents of the study of human disease. Not that this chapter is by any means a greater achievement than others in this remarkable volume; but, perhaps, in it successful inquiry is most amply rewarded by successful treatment.

When, on the other hand, we turn to glosso-labio-laryngeal palsy, we find the same acute clinical insight as before, the same refined and discriminating power of observation, but here comes with it no reward. A hopeless, fatal affection is set out before us with the pen of a master, but he does not attempt to give any promise of relief. On the contrary, he has no section for the head of "treatment" at all, and barely alludes to the subject. My own opinion is that a patient of my own who suffered from this sad affliction derived some benefit from the use of a gentle continuous current to the tongue and lips; at any rate, he thought it worth while to come three or four times weekly for some months, from a distance of some thirty miles to Leeds, to have this treatment applied. I certainly always noticed some improvement after the passing of the current, but this improvement gradually vanished until the next sitting, and thus, although the advance of the disease was not arrested, yet he gained considerable temporary alleviation. Faradism, which was thoroughly tried, did him no good whatever. In this case, as in all, the affection of parts not according to the grosser combinations of nerve trunks, but according to the higher and inner co-ordination of function, was very remarkable, and pointed necessarily to an affection of the centre of some definite group of movements. The patient had not palsy of the seventh nerve, nor of the eighth, nor simply of the ninth, but palsy of parts supplied by fibres running in all these trunks, fibres which subserve phonetic expression. The lips lost their expressiveness, the

tongue its movements, the larynx its intonations, and the thorax its breathings, so that the poor man slowly became "mute as a fish." These cases must be carefully distinguished from those in which the tongue undergoes "progressive muscular atrophy." In these the tongue falls away as other muscles do. We long had a man named Stevenson in attendance at the Leeds Infirmary in whom this palsy of the tongue was so early a symptom that an unwary observer might well have called his case one of labio-glosso-laryngeal palsy, as indeed was done by more than one of us. Such an early loss of the lingual muscle seems to be an unfavorable sign, for Stevenson's case, though a slow one, did not show itself so amenable to treatment as others of the same kind. The number of cases of progressive muscular atrophy which find their way into my wards is very large; I am seldom long without a case under treatment, and I have often two or three cases under my care at once.

It seems astonishing, therefore, that the disease should not have received any special recognition until the publication of the essays of Aran and Duchenne, Aran's cases and comments being indeed founded upon Duchenne's experience and clinical acuteness. It is needless to say that the chapter devoted to this subject in the present volume is most masterly and exhaustive. In a former number of this Review I had occasion to point out, or at any rate to express my opinion, that Benedikt had spoken adversely of Duchenne's conclusions, as given in his former edition, on false grounds; and that, so far from Duchenne's conclusions being wrong or defective, those of Benedikt, on the contrary, were in error, being in great measure due to positive mistakes in diagnosis. My own investigations in at least forty cases of progressive muscular atrophy have proved beyond doubt that from among the numerous local palsies which may similate it, the true instances of the affection may be detected by electro-muscular exploration; and that if Benedikt, or any other observer, described cases in which the loss of muscle and the loss of farado-contractility were otherwise than perfectly parallel, then he was describing a case not of progressive muscular atrophy, but of some quite different disease. I need not point out how important faradism therefore becomes as a direct test and exponent of the affection.

Duchenne makes no reference to Benedikt, indeed he has little first-hand knowledge of the German literature; perhaps he has been so engrossed by the perennial conflict with his old enemy Remak as to have neither eyes nor ears for any younger tilters, but nevertheless he proves over and over again the correctness of his old conclusions, having only to sacrifice his original hypothesis of the nature of the disease, which he does

sacrifice gracefully at the feet of Clarke, Charcot, Hayem, and Vulpian. Duchenne originally taught that the affection was essentially a disease of the muscle itself in a primary sense; and, if I mistake not, this opinion was adopted by Virchow in Germany and by Meryon, Greenhow and other English authorities. Clarke, however, showed in this Review (July, 1862), I think for the first time, that this view, and Cruveilhier's view that the affection had its origin in a wasting of the anterior roots of the spinal nerves, were neither of them true, but that the seat of the malady lay really in the cells of the anterior horns, which undergo an irritative and afterwards an atrophic change. This discovery of Clarke's has now been accepted by all competent observers, and may be regarded as tolerably certain.

The only other points of novelty to which attention need be called are to the occurrence of fatty deposits between the fibres of the muscle as well as in the fibres themselves, and to the appearance, noted by Hayem, of proliferation of the connective tissues in the muscles, which seems to point to some irritative process in the muscle itself. The reader will perhaps be scarcely aware of the distinct variety of progressive muscular atrophy which Duchenne has discovered in infancy; a variety it may be called, because it always begins in the face, and so is less difficult of recognition. Of the causes of progressive muscular atrophy the author says little, for he does little more than make a reference to hereditary and to previous over-exertion of the affected parts. He makes no allusion to syphilis, which at one time seemed to me to present itself as a cause, for I found that it had existed in a very large proportion of my cases; syphilis, however, is so common, and the absence of meningeal irritation in the cord is so unlike syphilis, that this cause can hardly be regarded as probable. Over-exertion has been distinctly traceable in some of my cases, the muscles involved being those which had been especially tried. These cases, however, showed little tendency to progressive invasion of other parts. In muscular atrophy we have again the satisfaction of thinking that clinical discovery has borne direct fruit in therapeutics. By faradism practised with care and patience we are enabled to do much to relieve or to arrest the malady if not to cure it. Duchenne has not found much benefit from the continuous current, and herein my experience is at one with his. I have tried the application of the galvanic current in three ways, first as galvanisation of the sympathetic (Benedikt), secondly as galvanisation of the portion of the cord affected, and thirdly as a local application to the muscles involved.

In the first two forms, however carefully and patiently tried,

I do not hesitate to say that it is useless. In the third form it has some power for good, but less, far less, than faradization, which must be applied with both rheophores in one hand and with well-moistened skin and sponges. Our successes have been most remarkable in this way, which is that prescribed by Duchenne, who, however, adds to it the use of the continuous current to the spine. My pupils have restored bedridden men to something like useful locomotion by constant pains, and the wonderful improvement in such advanced cases as the man Ash —the "soldier," as he was called, from his peculiar gait, due to atrophy of the sacro-spinal muscles—will return readily to the memories of many of our old Leeds students.

On one point I am anxious to gain more information, and here I regret to find little to help me in the present volume, namely, how long does a muscle thus renewed retain its amendment? Is the new muscle thus artificially obtained a really valuable and permanent growth, or is it like Jonah's gourd which perishes in the first heat? I have lost sight of most of those of my own patients who came from distant places; some, I know, have died, but others may still preserve their newly gained strength. That the disease itself often makes long halts we know, but what we lack is any extensive knowledge of the value of tissues which are the product of the induction coil.

Duchenne draws attention, by the way, to one remarkable character of progressive muscular atrophy which our experience also abundantly proves, and which may, in a way, be said to be a lucky chance, that is, that in these patients the affected muscles suffer a decided loss of electro-sensibility, so that they lend themselves freely to faradic treatment without distress.

The pathology of that remarkable affection myo-sclerotic or pseudo-hypertrophic palsy, the description of which we also owe to the penetration of Duchenne, has been advanced, or rather has been defined, to one more degree, as we learn from the chapter on the subject. In the text Duchenne is able to add little to his previous memoirs on the disease (the reader will perhaps remember that the author contributed a most important memoir upon this palsy to the 'Archives Générales de Médecine,' Jan. and Fév., 1868, and which is, indeed, more exhaustive than the chapter in the book before us). He describes the morbid anatomy of the muscles, and other features of the affection, and speculates little as to its essential cause. As his work passed through the press, however, an autopsy was made upon a fatal case of the disease, and the spinal cord (among the other parts) was examined under the direction of Charcot. Absolutely nothing was found; the cord in its

whole length proved to be quite healthy. The disease, therefore, cannot be classed with progressive muscular atrophy, but owes its origin to some totally different causes. This I should be disposed also to think probable, for while progressive muscular atrophy is positively common in Yorkshire, I have seen but two cases of pseudo-hypertrophic palsy, one of which I exhibited at the meeting of the British Medical Association, in Leeds. I am sorry to say that I never gave any treatment a fair trial in either case, and Duchenne says that in the fully-established forms treatment seems, so far, to be of little use; but, he says, the affection may be actually cured if caught at the very commencement.

Locomotor ataxy is now so commonly known and discussed, and the name and phenomena of it are so familiar, that the younger generation of practitioners, at least, will find it hard to suppose that it had not been defined twenty years ago, and that we owe the definition of it to the acuteness of Duchenne. Whether, as I am inclined to think, the affection will be found to shade off imperceptibly into many other forms of palsy depending upon local sclerosis, or whether it be found sufficiently constant in the grouping of its phenomena to deserve always a special name distinguishing it sharply from other maladies, in either case the facts brought into prominence by Duchenne will have a special interest and importance of their own. They are facts not only grouped in a special fashion, but of a very peculiar kind, and are obscure enough in their signification to exercise the wits of the ablest clinicians and physiologists, and as yet, in some measure, to have baffled them. On the side of pathology also the description of sclerosis of the posterior columns has led us to the knowledge of the various ways in which sclerosis may affect the central nervous masses, and may produce various symptoms according to the part attacked and according to the disposition of the morbid process in ribbons, disseminate masses, or in wider and less circumscribed districts. Between locomotor ataxy and general paralysis of the insane we may find almost every possible gradation. Sclerosis giving rise to symptoms grouped more or less in the form of locomotor ataxy are very common in the West Riding, and, probably, elsewhere, but they do not preserve nearly so uniform a course as they seem to do in France. We constantly have cases of unquestionable locomotor ataxy, for instance, in which the shooting pains have either been absent or so slight as to have escaped the memories of the patient, and the affections of the nerves of the orbit are often wanting likewise. On the other hand, many of our cases present a very "typical" course, as, for instance, in a woman whom I exhibited to Brown-Séquard and Charcot, who,

in addition to all the usual symptoms, had gone through the curious changes in the joints, visceral neuralgias and rigidity of parts of the limbs. Duchenne finds no benefit from any form of electricity, but seeks aid from internal medications. I have no hesitation in saying that the use of the continuous current is attended with very marked alleviation of the symptoms, or, at any rate, with great improvements in the gait and relief of the shooting pains. Dr. Althaus also has a very high opinion of the value of the continuous current for the relief of these terrible sufferings, and thinks it is not only very efficacious but is the only means for their assuagement which we have at our disposal. That this current has any power to extend the time of the duration of the disease, or to arrest it, seems, however, to be very doubtful.

Perhaps few affections are more wearisome to both patient and physician than the palsies of hysteria, and one turns with much interest to see what Duchenne's experience has been in this respect. As far as I can gather, his results have been much the same as those of others, often marvellously successful, but in other cases, apparently identical in character, utterly disappointing. He advises that the electric excitation be localised in each affected part, and that the cure be fixed, as it were, by a continuance of the means for a while after the apparent cure. On the other hand, he says, he, like others, has sometimes found a single operation amply sufficient for perfect restoration. He refers also to those cases of transient hemiplegia in women, which I described in the 'British Medical Journal,' Oct., 1870, and in which he seems to have found faradism more useful than I have done. I cannot call these cases hysterical in any practical sense. The cases collected by the author do not give any grounds for distinguishing those in which faradisation will bring about a cure from those in which it will prove useless. Hysterical palsy often does and often does not give way to faradisation. The paraplegic form is that in which the chances of success seem to be least promising. That the continuous current is of little use, except, of course, in hysterical contractions, when it is sometimes invaluable, is quite borne out by my own experience as well as by that of Duchenne himself. In accordance with his teaching, I have always been accustomed to push faradism in these cases, however alarmed or pained the patient may be, and even to this degree that I have several times administered a little chloroform rather than desist from it. So far from setting up crises and convulsions by its irritation, it actually calms them, as Duchenne also has observed. Duchenne, however, is disposed to use rare intermittences and very gentle currents at the commencement when

the electricity causes much pain, and so work on to higher degrees of intensity and more rapid intermittences.

We have found the perusal of this admirable and fascinating work to delay us to an unusual extent; but however ill I may have interpreted the parts selected, I sanguine enough to think that I cannot have failed to carry the reader willingly with me. One only part remains which I feel compelled to notice, and that is the chapter on neuralgia, which, I am bound to say, seems to me the only really defective chapter in the book.

Of the marvellous power of the continuous current of good constant batteries to arrest and cure the superficial neuralgias, no experienced electrician can now have any doubt.

Duchenne seems scarcely aware of what has been done in this direction by such accomplished electricians as Benedikt, Eulenburg, Ziemssen and many others in Germany, and by Althaus, Anstie, Buzzard and the American physicians. The therapeutic value of the continuous current is as striking as it is considerable, for in those many cases which admit of cure relief is always given at the very first sitting, and is farther established at each successive sitting. It is scarcely competent even for Duchenne himself to pretend to put faradism in competition with this method, though it is well known that many cases have been cured by it in a capricious kind of way. Gentle faradism, for instance, with the hand applied to the forehead and temples, is often a great relief in common headache, but in a case of sciatica or cervico-brachial neuralgia, it would for the most part be worse than useless. On the other hand Duchenne relates cures of epileptiform tic and of angina pectoris by means of faradism, which, if not incredible, are at any rate not likely to be repeated in the practice of others. In connection with neuralgia and "neuroses," the author deals with disorders and palsies of the special senses, which chapters are full of close and accurate research and of clinical acumen, but which are sadly marred by the author's ignorance of the work done by his fellow labourers in other countries. For instance, to write an essay on the electro-therapeutics of the ear without betraying any sign of having even heard of Brenner, still less of having mastered his treatise on the same subject, is to place one's self entirely behind the best knowledge of the day. Still, limited and defective as are some portions of this rare treatise, as a whole it is a monument of clinical and therapeutical skill which will not only preserve the memory of the author so long as medicine endures, but will do more than any contemporary work to honour the time in which it has been produced. If Duchenne may be proud of his own achievements, we in our turn have to be proud of him, for he has raised a memorial not to himself

only but to his people and his generation. And this we say the more, that not chiefly has he ministered to our scientific curiosity and culture, though these he has more than satisfied, but he has ministered to the sufferings of some of the most afflicted among men, and has perhaps done more for the art of healing than any one man in the present or in the past.

T. Clifford Allbutt.

IV.—Diseases of the Ear.[1]

Dr. Gruber has been long known for his contributions both to the anatomy and the therapeutics of the ear, and his book constitutes an important addition to the literature of the subject. Dr. Turnbull's, on the other hand, contains little original matter, but is a tolerably comprehensive compilation. The appearance of these two works, with that of many other less bulky publications, among which we may note three journals in Germany (one published also in America[1]) is a sign of the growing interest in the subject of ear disease which exists at least in some other countries than our own. They will enable us to give a brief outline of the present relations of the study.

Dr. Gruber's book consists of three sections:—An anatomical one; one on the general therapeutics of the ear; and a third on its special diseases. In respect to the anatomy, his own labours have been devoted chiefly to the study of the membrana tympani, and the development of the temporal bone. The anatomical details given are very complete, but cannot receive the praise of conciseness. It is open to question, indeed, whether the points to which attention is desired would not be more forcibly impressed on the mind by a briefer statement. The author remarks that it is much to be desired that every handbook on aural disease should contain also a description of the anatomy of the organ; to which we should reply that it is

---

[1] 1. *Lehrbuch der Ohrenheilkunde; mit besondere Rucksicht auf Anatomie und Physiologie.* Von Dr. Joseph Gruber.

Handbook of Diseases of the Ear; with especial reference to Anatomy and Physiology. By Dr. Joseph Gruber, Aural Surgeon and Lecturer at the General Hospital, Vienna. Wien. 1870.

2. *A Clinical Manual of Diseases of the Ear.* By Lawrence Turnbull, M.D., Physician to Department of Diseases of Eye and Ear of Howard Hospital, Philadelphia. 1872.

3. *Lectures on Diseases and Injuries of the Ear.* By B. W. Dalby, M.B. Cantab.

[2] The 'Archiv fur Ohrenheilkunde,' edited by Professors Von Troeltsch, Politzer, and Schwartze; the 'Monatsschrift für Ohrenheilkunde,' edited by Voltolini, Gruber, Ruedinger, and Weber; and the 'Archives of Ophthalmology and Otology,' edited by Drs. Moos and Knapp.

doubtfully desirable that it should describe to us every part of it as if we had never heard of it before ; as for instance thus :— "If we look more closely at that section of the anterior surface of the petrous bone which we have specially designated the inner surface of the tympanum, we remark a kidney-shaped opening, beginning a little below the outer end of the semicanal of the tensor tympani, with its long axis directed from within and above outwards and downwards, with smooth borders, 3 millimètres in length and 1·5 millimètres in height. This is the oval foramen, which turns its convex border upwards, its concave downwards, and leads into the labyrinth," and so on. Surely the important matters, and things not known before, run a risk of getting lost in this style of description: such matters, for instance, as that from the pyramid there often runs downwards to the lower border of the fenestra ovalis, or to the promontory, a small rod of bone to which the stapes may become attached by false membrane, and thus impeded in its movements; so that the varying issue of similar affections of the ear in different persons (often a matter of so much annoyance and perplexity to the surgeon, because not bearing any visible relation either to the history or to the constitutional condition) may be in some cases due to minute varieties of structure. This would seem to be more often the case with the ear than with many other organs (perhaps than with some not inferior to itself in delicacy of structure), and may be usefully remembered in practice as a key to anomalies in the result of treatment else inexplicable. For these slight variations of structure, of no importance in themselves, may render the very same pathological processes altogether different in their effect upon the hearing, by determining whether an adhesion shall or shall not take place. The same remark may probably be made also in another respect (though we do not see that Dr. Gruber has noticed it), in regard to these bands of adhesion so frequent in the tympanum after inflammatory affections. There is little doubt that thin fibrous connections often remain in the perfectly healthy tympanum as relics of a more continuous fibrous mass which exists in the embryonic state: and that they are results of an incompleteness in the process of retrograde change by which the tympanum is made an air-containing cavity. These bands are perfectly innocuous so long as no morbid process arises, but if inflammation sets in, even though it may be far too slight to give rise to any adhesive processes, these fibrous connections, already existing, seem to be very liable to be affected by it, and undergo changes which alter their character, inducing contraction and rigidity, and constituting them practically "bands of adhesion," though no adhesive inflammation has been present.

Dr. Gruber's chief investigations have been made on the membrana tympani, and to him we owe the discovery that the malleus is connected to the membrane by means of a distinct fibro-cartilage, thickest around the short process, where there exists a sort of half-joint, allowing a slight degree of motion between the membrane and the bone, a point which has been controverted, but on which Helmholtz is in agreement with our author. He has also pointed out the existence of certain branching fibres—"dendritric" he terms them—in the posterior and the inferior portion of the membrane, distinct from either the circular or fibrous layer, and to which he assigns the function of restoring the membrane to its normal position after it has been subjected to tension. On the disputed point whether the minute canal at the upper part of the Eustachian tube, immediately beneath the hook of the cartilage, is always open or not, he appears to agree with Rüdinger, that it is closed by folds of membrane. Dr. Brenner, however, in the April number of the 'Monatsschrift für Ohrenheilkunde,' quotes the case of a lady in whom the soft palate was wanting, so that the orifices of the tubes could be seen, and in whom closure of them produced immediate deafness; from which it is inferred that this smaller portion of the tube at least permits variations of air-pressure within the tympanum to take place with the utmost facility.

The description of the labyrinth is very complete, but contains nothing essentially new or that throws fresh light upon its functions. He notes that the conveyance of vibrations through the ossicula is now proved by the experiments of Politzer, Lucae, and others; and holds that the membrane, through its various connections, greatly *damps* the passage of sound, and the more owing to its oblique position; but Helmholtz and Wolf have shown that not any considerable amount of force is needed to be transmitted to the labyrinth, but only a moderately equal proportion of every form of vibration.

In the examination of the patient Dr. Gruber advises that an investigation of the general condition should precede any examination of the ear. But without under-estimating the necessity of a complete inquiry into the patient's health, nor the temptation that exists to regard local affections from a merely local point of view, it must be held that a stringent rule like this defeats itself. An exhaustive analysis of every organ would be undertaken sometimes as a preliminary to the removal of a plug of wax. We venture to be sure that our author does not rigidly follow his own precepts here, nor even in the patience with which he recommends us to listen always to the

patient's own account, without putting questions. Assuredly in that way we do not always attain a "chronological" account. Nothing is more frequent than to find the early symptoms wholly left out; as indeed our author notices that the duration of the disease is constantly under-estimated, years often elapsing between the real cause and the occurrence of inconvenient symptoms. Among more established methods of testing the hearing, he recommends words containing similar consonants at the beginning, as *t* and *b* before *i* (*tin, bin, fin,* &c.); and that a *scale* of voice sounds should be acquired by practice. In the use of the rhinoscope he recommends the uvula, if necessary, to be held forward by a loop of thread, introduced through a canula and attached to a forehead band, so leaving the left hand free.

For diagnostic purposes he still uses the Eustachian catheter and an improved air-press, and attaches much importance to the sounds produced in the tympanum and tube by the air-stream. He had previously called attention to the value of what he terms "secondary auscultation sounds;" that is, those produced by the return of the tympanic structures to their former condition after the stream of air has ceased. In the healthy state this is a slight click of short duration, but in morbid conditions it may be much prolonged. In respect to the introduction of air during swallowing, by means of what is now termed Politzer's bag, Dr. Gruber affirms again his claim to have used essentially the same process long before it was made the subject of Politzer's memoir. Into these personal disputes, however, it is no part of ours to enter, and we can but wish that, at least in a profession like our own, all ignoble rivalry were wanting. As is well known, the method consists in passing a stream of air by means of an elastic bag into the nostrils, during the act of swallowing, whereby, the tubes being opened, it may pass into the tympanum. Our author urges three points that detract from its diagnostic value: (1) that the air may pass so easily owing to the opening of the tube by swallowing as to deceive us as to the amount of swelling; (2) that in cases in which the velum does not completely close the pharynx, the air escapes into the chest and stomach, with very unpleasant effects, instead of entering the ear; and (3) that as the air passes most easily on the least obstructed side, it may happen that when one tube is simply narrowed no air may be heard to enter it, and may so give rise to a false impression of its total closure. Finally, he urges that the suddenness of the impact on the membrane tends to produce rupture of it much more readily than the use of the catheter: so that, on the whole, the catheter remains essential for a trustworthy

diagnosis in all cases in which auscultation by means of a stream of air is called for. In these remarks it seems to us that an excess of weight is laid upon difficulties which a little patience suffices to reduce to unimportance. The unpleasant choking sensation from the air escaping downwards seems in some cases unavoidable at first, but it by no means necessarily recurs on repetition; while, according to our experience, it is perfectly possible to recognise the difference of sound indicative of the passage of air through a tube closed by swelling of its mucous lining; and by closing the meatus of the sound ear, it is very seldom indeed that clear indications cannot be obtained of its entrance into partly obstructed ones. Indeed, we have been often struck with the great variety of the sound that can be detected by this method, due quite evidently to the various conditions of swelling, secretion, rigidity of the tube and tympanum, and the position and attachments of the membrane. In some respects there may be fairly claimed for it even a superiority over the catheter itself in these respects. The *sudden* entrance of the air will often betray the presence of conditions—as of adhesion, or slight amounts of secretion—that the more uniform, even if protracted, stream of air from the catheter would fail to indicate. Perhaps the chief drawback to the plan is that it cannot be confined to one ear; but if in cases in which one ear alone is affected the meatus on the healthy side is firmly closed by the finger, so as to give the membrane what support is possible, no evil appears to result from its moderate employment. In respect to the danger of rupture of the membrane from the sudden impulse of the air, it is very doubtful if this ever occurs when the membrane is healthy; and in its morbid states, as Politzer has adduced cases to prove, such ruptures are frequently attended with great improvement in the hearing, which may often be made permanent; as for example in the case of healed perforations, attended with adhesions of the membrane to the tympanic wall, &c. It will sometimes happen also that a membrane thinned by the continued presence of abnormal secretion within the tympanum will give way on inflation by this method and the secretion escape to the great advantage of the patient. It is therefore by no means certain that a greater tendency to rupture the membrane—when it is capable of being ruptured so—would be any argument against the use of Politzer's bag as compared with the Eustachian catheter; and it is farther to be observed that rupture from the use of the catheter has also been reported, and with similar good results. Perhaps it should be said, in reference to the relative value of the two methods of inflation for purposes of auscultation, that our author lays stress on several points in

respect to the sound elicited by the catheter; as whether the rushing sound be near or distant; full or thin; variable or uniform; whether any feeling of tremor or change of temperature is experienced in the ear of the auscultator; whether the same on repetition; and whether catheters of different sizes elicit different sounds. On the use of the tuning fork and other similar methods, and on that of bougies, our author gives us nothing fresh.

In reference to the introduction of liquids into the tympanum through the catheter, our author makes a curious historical oversight, ascribing the demonstration that they can be made to enter it without perforation of the membrane to Kramer, among others; whereas, in fact, Kramer affirmed the contrary, and not only maintained that no liquid could be passed through the Eustachian tube into the tympanum while the membrane was complete, either during life or after death, but succeeded in inducing a most scientific committee at Berlin to confirm the statement. Schwartze, of Halle, first gave fresh proof of the contrary; others followed him, and the question has been long put to rest. It is not attended with the slightest difficulty to a practised hand. But Dr. Gruber lays more stress upon a method of introducing liquids into the tympanum, to which he was the first to call special attention: syringing the liquid through the nostrils without the catheter. The patient's head being inclined horizontally towards the affected side, a dram or two of the liquid is taken in a small syringe and syringed with moderate force into the corresponding nostril. The patient instinctively raises the soft palate, and by the action appears also to open the mouth of the tube; the degree of force with which the liquid tends to enter the tympanum may be regulated by more or less completely closing the opposite nostril. Or the same result may be attained by gently syringing a little of the liquid so as to cause it to lie in the fossa of the Eustachian tube, closing both nostrils and directing the patient to blow with his mouth closed. A little of the liquid, if the tube be fairly pervious passes into the drum, and may be heard to enter it; or if the membrane be perforated, will run out of the meatus. After the nose and pharynx have been well cleared, this method is very valuable for evacuating collected mucus from the tympanum in cases of old perforation with long standing discharge. But we do not find that our author refers to another method not less useful in such cases; that, namely, of passing a stream of liquid (a weak alkaline solution is, perhaps, the best) from the meatus, so as to escape by means of the Eustachian tube. If the patient leans his head forward and breathes through the mouth, this procedure

occasions him no inconvenience; the Eustachian tube, of course, being first ascertained to be freely open. The nozzle of the syringe is made large, and covered with a piece of india-rubber, so as to close the meatus. The objections to Dr. Gruber's plan of introducing liquids into the tympanum are, that it is difficult to regulate the force with which they pass into the cavity; and (as he remarks) to prevent them from entering the other ear as well, especially if the tube of the diseased side be narrow. With the view of diminishing this risk it is well, perhaps, always firmly to close the meatus of the side on which the entrance of the fluid is not desired. There is least risk of this result when the patient himself blows. Dr. Gruber makes no reference to Weber's nose douche; but it is to be borne in mind that serious inflammations of the ear have been witnessed by American physicians as a consequence of its free employment, even when all precautions appear to have been taken.

Coming to special diseases, Dr. Gruber holds that the hæmatoma of the auricle, so frequent in the insane, is not always a result of violence. He treats it by evacuating the blood by a trocar before coagulation, or by incision after it, and dressing with charpie. For eczema he adopts Hebra's treatment. Alike for the circumscribed, and the diffuse inflammation of the meatus, he recommends early incision; perhaps without allowing, in the latter case, sufficient scope for other means. In the later stages of diffuse inflammation in weak or morbid constitutions he has seen diphtheritic exudations form; and in ill-nourished children not unfrequently extensive gangrene extending to the surrounding parts. For aspergillus in the meatus (which he believes to be a cause and not a mere result of inflammation, and to lead to destruction of the membrane) he prefers, among all the solutions that have been recommended, carbolic acid (gr. v,—℥j); but Dr. Orne Green, of Boston, who has suffered himself from the affection, thought that washing freely with water alone was inferior to none of them. Gruber found the aspergillus in the ear of a rabbit, or rather spores and filaments of a smaller size, such as on only one occasion he discovered in plates removed from the surface of the membrana tympani.

He thinks Toynbee in error in ascribing absorption of the bony walls of the meatus to the mere pressure of cerumen, which can at most excite inflammation. Cholesteatomata, on the other hand, cause absorption, and may even (as Toynbee showed) protrude into the mastoid cells or cavity of the skull, and threaten, therefore, the most dangerous results. He does not refer to the mischief that is done in this respect sometimes,

perhaps very often, by the injudicious custom of cleaning out the ears of children by the rolled-up corner of a towel, which, when flakes of loosened epidermis are present, presses them down into dense masses upon the membrane. To all appearance some cases of "cholesteatoma" begin in this way.

In reference to ruptures of the membrane, the case of a soldier is related, who was thrown by a comrade on some straw. Fifteen years after Gruber removed from the ear a portion of straw which occupied a hole in the anterior part of the membrane, the removal displaying the red and swollen mucous lining of the tympanum. Having tried in vain to rupture the healthy membrane after death, by means of a powerful air press, either from the side of the tube or the meatus, he holds that ruptures occurring during hooping-cough, vomiting, &c., imply a diseased state of the membrane.

Although unfrequent, inflammation of the membrana tympani alone does occur sometimes, its causes being chiefly local irritations or chills, especially the entrance of cold water into the meatus. It is apt to lead to formation of abscess in the walls of the membrane, which should be speedily opened, as they not only cause perforations, but, by degeneration of their contents, lay a basis for chalky concretions, or other abnormal deposits, in the membrane. If leeches, &c., do not subdue the symptoms superficial incisions should be made through the swollen dermoid layer; irritating lotions should be avoided, and even in the later stages it is better to apply lotions carefully by a camel's-hair brush. A single grain of Plumb. Acet. to the ounce, with a little morphia, is the only one recommended. If perforation ensues, a chief object is to avoid adhesions to the inner wall of the tympanum in the process of healing, for which purpose the air-douche should be used frequently, and even a small bent probe may be introduced; but sometimes all is in vain.

In reference to exostoses occluding the meatus Dr. Gruber alludes to the removal of a portion of the bony mass by a hand-trepan as the only operative proceeding, and without referring to any successful case but the one recorded by Bonnafont, of Paris;[1] but there appears to be a good foundation for the expectation that the continuous current, applied by needle introduced into the mass, will be found a safe and effective means for their removal.

For perforations that cannot be healed (and Gruber admits that when the edges of a perforation are skinned over this has seldom been brought about, even by a complete paring off of

1 'Mém. de l'Acad. Imp. de Méd.,' May, 1868.

the edge) he advises the use of the artificial membrane, giving the preference to the form used by Mr. Toynbee. He even employs it to protect the exposed parts, although it may effect no improvement, giving it the preference to cotton wool. We read this recommendation with some surprise, but do not doubt it may be justified by results. Certainly, we have never seen any ill results from the use of the artificial membrane that would lead us to avoid its use. The only danger, if the ears be kept clean, is that of the disc becoming detached and remaining in the meatus, if it be worn too long.

Opacities in the membrane may have their seat in each of the layers, but our author has detected a form not before described, namely, an *adhesion* of the folds into which the mucous layer is thrown, when, by alterations in the position of the malleus, portions of the membrane become greatly relaxed. These are most frequent in the upper and posterior portions of the membrane, the malleus being most frequently drawn upwards and backwards. The superficial opacities may be removed by pencillings with iodine (Pot. Iod. gr. xx, Iodinii gr. ss, Aq. ʒij). For the deeper ones myringodectomy is the only remedy.

In treating the inflammations of the middle ear Gruber begins by stating that he finds it less and less possible to distinguish absolutely between the symptoms peculiar to the tympanum and those which denote an affection of the labyrinth, which latter he holds to be generally more or less implicated. Indeed, he suggests that small extravasations of blood in the labyrinth "not seldom" take place, even in catarrhal inflammations of the tympanum; having been forced to this view by the frequency with which deposits of pigment—which he believes to have its origin in effused blood—are found in the membranous labyrinth. In this view, however, he stands alone, and opposed to the positively expressed opinions of many others, as Von Troeltsch, Kolliker, Lucae.

His division of inflammations of the tympanum is into catarrhal, suppurative, and plastic, the last being that long-standing dread of the aural surgeon—the rigidity or thickening of the mucous membrane of the tympanum. In respect to catarrhal affections little that is new is given us. The peculiar loudness or ringing or "empty" sound of their own voices, of which some patients complain, he ascribes it to a freer conduction of sound, due to swelling of the mucous membrane of the Eustachian tube while the tympanum still contains air. Of the use of the tuning-fork as a means of diagnosis he speaks, perhaps, somewhat too slightingly, although it is unquestionable that too much reliance has been placed on it as distinguishing nerve affections from those of the conducting media merely.

Especially it has tended to a too unfavorable prognosis, since good recovery often occurs where the conduction through the bones of the skull, as tested by the tuning-fork, appears very small. In this respect, however, it is to be observed that much more reliable results are obtained by placing the fork upon the teeth than upon the bones of the skull. The conduction of the sound from the teeth appears to be much less interfered with by accidental circumstances. Used so, with due regard to other indications, we are of opinion that great weight may be attached to the results afforded by it. At least, if it be fairly well heard, we are assured that the nerve-function is not affected; and if, in addition, we test the effect upon the sound of gentle closure of the meatus, we seem able to attain, in some cases, reliable evidence of the condition of the tympanum; for if closure of the meatus distinctly augments the volume of the sound, we may be assured that the conducting media are not much at fault. In this respect, therefore, it seems to us that our author hardly does full justice to our means of diagnosis, which, in other aspects, he so exhaustively discusses; as especially, for instance, in respect to the sounds heard on auscultation by the catheter, which he classifies under six heads, according as there is little or much swelling and mucous accumulation in the tympanum or tube, or displacement of the membrane. He says truly that the practised ear may detect whether an obstruction of the tube is at either orifice, or at what part of its course. In treatment he lays great stress on the constitutional condition of the patient, and the state of all adjacent organs; prefers the catheter to Politzer's bag for all cases, except when the naso-pharyngeal mucous membrane is in an irritable condition; advises leeches for obstinate hyperæmic conditions, disbelieves in vesicants, gives injections of fluids the distinct preference to that of vapours, as being at least equally efficient and safe, and much more simple in their use, if either are indicated. Finally, he finds that the "pneumatic cabinet," in which the patient is placed for hours, in a compressed atmosphere, is no more effective than other ways of applying compressed air.

In the purulent form of inflammation of the tympanum, mostly an exacerbation of the catarrhal, Gruber lays most stress on a sufficient evacuation of the collected matter. He does not, however, expressly adopt Schwartze's recommendation, that if the symptoms do not show signs of mitigation, the third day should be chosen for incision of the membrane. Our own experience has not been so decisive on this point. In some cases of double-sided purulent otitis, in which incision has been adopted for one side and omitted for the other, it has been by no means clear that it has had any beneficial influence. Still there is no

doubt that lives are lost for want of even so simple an operation, and it may be that a quite early incision when once there is clear evidence of collected matter that cannot escape through the Eustachian tube is always indicated. The incision is sure to heal speedily when once the suppurative process has ceased. It should be made where the matter bulges. In regard to the use of cold or warmth externally Dr. Gruber fears the effect of cold, but speaks very highly of the combined use of warmth to the affected part, and cold at the same time to the other parts of the head.

To the plastic inflammation of the tympanum our author has given special study, and we cannot give a better specimen of his work than his description of its pathology:

"Occasioned by some injurious influence there arises in some portion or the whole of the mucous membrane of the middle ear a great hyperæmia, with swelling; and, in fact, also new formation of vessels, and increase of the intercellular fluid. The corpuscles of the connective tissue multiply by division, and perhaps through other processes also. These results now increase in the substance of the mucous membrane, the epithelium of which is swollen. It is generally less moist than in the other forms, and the free exudation, which is formed in part from the protoplasma of the epithelium, is much less profuse; in many cases it is not discoverable. Sometimes the inflammation leads to increased formation of epithelium, which then undergoes farther metamorphoses, and may lay a basis for choleastomatous formations."

As the process advances, the newly formed elements (which are either uniformly distributed or found in groups) receive partly or altogether a higher organization. By outgrowth and division of their processes, meeting with those of others, there is formed a complex intimately interlaced soft connective substance, either distributed uniformly, or constituting single or manifold granular excrescences, or even running on to polypi. It should be expressly mentioned, that in the continued development of connective tissue and corpuscles, it often happens that they are not confined to their place of origin, but that they extend to quite other surfaces, either by means of the epithelium of the part or by their freer growth, if this be already discharged. This occurs in the formation of polypi, but is shown still more strikingly in this affection by the connective threads and membranes which extend in every direction from one part of the tympanum or mastoid cells to another.

"Thus far we have described the developments of the newly produced elements; but it must be observed also that many of these elements undergo a retrograde metamorphosis. They are molecularly disintegrated, become fatty and are absorbed, or as sometimes happens in very chronic cases, become chalky. (A figure is given of a section of the mucous membrane of the tympanum from a person with hypertrophic inflammation. It is thickened fivefold, and contains

scattered chalky masses which show corresponding changes on the addition of hydrochloric acid.)

"Even the newly formed as well as the simply distended blood-vessels ulcerate in the course of the process, whereby they are either reduced to simple fibres, or grow fatty or chalky, together with their contents, and gradually become absorbed" (pp. 514-516).

Since this affection is one great difficulty of the surgery of the ear, we append also our author's treatment:

"We must endeavour to prevent the farther development of the new-formed elements, or even to destroy them. For this purpose we possess various methods, according to the portion of the tympanum affected by the inflammation, and the condition of the ear in other respects. If the tube is chiefly involved we should seek to maintain its patency by the air-douche, or by the introduction of laminaria or other bougies; at the same time the pressure of the bougie may promote absorption. If the affection is slight in its degree, and has extended over the tympanic mucous membrane, benefit may be gained from injections of caustic potash (gr. ½—1 to ʒj) daily or at longer intervals; especially by these the epithelium is softened and brought to a speedier exuviation, the air-douche being then brought into operation to aid the removal of the free product.

"In this form of inflammation also dilute acetic acid, used in a similar way, is sometimes useful in particular cases. Also acetic ether, iodic ether, and vapour of hydrochlorate of ammonia, give satisfactory results, and in syphilis the vapour of weak solutions of perchloride of mercury (gr. ½—1 to ʒj). If the disease is more intense, or if from the very commencement it is attended with very considerable increase of substance of the membrane, a more or less concentrated solution of nitrate of silver (gr. 1—5 to ʒj) is indicated. These solutions are to be syringed through the Eustachian tube by the catheter, or applied as spray locally by the very small catheter, which Dr. F. E. Weber recommends to be introduced completely into the tympanum."[1]

On the efficiency of the above-described means too much reliance should not be placed; but a good success certainly very frequently attends them. For our own part we prefer to any other solution, for injection into the tympanum in the majority

[1] This instrument, which Dr. Weber terms the "koniantron," is a woven silk tube covered with caoutchouc, made so small as to pass the whole length of the Eustachian tube. It is introduced through an ordinary catheter, and at the end has an opening placed laterally, through which liquids, forced out by an elastic bag, escape in the form of spray. This spray may, by careful application of the tube, be directed to special parts of the tympanum; and so, if the diagnosis can be made sufficiently exact, may ensure an exact application of the remedies. And it is hardly doubtful that, with advancing knowledge and careful clinical study, it will be often possible accurately to fix the *position* of abnormal conditions within the tympanum; whether there be, *e.g.* rigidity of the ossicula among themselves, or of the stapes in the fenestra ovalis, or hindrance from any cause to the movements of the membrana rotunda.

of cases, that of the iodide of potassium (gr. iij—5 to ℥j); and frequently find great benefit. Politzer expresses the decided opinion that the use of liquid injections should be alternated with that of the air-douche; which, of course, in most cases might be entrusted to the patient to carry out by means of the india-rubber bag. Whether many cases of supposed thickening of the mucous lining of the tympanum are not really cases in which formerly effused secretions have become dry and caused rigidity Dr. Gruber does not give any opinion, but it is a question that clinical observation suggests, and of course the latter affection affords much better grounds for hope in treatment.

Not much that is new is given us in respect to trephining of the mastoid process. In general Dr. Gruber thinks that affections of the bone arise much oftener from inflammations of the external parts of the ear than of the more internal, and to this cause he ascribes it that the disease is not more frequently fatal.

An examination of the secretion for particles of bony substance may aid the diagnosis, but a negative result must not be too readily relied on.

For adhesions of the membrane and ossicula to each other or to the inner wall of the tympanum the air-douche is of course recommended, or the patient's own injection of liquid, which is said to be sometimes more effective, and these failing, their division where possible by means of a small curved knife. Dr. Wreden, of St. Petersburg, has introduced one blunt at the end, but cutting on either side, and capable of being turned at any angle. On true anchylosis of the stapes there is nothing fresh. The author believes that it may, alone, destroy the power of understanding speech, and thinks that when no visible lesion exists, its presence may be inferred if the hearing is better without than with a hearing trumpet; that is, better when the sound has the most free access to the bones of the head.

Dr. Gruber pleads, and very justly as it seems to us, that operations on the ear have not yet had justice done to them. Incision of the membrane he recommends for escape or removal of hurtful secretion; as a means to other procedures; and lastly, *it being entirely innocuous*, even as an explorative means. For it often happens that the mere incision itself produces immediate benefit (owing, so far as our means of judging go, to a change in the tension of the membrane). And though this may almost always be lost again when it closes, yet it gives us knowledge; perhaps even grounds for farther procedures. In some cases he has known improvement follow the healing of such an incision, though no effect was produced at the time. But his favourite operation is myringodectomy, or excision of a portion of the

membrane (his instrument is constructed after the fashion of a minute guillotine, but with a circular knife: the excision, however, can be carried out with a sharp cutting instrument merely). He has recourse to it when the symptoms otherwise seem to demand it, (1) in unrelievable structure of the tube; (2) for great thickening, or relaxation, or lax scars, of the membrane; or for bony or cretaceous deposits in its substance. In spite of the strong tendency of the membrane to heal, he sometimes keeps the orifice open by the repeated introduction of bougies for an indefinite period. In some cases its edges become attached to the inner wall of the drum. Unhappily, he is not above an ill-natured sneer at Politzer for his suggestion of small gutta-percha rings to effect this end; which, however, it is true do not satisfactorily effect it. No account is given of the division of the tendon of the tensor tympani, as carried out with apparently good results by Weber of Berlin and others.

In respect to polypi, Gruber finds that small granulations may often be removed by pencilling with tincture of opium; so sparing much pain. For removal he uses Wilde's snare.

There is much clinical evidence, in Gruber's opinion, in favour of Voltolini's view, that a primary and uncomplicated inflammation of the labyrinth occurs in children, with symptoms like those of brain affection; and that the cases to which he drew special attention are not instances of cerebro-spinal meningitis. As bearing upon the nature of Meniere's disease, the following remarks are interesting:—

"I have had to examine four syphilitic patients (all soldiers), and two of them were long under my treatment, who had suddenly become totally deaf, after a simple chill or overheating, as they thought, having had very tolerable hearing before. At first they suffered from attacks of giddiness, which afterwards ceased; but the deafness remained unrelievable. One of the patients died, shortly after the access of his deafness, of typhus, and the examination of the ear showed (together with considerable hyperæmia of the mucous membrane of the tympanum) great injection of the vessels of the membranous labyrinth, which was also very much thickened. The labyrinth-fluid was abundant and tinged with blood, &c. Ought this condition to be regarded as an inflammation attended with hæmorrhagic exudation, specially liable to occur in syphilis?" p. 617.

It is of course a question whether this condition was not rather due to the typhus. For the rest, his remarks on nerve affections seem to be rich in ingenious suggestion. But in respect to its diagnoses (to be made chiefly by the way of exclusion) we miss a few things to which we have learnt to attach a certain value: *e. g.*, the appearance and voice of the patient, and, in some cases, that closing the meatus seems to

diminish, instead of increasing, the sound of the tuning-fork placed upon the head or teeth. Also we do not perceive any reference to the apparently very frequent occurrence of a distinct nerve-deafness after mumps, which may not even have been severe; nor to the question whether in inflammatory affections of the tympanum the labyrinth does not seem distinctly more liable to be affected if the membrane does not give way; an additional reason, of course, for practising incision as soon as the demand for it has arisen.

We have made no reference to Dr. Turnbull's volume, but we may say that the extracts it contains are not badly chosen and put together, and that he endeavours by a brief summary of symptoms and treatment to mitigate the somewhat perplexing result he might otherwise produce on the mind of a person unacquainted with the subject. It is well illustrated, and a full bibliography is appended. Dr. Gruber's book also is well illustrated, and contains twenty-four chromo-lithographs of the morbid conditions of the membrane. These seem well executed, but, like all the others we have seen, do not seem as if they could possibly convey a real idea of the object to one who has not seen it.

Since the foregoing review was written, Mr. Dalby's new book, —'Lectures on Diseases and Injuries of the Ear' has reached us. It is evidently the work of an accomplished and observant man, and perhaps, as an outline of the subject, is nearly all that can be desired. Completeness is not aimed at. Accordingly we find no reference to Weber's (of Berlin) operation for the division of the tendon of the tensor tympani, in cases of long standing catarrh, presumably attended with contraction of that muscle, as indicated by tinnitus and giddiness; nor to the same physician's suggestion of withdrawing mucous secretion from the tympanum by suction, through a flexible catheter, small enough to be passed the whole length of the Eustachian tube; nor to the much disputed question of the remedial value of electricity in all its forms. This latter question is one on which we greatly need definite information, and we trust that Mr. Dalby may be induced to turn his attention to it, and give us hereafter results based on a solid foundation. The use of the galvanic cautery also is barely done justice to. But on some other subjects the information given is fuller and of the greatest interest. We may refer to the chapter on "Polypi," which contains drawings and descriptions of great value. Here, however, we think the author under-estimates the frequency with which these growths are found in the external meatus, without rupture of the membrane. Mr. Dalby adopts as a caustic the chloro-acetic acid, which is doubtless a very effective

treatment. But we could have been glad to know his experience of some other methods of treatment—that by injecting perchloride of iron, for example. One of the most valuable chapters is that on "Deaf-mutism"—a subject to the educational branch of which the author has paid special attention; and the reader will find in it a lucid explanation of the method of lip-reading, and a description of its relative advantages over other methods of instruction.

V.—Haughton's Animal Mechanics.[1]

In this new work Professor Haughton has given fresh proof of the extent and versatility of his powers. His subject is one requiring a rare combination of knowledge, and enters upon a field which has been much neglected, but which holds out promises of fruit of the highest value.

The main defect of the system of higher education in this country has been the almost absolute separation of practical and theoretical training. Thus we have very few men like Professor Haughton, able to bring high mathematical attainments to bear upon physiology, which, until recently, has been little studied except by those who have adopted medicine as a profession.

Again, the extreme difficulty of obtaining numerical results at once accurate and general must have deterred many possessing the requisite ability from the application of mathematics to biology. This difficulty is unusually great in the set of questions attached by Professor Haughton in his present work.

It was thus with the highest hopes that we began the examination of a work of such difficulty by one so singularly fitted for it as Professor Haughton, and our disappointment on the discovery of certain defects, which we ought, perhaps, to regard as almost inevitable, is, on this account, great.

As will appear in the course of our remarks, a weakness underlies the whole work. In his haste to obtain a basis for mathematical reasoning, Professor Haughton, in too many cases, either assumes some of his data upon insufficient grounds, or omits some details from consideration which, while they would no doubt greatly complicate the mathematical problem if introduced, can certainly not be safely ignored.

Professor Haughton's investigations have, however, a value

[1] *The Principles of Animal Mechanics.* By the Rev. S. Haughton, F.R.S., &c. London, 1873.

apart from that of their results. No one can read his works without having suggested to him a number of ideas well worth following up, and without being delighted by the freshness of his style. He has also collected together a large number of facts of high interest, apart from the theories which he bases upon them.

Prof. Haughton begins by a brief description of muscle, in which he suggests that the transverse striation of muscular fibres and tendency to split into discs is a phenomenon of the same class as cleavage in rocks or wax subjected to pressure, which occurs in planes perpendicular to the direction of pressure.

He says that it is a kind of cleavage "due to their repeated contraction between two fixed or nearly fixed points of origin and insertion," which produces the necessary pressure! and that the state of things is the same in those muscles which form closed circuits. On the other hand, it is not found in the muscles of the bladder, uterus, intestinal canal, scrotum, &c., because their contraction "is vermicular, and not a constant strain from end to end"!

Now, firstly, the muscular fibre when contracted is in a state of longitudinal tension, and any compression there may be is perpendicular to the direction of the fibres and due to their thickening; secondly, the theory gives no functional value to the structure, which there is every reason to suspect, so that we fear we must reject this application of physical geology to physiology. Prof. Haughton's remarks on muscular contraction have no special importance; but, while on the subject, he favours his readers with an interesting digression on the art of hanging, which we hope will receive the attention it deserves, as there can be no doubt that, as usually conducted in this country, hanging is a slow and brutal method of terminating the life of a criminal. In fact, as Prof. Haughton points out, it was only intended as an anæsthetic preparatory to drawing and quartering the criminal.

He gives an empirical rule for the height of the drop required to cause instant death. Divide 2240 by the weight of the patient in pounds, the quotient is the length of the drop in feet. This might probably be followed with great advantage, although we deny that the strength of a man's neck is in any degree proportional to his mass. Prof. Haughton may rejoice to think that, should an insane person be hung, if his bones display their characteristic brittleness, his death will be more than usually rapid.

Prof. Haughton obtains the period of muscular contraction; that is, the time in which each portion of a muscle contracts and

relaxes until it has returned to its initial state, from the note of the muscular susurrus. He finds it to be $\frac{1}{3\frac{1}{2}}$ sec.; from this he endeavours to calculate a formula to represent his law of muscular fatigue in the case of the arms extended horizontally, by considering what would occur if all the fibres of the muscles acted simultaneously, when, every $\frac{1}{3\frac{1}{2}}$ sec., the muscles would have to raise the arm through the arc, through which it would fall in that time under the influence of gravity. Unfortunately he finds on plotting the curve given by the formula obtained in this manner, that it does not comply with the known facts of the case. Giving up the above ingenious hypothesis, he finds a cubical hyperbola which satisfies the required conditions.

Professor Haughton goes on to consider the work done by muscles in rowing, climbing, walking, and in labour employed in raising weights, &c. His results are of considerable interest, and accord fairly well with common experience as to the comparative exertions required in different kinds of work. In the course of his remarks he points out that in walking horizontally the work is done by raising the centre of gravity of the body at each step, and allowing it to fall again, and quotes W. and E. Weber for the fact that when the length of step is 28·740 in., the mean elevation of the centre of gravity of the whole body is 1·248 in., and that this proportion is constant for different lengths of step. Therefore,

$$\frac{\text{Vertical elevation}}{\text{Horizontal transport}} = \frac{1248}{28740} = \frac{1}{23{\cdot}03}.$$

He also finds values for this ratio from a consideration of the forward inclination of the body in walking, as given by the same authorities.

The next subject considered is the absolute force of muscles, which he brings out as about $95\frac{1}{2}$ lbs. per square inch of section for the living muscle, allowing for the wasting observed in most subjects after death. The maximum absolute force for a single spasmodic contraction he brings out as somewhat greater, probably exceeding 100 lbs. av. per square inch of section.

Professor Haughton passes naturally on to consider the strength of tendons, which, as he points out, ought to be proportioned to that of the muscles with which they are connected. He looks upon the friction undergone by the tendons in passing over pulleys, &c., as a very important element in the case, and considers that the differences between the united thickness of the tendons distributed to the digits, and the thickness of the tendon of the muscle from which they diverge,

in the fore and hind feet of animals, represents the amount of this friction. Notwithstanding Professor Haughton's reasoning, and the large amount of statistics which he gives on the subject, we cannot believe that friction is ever sufficient to account for the differences observed, amounting to over 50° per cent. in some cases. Other points seem to us more important, such as differences of strength per unit of cross section in the tendons, and the possibility of the greater part of the strain coming upon one digit only in any case, and thus requiring a surplus strength in the branches of the tendon.

All who have had much experience in dissection are aware of the great variability of muscles and tendons in men and animals. Variations in relative size admit of being averaged, but those of arrangement do not; and if it is found that an arrangement of any important muscle and tendon, differing radically from that ordinarily found, is compatible with the ordinary use of a limb, all elaborate investigations into the functional importance of the ordinary arrangement are shown to be of small importance. Prof. Haughton gives an interesting instance of this variability, p. 122, in a case in the human subject in which the *flexor pollicis longus* muscle was entirely wanting, and its tendon was attached opposite to the base of the third metacarpal bone to the tendons of the *flexor digitorum profundus* on their superficial surface, an arrangement identical, as he points out, with that found in the macaques and other Old World monkeys.

Prof. Haughton adds the following sneer:[1] "Whether this man was a Macaque passing upwards into a man, or a man passing downwards into a Macaque, must be decided by the reader, according as his sympathies are with Lamarck or Buffon." This we cannot but regret, while we acknowledge his liberality in mentioning a case so conflicting with its attempt to draw any anti-Darwinic conclusion from the mechanical perfection of muscular structure.

Mr. J. Wood says on this subject that "notable departures from the ordinary type of the muscular structures run in grooves or directions, which must be taken to indicate some unknown factor of much importance to a comprehensive knowledge of general and scientific anatomy."

Mr. Darwin claims that this unknown factor is reversion to a former state of existence.

Prof. Haughton would, we suppose, allege on the other hand that the mechanical problem, in the case of any member, is one

[1] Professor Haughton has put this remark into a form much more pungent than that in which it first appeared in the 'Proc. Roy. Irish Acad.,' June 27, 1864, p. 715, perhaps on account of Mr. Darwin having taken him at his word in his remark on the passage.

admitting of a certain limited number of solutions;—that while the ordinary arrangement in a given animal was that best adapted to the special form of the problem in its case,—of the other solutions possible some have been embodied in the structure of other animals, each in the case to which it is best adapted. Thus, if a deviation from the normal structure exist, we may, not improbably, find an arrangement characteristic of another animal. This argument is, we think, unanswerable so far as it goes, but it must be remembered that it does not exclude Mr. Darwin's view, though it is inconsistent with the doctrine, *Natura non facit saltum.*

Were it not for this sneering remark we might have hoped that Prof. Haughton's hatred of Darwinism had somewhat abated, as he says in his preface, p. vi:—"I cannot see why there may not be in organic life a similar process of evolution of higher from lower forms of existence; but it is a teleological evolution in which every step and every result was foreseen and planned beforehand. The laws of such an evolution appear to me, in the present state of our knowledge, to be entirely unknown."

This last sentence certainly does scanty justice to Mr. Darwin and Mr. Herbert Spencer. It can hardly be denied that natural selection, including sexual selection, is a *vera causa;* and it is only by the study of known causes and their effects that we can discover what remains as the effect of unknown, and possibly unknowable causes.

No one admitting the omniscience of the Deity would question that every step and every result of evolution was formed and planned beforehand. The searcher after the laws of evolution assumes their existence, and considers that the present state of things represents their necessary result, and whether he seeks to know the Ordainer through the laws which He has ordained, or with Mr. Herbert Spencer conceives Him to be unknowable, makes no difference to the point of view from which he approaches those laws, however it may affect his religious belief.

While speaking of the relation of Professor Haughton's work to Darwinism, we must protest against the claim which Professor Haughton makes in his preface, to have explained the similarity of muscles and bones in the vertebrate animals on simple teleological principles, without making use of the unproved hypothesis of their descent from a supposed common ancestor."

To show in a large number of cases that the existing arrangement carries out its object without loss of work is a very different thing from showing that a totally different arrangement would not also possess this property; and this Professor

Haughton has not even attempted, except in the case of the heart, to which we shall recur.

Professor Haughton's investigation of the mechanics of parturition is highly interesting. In the investigation of the action of the muscles of the uterus itself, Lagrange's solution of the problem of the equilibrium of a flexible membrane, subjected to the action of given forces, is employed. He goes on to consider the action of the abdominal muscles in parturition. He treats of the mechanics of these muscles at some length and with great ingenuity, when he comes to the subject of muscles forming ellipsoidal surfaces. His results show that the different portions of the abdominal walls, and the diaphragm, possess different powers of compressing the contents of the abdominal cavity. But as these are mobile, so that the case is one of fluid pressure, the power of compressing the contents of the abdomen is limited to that of the weakest part of its walls; we cannot, therefore, accept his conclusions.

From the action of the abdominal muscles in parturition Professor Haughton infers that the use of anæsthetics in parturition is highly dangerous: he seems to think their moral influence still more so when the pain to be relieved is due to "the exercise of a purely natural physiological function." This stoic doctrine may command respect, but not general acceptance.

We now come to the main portion of the book, on the mode of action of muscles generally. They are classified for study according to their geometrical form, on which, as he points out, their mechanical action depends.

The case of muscles with parallel fibres is simple enough; but when we come to "muscles with their fibres rectilinear, lying in the same plane, and not parallel to each other," there is considerable room for differences of opinion.

We must premise that muscles of considerable thickness are included under this head; they may often be resolved into a set of parallel triangular muscles. Our author's use of other geometrical terms, such as 'point,' 'right line,' as applied to muscles, is rather lax. He defines triangular muscles thus: "In these muscles the origin is a point, and the insertion of the fibres is a right line."

In these muscles he attempts to show that the direction of the resultant of the force of the muscle bisects the angle at the apex or point of origin of the muscular fibres. To do this he assumes that each fibre contracts with the same force whatever be its length. This is very true of the ultimate force of a muscular fibre; that is, of the force required to break it or prevent its contraction; but in the case of a normal contraction, supposing any two fibres of different lengths in the muscle to

contract alike throughout their lengths, each will tend to produce a velocity in its point of insertion directly proportioned to its length. Now assuming this, and that the muscular fibres in a triangular muscle are inserted uniformly along its base, and not as he assumes, that they radiate uniformly from the point of origin, it follows that the direction of the resultant velocity due to the contraction of the muscle is the line joining the origin with the middle point of the base.

We do not set up this result in opposition to Prof. Haughton's, but state it to show the extreme danger of reasoning upon doubtful assumptions. All Prof. Haughton's remarks about loss of work in muscles with their fibres not parallel, which so ill accord with the principle of least action in nature which he so warmly supports, depend on this view of the action of triangular muscles, and therefore stand or fall with it.

Prof. Haughton's remarks on the waste of power in the radial muscles of the iris are not only inconclusive in themselves, but exhibit a *reductio ad absurdum* of his view of radiating muscles. We cannot discuss them for lack of space.

Some valuable remarks follow on the mechanics of sphincter muscles, especially on the complex system of muscles of the human mouth.

After the observations before referred to upon muscles forming ellipsoidal surfaces, we come to his consideration of skew muscles. He divides this very difficult head into two parts, assuming that their action tends first to bring the skew surface into a plane; secondly, to produce motion in that plane.

We next come upon an extended investigation into the action of quadrilateral muscles in relation with bones and joints. This is based upon our author's theory of triangular muscles, and upon the following postulate, p. 238:

"The Framer of the Universe (Δημιουργός τοῦ κοσμοῦ) has constructed all muscles on the principle that each shall perform the maximum of Work possible for it under the given external conditions."

This he considers will be admitted either as an *à priori* truth, or an inductive truth demonstrable by instances. Although the ingenuity of his reasoning is very considerable, and his data and instances of high value as facts for the comparative myologist, we cannot feel much confidence in his deductions, owing to the uncertainty of their foundations.

The theory of the hip- and shoulder-joint occupies the next portion of the work. The results of the observations are summed up in the form of four propositions—to which he says he has never met with any exceptions—to the general effect that the

combined arrangement of muscles, bones, and joints in all animals is such as to perform its work in the most advantageous manner possible. On these propositions he bases six conclusions (p. 387):

"1. That a foreseeing Mind planned the type of the limb, and of its actions.

"2. The idea of the limb and of its necessary actions being given; the number, shape, and arrangement of the necessary muscles can be calculated and predicted with as much certainty as an astronomer can predict an eclipse.

"3. That the shape and arrangement of the bones follow of necessity, from the necessary arrangement of the muscles.

"4. That any alteration, however slight, in any part of the combination of bones, muscles, and joints, would entail a loss of work, and lead to a less perfect mechanism.

"5. Hence the permanence and stability of each species (as far as relates to bones, muscles, and joints) is absolutely secured, on the principles so admirably laid down by Mr. Darwin.

"6. The profound study of the mechanism of joints lends no support to the postulate, that the similarities found to exist in the bones, muscles, and joints of animals may be explained by common descent from a supposed common ancestor."

That variations are commonly found seems fatal to 4, while 5 and 6, with their covert sneers, fail for want of support.

We wish Professor Haughton joy in his discovery of these "principles" which nobody laid down, and this "postulate" which nobody made.

It must be remembered that most of all men the believer in evolution would maintain that everything as it now is, is the necessary consequence of everything as it has been, and, therefore, that every result of evolution must have been foreseen by an Omniscient Being.

We fail to see how Professor Haughton can reconcile his views with the very numerous cases of organisms differing most widely in the principles of their construction, adapted to fulfil the same or nearly the same conditions. For instance, from his point of view, we should not expect to see the noctule and the swift feeding side by side on the same insects, and moving in a manner so nearly similar that they are indistinguishable at a moderate distance. The Creator of a ready developed universe would surely have fixed on one Type of mechanical structure as the best possibly adapted to the conditions which, on the direct-creation hypothesis, might be combined with internal organs adapted to viviparous or oviparous reproduction, to hibernation or migration. The same might be said, *mutatis mutandis*, of humming birds and humming-bird moths, and of the jumping arrangements of fleas and poduræ, and endless other cases.

Under the head of Muscular Types Professor Haughton gives a large amount of statistics as to the proportional weights of the muscles of the limbs of mammals and birds and of a few reptiles, with some interesting remarks on their functional importance.

No part of Professor Haughton's work is more interesting to the physiologists than his application of the principle of least action to the arrangement of the muscular fibres of the heart.

It has long puzzled descriptive anatomists to ascertain accurately what this arrangement is, but the elaborate investigations of Pettigrew seem to have untied this Gordian knot. Professor Haughton, however, makes no use of Pettigrew's researches, but proceeds to cut it with the help of his theory, supporting his views, however, upon dissections made for the purpose, and being apparently in accord with the views of Dr. Sibson. It is very difficult to do justice to Professor Haughton's view in the short space at our disposal, but the interest of the subject seems to demand a few remarks.

Professor Haughton begins by pointing out that, in order that the principle of least action may be satisfied, the arrangement of the fibres must be such that each fibre shall be free to contract proportionally to its length. After showing that one simple arrangement would not satisfy this, he gives a brief description of what he conceives to be the actual arrangement of the muscular fibres of the ventricles.

He divides them into—

1. *Common* fibres passing round both ventricles.
2. *Proper* fibres passing round the left ventricle.
3. *Proper* fibres passing round the right ventricle.

He says, p. 435, *et seq.*, that—

"All the muscular fibres of the ventricles take origin from the tendinous zone," which surrounds the orifices of the heart, "and having wound round the heart in the manner presently to be described, either return directly to the tendinous zone, or indirectly through the intervention of the *columnæ carneæ* and *papillary muscles*."

The common fibres of the heart arise from the outer side of the tendinous ring, and after describing a spiral course, turn in at the apex and pass up to the tendinous ring on the interior, describing in their course a complete circumference, and returning to a point opposite their origin. In the cavity left between their outer and inner course lie the proper fibres of the two ventricles, likewise inserted at their two extremities in the tendinous rings, but describing a complete circumference, and 72° between their origin on the outer side and their insertion on the inner side of the tendinous ring; thus, these proper fibres in their contraction tend to twist the heart, and, as it were, to wring it out.

Thus far Professor Haughton's account is not opposed to that given by Pettigrew; the remaining points on which his theory mainly depends rest on his authority alone.

Professor Haughton finds that the fibres both of the common group surrounding both ventricles and of the two proper groups are equal among themselves, within the limits of errors of observation. He then points out that as the change of volume of the contents of the common fibres, *i. e.* that of the two ventricles, is twice that of the contents of one set of proper fibres, say those of the left ventricle, the ratio of the lengths of the common and proper fibres must be as the cube root of two to unity.[1] In two specimens in which the common fibres were $13\frac{1}{4}$ and $8\frac{1}{2}$ inches in length respectively, the calculated and observed lengths of the proper fibres of the left ventricle differ by less than a quarter of an inch in each case, a most remarkable approximation, and one deserving most careful consideration and an extensive series of observations.

We should be glad to know more details of the measurement of the fibres, as on this turns the whole point; for although the equality of length of the fibres of each group is not essential in order that the law of least action should be complied with, if the arrangement be modified accordingly; unless there is this equality there can be little hope of demonstrating that it is complied with.

There can be no doubt that the heart is a most efficient machine, and therefore that the law is very nearly true.

There are in an earlier portion of his work, pp. 137—151, some interesting investigations into the mechanical work done by the heart.

One method of ascertaining this is original, but we fear not

---

[1] Let L = length of common fibres,
l = proper fibres of left ventricle,
$\lambda\rho$ = vols. of left and right ventricles,
and let L, l become L', l' when contracted;

then $$\frac{L^3 - L'^3}{l^3 - l'^3} = \frac{\lambda + \rho}{\lambda}.$$

If n = coeff. of muscular contraction we have—

$$\frac{L^3(1 - n^3)}{l^3(1 - n^3)} = \frac{\lambda + \rho}{\lambda},$$

or $$\frac{L^3}{l^3} = \frac{\lambda + \rho}{\lambda};$$

but probably $\lambda = \rho$ very nearly in life;

therefore, $\frac{L^3}{l^3} = 2$, or $\frac{L}{l} = \sqrt[3]{2}$.

See 'Principles of Animal Mechanics,' pp. 439—441.

very much to be relied on. It depends on the distance to which blood was spurted when a large artery was divided in an operation. It is a good instance of Professor Haughton's extraordinary acuteness.

The conclusion of Professor Haughton's work is devoted to the consideration of the three following laws of muscular action laid down by him (p. 442):

"Law I. In comparing together different muscles, the work done in contracting is proportional to the weight of each.

"Law II. In comparing the same muscle (or group of muscles) with itself, when contracting under different external conditions, the work done is always constant in a single contraction.

"Law III. When the same muscle (or group of muscles) is kept in constant action until fatigue sets in, the total work done, multiplied by the rate of work, is constant."

Law I he calls Borelli's law, as it corresponds to his 121st, 122nd, and 123rd propositions, but Borelli, more cautious than his followers, only propounds it of the muscles *ejusdem animalis*, and so far only can we grant it, for muscle differs greatly in quality.

Instead of basing Law II upon laboratory experiments admitting of high accuracy, and not involving nervous energy, at least as much as muscular energy, Professor Haughton supports it on some experiments of Mr. Stanley Jevons (in 'Nature,' 30th June, 1870).

In throwing increasing weights the accordance between Professor Haughton's theory and Mr. Jevons' practice seems remarkable, but as it follows from the theory that Mr. Jevons' maximum throw when the weight thrown vanishes is $29\frac{3}{4}$ feet, while it is well known that any good thrower can throw a cricket-ball weighing over $5\frac{1}{2}$ oz. 100 yards, the numerical results of Professor Jevons' experiments appear open to criticism, and we confess ourselves astonished to find Professor Haughton setting down as a conclusion that *the* throwing muscles can produce on *the* hand a velocity of 30·9 feet per second.

Law III. The law of fatigue is the most important of Professor Haughton's laws. It is supported by experiments of Mr. Jevons in raising weight with a pulley, and of Mr. F. E. Nipher, of Iowa University, Dr. Macalister, and Mr. Gilbert Haughton, in raising weights by swinging them up with the arm alone, and especially by experiments on holding the arms out horizontally with different weights; these latter probably involve the fewest sources of error.

In all these experiments, however, nervous energy as well as muscular energy is tested, so that this law of fatigue, however well supported by them, can scarcely be called a law of mus-

cular action at all. Until laboratory experiments are carried out affording sufficient data for the support of these or other laws, we are not in a position to pronounce upon them.

From Mr. Stanley Jevons's point of view, as guides for the economical application of labour, Professor Haughton's reasoning and experiments are worthy of all consideration, as are his concluding observations on the law of refreshment, which are specially interesting, but on which we can find no space to dwell.

We have endeavoured to give a brief account of Professor Haughton's book, and if our remarks seem on the whole adverse to its value, it is due to a desire to caution readers against too hasty an acceptance of some of his conclusions, which seem to us to rest at present on insufficient grounds, rather than to any wish to detract from the real value and interest of this most suggestive work. It should be read by all those interested in this highly practical and important branch of physiology, who, whatever they may think of some of the conclusions arrived at, cannot fail to have suggested to them at every turn important subjects for investigation, and to have their attention kept up throughout by Professor Haughton's brilliant and amusing style.

VI.—Military and Naval Reports.

We have grouped together some books which have many points of resemblance with each other, and also many points of difference. In several of the individual works the contents are of very varied character, embracing dry statistics with scarcely a word of comment, and also full histories of disease. The exhaustive abstract of the year's Hygiene, by Professor Parkes, has little analogy with an ordinary official report, and is not capable of analysis by us. Fitzgerald's elaborate reports on Prussian army organization would form the basis for a separate review, and might be compared with advantage with Inspector-Gen. C.

[1] 1. *Statistical Report of the Health of the Navy for the Year* 1870. London, 1872.

2. *Army Medical Department Report for the Year* 1870. London, 1872.

3. *Eighth Annual Report of the Sanitary Commissioner with the Government of India for* 1871. Calcutta, 1872.

4. *Report of the Sanitary Commissioner for Madras for the Year* 1871. Madras, 1872.

5. *Memorandum on the Cholera Epidemic of* 1872 *in Northern India.* By J. L. Bryden, Statistical Officer. Simla, 1872.

6. *Traité de Climatologie Générale du Globe, Etudes Médicales sur tous les Climats.* Par Le Docteur Armand. 8vo, pp. 868. Paris, 1873.

Gordon's valuable experience of French army organization, or with the account of the latter subject given in the last number of this Journal.

We shall pursue mainly the same course as we did last year, and run through the points which appear to us to be of chief interest, especially with reference to the geography and the climatology of disease, entering a little more fully on some of them, that are imperfectly understood.

The Indian Reports, valuable though they are, are of less importance than the others with respect to the particular object in view, and we shall only have occasion to allude to them incidentally.

The health of the navy was good during the year 1870, and it follows almost as a corollary, that there was no great variety in its medical history; accordingly, there is nothing very striking in the reports for the year. In the report for the Mediterranean station there are some interesting remarks on intestinal worms —tænia and round worm were of most frequent occurrence. Surgeon Littleton, of the Bellerophon, very justly remarks, that while worms give indication of their presence in women, and still more in children, they are often unobserved in men. During twenty-two years' service he only once knew of mischief occuring in one adult from this cause—he died in a convulsive fit. We can bear testimony to the extreme frequency of the occurrence of round worm in sailors of merchant vessels in the tropics. Indeed, in many places in the tropics, their presence, both in sailors and in children, is frequently only discovered after death. One surgeon considers that diseased meat in sausages has been the common cause of tænia; while another attributes it to water got at the Piræus and other places.

Deputy-Inspector-General Donnet continues his interesting reports on yellow fever, which was fortunately less prevalent than the year before; but there is nothing very novel in them, or in Surgeon Turnbull's account of the same disease, except that the latter is able to record an outbreak, of what was believed to be yellow fever, at Asuncion, in Paraguay, which is more than six hundred miles, by river, from Monte Video. The old difficulty, of yellow fever being complicated with malarious poisoning, recurs, and the excessive liability of unacclimatised men to suffer from yellow fever is commented on.

On the China station there was, as usual, an immense deal of chronic diarrhœa. If it only caused two deaths, it was the occasion of much invaliding. Surgeon Buckley observes that it is very difficult to understand, how this constant diarrhœa flux can go on day after day, for more than six months, without causing some organic and serious injury to the internal mucous membrane. It usually in the end does so.

The account by Dr. Macleod, of the Royal Naval Lunatic Asylum, is again full of matter of interest. He bears ample testimony to the calming and soothing influence of chloral, and to its special usefulness in cases of general paralysis.

The report on Sheerness gives a very full account, by Staff-Surgeon Haran, of the meteorology of the Isle of Sheppey. Its mean temperature is somewhat higher, than that of the United Kingdom or of Kew. The condition of Sheerness is most unhygienic—it is full of cesspools, and its low level presents an insuperable obstacle to drainage. Nevertheless, the death-rate of 17 per 1,000 is particularly low. No length of residence appears to give immunity from the effects of malaria; but the ague is not very acute, and it leads more often to enlargement of the liver, than of the spleen.

Turning next to the Army Returns, the report of Dr. Maclean from Netley is very interesting. It contains elaborate accounts of many cases of pulmonary phthisis, and he is able to say, that malarious districts contributed quite as many cases of that disease, as non-malarious ones. It is worthy of observation too, that Dr. Bryden has established on a statistical basis the fact, that phthisis is one of the three diseases, by which unacclimatised regiments suffer most in India.

A very remarkable history of hepatic abscess is described, in which no fewer than 600 oz. of pus were withdrawn by Dr. Dieulafoy's aspirator by successive operations, and the patient's life prolonged for five months. Dr. Maclean also gives us the results of his experiments in the hypodermic injection of the neutral sulphate of quinine dissolved in warm water, which is less irritant, than the usual salt dissolved in an excess of acid.

He complains of relapses in diarrhœa and dysentery in patients from the tropics, owing to their arrival in England in cold and wet weather, without being sufficiently protected by suitable clothing.

He also offers some important remarks on the subject of invaliding, and discharge from the service.

It has been remarked, that for some years past the number of the men invalided from India, has been greatly increasing. We need not inquire here into the question, how far this accounts partially for the diminished mortality of troops in India. Men appear to be invalided, either for final discharge from the service, or for change of climate.

That there is considerable mortality among men sent from India, is very certain. Thus, we learn from a return of effective men returning from India, *viâ* Egypt, that the mortality amounted to 25·2 per 1000 of the mean amount of strength, chiefly from diseases of the digestive organs; while among

troops from the Cape of Good Hope and Australia, it was 13·1, and from the West Indies, 16·8.

In the medical division of Netley in the year 1870, out of a total of 1582 invalids, 52 died, 1192 were discharged the service, and only 250 men were returned for duty. The explanation offered of this, is the extreme gravity of the cases. Dr. Maclean thinks, that many of the men might have eventually become fit for service, if they had been kept for eighteen months at home doing light duty, but they would, if returned fit for duty, have all been sent back to their regiments in India, and very likely to the spot where they had contracted their illness, and would have been quite sure to break down. Under such circumstances, it was more for the interest of the men themselves, and of Government, that they should be discharged the service.

We glean some further information of interest, on a point materially influencing the health of soldiers in India.

On the water supply of India Sir W. Muir offers these judicious remarks:

"An opinion seems to have got abroad, especially in England, that the European troops in India are very indifferently supplied with water of a pure and wholesome quality, and that most of the disease to which they are subject, is traceable to this source. He believes this idea to be an exaggerated one. With few exceptions, the supply is adequate and wholesome. The exaggeration, to which he has alluded, has been due in a great measure to the water question being mixed up with the theories of cholera, and of other epidemic diseases. While freely admitting that a pure water supply is one of the first conditions of health, and must be worked up at any cost, it is due to the authorities to state, that great exertion has been made to secure this boon to the British soldier serving in India, and generally with a success that can not be gainsaid."

The India Sanitary Commissioner states, that the utmost attention continues to be applied by the authorities to the water supply in all stations.

One case is recorded by Staff Assistant-Surgeon W. K. Stewart of that very horrible affection, well-known in some parts of India, the development of larvæ in the nasal passages, otherwise, the generation of maggots. This fatal disease is not very uncommon among natives, but a sergeant of the 60th Rifles is one of the first Europeans that has succumbed to it. In natives, the ears as well as the nose are often attacked, and the sufferings are so intense, that the unfortunate patients often commit suicide.

In this instance, it was found on examination after death that, besides the soft tissues being furrowed by many canals, the nasal bones and upper surface of the palate bones, and the

inferior aspect of the ethmoid, were denuded of periosteum, and covered with a blackish, putrid substance. The turbinated bones were in great part destroyed; the frontal cells were free from larvæ, and no communication with the encephalon was discovered. In other cases the encephalon has been reached.

It is believed that the larvæ were of the fly *Sarcophaga ruficornis*, a well-known Indian species. In this particular case, no ulceration or open surface had been observed on the nose; the treatment by various injections was entirely without result. Where maggots are developed in ordinary wounds, they can be picked out, and turpentine may be made to reach them, but there is no way of dealing with the spongy bones of the nose. We know of no more hopeless affection than this; the treatment, so successful in the management of ordinary wounds, when maggots make their appearance, can not be efficiently applied in these labyrinthine passages.

Assistant-Surgeon Batho gives a very interesting account of the endermic hæmaturia of certain districts of the Cape Colony. Its connection with the presence of Bilharzia was shown some years ago by Dr. Rubidge, of Port Elizabeth, and more recently Dr. Harley called attention to the subject.

Some circumstances are remarkable—the occurrence of the disease only in males and in boyhood; its not affecting the general health materially; and its disappearance in adults. As yet no explanation of these peculiarities has been offered. No mode of treatment appears to influence the course of the affection, which disappears of itself.

Connected with this, are the recent researches of Dr. Lewis in Bengal, showing the presence of some forms of hæmatozoa in chylous urine, and also in the blood of patients suffering from it. It is a disease of not unfrequent occurrence in tropical districts, in India, in the Mauritius, in Madagascar, in Senegal, in Brazil, and in the West Indies, and occasionally interchanges with hæmaturia. The connection of the two requires further investigation. It also is by no means amenable to treatment. Change to a cold climate usually operates beneficially, though it may not produce a cure. Although the disease occasionally terminates in death from exhaustion, it may go on for many years, without affecting the general health very materially. In India, the disease is more common in white men, who have been born in the country, than in those born in Europe.

Dr. Barry contributes a full report on the occurrence of Beriberi in Borneo, which, if it contains nothing absolutely new, at least supplies a more careful record of cases, than is usually furnished. It has induced us to look a little into this complicated

subject, and a review of such materials as we have before us, has led to the conclusion, that two conditions of the system have been confounded under the name of Beriberi or Barbiers, namely, one of general paralysis, and one of general dropsy. Though they meet each other in some of their symptoms, they are so different, that they should not have been confounded.

Bontius, 240 years ago, described in Java a form of paralysis, which, he said, went by the name of *beri beri* (which the French afterwards altered into Barbiers), as the gait of those suffering from it was supposed to resemble that of sheep, for whom *beri* is the Hindostanee name. He describes the disease, as a gradual loss of power and sensation in the hands and feet, with general tremor, lassitude of the system, and loss of voice. It is usually gradual, but sometimes comes on suddenly. He thought it the result of cold and damp on a debilitated constitution—one in a state of cacochymia. He mentioned, he says, only the paralytic symptoms, but there were others indicating cold and tenacious humours. He does not hint at dropsy or at convulsions, though he has full chapters on dropsy and spasms, both of which he represents as being of very common occurrence. Lind, in 1750, and Clark, in 1790, like most authors, do little more than give a translation of what Bontius said. Lind added, that he had heard of a native cure of the disease, by burying the patient, all but his head, in the earth.

Dr. Heymann[1] gives an account of the form of disease described by Bontius, and as he writes of the district, where it was originally observed, his notice of it is worth considering, especially with reference to the epidemic occurrence of the disease.

He says it is endemic in Sumatra, and shows itself sometimes in single epidemics in the Moluccas, but much more rarely in Java. The malady consists in a gradually invading paralysis of the lower extremities. The patients have at first a feeling of creeping of ants along the spine, and tolerably strong pressure between the twelfth dorsal vertebra and the os coccygis, causes such acute pain, that the patient can scarcely stand. There is no fever in the commencement, but it associates itself, when the disease is further advanced.

Several epidemics have prevailed in the Spice Islands in the years 1839, '41, and '44. The first two of short duration, the last of longer. Without any previous warning, people were seized suddenly with a feeling of formication in the lower extremities. This feeling, which spread upwards, soon increased to pain, loss of sensation, and paralysis. In the worst cases the upper extremities were attacked also. Along with these symp-

[1] 'Krankheiten in der Tropen-ländern,' 1855.

toms there was obstipation. There was a feeling as of a tight cord round the abdomen, which gradually rose and led to oppression about the præcordia, embarrassed respiration and difficulty in making water. Œdema of the extremities was not rare. The mortality was very great. Few women or children or old men were attacked. The disease was ascribed to heavy rains and cold winds.

M. Vinson, in 1853, gave an account of beriberi, as it prevailed in the island of Réunion. From the commencement of the century there have only been four epidemics, in 1805, 1828, 1838, and in 1847. The last two epidemics were slight, but the former ones were violent. Vinson describes two forms of the disease.

The pains in general precede the paralysis, but sometimes the paralysis appears spontaneously, and without any observable precursors. It is temporary or permanent; sometimes it comes on suddenly, at other times gradually. In the latter case, it generally attacks both the legs at once; the arms are less frequently attacked. It sometimes extends to the muscles of the trunk, and thus interferes with the functions of the abdomen and of the thorax. Dyspnœa and oppression of breathing, and sometimes difficulty of deglutition, supervene; in some very rare cases slight convulsive movements are superadded, as in the acute form of the disease now about to be described. There is much more loss of power than of sensation, and there is a notable loss of heat in the lower extremities. The acute form of beriberi is characterised by fever, by violent pains in the legs, and in the loins along the vertebral column. Sometimes the invasion of the disease is announced by heaviness of the head, general malaise, and feeling of formication over the whole body. On this the fever supervenes, and is accompanied by spasmodic or convulsive movements of the legs and arms, which sometimes extend to the trunk. At the same time paralysis shows itself. But usually beriberi is ushered in by violent fever and acute pains in the loins. M. Vinson does not make any allusion to dropsical complications.

After this account of violent fever associated with beriberi, one is not surprised to find, that the doctors of Bahia and Rio, doubted what was the form of disease, since described by Dr. Lima as beriberi, which broke out among the blacks of the army of Paraguay, and generally took it, for an epidemic spinal meningitis.

Dr. Lima has lately described in Bahia a form of paralysis exactly agreeing with that of Bontius, and with the milder form of Vinson. He has probably gone a little too far, in adding to the forms of beriberi, a paralysis of the lower extremities, which

is not very rare after confinements in the tropics, and, indeed, occasionally occurs in Europe.

We must not leave this paralytic form of the disease, without remarking, that there is an affection having considerable resemblance to it, produced in many parts of India by using as food a variety of Lathyrus or vetch. Paralysis of cattle, and what is commonly called going in the loins of horses, is sometimes attributable to this cause. But, as far as we know, its effects are never fatal, and it is in no way connected with either local or general dropsy.

But Dr. Lima also mentions an œdematous form of the disease, and this is the malady, for the last forty years described by English writers, chiefly under the name of beriberi. Official reports of deaths in the English army in 1843 describe the beriberi of Burmah, of Ceylon, and of the Mauritius, as a dropsy sometimes attended with loss of power in the extremities. Such is the usual description of beriberi. Œdema, general dropsy, and dyspnœa, we have often enough observed in sepoys who had long suffered from fever, or in coolies, come off a long voyage, with scorbutic symptoms; but though there was often heaviness and some numbness in the limbs, we confess that we have not seen what could be called paralysis; and it is a question—how far true paralysis is present in such cases.

We can scarcely pretend to have materials yet, for the full solution of these questions, but it seems to us that general dropsy, because it may be accompanied by some loss of power in the extremities, is not general paralysis, any more than is paralysis, because there may be some œdema of the extremities, general dropsy; but we would venture to suggest, and it is mainly what Morehead has advanced, that the root of the confusion lies in this, that the œdema which occasionally accompanies the last stage of the paralytic affection, has been looked on as a primary, not merely a secondary incident; and that, in consequence, many forms of anasarca and dropsy (of the frequency of which in those districts Bontius was fully aware), in which there is necessarily from mechanical pressure impaired movement of the lower extremities, and oppression about the chest, have been mixed up with the true paralytic affection.

Armand, in his work, which we shall have presently to introduce to our readers, although all his remarks on the subject are not very consistent with each other, makes this observation: —"By what strange confusion has beriberi been mixed up with anæmia and scorbutus?"

We are much in want of post-mortem results to assist us in clearing up the subject. Usually, only the ordinary appearances connected with dropsy are observed. Dr. Barry, in two

cases, although there was more fluid than usual present in the theca, in both instances found the cord firm and natural. M. Vinson does, indeed, say that he found softening of the cord, but then this was found in those acute doubtful cases mentioned above; besides, he had, like Malcolmson, made up his mind that the cause of the disease was myelitis. Lima, in one case, thought the cord of less than the normal consistency. Were the affection structural, we could not have the cures that are constantly effected by simple change of air.

With respect to the etiology of the disease, the same causes have been assigned for both forms of it. It prevails more in adults than in women and children, and is unknown in old men. It chiefly occurs in persons, who have been for some time in the place. It supervenes mainly, where the blood has been impoverished by fever or by tropical residence, but it sometimes occurs in those who are quite healthy. Mental depression and nostalgia favour its development. The disease appears to be curiously limited to the equatorial region and to a few degrees on either side of it. It is known in Southern India, in Burmah, in Ceylon, a little in Java, more in Japan, in the Mauritius, and latterly there seems to be no doubt that the disease has developed itself in South America.

We turn now to the result of the life-long studies of a distinguished French army surgeon, from which we have already extracted Dr. Vinson's account of beriberi.

Dr. Armand's book, although its title is a very extended one, and embraces very wide subjects, may appropriately be noticed here; as the work, at least the portion of it in which he speaks from the results of personal experience, is made up of reports on Algiers, on Rome, on the Crimea, and on Saigong; in short, on the parts of the world, where he has had to serve with the arms of his country. He has, however, brought together from every quarter a great deal of information, including accounts of the climates of North and South America: information which is not to be found collected elsewhere. It is easy to perceive a want of unity in the book, a great disproportion of its parts. We have only two and a half pages about Great Britain, and eleven about the Arctic regions. India is represented by an account of Pondicherry and of Ceylon; and with English works and Reports generally Dr. Armand has but a scanty acquaintance. We have some fifty pages about fibrinous concretions of the heart, under the head of the climate of Lyons; long dissertations on opium smoking, and accounts of the popular medicines and superstitions of Bretagne. There is a general introduction on climatology and anthropology, occupying 150 pages, which was scarcely needed here. But in spite of all faults of excess on

some points, and deficiency on others, the volume may be consulted with advantage by all, who study the general relations of disease.

Many of the views propounded of late years by various of the younger writers of India as novelties (and not one of them is really a novelty in Indian medicine), have been insisted on by Dr. Armand in his writings for the last twenty-five years. Thus, he regards fever, cholera, and dysentery, as all arising from the same cause. He has no doubt about cholera being a pernicious intermittent fever, and trusts in quinine, as being the best remedy for it—indeed almost a certain one. He believes in marsh poisoning, but not in its acting by the generation of malaria. He does not believe in the existence of any such vegetable poison. He attributes the causation of all tropical diseases to heat and cold and damp, or, as he calls them, thermo-electro-hygrometric influences.

He accounts for the periodicity of attacks, towards the explanation of which the malaria theory offers no help, by saying that in fever the nervous system is the part primarily involved, and that all nervous affections have a tendency to periodicity.

He looks on relapses as fevers newly contracted, and often occasioned by very slight causes, owing to increased individual susceptibility, which is the consequence of previous attacks.

Wide questions like these we cannot discuss here, but some of his general arguments against the existence of malaria, are drawn, from the trifling extent of the marshes in many malarious countries, from the small amount of malarious diseases in the southern hemisphere, even in spots which, according to all theory, should be hotbeds of them; for instance, Valparaiso, and New Caledonia; and again, the prevalence of malarious disease in Sierra Leone, where there are no marshes or stagnant water. The subject has been fully illustrated by Dr. Oldham in his ingenious work on malaria.

Dr. Armand has little belief in contagion in any shape, and is against all quarantine, which he considers leads to inhumanity. He does not believe that under any circumstances of putrid emanations from the excretions, dysentery becomes contagious. He looks on cholera as generated like other fevers, mainly by solar heat, and as air-borne. It does not appear that he has ever heard of polluted water, and still less of *grund wasser*. He does not allude to the excreta as noxious.

Armand tells us with great gusto, how the inhabitants of the Chincha islands, who live in an atmosphere of guano, are particularly healthy, and how the vapours disengaged from that substance on board ship, cure the bronchitis of the sailors.

With reference to his military experience, we could have wished that Dr. Armand had said more about invaliding and change of air for soldiers. He discusses but slightly change of air by alteration of latitude or of altitude.

On the first head, he does not like sudden changes. He would even have some intermediate point between Algiers and France, as Corsica, or Majorca, and Minorca. At all events, he would, in the first instance, detain at Toulon or Marseilles natives of the north of France, before they were allowed to proceed to the north. The rapid changes from England to India, and from India to England, which are the result of our Red Sea communication with the East, would be too sudden to meet his views. And here we may remark, that the volume generally gives no information on the subject of choice of climate, and that the account given of Algiers, with reference to phthisis, is not very encouraging, so far at least as he considers the disease to be common in that country, as well as in Toulon and the South of France.

Nor on change of altitude, does Dr. Armand say a great deal more. A slight change of altitude, as to the hills near Rome, was often found sufficient by him in cases of fever; but he gives Coindet's experience in Mexico, which it is worth while to compare with our own observations in India.

Coindet's conclusions are these:—1. Intermittent fevers profit more by a residence on the high plateaus, than the diarrhœas and dysenteries contracted in the low country. 2. All affections contracted in the low country, improve while they are still tolerably recent, and the strength of the patient is fair. 3. The beneficial effect of a high climate is less marked, when a case is already chronic, and the condition of the patient is cachectic.

We believe that all this agrees exactly with the experience of medical officers in India. Nothing is better than removal to the hills in convalescence from a sharp attack of illness, but congestions and enlargement of the abdominal viscera and diarrhœas or dysenteries often derive no benefit. Diarrhœa and dysentery are both frequent among the Mexican plateaus.[1]

Coindet considers, that the races who inhabit such heights as 13,000 feet, are, on the whole, anæmic and feeble. He is of opinion that Europeans can thrive best at altitudes below 6000 feet, and that a European race may do well there, but that above that height children are not so robust, as below it. At lower

[1] In the Army Reports before us, Assistant-Surgeon Davidson says of Wellington, at a height of 6000 feet in the Neilgherries, that men who have suffered from fever generally derive benefit from a residence in the hills. The diseases which do not suit the climate and are generally aggravated, are diarrhœa and dysentery, secondary syphilis, rheumatism, and heart complaints.

heights, those of about 5000 feet, it has been observed, in Europe, that there is a good deal of chlorosis and anæmia.

Armand gives a full and satisfactory account of that peculiar fever marked by a red rash and excessive rheumatic suffering and prostration, which has lately pervaded India to an extent unknown in former visitations. It has prevailed at different times in Réunion under the name of *red fever*, in Brazil under that of *polka*, in Senegal, in the Canaries, and is nearly identical with the *rheumatic* fever attended with *urticaria* in Algiers, or miliary eruption in Italy; in Senegal called *n'daga-monte*, elsewhere *courbaturale;* it is known at Aden and at Port Said, at the latter place under the name of *date* fever, from its usual season, when the dates are ripe.

It has been well and sufficiently defined by de Méricourt:

"Dengue is a febrile malady which at various times shows itself in an epidemic form, in tropical and subtropical countries of the new and of the ancient world. It is characterised by febrile action, gastric disturbance, by articular and muscular pains of rheumatic nature, by an exanthema approaching at once to scarlatina, rubeola, or urticaria. The convalescence is marked by a stage of extreme prostration. The prognosis is not serious. When the eruption is absent there is sometimes difficulty in distinguishing this fever in its early stage from yellow fever."

Without entering into anything that is controversial, we may notice a few facts mentioned in Armand's volume, respecting cholera.

Armand finds that cholera is endemic in the vast delta of Cochin China, which is watered by the net of rivers of the thousand branches of the Cambodge and of the river of Saigon.

It becomes epidemic in the dry and hot season of the year, that is from November to May, but especially during the months of February, March, and April. This fact is notorious to all doctors who have resided there, but, concurrently with cholera, fevers and dysentery are equally endemo-epidemic. Thus, cholera reigns in permanence in the deltas of the Ganges, and of Cambodge, which are also endemic seats of various forms of fever.

"In 1861," says Armand, "we had epidemic cholera during the dry season. Later, the cases became less numerous, when the rainy season was fairly established, and cholera then passed into its endemic condition, supplying a few sporadic cases only."

How completely does this accord with the course of cholera in Calcutta, as described by Macpherson and others!

Further, Bankok is a regular nest of cholera. It has had several frightful epidemics. Often, says Friedel, marsh poison shows itself in the choleraic form, and it resembles black

cholera so closely, that one often makes mistakes, but in these cases quinine succeeds. "Thus," says Armand, "because quinine succeeds, it is not cholera; it is only marsh fever of a choleraic form. I note this illogical conclusion, because it is one proof more that at Bankok, as in Cochin China, as everywhere, cholera is only a pernicious algide fever; the only efficacious remedy for which, that we know of as yet, in the great majority of cases, is sulphate of quinine, if it is given early enough and in sufficient quantity.

At Tjelatjah, in Java, the country is often desolated by cholera; it appears in two forms, the common or erethic form, and in an occasional one, the cholera sicca. In it there is no vomiting or purging. The prostration, the absence of pulse, the refrigerative character of the malady, mark it; all cases of this kind are fatal in a few hours. We should have liked to know whether such a form is epidemic at any time. Usually, such cases occur in India rarely and sporadically, and the more minutely such cases are examined, the more appearance is there that recorded cases have been observed imperfectly.

There is, of course, a great deal about cholera in the Indian Reports. We shall only offer one observation on them, and this is, that two of their chief authorities, Dr. Cornish and Dr. Bryden, while they differ on many points, seem always to look to a distant endemic source, to account for each outbreak of cholera. This is the more remarkable when we have, in all probability, independent sources of disease in other parts of the East, in those just mentioned by Armand; when the outbreaks of recent years in Russia, and the last and most recent one in America, are no longer ascribed to fresh reimportation from India; and seeing that it has been proved, beyond all doubt, that in former times cholera prevailed in districts of India, very different from those, which are considered at the present day to be its familiar and only sources.

Dr. Cunningham's report states, that there is a growing return to the old belief, that it is impossible to account for the spread of cholera by human intercourse only, and that it is not even one of the chief modes of its propagation.

Respecting what has of late years been called the typhoid of young soldiers in India, it has been pertinently asked, if typhoid be mainly the result of fæcal poisoning, why should it attack only the young and the unacclimatised? Armand, while admitting its presence in many parts of the tropics, is induced to believe that it is in its essence a variety of remittent fever, and he considers that it is amenable to the usual quinine treatment, an opinion in which the later Indian authorities differ from him.

From what has been already said on the nervous affection in beriberi, it might have been inferred, that there is something very peculiar in the extreme sensitiveness of the nervous system in tropical climates, and in the readiness with which convulsive or tetanic spasms are induced,

Dr. Letzer has observed at Bima an epidemic of rheumatismal tetanus. The history of it is the more interesting, because Bontius gives an account of a similar affection, and almost all the early voyagers describe sailors as apt to get such attacks. According to Dr. Letzer, this attack, without any distinct warning, surprised individuals in the midst of their ordinary occupations. Suddenly they were seized with a feeling of extreme weakness, which passed into a state of sleepiness. No febrile disturbance accompanied this first phase of the malady. But after that, an access of tetanus developed itself suddenly in the form of opisthotonos. After repeated accesses of convulsions, a period of remission of them came, but the state of stupor remained, and often ended in death. This account differs from that of the spasms described by Bontius in the state of stupor, which he did not observe. This condition seems to resemble much the *fièvre pernicieuse soporeuse* of Torti, and the *hypnosie ou maladie du sommeil des nègres,* which has been thus described in Senegal, and only of late years, as special to that coast.

It appears specially along the western coast of Africa, between the 15° and 20° latitude north and south. The sleep becomes every day longer and more profound, and finishes by becoming continued. The patients cannot be awakened by any stimulation, and die in coma; this is the invariable termination. There has never yet been a case of recovery. Autopsy reveals a state of congestion of the brain and its membranes. The cause of this disease is unknown, but Armand thinks there is little difficulty in diagnosing it as fièvre pernicieuse comateuse, which should be cured by employing quinine in large doses, *intus et extra.*

A chronic form of hypnosis, said to be caused by the pressure of enlarged cervical glands, and which the negroes cure by excision, has been recently described in the 'Medical Times.'

There is another tropical affection in which, as in the case of beriberi, much confusion has arisen from the occasional presence of paralytic symptoms, viz., dry or vegetable colic. It has been observed in the East Indies, in Java, in Tahiti, in parts of China, in French Guiana and Cayenne, in Senegal, in Valparaiso. In many of these places there is no paralysis with it. It is often attributed to vegetable diet, but apparently without sufficient reason; it is an enteralgia, probably connected with

neuralgia of the ganglionic nerves, and usually the result of febrile anæmia.

But in a certain number of cases paralysis is also present, and one result of this has been, that the disease is usually considered only a form of lead poisoning, and undoubtedly in many instances it has been proved to be so. Still we have no doubt that the disease is an independent one. If the disease were simply lead poisoning, why should it be almost entirely confined to tropical and subtropical districts—why does it diminish on leaving the tropics ?

Dry colic further differs from lead colic in its more sudden onset, in its earlier cessation, in prevailing at particular periods of the year, for instance, in Senegal, in the fourth quarter of the year, and in its especial prevalence at particular places, such as Gabon.

The line on the gums, characteristic of lead poisoning, is absent in dry colic; but yet, as the presence of scorbutic symptoms may make this sign less distinct, M. Ballot has laid down the following characteristics of the pulse, as diagnostic of the two diseases.

In lead colic the pulse is hard and vibratory, and becomes slower as the poisoning increases, and becomes quicker again as the poisoning diminishes.

In dry colic the pulse is feeble and regular at the beginning, and increases in frequency and in irregularity in proportion to the intensity of the attacks of colic, and becomes slower and more regular as the attacks are less frequent.

Another diagnostic sign would be, the readiness with which, according to French authorities, dry colic, which they consider a form of paludal poisoning, yields to treatment by quinine.

There is no reason for assuming, that because colic and paralysis are associated in lead colic, their association must necessarily in all cases be connected with the action of lead. Indeed, the immensely wide diffusion of dry colic excludes the possibility of its being solely attributable to metallic poisoning.

We have had occasion to observe many cases of dry colic in natives of India, and several in Europeans, in which there was not a trace of paralysis, and in which, as they lasted over many years, and sometimes did not yield to change of climate, there could have been no lead ageney.

Armand gives a useful chapter on the diseases of Northern America, founded on documents of medical statistics relating to the United States, translated by M. Vauvray. It contains a curious table, contrasting the relative frequency of deaths from particular diseases in the United States among whites and among blacks. As we are not told from what sources it is

compiled, we shall only state shortly some of its results. Cholera is about equally fatal in whites and blacks, but infantile cholera is far more frequent among whites. Blacks suffer much less from yellow fever, typhus fever, variola, scarlatina, and somewhat less from intermittent and remittent fevers; also less from affections of the liver and intestinal canal. They die much less from scrofulous and tubercular affections, and from phthisis. The blacks die very much more of acute affections of the respiratory organs. There is an enormous excess of mortality among the blacks from hooping-cough, dentition, scrofula, rickets, and verminous affections. Although blacks die in five times the proportion of whites from tetanus, yet the white race suffers much more from affections of the nervous system generally. Although blacks die more in their confinements, puerperal fever is more common in whites.

It would be interesting to enquire how far these differences are dependent on race, on climate, or on mode of living.

Armand has a good many scattered notices about leprosy, which in the main bring out very distinctly that the disease is non-contagious, but is hereditary—points on which the solid judgment of Dr. Gavin Milroy has satisfied him, as the result of his visit to the West Indies. Further, they show that syphilis and leprosy are in no degree allied to each other. We confess that we scarcely see on what grounds Dr. Milroy has made out any close analogy between leprosy and scrofula.

Yaws, a form of disease [which has always struck us as having a resemblance to a form of ulceration in the horse common in India, and going by the name of the *bursat*, as it occurs in the rainy weather], is distinctly contagious, where there is close contact; it is separate from syphilis, but often associated with it. The Sibbens, of Scotland, seems to have been more closely connected with syphilis. Dr. Carter's visit to Norway may throw some new light on these subjects.

Notwithstanding what has been said above, regarding Armand's views of the non-connexion of leprosy and syphilis, there are indications of his having adopted the view, that syphilis has developed itself out of leprosy. He goes into some length as to the antiquity of syphilis, and scouts the idea of that disease having been introduced from America. Notwithstanding all that has been written on the subject, the history of syphilis may yet be made clearer by a dispassionate investigation, or a reconsideration of facts.[1]

[1] As an instance of early confusion of dates, we may mention that Montanus in the first half of the sixteenth century, says, that Spaniards from Calicut brought syphilis to Europe. The Portuguese did not reach India till 1498, and the siege of Naples was in 1494.

In bidding good bye to these medical reports, we must not forget that, though their form, and their substance too, are often somewhat dry, still, independently of their importance for testing the condition of our soldiers and the best means of preserving their health, they are the main sources from which we can gather any new information respecting the geography of disease, a subject illustrated by Boudin, Hirsch, and other foreign writers, but, we believe, never yet treated of at large by any English one.

VII.—Lane on Old Medicine and New.[1]

IN this essay the author professes to examine "the present state of the medical art in Great Britain, with a forecast as to its future." Unfortunately, the stand-point he has chosen turns out to be within the area of a special theory, and, consequently, his review of what is passing beyond it is coloured and confused. In short, Dr. Lane is an ardent disciple of hydropathy, so called, and zeal for the cause of his own system of treatment has dictated the matter and manner of this brochure. His plan is sufficiently simple and much to the point aimed at. It consists in depreciating what he is pleased to call "old medicine," and in extolling "new" physic, *i. e.* hydropathy. To effect the former object, and to lend weight to his argument beyond what it would otherwise possess, he quotes approvingly from an article entitled "Aims of Modern Medicine"—that appeared in the 'Quarterly Review' upwards of three years ago—numerous paragraphs exhibiting the great changes that have of late years occurred in the estimate of the nature and in the treatment of disease. To these he adjoins a quotation from an article published in the 'Edinburgh Review' last October to the same effect, and further alludes to Sir John Forbes's opinions in his work on 'Nature and Art in the Cure of Disease,' and to views at various times expressed by Mr. Skey, antagonistic to former popular theories of medicine, and especially to drug-giving.

From this collection of authorities he would have us gather the inference that "old medicine" is rapidly breaking down as a system and ready to perish. But he forgets that this throwing off old doctrines, as the exuviæ of a past existence of restriction and bondage to dogmatic assertions, and the development of a "biological basis" which he himself is pleased to recognise as the correct one, is not a sign of decadence or of death, but of new and active vitality.

He catches at some modifications of views respecting certain

[1] *Old Medicine and New.* By EDWARD W. LANE, M.D. London, 1873.

remedial agents and utilises them, just as is done in popular articles on medicine, as evidence of an entire subversion of opinion relative to their use and value. Thus, for instance, he refers to bleeding and to mercury, and would lead his readers to imagine that we are now ashamed of our forefathers' opinions and practice, and discard altogether those remedial measures from use. But he would have more correctly represented modern views had he stated that venesection, instead of being resorted to on empirical and purely hypothetical grounds, is at the present day recognised as a valuable agent in dealing with some forms of disease, its use being determined by physiological and pathological considerations. So, likewise, with respect to mercury, the impression Dr. Lane would convey is erroneous; for although experiments may show that this mineral does not affect the secretion of bile, as believed of old, yet medical men still regard it as a most valuable medicine, and are able clinically, if not experimentally, to exemplify its utility in various morbid conditions.

It must be, indeed, candidly confessed that the physiological mode of action of very many drugs is yet unknown, and that with regard to some, at least, of them, this action will in all probability for ever remain undiscovered; yet, notwithstanding Dr. Lane's contempt for therapeutical experience, the profession will still be guided by it in selecting and employing such agents. Doubtless, the blunders and disappointments relative to the properties and uses of drugs are many, but this circumstance does not discredit the employment of such substances. It only teaches the necessity of more discrimination, of more patient investigation relative to their use; and amid the wreck of a host of vaunted remedies many are there that have retained their position and have their value certified both by regular and irregular practitioners. Even Dr. Lane recognises the discovery of chloroform to be a valuable one, and towards the close of his tirade against drug-giving, he is forced to admit "the great practical value of many drugs," and cites "opium, quinine, the iodide and bromide of potassium, to say nothing of castor oil," as possessing great virtues. By this admission he, indeed, practically surrenders the principle he contends for, viz. that "old medicine," considered as a drug-giving system, is founded on empiricism and erroneous hypotheses, and that new medicine or hydropathy is a perfect and complete system, based on exact physiological truths, and destined to replace the former. But if the drugs enumerated and the "many others" so liberally thrown in as make-weights, possess the great virtues allowed by the author himself, why may not still "many others" have like virtues, even if not so considerable;

and, consequently, why may not the practitioners of "old medicine" go on in peace prescribing those virtue-laden drugs without bringing down upon themselves the scorn of hydropathists, who are fain to call to their aid the selfsame agents in the selfsame doses? or would Dr. Lane have those practitioners appeal to him to draw the line between drugs that may and that may not be used without offence to the physiological sensitiveness of hydropathic medicine? No. But assuredly with more loyalty will the practitioners in question observe the admirable maxim he quotes from the great Latin poet, "Nullius addictus jurare in verba magistri," than those who follow the principles and teachings of the author of this essay, which are the fruit of dogmatism, and of a narrow survey of physiological and of pathological facts.

Further, Dr. Lane must know that, in the science of medicine as understood by the great body of practitioners in this country, and, we may add, in the world at large, the motto "contraria contrariis" has no place; and it is unbecoming a physician so sensitive as he is to having the name hydropathist applied to himself in a wrong sense, to represent that motto as applicable to present medical theory, and to attach the name of allopathists to those who embrace it. Moreover, he is unfair to the mass of intelligent medical men of this country in representing them as blind routinists, cognisant only of an empirical drug medication, attributing the cure of disease to "some special virtues residing in a drug," and ignoring practically the salutary efforts of nature and the value of the new "hygienic medicine." The adherence of medical men "to prescriptive tradition and antiquated routine" is an assertion contradicted by the author's own approved quotations relative to the great changes that have taken place in the profession in the treatment of disease. And as to their ignoring the "vis medicatrix naturæ," this charge, again, is contradicted by himself where, in an after page, he bewails the extension of the doctrine of expectant medicine amongst physicians, and the evils that may follow a too implicit trust in the good offices of nature. Lastly, their supposed oversight of "hygienic medicine" Dr. Lane has equally little ground to animadvert upon. It is simply not true. Every-day observation contradicts it. The case is rare, indeed, where the medical practitioner does not prescribe the use of water therapeutically as well as dietetically, together with exercise and appropriate food, and, where necessary, change of air and scene. In fact, the non-professional public are disposed to satirise their physicians for the freedom with which they order change of air and a resort to baths and wells. And with regard to exercise, we are bold enough to assert that

the "hydropathists" do not more highly prize it as a means of retaining and of regaining health than do ordinary practitioners. Respecting all the charges Dr. Lane has brought against "old medicine," we feel that we have so sufficient a reply that we can afford to suggest to him that he has too slightly touched upon one, viz. inattention to the diet of patients. Compared with the attention bestowed upon this matter, that accorded to the swallowing of drugs and to the traditions of their special virtues has been out of proportion. However, we can assure him that even this regrettable negligence in prescribing suitable food is on the wane, and that the many excellent published teachings concerning diet are bearing fruit, so much so, indeed, that "old medicine" may ere long attain results from regimen equal to those boasted by "hygienic medicine."

We have taken some pains to show that "old medicine," however decrepid it may be in Dr. Lane's estimation, has some vitality left, exhibited by an active process of rejuvenescence, whereby it throws aside that which no longer serves its purpose but impedes its growth, and in so doing makes use of physiological principles which its detractor arrogates as the peculiar property of his own system of hydropathy. What, then, it may be asked, does Dr. Lane propose to put in its place? What is there special in the principles and practice of the "perfectly physiological system of medical treatment" so obtrusively and perseveringly thrust upon our notice?

We feel justified in remarking of the much lauded system that "what in it is new is not true, and what is true in it is not new."

But first let Dr. Lane himself explain what he means by "hydropathy," for he complains, and not without reason, that the word does lead to erroneous conceptions respecting the treatment the present race of hydropathists generally pursue, and the theoretical opinions they hold. The faulty apprehension of the system he attributes to the name imposed upon it at first by its inventor, Priessnitz, who, in his simplicity and enthusiasm, as a non-medical man, mistook a part for the whole, and originated the term "hydropathy," being "dazzled no doubt by the extraordinarily curative virtues of water, which he was daily practically discovering."

Dr. Lane comes to the rescue of the Apostle of Hydropathy, preaches a more comprehensive doctrine, and conjoins with the water-cure, as necessary parts, "pure air, exercise, simple diet, and nervous repose," so constructing to his own satisfaction a truly physiological system of medicine. Further, lest his readers should object that hydropathy, so understood, is tantamount to hygiene, he defines the difference obtaining between the two,

teaching that "in the latter the natural agents referred to are used to preserve health; in the former are employed, much more systematically, to cure disease." There is a refinement in this distinction not very cognisable to less astute individuals, who may contend that hygienic agents are simply hygienic agents, whether acting in the preservation of health or in the relief of disease. Common-place doctors send patients to the seaside for better air and for exercise, enjoin careful regimen, and prescribe baths, but would be surprised to hear that in ordering such "more systematic" or efficient hygienic measures than they could prescribe for their patients at home, they were practising "hydropathy." They will, moreover, be further astonished to learn that in resorting to such measures to cure their patients, instead of, as they suppose, merely following the precepts of the Father of medicine, they have actually adopted "an organized attempt, ... *the very first in the long history of medicine*, to cure diseases after a purely physiological fashion," and have thereby unwittingly embraced a grand moral principle—the outcome of the nineteenth century, proclaimed to the world as the system of hydropathy.

Dr. Lane further assigns much importance to the right conception of the fact that hydropathy implies hygienic agents *intensified* with the object of curing disease, and that this intensifying of the action of the natural agents of health "explains in a general way the *rationale* of the hydropathic theory and practice in the treatment of chronic diseases."

We trust we have made sufficiently plain Dr. Lane's notion of what hydropathy really is. There rightly remain for examination the doctrines whereon the system is based and on the strength of which it is recommended to adoption as the "new medicine" of the day. But to fulfil this object would carry us beyond the limits we can appropriate to this article. It will be enough, therefore, to briefly signify the nature of the grounds whereon this supposed grand superstructure is reared. First of all, we meet with a theory of disease, admirable by its simplicity, but which our readers generally will recognise as an old acquaintance. Disease, says the author, is the result of the transgression of the primary laws of health—"the necessary conditions of healthy existence imposed on the human being constituting in their totality what is known as hygiene." The terms of this proposition are sufficiently elastic to allow of considerable debate respecting their limitation or precise definition. For instance, there is a very wide range of conditions compatible with healthy existence.

But, admitting the general truth of the proposition, it is not nearly wide enough to include many of the ills the lot of mortals.

Dr. Lane could not fail to see this, and so adds a paragraph of exceptional diseases, as to which erring man is not so blameable for their occurrence. In fact, he could not fail to see that a very large proportion of diseases, comprising contagious maladies, acute disorders consequent on vicissitudes of heat and cold, and not a few others, *e.g.*, malignant and other tumours, degenerations of tissue, &c., elude classification under the head of transgressions against the primary laws of health. Consequently, "the central dogma" of hydropathy, "the only rational plan of treatment," an unconditional return to the laws of health, which have been violated, as the road to recovery, sinks in significance from a general to a particular mode of treatment.

Another simple division of diseases, claiming a pathological basis, is advanced by Dr. Lane, but whether he would have us view it as a novelty it does not appear. Indeed, as to this point, we seem to have read of some ancient divisions of diseases, or diseased processes, akin to it.

However, let the author speak for himself:—

"As a general rule (he writes) which knows but few exceptions, diseases are characterised by one or other of two great distinguishing features—either they are of the febrile, inflammatory, plethoric type, with an (apparent) or temporary superabundance of strength; or they belong to the opposite class, and are marked by debility."

And, he proceeds—

"The physician's first and main duty should be to aim, in both cases, at restoring this lost balance, and to endeavour, as to the former, to *reduce the standard of health;* in the latter, to assist in elevating to it."

The means to this end is hydropathy.

This pathology and the principles of practice possess the charm of simplicity, but in this quality they are rivalled by the "system" of the "Hygeists," and of many other drug-compounders, who proclaim all disease to be in the blood, and to be eradicable by the simple art of swallowing so much patent medicine. In short, neither the pathology nor the principles of treatment propounded will bear criticism, and we regret to see an educated physician exhibit contentment with the crude hypotheses they rest upon. Whatever might be the verdict of the "committee of the most enlightened physiologists," "a college of unprejudiced men," respecting the practical value of the hygienic measures Dr. Lane so vehemently advocates, we are confident they would reject as worthless the fabric of loose statements and hypotheses which he presents as the *rationale* of his system of treatment.

The taunts which the author so freely indulges in towards the medical profession respecting its blind adhesion to the principles

of medicine sanctioned by a hoar antiquity, its rejection of physiological teaching, and its blindness to the true light of hydropathy, would have fallen with some force had he been able to exhibit his so-called system to be based on a sound physiology and pathology, and to demonstrate that medical men were really unconscious of the therapeutical value of water and of hygienic agents and opposed to their employment. But in neither of these two courses has he succeeded. His attempts to theorise are miserable failures, and his charges against the profession are unjust.

To arrogate to the water-cure practitioners attention to hygienic measures as a distinctive feature would be simply amusing were it not impertinent and untrue. And yet this notion is diligently impressed upon the readers of the essay, who are farther warned against medical men as merely drug-administrators, with no better guides for prescribing than empirical precedent, and the exploded hypotheses of past ages.

However, the whole history of the profession at the present day in its relation to sanitary work and to hygiene is enough to contradict these misrepresentations. In fact, the rebellion of medical men (itself made so much of by the author when desirous to prove the decadence of the profession) against indiscriminate medication and over-dosing, is a sufficient reply to the representation elsewhere made of their following in the groove of an old, unscientific medicine.

Dr. Lane must be very ill-informed concerning the practice pursued by medical men in the present day, for otherwise he could not represent them as he does, as ignorant of and opposed to the therapeutical uses of water, whether this element be crudely regarded, as is usually done when the "water-cure" is spoken of as the sole factor in operation, or be looked upon from a medical point of view, as Dr. Lane would have us do, in connexion with those useful qualities which render it the best vehicle for applying heat and cold as curative agents. The facts are quite otherwise, and the physician in question ought rather to have congratulated himself and his readers that there is an increasing recognition of the virtues of baths, of wet sheets, of packing and other modes of using water, which have formed the stock-in-trade of hydropathy in its general acceptation. Nay, more, some eminent members of the profession have stolen a march upon hydropathists, and have startled their brethren by plunging patients into ice-baths.

We have the conviction also that more distinct homage would have been done to the uses and services of hydropathy had its practitioners always comported themselves towards the medical profession at large in a strictly honorable and professional

manner, and had they taken as great pains to arrive at and illustrate the value of their various methods of employing water curatively, as they have done to erect themselves into a special craft, distinguishable by a peculiar practice and the sole knowledge of particular processes of cure, and otherwise marked off as antagonistic to the great body of medical practitioners.

What is wanting is a genuine, trustworthy investigation of the results of the treatment of disease by hydropathy, both in its common acceptation as the "water-cure," and in the wider meaning Dr. Lane assigns it as the entirety of hygienic medicine. What has been written in this country by hydropathists on the uses of water therapeutically, has been tinged so much by matter addressed rather to the public than to the profession, as to create suspicion of its genuineness and value scientifically. Hydropathists are cursed with "establishments," which it is their bounden duty to have filled with patients, and which tend to overwhelm, even, it may be, against their will, their status as medical and scientific men, and to convert them into boarding-house keepers, with whom profit is preferable to physic.

The *rapprochement*, therefore, that Dr. Lane professes to be anxious to see, between hydropathists and the followers of "old medicine," must begin in the ranks of the former.

The profession is not in a state of dissolution, without principles to hold it together, as that gentleman would have the world infer, although, on the other hand, it is held in bondage by no dogmas, and is prepared to receive from any sources what can be scientifically established and be shown to contribute towards its advancement.

In conclusion, we cannot congratulate Dr. Lane on having helped forward the *rapprochement* he desires, or on having written anything to commend hydropathy to the better consideration of medical men. He has written for the public; his style is that of a special pleader; his matter will not bear critical examination; his sole aim is to extol the practice of hydropathy. He introduces himself as a calm, dispassionate examiner of "the present state of the medical art," but soon assumes the character of an advocate of a special mode of treatment, blind to adverse opinion, overbearing in the enunciation of his own.

But enough, and probably more than enough, has been said respecting this essay. That it has secured so much consideration is attributable, not to its excellence, but to the occasion it offered to bring forward the subject of hydropathy, and to show its aspirations and pretensions. As a specious plea for a falsely called "system" of medicine, attractive by its title, to be met with on every railway book-stall, it also called for criticism.

VIII.—A System of Midwifery.

THERE has been no greater desideratum for many years in medical literature than a systematic work on midwifery, such as might safely be relied on as a guide in the emergencies of practice.

All our older authors were much behind the present state of obstetric science; the excellent work of Tyler Smith, which was the only satisfactory representative of British practice, was out of print, and its author's numerous avocations had prevented his bringing out the new edition which was always hoped for, and which his untimely and lamented death has rendered an impossibility. Obstetrics have, year by year, been making such rapid strides in advance that it is hardly a matter of astonishment that our leading accoucheurs shrank from the labour of condensing the mass of new material into the compass of a volume. Those who had to teach the subject must have seriously felt the want of a thoroughly good text-book, and many, we believe, have been compelled to recommend to their students the American translation of Cazeaux's elaborate treatise, or some of the numerous American works on midwifery, simply because there was no English book which would satisfactorily supply their place. It was, therefore, with great pleasure that we saw the announcement of Dr. Leishman's 'System of Midwifery.' That gentleman was already favorably known to the profession by his very able essay on the mechanism of parturition, and his experience as a teacher gave good ground for supposing that the work had been undertaken by one thoroughly well suited for its performance. It is satisfactory to be able to record our conviction that this hope has been well founded, and that the stigma of not having a really good and scientific work on so important a subject need no longer rest on us. The large and handsome volume before us is one which reflects much credit on its author's knowledge and literary skill. It is terse and clear in its diction, remarkable for its condensation, easy to read and understand, and, on the whole, well up to the most modern views. If, in running over its contents, which space will only admit of our doing in the most cursory way, we venture, here and there, to differ from the author, and to point out what seem to us faults and omissions, we beg him to believe that it is done in the most kindly spirit, and in the hope of enabling him still farther to improve his "magnum opus" in the future editions, which, we do not doubt, will ere long be called for.

[1] *A System of Midwifery; including the Diseases of Pregnancy and the Puerperal State.* By WILLIAM LEISHMAN, M.D., &c. Glasgow, 1873.

The first chapter commences with an interesting *résumé* of the history of midwifery, which does not profess to give a complete account of the subject, but which points out certain epochs of belief and practice in obstetrics which have had an important influence in their progress. Thus we have, first, the Hippocratic doctrine that the head was the only natural presenting part, and its corollary,—that when other parts presented an attempt should be made to replace them by the head. This singular, and, one would think, almost impossible error, strangely enough, seems to have held its ground for a period little short of two thousand years, and must have very materially retarded the progress of midwifery. The knowledge of the ancients of various obstetric instruments, one of which seems to have been very closely allied to the forceps, is also well described. Then we have the labours of the Parisian school, under Paré and Guillemeau, when the operation of turning acquired its greatest development, and from which the commencement of modern practice may be dated. Next comes the important change in obstetrics arising from the discovery of the forceps, and finally, the study of the proper mechanism of delivery, originating with Sir Fielding Oulde, and so ably carried on by Naegelé, brings us down to our own day, and to the many important improvements we can boast of, among which turning, and the increased resort to instrumental interference, may perhaps fairly claim the first rank. We are rather pleased to see that Dr. Leishman tackles the question of the practice of midwifery by women in what we have always considered to be the proper way, namely, that the thing itself is right and proper, provided the women so claiming to practice have had the same complete training and extensive knowledge of the profession which alone qualifies men to fill the functions of an accoucheur, but that anything short of this, any tampering with the responsibilities of midwifery by permitting it to be pursued by those who have not gone through a complete training, except in the subordinate capacity of an ordinary midwife, is strongly to be reprobated. Nothing is more obvious than that those who do not know what dangers and abnormalities are cannot be trusted to recognise them, and are, therefore, unfit to attend even natural cases, since no one can tell that even the most natural case may not pass into one that is most difficult and hazardous.

The remainder of the chapter is occupied by a very interesting discussion of the comparative anatomy of the pelves of various classes of mammalia, which is a novelty in a work on obstetrics, but which contains much matter of importance, and tends to throw considerable light on the process of parturition in the

human race. The author's reasons for introducing this topic will meet with the approval of all.

"We must," he says, "of course admit that parturition is a physiological function. But in the discharge of this function there exists in the human species peculiar conditions which exercise, as compared with the lower animals, a special influence on the progress and issue of labour. What these conditions are will be best understood by a reference to one or two points in comparative anatomy which reveal certain analogies, the appreciation of which will clear away many difficulties, and a knowledge of which is, in point of fact, almost essential to the student of midwifery."

The anatomy of the pelvis in the lower animals, as the author proceeds to point out, shows very clearly how that portion of the bony skeleton is formed not only to suit the requirements of the particular animal, especially as regards its mode of locomotion, be it running, leaping, or swimming, but also with special reference to the propagation of the species, the protection of the generative organs, and the performance of the acts of parturition. Thus, in the Chevrotains, a little deer-like group of animals, the two ischia are united to the elongated sacrum by ossification of the sacro-ischiatic ligaments, which, in the female, retain their extensile character. So in the guinea-pig, cow, and many other animals, special arrangements of the ligamentous structures exist which greatly facilitate labour. There is, however, a marked difference between the pelvis in man and any of the lower animals, even the highest apes, connected with the arrangements for maintaining the erect posture of the human race, which render the process of childbirth more difficult and more liable to accidents than in any other species of animal.

"The function of the pelvis being in every case a complicated one, is so in the human female in an especial degree. The unyielding nature of the structure, essential to the effectual support of the trunk, and the curving of the cavity, for the reason above stated, render child-bearing in this instance exceptionally liable to danger of various kinds, and thus arises the necessity for that thorough training which can alone engender confidence and develop skill."

The whole of this portion of the book will well repay perusal, and will give information otherwise not readily accessible.

Proceeding to the vexed question of the condition of the pelvic articulations in pregnancy, the author adopts the opinion, which is now very generally admitted to be the correct one, that there is a distinct process of relaxation and softening going on in the latter months of pregnancy, which facilitates parturition by admitting a certain increase in the size of the pelvic diameters during labour. The precise mode in which this is effected is said to be very similar to what takes place in the cow, the

sacrum acting as a wedge and separating the ossa innominata, and causing the symphysis to open with a hinge-like motion; in addition to which the whole sacrum may probably be driven back to a trifling extent. Passing over the description of the bony anatomy of the pelvis, which is clearly and well given, but contains nothing requiring special comment, we come to chapters on the anatomy of the organs of generation, and the changes which are produced in them by pregnancy.

It seems to us that in his desire to be concise on these points Dr. Leishman has not entered into them with the fulness one would expect in a work of this class. We might cite several points in which his description seems faulty, but here only refer to one or two. Thus, for example, in his description of the broad ligaments, and their intervening structures, he mentions cursorily the par-ovarium or organ of Rosenmüller, but he makes no allusion to that layer of muscular tissue which has been described by Rouget and others as existing between the folds of the broad ligaments. This is said to form a continuous muscular envelope, embracing the uterus, Fallopian tubes, and ovaries, and which is believed to have an important physiological influence in bringing all these structures into harmonious action, and to explain the precision with which the fimbriated extremities of the Fallopian tubes grasp the ovaries during ovulation and the venereal orgasm. Possibly Dr. Leishman may have good grounds for doubting the existence of this structure, but the fact that it has been carefully described by anatomists of eminence should have at least ensured some mention of it. Then, again, in the description of the mucous lining of the Fallopian tubes, we find no allusion to the curious plicated arrangement of the membrane, by which it seems converted into a series of capillary tubes, which facilitate the progress of the ovum towards the uterine cavity.

Proceeding to the description of the gravid uterus we find that the anatomy of its various component parts is less fully given than it ought to be in a standard work. For example, the anatomy of the placenta is far more curt and brief than the importance of the topic demands. The ordinary or Hunterian view of the anatomy of this organ is adopted, and with this we have no fault to find, believing it to be the correct one. But surely the fact that this description of the anatomy of the placenta has been altogether denied by so eminent an authority as Braxton Hicks should have received at least a passing mention? If he devotes to the denial of the existence of the curling arteries of the uterus, and to the entrance of the maternal blood into the substance of the placenta, an elaborate paper, the fruit obviously of many years' labour—a paper which has been

deemed worthy of an equally elaborate rejoinder by the Professor of Anatomy in the University of Edinburgh—surely the subject should not be passed over in silence in a work published six months after the appearance of the paper in question?

Proceeding to the more practical part of the book, we come to the chapter on the signs and diseases of pregnancy. In describing the changes which take place in the cervix the author adopts, with little or no modification, the views of the older authors as to the shortening of the cervix as pregnancy advances. He tells us that in primiparous patients, after the twenty-fourth week, the cavity of the cervix gets gradually taken into that of the uterus, until, at the full period, none can be made out at all. In pluriparæ he admits that these changes do not take place nearly to the same extent. It will be observed, therefore, that Leishman differs from Stoltz, Cazeaux, Duncan, and the majority of recent writers, who maintain that shortening of the cervix is more apparent than real, and that it is not until quite the end of pregnancy that it really occurs. If, indeed, we are to take the diagrams by which Leishman illustrates these points, there would be no room to question them, for nothing can be more evident than the progressive shortening of the cervical canal that is there shown. But, unfortunately, they have a suspicious likeness to the drawings which used to be found in our midwifery text-books, and the accuracy of which has been so strongly impugned by those who maintain the fallacy of the general view. We should have been glad of some assurance that these diagrams do not owe much of the shortening of the cervical canal they show to the skill and imagination of the artist. For our own part we much prefer those which are to be found in Matthew Duncan's elaborate paper "On the length of the cervix uteri in advanced pregnancy," which have the advantage of being drawn from nature, and which seem to us much more reliable representatives of the real state of the parts in pregnancy. We observe also that in describing the signs of pregnancy, Dr. Leishman does not mention the regular hardening and contraction of the uterus from the earliest time at which the uterine tumour can be felt, on which Dr. Braxton Hicks has recently laid so much stress. Since reading that gentleman's paper on the subject we have paid a good deal of attention to it, and, although we are not inclined to give it the same paramount importance as an indication of pregnancy which Dr. Hicks claims for it, yet we feel sure that it is very constantly present, and that it is often of material assistance in diagnosing the tumour of pregnancy from other forms of abdominal growth, and we regret that the author has made no allusion to it. In

treating of morning sickness we notice that it is attributed, very properly as we think, to sympathetic derangement of the digestive organs; and it is, therefore, obvious that Dr. Graily Hewitt's singular view of its depending on uterine flexions does not find favour in Dr. Leishman's eye. Farther on, in treating of excessive vomiting, a judicious attitude seems to be taken with regard to the induction of abortion in extreme cases, and it is pointed out that, while the operation in some rare and exceptional cases may be required, these can occur but very seldom. It certainly would lead to endless abuse if so extreme a measure was recognised as one which was frequently called for. We should also have been glad if Dr. Leishman had warned his practitioners never to resort to it without the sanction and approval of another practitioner. One other morbid condition connected with pregnancy we allude to, because Dr. Leishman seems to take a lighter view of it than is customary. We have sometimes seen it laid down as an axiom that no woman should be allowed to go on to labour with albuminuria, but that its detection should be held to be an indication for the induction of premature delivery. This is a rule not likely to meet with general favour, but we confess to regarding albuminuria with somewhat greater anxiety than Dr. Leishman seems to find necessary.

In the description of extra-uterine fœtations we have some faults to find. The existence of true ovarian pregnancy, that is, the impregnation and development of the ovum within the Graafian follicle, is stated to be a proved fact. "Careful dissection has discovered," he says, "in the sac of these pregnancies not only the peritoneum, but beneath it, the proper tissue of the ovary, facts which it is difficult, if not impossible, to reconcile with an idea other than ovarian pregnancy." No reference is given to the authority referred to, and, therefore, we cannot say much about it, but it is certainly more easy to believe that an ovario-tubal has been mistaken for a true ovarian pregnancy, than to understand how it is possible for the spermatozoa to penetrate not only the peritoneum, but also the dense coverings of the Graafian follicle, a penetration which is essential for the occurrence of the particular variety of extra-uterine gestation referred to. In describing the symptoms, the only one which would appear to give much hope of ever recognising this obscure condition in its earlier stages, namely, the occurrence of irregular uterine hæmorrhage in conjuction with the usual signs of pregnancy, is entirely omitted. In regard to treatment, the author is of opinion that this can only be palliative, even in "an exceptional case, in which all diagnostic difficulties were removed, and the existence of an extra-uterine pregnancy

revealed to us in a manner which excluded the possibility of doubt." To so lame and impotent a conclusion we must decidedly demur. That the difficulties of diagnosis are so overwhelming as to render the case supposed one which rarely, if ever, would occur, we readily admit. But if it should happen, as Dr. Leishman supposes, that a tubal pregnancy before rupture has been recognised "in a manner which excludes the possibility of doubt," it would surely be a fatal mistake, in these days of advanced abdominal surgery, to fold our hands and wait patiently for rupture and inevitable death.

For our part, in a case of this kind, we should have no hesitation in advising the patient to submit to the risk, great as it certainly would be, of gastrotomy, with ligature and removal of the tube and its contents. In such cases before rupture there are no adhesions, and the procedure itself would be no more difficult, and, probably, not more hazardous than an ordinary ovariotomy, while the alternative is certain death. Unfortunately the thing has not yet been done, but that it will be done some day we do not doubt. Then there is the expedient, which the author mentions, of perforating the sac from the vagina, apparently being unaware that this has actually been done successfully in London within the last few years. In abdominal pregnancies the author also deprecates the resort to gastrotomy, on account of the great and special risk of "the removal of the placenta, attached, as it probably is, to an undulating and irregular surface." But years ago it was laid down as a positive rule by Ramsbotham that the placenta should never be interfered with in such cases, but left to detach itself. Numerous successful cases in which this rule has been followed — amongst others, we would refer to one recently published by Lawson Tait—prove, we think, that the operation is not so hopeless and inadmissible as the author supposes.

While considering the subject of gastrotomy, we may observe with satisfaction that Dr. Leishman approves of that procedure in preference to turning, when the child has escaped into the peritoneal cavity after rupture of the uterus. We agree with him that Trask's statistics, which make out that two thirds of the cases so operated on have recovered, must be taken "cum grano," but even allowing for this, the results are infinitely more favorable than when the child is forcibly dragged through the torn walls of the uterus. One reason for the superiority of gastrotomy is that it admits of the extravasated blood being removed from the peritoneal cavity, which the experience of ovariotomy shows to be of importance. At any rate, we are pleased to have the practice recognised as justifiable, having

long felt convinced that it offers the best hope of safety to the mother.

In the chapter on the management of natural labour we find simple and judicious directions laid down for the guidance of the practitioner. We feel bound, however, to criticise Dr. Leishman's rules for the management of the third stage of labour. It is marvellous to us how little known Credé's system of expression of the placenta is, even now, amongst us, in spite of all that has been written and said on the subject. We feel sure that if Dr. Leishman had ever accustomed himself to practise it he would have convinced himself of the almost invariable certainty with which the uterus can be made to expel the afterbirth a few minutes after the birth of the child, and would never have penned the directions he gives:—

"If we do not feel that the uterus is firmly contracted behind the symphysis, we should now attempt by friction over the fundus to excite it to contraction; if, *on the contrary, it is quite firm, the case should be left absolutely to nature. If, in the course of fifteen or twenty minutes,* no attempt at expulsion shall have occurred, we should pass the finger into the vagina."

And then follow directions for aiding the removal of the placenta by traction, or hooking it down from the vagina.

Now, the passages we have italicised conclusively show that the author either does not understand, or does not adopt, expression of the placenta. Credé's method does not contemplate irregular uterine friction, "if the uterus does not contract," but its essence is to keep the hand on the uterus in all cases after the birth of the child, and to apply very strong pressure whenever the uterus is felt to harden, so as to squeeze the secundines out of its cavity. This is the plan we have adopted for years, and we can confidently assert that ever since we have done so troubles in the third stage of labour have ceased. In nine cases out of ten the placenta will be expelled, by uterine contractions alone, within a few minutes after the birth of the child, and it is rare indeed with us ever to have even to introduce the finger into the vagina. The risk of post-partum hæmorrhage becomes reduced to a minimum, and although the pressure may at the moment be irksome to the patient, she will soon reap the advantage of it by the comparative absence of after pains, which the firm contraction, thus produced, ensures. We feel confident that no one who has once made himself acquainted with this method of managing the third stage of labour will ever follow the directions which Dr. Leishman lays down.

The mechanism of delivery being a subject which Dr. Leishman had already made his own by his former excellent work on

the subject, it is not surprising that he should devote considerable attention to it, and his chapter on this topic will well repay careful perusal. The description is carefully and clearly given, and yet with a simplicity which will greatly aid the student to master a branch of midwifery which is not so generally understood as its paramount importance merits. We cannot now enter into it, but it will suffice to point out that here, as in his former writings, Dr. Leishman strongly controverts various views generally assumed as certainly correct in our handbooks. This is especially the case with regard to the lateral obliquity of the head described by Naegele, and hence pretty generally adopted by all British authors. Dr. Leishman strenuously upholds the views of Cazeaux, Duncan, and many American authorities, that this assumed obliquity does not in reality exist, and that the head rarely enters the pelvis with the parietal bones on the same plane.

Passing on to breech presentations, we may point out that in treating of difficult cases in which the breech is impacted, and we know of few conditions which more severely tax the skill and knowledge of the accoucheur, Dr. Leishman very properly objects to the forceps and blunt hook, and advises rather a "fillet" passed over the groin, such as a soft handkerchief or yarn of cotton; no doubt this is an excellent and often a very useful contrivance. It seems curious, however, that no mention is made of the method of dealing with these cases specially advocated by Barnes, which consists in bringing down one of the feet, so as to break up, as it were, the presenting part. The operation is often unquestionably difficult, but it enables us sometimes to cope with cases not otherwise easily dealt with, and it should certainly have been discussed and described.

In modern midwifery the more frequent use of the forceps as a means of sparing the mother much useless suffering, and of greatly lessening fœtal mortality, has properly attracted so much attention that we naturally look to what is said on the subject, and we regret to be obliged to differ more from Dr. Leishman's teaching on this important point than on almost any other. In the first place we are surprised to find that the author says nothing as to a more frequent use of the instrument than has been customary, or as to the advantages to be expected from it. Indeed he seems to be still impressed with the too prevalent notion that the use of the forceps should be delayed as long as possible, and to dread a resort to the instrument as a somewhat formidable operation. Such at least is the impression that the chapter on its use has given us. Then again he expresses a preference for the straight short forceps, reserving the curved forceps for the high opera-

tion, in which he thinks it essential. We can understand the teaching of the Dublin school, that the pelvic curve is altogether unnecessary, and that even at the brim long straight forceps are preferable, although we believe it to be erroneous; but why the student should be taught to deem it necessary to have two instruments, a straight one for the low operation and a curved one for the high, passes our comprehension. We firmly believe that no one who has ever accustomed himself to the use of a long double-curved instrument in all positions of the head, will ever again use the short straight forceps. Nothing is so important as a thorough familiarity with the instrument to be used, nor is it advisable that the practitioner should encumber himself with more than are necessary; and it is surely preferable to habituate ourselves to that which is applicable to all cases alike.

The only possible instance in which the straight forceps are superior is, to our mind, in occipito-posterior positions, provided we deem it necessary to attempt the rotation of the head. In such cases, Dr. Leishman says, "we should always try to effect rotation by the forceps previous to attempting direct extraction." Although this is the common teaching, we have grave doubts as to its correctness. It is one thing for nature to turn the head spontaneously, but it is quite another thing for the accoucheur to seize it in the blades of his instrument and turn it round, regardless of the accurate adaptation of the head to the pelvic cavity. In the large proportion of occipito-posterior positions in which forceps delivery is necessary, simple extraction is, we believe, greatly preferable to attempting artificial rotation, and it rarely fails. We would venture to quote the high authority of Dr. Barnes as corroborating our views on this point. "*Extraction*, then, *simply*, without troubling yourselves about rotation, is all that is necessary. If nature prefer or insist upon rotation, your business is to consent. As the head advances the occiput may come forward, and you will feel the hands of the forceps turn on their axis. But in a large proportion of cases nature will not insist on bringing the occiput forwards, and here again your part is simply that of a minister of nature. The forehead will emerge under the pubes; the cranium will press the sacrum and perinæum." These directions are surely less puzzling to the student, and less likely to tend to injure the mother and child, than Dr. Leishman's. So again with regard to the position of the forceps, Dr. Leishman unhesitatingly lays it down as essential that they should be applied in relation to the child's head. We cordially agree with Dr. Barnes that nature will guide the blades to the sides of the pelvis, even when the practitioner is under

the impression that he has applied them in relation to the pelvis, however low the head may be. This is the doctrine taught in many foreign schools, and it seems to us far simpler and better than the one which Dr. Leishman adopts. It is certain that much of the dread of forceps operations, which is so prevalent, can be traced to the complex and confusing directions for their application, which are to be found in most British works on the subject. While on the subject, we may refer to figs. 148 and 152, where the operator is represented as holding the blade in the hollow between the thumb and forefinger, much in the way a javelin would be held for throwing. The correct way of holding it is, to our mind, between the tips of the thumb and two first fingers, with the end of the handle looking to the palm of the hand. In this way we have a far more delicate perception of the exact position of the tip of the blade than if it is grasped in the somewhat clumsy way figured in the drawing.

In treating of labour delayed by rigidity of the cervix, we are advised to try the old-fashioned remedies of bloodletting, tartar emetic, and the warm bath, to which are added the much superior one, administration of chloroform. No mention, however, is made of the use of chloral, of which no long experience is required to show us its great superiority over all other agents at our command. We have often been surprised at its really remarkable effect in tedious cases of this kind, and it is seldom indeed that even an obstinately rigid os will not yield after one or two fifteen-grain doses of chloral given at intervals of twenty minutes. As an anæsthetic, too, this dose is of great value in the first stage of labour, and is much less likely to interfere with the proper course of the labour than chloroform. Its use is, indeed, casually mentioned under the head of anæsthesia, but Dr. Leishman does not seem to have convinced himself practically of its occasional great value.

Proceeding to the subject of tedious labours and oxytoxic remedies, Dr. Leishman seems to place greater faith in ergot than we should be inclined to do. The disadvantages of the continuous uterine contraction it is apt to produce are mentioned, but the evil effects resulting from it are held to be over-stated. In this we can hardly agree with the author. Ergot, indeed, is a most valuable drug in midwifery when we want to produce persistent uterine contraction, and a full dose of it may, we think, be given with great advantage in all cases after the expulsion of the placenta; but before the birth of the child, we think, it would be better to use it only very occasionally and with great caution. No allusion is made to a far more effective and more easily manageable oxytoxic, to which attention has been of late years directed, both

in this country and abroad. We allude to pressure on the uterus with both hands along with the pains. This is an agent so easily applied, so completely under the control of the practitioner, and of such great value in increasing the force and intensity of the pains, that it seems a pity that no mention should be made of it.

The extent of the observations we have already made prevents our saying more than a few words on the important topic of the diseases of the puerperal state, but we would particularly recommend a careful perusal of the chapter on puerperal eclampsia, in which the subject is most judiciously handled, and excellent rules for treatment laid down. We are surprised to find that no allusion whatever is made to thrombosis or embolism, or any of the conditions which lead to those appallingly sudden deaths after delivery, which all practitioners of experience have met with. The subject of puerperal fever is so confusing and difficult that we are not astonished to observe that Dr. Leishman confesses the difficulty of treating it in a satisfactory manner. He acknowledges the fact of its general dependence on a puerperal poison, and that the numerous varieties which have been described, and which prove so perplexing to the student, all have a common relation to a septic source. This part of the subject is unquestionably well handled, but we can hardly say as much for the directions as regards treatment. We more than question whether the recommendation of occasional bloodletting is judicious; we observe no mention of turpentine, so strongly recommended by some authors, and the value of which we have ourselves often convinced ourselves of; and, above all, we think that sufficient stress is not laid on the necessity, so general in these cases, of a free administration of stimulants.

We have ventured freely to point out what seem to us the faults and omissions in Dr. Leishman's work. That these exist is only what is to be expected, and in spite of them we can strongly recommend the work as unquestionably the best modern book on midwifery in our language. It will largely add to the author's already high reputation, and will form an excellent manual for all who devote themselves to the practice of this important branch of medicine.

IX.—Sheppard's Lectures on Madness.

THE movement made within the last ten or twelve years in our medical schools to impart some knowledge of mental disorders and their treatment must be held in all honour. It was indeed a marvellous circumstance, explicable upon no discoverable grounds, why bodily maladies of every sort should be lectured upon and illustrated, whilst disorders of the mental functions were altogether ignored, as though they had no existence, or were incorporeal, and medical men were not called upon to treat them.

In our principal metropolitan medical schools this extraordinary hiatus in medical teaching has been filled up by the appointment of professors or lecturers on "psychological medicine." King's College has followed at some distance in time the example set elsewhere, and Dr. Sheppard, a former student of the college, and for many years one of the superintendents of the enormous asylum at Colney Hatch, has been appointed lecturer. In the book before us he records the instruction conveyed to his class by a course of seven lectures.

In his opening sentence he presents himself, as it were, with a jump, and reminds us of the customary prelude of the fun of a pantomime—"Here we are again!" For, surely, there is the same life, motion, and frolic in "Gentlemen, this is a new chair, and I am a new professor." As in the well-worn witticism alluded to, it is suggestive of some playful approach, on the part of the professor, to the chair or performance thereon. We will, however, take it for granted that the sober seriousness pervading the lecture-halls of the college forbad more than the suggestion. Nevertheless, this opening paragraph has its character repeated at frequent intervals throughout the book. Dr. Sheppard has sought to be facetious, and too often after a fashion to be deplored and reprehended. He has not always distinguished between wit and slang, and has indulged in metaphorical facetiæ, blemished by vulgarity. In our estimate it is mere slang, when speaking of the necessity of having the brain in perfect order to fulfil its high functions, to say to his audience, "You see, then, how important it is that you should be 'well-thatched,' and have your 'intellectual tenement' in good repair." It is to go much out of his way to describe the degradation of the insane intellect, in the loss of the sense of decency, by a quotation admittedly from "Scripture," particu-

[1] *Lectures on Madness in its Medical, Legal, and Social Aspects.* By EDGAR SHEPPARD, M.D. London, 1873, pp. 186.

larly as it is not apposite to his subject. Speaking of the frequent disappointing relapses in maniacal and other cases, he, among other things, remarks, "He (the patient) may have destroyed his bedding, daubed himself with his own fæces, or (in the language of Scripture) 'pissed against the wall.'" This, as just said, is a gratuitous outburst of bad taste, and is more offensive to the reader than even the vulgarism of such phrases as being "stunned by the noise and bustle and everlasting spin of this six-mile cannon age;" keeping a family "in constant hot water, as well as in bread and meat;" or of the picture he gives of the rectum as "the least interesting of our visceral territories, (which) does contain vaster mines of that which may fertilise the soil when bounded anteriorly by a uterus than when fronted with spermatic cords."

An amount of pedantry also pervades the author's style, exhibited in the selection of magniloquent words and phrases, which rather tend to darken instruction than to facilitate it. Thus we have sudorification for sweating, conditioned for caused or produced, and such sentences as—"It is pleasant to hail the dawn of a long-occulted reason, to welcome back the first silver streaks of returning day," the meaning whereof remains open to analysis. It indeed makes matters worse to be confused by Dr. Sheppard's rhetoric and grammatical construction respecting his meaning. The following ambiguous paragraph may be further quoted as needing an interpreter:

"In some (patients) there is a quiet and orderly passivity, which, though associated with delusions, finds no outward expression, as under those acute ideational disturbances which once rioted there, and have now made shipwreck of the supreme centre of intelligence. These subjects will state their delusions when under pressure, and affirm the reality of their convictions with a temporary and emotional excitement; but you perceive they lack the intensity which they once possessed, and are babbled out with a childish reiteration contrasting strangely with the fervour and sparkle of earlier days."

Enough has been said of the author's style; something must be added concerning his matter. On this again we regret to be unable to compliment Dr. Sheppard. It is superficial, has no claim to originality, and only adds another book unnecessarily to our store of publications, neither contributing to our previous knowledge, nor conveying recognised facts in a better shape. Considering the vast field of experience afforded to the author, the marvel is that the information gained (judging from the contents of this treatise) has been so small. Here and there we meet with a practical conclusion arrived at, and in the recounted histories of the few cases quoted in illustration of the features and treatment of some forms of insanity, we gladly

recognise the hand of a well-experienced practitioner ready in resources and prudent in management. The descriptions, moreover, of most of the well-recognised varieties of mental disease have the merit of perspicuity.

As examples of distinct practical opinion, we may with satisfaction refer to the remarks on the value of the wet sheet and packing, of the Turkish bath, particularly in melancholia, of digitalis in the active delirium of mania. The last-named drug he gives in large doses to produce quiet and sleep, recommending two or three drachms of tincture of digitalis for a dose when the pulse is quick and bounding. He prefers this medicine in such cases to chloral, which he regards as less certain in its action, as requiring repetition, and as attended with danger to life when given in large doses (from forty-five to sixty grains). The bromide of potassium, in single doses, he does not reckon a hypnotic, although in the excitement of chronic and epileptic mania he considers it suited to allay excitement if persevered with three times a day. With respect to opium, his experience induces him to pronounce against its value generally in the treatment of the insane. Even in the treatment of melancholia, in which its reputation stands high with many practitioners, opium meets with no favour from Dr. Sheppard. We would commend this drug to a fuller trial at his hands, especially in recent cases of melancholia; for experience would, we believe, disabuse his mind relative to the constant sequence of the unwelcome symptoms, dry and parched tongue, constipation, nausea, vitiated secretions, &c., which he represents as invariable effects.

The contents of the first lecture consist of superficial observations on the causes of insanity, the prevalence in the population of mental disorders, and the relative proportion of the two sexes affected by them. In what he says concerning asylum statistics he will have the concurrence of most people who have paid attention to them. He pronounces them formulated falsehoods, and utterly unreliable, especially in reference to the etiology of insanity and its assigned duration at the time of admission into asylums.

On the much debated question of the increase of insanity in the population, he is on the side of those who regard it as real. On another disputed point it is not clear what his opinion really is. Alluding to physical causes of insanity, he states, "without doubt the most frequent of these is intemperance;" but, on the next page, he quotes approvingly the views of Moreau and others, which are adverse to this statement, and he refers to intemperance not as a cause, but as a symptom, and usually one of inherited insanity; and, finally, on winding up

his remarks, he tells his pupils that the "etiological bearing" of intemperance "is invested with very much uncertainty."

In dealing with the varieties of mental disorder, Dr. Sheppard has adhered generally to the classification of Pinel and Esquirol, and manifests much contempt for the hair-splitting divisions and sub-divisions some writers would inflict upon us. In regard to this matter, the author presents some good arguments to support his views, and might, indeed, have advanced more. Our knowledge of brain function, of the causes affecting its exercise, and of the material changes that take place in the brain diseased, is far too limited and insecure to build a rational or scientific classification upon; and as matters stand, we must be content with a symptomatic grouping based upon clinical observation.

Here we must leave Dr. Sheppard's volume, regretting its publication.

X.—Caseous Pneumonia and Tubercle.[1]

THE recent discussions as to the nature and affinities of pulmonary consumption derive interest not only from the importance of the subject itself, but as exhibiting in a very complete manner the present position of pathology, and affording illustrations both of its weakness and of its strength. With pathology it is especially true that the path of progress is beset by obstacles that have been left by former explorers. There resides in the names and categories bequeathed to us by a former generation, a directing influence which, for a time at least, must shape unduly the course of further inquiry. Together with the old phrases the old opinions linger, or, where that is no longer possible, we seek to carry the old words along with us tacked to a new use, and are sooner reconciled to a new view than to new words for expressing it.

Thus, mere words acquire after a time a supremacy of their own beyond the bare value of the facts they were meant to symbolise. We apply ourselves laboriously to discover what is the present signification to be attached to some term devised in older times upon an imperfect or erroneous conception of the thing to be named, and do not rather surrender the word to its historical use. We have seen the process in the case of in-

---

[1] 1. *De la Pneumonie Caséeuse.* Par le Dr. R. LÉPINE. 1872.
2. *The Anatomical Relations of Tubercle; a Debate at the Pathological Society of London during the Session* 1872-3.

flammation, where by successive manipulation everything of the old meaning has been allowed to escape; we see it in the case of tubercle, where a term which served for a time to identify a certain product within the lungs was next removed from that and conferred upon something else, and is at the present moment in suspense till the exact measure of its use is determined. The expressions used of tubercle are not definitions of it, but only so many illustrations that an absolute histological definition is impossible. "Let any one get inflammation," says one, "and he is already on the high road to tubercle; let but certain causes come into play upon his inflammatory products, and he becomes at once sown with tubercle." "Caseation," says another, "the same anatomically is not the same in its effects; it is sometimes tubercular and sometimes not." Arbitrarily chosen and arbitrarily shifted from one meaning to another, the word tubercle is difficult just at present to provide for, yet its existence, notwithstanding Dr. Bastian's well-intentioned efforts, can hardly be said to be seriously threatened. It is as though some great truth lay hidden in a word, which, like the terms in theology, was for ever capable of new exposition.

There is an obvious reason, however, for this persistence in the use of names which have ceased to be apposite. The language of pathology must adapt itself as far as possible to the language of practical medicine, and this latter clings to a fixed and authorised nomenclature which can hardly be changed without the intervention of great corporate bodies. Thus, the teacher finds himself bound down to a set of names, and compelled to teach up to the terms of a nosological table. In the debate to which we propose to call attention it was urged as a distinct source of embarrassment beyond the inherent complexity of the subject itself, that when the student asked, "What is tubercle?" it was impossible to give him an answer; the name was there, but the substance had gone out of it.

To escape as far as possible from this dominion of names, let us approach the question from another point, and inquire, not what is tubercle, but what are the bonds of connection between the several processes by which the lung becomes disorganized; upon joint histological and clinical considerations, how can we best group the several forms of disease which concur in certain broad features, entitling them to the general appellation of phthisis?

It may be possible, though certainly it will not be easy, to exhibit the present state of opinion upon this subject, freed from mere nominal differences, so as to define the precise extent of what is known and assured, and the points at which, for lack of things being known, they begin to be conjectured.

Take that stage of acute pneumonia where the alveoli are packed with a variety of cell forms while the alveolar walls remain unchanged. The condition is one not peculiar to acute pneumonia, and from the bare observation of it, supposing that were possible, no certain conclusion could be arrived at as to the ultimate fate of the lung. In the acute disease we should have ample reason for believing that by a process of liquefaction and disintegration the alveolar contents would after a while disappear, and the air cells resume their functions quite uninjured. That is so uniformly the case in simple pneumonia, that but little danger to the alveoli is implied from their being thus filled. Sometimes, however, the alveolar contents are not thus disposed of; the cell forms may remain clogging up that particular portion of lung, and may undergo and induce further changes, so as to alter or destroy its minute structure. And this, which is the exception with acute pneumonia, is the rule in other cases, as, for instance, when the packing of the alveoli is the result of mitral obstruction. We have here a condition consequent upon a permanent impediment, and which is apt to abide, just as its cause, or its part cause, abides. In those spots of hepatization which occur in connection with such obstructions, there arises after a time a further change, viz. thickening of the alveolar walls. In many instances this event may be said to be the ultimate one, so far as the hepatised lung is concerned; it is the condition in which the lung is found after death, even when a length of time has elapsed from its first becoming solid. Excluding extravasation and its results (which is a thing apart), there is no breaking down of structure, no caseous softening, no sign of those further changes by disintegration, which follow elsewhere in the track of hepatization. If we choose to call the exudation and epithelial production by the name of pneumonia, we have in this case pneumonia and something more; the filling up of the alveoli has been attended by a material thickening in the cell walls, and the structural integrity of the organ is for ever lost.

We have here, then, one result which is apt to follow the prolonged occupation of the alveoli by inflammatory contents. There are others in the same direction which do not end here but go on to the complete destruction, or the complete metamorphosis, of the lung tissue. Thus the partition walls may break down, and the contents of a number of alveoli become merged into a single mass or otherwise these walls themselves, under the stress of a long abiding accumulation, may undergo changes of their own by the development within themselves of fibre tissue, or of massed nuclei, or of both together and side by side. The fate of the lung entirely depends upon which of these issues

shall befall it, whether, that is, the corpuscular change or the fibrous change shall predominate. In the one case the part becomes destroyed by caseation, in the other it indurates and shrivels, and at last, by a species of fibrous transformation, becomes obsolete.

Two things are here to be noted; first, the great variety of ways in which these accumulated products,—leucocytes, epithelial proliferation, red corpuscles,—at first originate within the alveoli, and, secondly, be their origin what it may, the great diversity in the ulterior changes which these same elements undergo and excite. It is not our intention at present to discuss the propriety of the term pneumonia in all its uses—in its application now to the alveolar contents, and now to a "growth" within the alveolar walls. We may notice, however, two of its many forms, which have a special tendency to develope ulterior changes, and so to bring them at once within the wide territory of phthisical affections. The one is the form which arises in the collapsed lobules of weakly children and leads commonly to caseation, the other is that which is induced by the irritation of stony or metallic particles, and ends commonly in fibroid transformation. Thus, while it is notorious that acute pneumonia has no tendency to remain or to leave indelible marks behind it, we have here two well-recognised modifications or modes of pneumonia, which are unquestionably associated with as many forms of phthisis.

In the possible issue, therefore, of what we are in the habit of calling a pneumonia, catarrhal or not (for that the proportion of catarrhal elements is always a guide to the result admittedly cannot be maintained), we have almost the whole range of processes by means of which the lung becomes disorganized. Various as are the results they own a common origin, and that origin is inflammation. Inflammation more or less "intense," attaching itself to this structure or to that, and advancing at its own rate, is competent to explain not only all the varieties of phthisis, but the reason why phthisis is sometimes encountered and sometimes not. Different, however, as would be the ultimate fate of any lung according to the rate and progress of the pneumonia attacking it, it is not in its histological elements that it would be different. The same elements are exposed to a variety of changes and events which are not expressed by anything to be observed in themselves. All that pathological observation can determine is the range of change which is possible to particular structures—their capacities, so to speak, of development and decay. It cannot predict from the elements the event that is about to happen, or foresee the accidents which may either arrest or hasten any particular issue.

That being so, it seems pertinent to remark that in referring all phthises to pneumonia or inflammatory action modern investigation has somewhat exaggerated the importance, or at least the sufficiency, of that observation. If from this common starting point so many issues proceed, and if the particular issue is in no way marked at the commencement by any special appearance of the alveolar contents, it seems but a small thing to be able to say all phthisis has this common origin. Granting that the exudation of leucocytes or the proliferation of so-called epithelium is always the first step, and that all subsequent changes proceed from this one, (which is granting much more than can be proved,) it is still possible to lay undue stress upon an event so common and so easily determined, and which owes all its future to the influence of special and as yet unseen agencies.

In reviewing from a purely histological point of view the endowments of the several structural elements and their capacity for undergoing proliferative or retrogressive changes, we are brought to consider all the possible changes of which the same tissues are capable under all circumstances whatever. Out of a very few histological elements we have the factors for a great variety of morbid processes which are, from this point of view, intimately connected with one another. The liquefaction of epithelium, more or less, makes the difference between absorption and caseation; the overgrowth of corpuscular elements which in one place we call tubercle may in another be so exuberant as to deserve the name of cancer; there is no hard line of demarcation whatever, the appearances blend into one another. These several results, however, which, regarded by themselves, anatomically, seem no more than successive steps in a single process, are in fact sharply dissevered by nature. They belong to different constitutional states, and are, indeed, incompatible with one another.

In this question of phthisis we have particularly to complain how that point is overlooked. While pathological events of quite different characters are violently yoked together under the hypothesis of a common origin, the wide difference in the ultimate results is sought to be explained by the use of language which is a mere evasion of the point in difficulty. It is quite true that epithelial proliferation is capable of no higher development, and that it is apt to caseate; it is equally true that under irritation the alveolar walls are apt to thicken and suffer changes in fibrillation and nucleation, and that in the varying combinations of these changes we have represented all the forms of so-called non-tubercular phthisis, but it is not true that to know the mode of origin and to trace the steps in

the process of decay or obsolescence brings us at all nearer to a knowledge of the special agency by means of which either result is produced.

We may quote, in illustration of our meaning, the words of Dr. Green, the more so inasmuch as a small treatise of his on morbid histology is deservedly coming into use as a student's text-book—

"I wish to express," he says, "that the various anatomical changes in phthisis are the result of inflammation, and that the differences in their anatomical characters and the subsequent history of the newly formed elements are mainly due to differences in the intensity and duration of the inflammatory process."

The greater the "intensity" of the inflammation, Dr. Green goes on to say, the more the proliferation of large cells, the less the intensity, "the more its influence tends to be limited to the elements in the alveolar walls and interlobular tissue." It is in the same spirit that Dr. Moxon declared at the same meeting that any one may become tuberculous if only the inflammation that attacks him behaves in a certain way.

We are all prepared to see great liberties taken with inflammation, but if inflammation does all this we must at least have some independent means of identifying the process under all its aspects. What, it may be asked, are the recognised degrees of inflammation, and upon what authority is it to be regarded as "intense" when its effects are temporary and less "intense" when they are permanent? In taking advantage of the very large capacity of inflammation to comprehend under one category two different processes the achievement is a mere verbal one; it does not in reality bring the two things nearer together or indicate their relationship. Even if it could do so, no more would be accomplished than to shift the difficulty further back. Pathological processes do not act by an instinct of their own, working the body according to their own will, the inflammation is a morbid phenomenon of a particular organism; it is not a specific entity which fastens upon it from without. If, therefore, an inflammation is "intense," whatever the word in that use may strictly signify, that is due to the circumstances of the individual. In the use of such a term we are implying a difference which has still to be explained. The error of Laennec and the error of our time would seem to arise from causes which the different circumstances of the two periods sufficiently explain. He was led to regard certain material products of disease as distinct and specfiic from observing the peculiarities of their deportment and the intimate association of certain morbid events with certain personal characteristics. Occupied with this view, the only one accessible to his day, he overlooked the capacities

of the normal histological elements for undergoing conversion into many forms of lesion whose real nature he, in consequence, mistook. Our error lies in seeking to rest everything upon this purely histological basis, ignoring the paramount weight of those specific influences which he so fully and exclusively recognised.

Wherein does the inflammation which terminates in caseation differ from ordinary inflammation? It is interesting to notice the many expedients which are resorted to in order to avoid the necessity of throwing the whole stress of the explanation upon so vague an influence as diathesis. There may be something discoverable in the physical state of the lung or the circumstances of its blood supply which shapes the particular issue, or at least explains it in some more definite way than by saying that such is the habit of certain lungs.

M. Lépine, who describes with admirable precision the gross and minute appearances of caseous pneumonia, adduces many considerations bearing upon its mode of origin. He observes particularly "that an imperfect supply of blood to the lung will promote, while a permanent condition of hyperæmia will retard or disfavour, the production of caseation." Twenty-one cases are quoted from various authors to show the frequent coincidence of congenital narrowing of the pulmonary artery and pulmonary phthisis. "Does this arise," he asks, "exclusively from a general condition of the organism resulting from disturbed circulation? That is little likely, for in mitral lesions, which also embarrass the circulation, we see nothing of the kind; indeed caseous lesions in such cases are of excessive rarity. It appears to me highly probable, then, that in cases where the tuberculous diathesis has not been positively established caseation has been favoured by an anæmic condition of the lung."

Arguing thus, M. Lépine would be disposed to say that a "local debility" causes the inflammation to end in caseation. He adds, with much frankness, "This is, I confess, no more than a pure hypothesis, but has nothing in itself improbable. At all events, it satisfies me as well as the *scrofulous vice* which is so gratuitously attributed to patients who have absolutely nothing of scrofula in their antecedents." An hypothesis truly, and, as stated, hardly to be maintained, for while the connection of phthisis with narrowed pulmonary artery is at least noteworthy, and the absence of phthisis in cases of mitral narrowing is a notorious and highly significant fact, it is obvious that under both conditions alike the function of respiration must suffer, and the term debility is as applicable to the gorged lung as to the anæmic.

"I incline to the belief," he says, further on, that "caseous inflammation is to be regarded as special from the commencement in the greater number of cases. It certainly always is so, for the typical caseous inflammations."

We need not stop to consider how far the view is cleared by adopting the phraseology of Niemeyer, of "vulnerable constitution." Cheesy pneumonia, according to him, is "quite an unnecessary creation." Every form of pneumonia may so terminate, and while none will be certain to end in that way it is the result to be expected in the case of chronic catarrhal pneumonia. What then? Is chronic catarrhal pneumonia an unnecessary creation also? or is the phrase which marks the special nature of the process only removed a step back?

It may be safely affirmed, that those changes which lead to caseation of the lung or to its induration by fibroid thickening are due to a special tendency of the system itself. Such a tendency is not the less real because, in the one case, the caseation can be traced to an earlier stage of alveolar catarrh, or the induration to the direct contact of metallic particles. The occurrence of a catarrh does no more than favour in a very small degree an inherent predisposition. The passage of such catarrh into a chronic state, with the subsequent degeneration of the alveolar contents, and destruction of alveolar walls, is not to be regarded as an untoward accident; it is a circumstance which marks the peculiarity of the individual constitution. It is the same with the induration which seems traceable to inhalation of metallic dust. Different individuals respond to that irritation in different degrees, and if the ultimate injury is more definite and frequent in this case than in the other, the reason is that the source of the mischief is more uniform and persistent.

There is a body of clinical evidence to show that external circumstances offer but one, and that the least important factor in the production of pulmonary phthisis, that there must come to meet it from within a predisposition or a vulnerability, or a diathesis, call it what you will. While the histological elements concerned in these disorganizing processes, and the part taken by each in promoting the common ruin are distinctly recognisable, the initial cause lies hidden under well-known popular expressions.

"I admit," says M. Lépine, "that caseation is not the result of a simple accident, but of a special process, that of caseous inflammation. Seeking as to what it consists in, I must admit an alteration in the process of recent inflammation, a vital modification of the vessels producing necrobiosis, perhaps by the agency of arterial thrombosis."

The conclusion is little precise, and leaves the matter pretty much where it finds it, but the language is to be commended as distinctly recognising what a certain school is engaged in concealing, viz. the paramount influence of diathesis. An author of our own,[1] who regards alveolar catarrh as the first stage of phthisis, and traces it with a precision much to be envied through its "catarrhal pneumonic, tuberculo-pneumonic, and tuberculo-fibroid" stages, declares that "if it be early recognised and rationally treated, its progress may in a large number of cases be entirely checked, and the more adverse the circumstances which have led to the development of the disease, the more hope is there on their removal of convalescence." The last part of the sentence is to us unintelligible; for the rest, it is undoubtedly true that by detecting a disease early enough any of us may enjoy the satisfaction of curing it. For all that we must be permitted to doubt that phthisis may be arrested if taken in time, or that the "rational treatment" of phthisis is capable, as yet, of definition.

We have seen that the formation of caseous spots within the lungs results from retrogressive changes in the proliferating elements of alveolar catarrh, and that the belief of Laennec, that these masses represented extraneous bodies undergoing a decay of their own, must be abandoned in the light of more minute methods of research. But the process is not confined to the catarrhal elements. It is, indeed, by the different degrees in which other changes intrude and other parts become implicated that the varieties of caseation are produced; especially does the implication of the alveolar walls determine the ultimate issue, whether in the direction of caseation or in the direction of induration.

Where caseation is only commencing, we may, in some instances at least, observe no more than the stuffing of a number of contiguous alveoli, with more or less altered catarrhal products, the containing walls are unchanged, except that in places they may be wanting altogether, broken down, as we may suppose, by the bulk of their contents. In marked caseation, however, the walls of such air-vesicles as remain (those probably around the margin of the spot) are obviously thickened, and themselves occupied by a nuclear, small-celled material, by whose presence the capillary circulation is arrested, and the ultimate destruction of the lung-tissue insured. Along with this appearance, the walls may exhibit traces of fibrillation, more or less extensive, more or less productive of obvious lines of hardness, interrupting the uniform consistency of the caseat-

[1] Dr. Powell on 'Pulmonary Consumption.'

ing mass. It is upon the extent of this hardening, as well as upon the extent to which the walls themselves by their thickening encroach upon and shut out the alveolar spaces, that it will at last depend whether we are to have a fibroid development with its attendant shrinking, and ultimately obsolescence, or an almost uninterrupted progress of softening ending in the formation of a cavity, or whether the two sets of changes are to proceed in combination.

Now, without invoking the name or the associations of tubercle, it was impossible at this point that the fact should be overlooked that this nuclear or fibro-nuclear product, occurring amongst the elements of what the new pathology sought to describe as purely inflammatory, was precisely the product known and described as constituting the grey granulation of Bayle, or that the observation once made should fail to revive the old belief that a specific new growth was really the essence of phthisis, and that the numerous varieties of that affection might still be appropriately distinguished by some name in virtue of that common feature. It is thus not a little remarkable, as Dr. Bastian has pointed out, that among "the ruins of the pathology of Laennec" has been picked up an evidence whereof Laennec himself never dreamt, which, long after the destruction of all that he meant by tubercle, tends once more to set up these caseous masses as having the nature and deserving the name that he first claimed for them.

It is to the nature of this nuclear product, and its sufficiency to blend together all the forms of phthisis, that the debate at the Pathological Society is mainly directed. We cannot allude to Dr. Wilson Fox's part in that discussion without recalling at the same time both the ability and the candour of his observations. While advocating his own views with perfect distinctness, Dr. Fox is singularly careful and conscientious in weighing the statements and opinions of others, and invariably courteous in the expression of dissent. Such is the temper which pathological questions more especially demand: that it is not the treatment they always receive, the evidence of this very Society may be brought to prove.

At the risk of doing injustice to Dr. Fox's views in the attempt to summarise them, we may state his contention to be that the appearance of this small-celled reticular growth, both in the grey granulations, in the tubercular infiltrations, and in the alveolar walls bordering upon caseous masses, is an indication of the essential unity of all the forms of phthisis. *The implication of the alveolar wall after this manner is the one constant characteristic of all such cases;* it appears in the most typical instances of tuberculosis, and is only absent where

fibroid change has taken its place, or where ulceration has succeeded to collapse or lobular pneumonia. While, further, the process by which scrofulous pneumonia is developed is identical, step by step, with the majority of the processes which occur in acute tuberculosis. It was Dr. Fox's anxious wish, he tells us, in earlier life, to distinguish a great many varieties of phthisis, but he has abandoned that hope on the ground that this constant anatomical element crops out in all of them.

Now, it is pertinent to observe that whenever, as a matter of mere convenience, we fasten upon some prominent feature of disease by whose aid it may be parcelled out into distinct groups, we must always incur the risk of dissevering each group differing from much of its own real kindred, and linking it with the kindred of others. As Dr. Fox himself says: "There is scarcely a single definition of disease that precisely excludes the phenomena of other diseases." Classification must be made, notwithstanding; and when the feature selected for the purpose is one of anatomical structure we must be assured that the particular appearance thus brought into prominence is itself distinctive. Thus it would be useless to fix upon pustulation merely, or upon extravasation merely, to furnish names; those events are common to a variety of circumstances, and nothing would be gained by such a mode of classification save confusion. In the particular case before us the same considerations would seem relevant. The associations of this small-celled growth are so various, its range of meaning is so wide, that no trustworthy classification can proceed on the sole authority of its evidence. A dense corpusculation of the lymphatic gland type is the nearly constant product of an irritative inflammatory overgrowth. It is met with in various parts of the body under many circumstances, and from almost every kind of irritation, in leucocythæmia, in the early stage of cirrhosis of the liver, from the inhalation of dust and the poison of typhus.

Such wide diffusion of a product of definite anatomical structure might favour the belief that in all the cases the same histological elements were concerned, but the great variety of ways by which the proliferation could be provoked would at least contradict the notion of a "specific" cause. Coexisting as it does with a variety of pathological changes, it will be to its accompaniments rather than to itself that we must look to interpret its meaning in any particular case; alone it would seem trivial and unimportant. Presumably, though not certainly, it signifies no more than the making evident by means

of a hyperplasia of a pre-existing tissue largely distributed over the body, and especially rich in the lungs.

But we have here to encounter another set of facts, of which the tendency undoubtedly is to establish a certain relationship between the adenoid growth and that special form of lesion to which the name of acute tuberculosis is universally applied. As Dr. Wilson Fox insists, "the grey granulation, the typical tubercle of Virchow, does not exist alone." The association of caseation, of infiltration, and of grey granulation is, indeed, so constant that the nature of the relationship needs to be accounted for.

According to the hypothesis upon which the whole pathology of the question is allowed to rest, acute tuberculosis has its material part in a proliferation of those lymphatic tufts which are believed to exist in the lungs as well as in the serous membranes of the brain and intestines. What is the origin of the granulations may be equally the origin of the spots of infiltration which so frequently attend them. It must be borne in mind, however, that for reasons which need not here be recapitulated, notably for their definite shape and mode of arrangement, there is more warrant for asserting that the granulations are the result of a hyperplasia of pre-existing lymphoid structures than for saying the same thing of the intra-alveolar wall-growths of the infiltrations. These latter may represent no more than an irritative overgrowth of connective tissue. "The two kinds of changes," as Dr. Bastian points out, "occur in intimate union, the lymphoid hyperplasia shading away insensibly into the ordinary nuclear overgrowth of the connective tissue elements."

Again, acute tuberculosis has special phenomena of its own; it is characterised by the simultaneous implication of a number of organs, while the organ which suffers most conspicuously is not the lung, but the brain. In this respect, as Dr. Bastian has shown, no less than in its sudden accession and rapid course, acute tuberculosis claims recognition as a distinct disease, to be carefully separated from the lesions which can be artificially produced in some of the lower animals.

Dr. Fox resists this conclusion, and labours to show that acute tuberculosis is, in fact, a disease capable of considerable variation. Admitting such to be the case, we are yet bound to judge of the two affections by placing side by side typical examples of each. There are no two diseases, probably, which do not comprehend some common ground. Viewed as a whole, however, the usually chronic course of the brute affection and the usually

successive invasion of the several organs would alone suffice to distinguish it from acute tuberculosis, notwithstanding that the course of the latter is sometimes exceptionally slow and the area of the tubercular deposit exceptionally limited.

Yet the importance of recent investigations upon artificially produced tubercle is not to be underrated. They establish the fact that, in certain animals under domestication, and notably in the guinea-pig, almost any kind of injury is apt to be followed sooner or later by the appearance of granulations in several organs, and that these in great probability owe their origin to an abnormal nutritive activity of the natural tissues. The observations are of value as indicating one of the avenues by which a particular set of tissues may become infected, as well as the comparative potency of a number of agents so to infect them. They exhibit (subject to the original hypothesis) the highly suggestive fact that the liability to suffer hyperplasia under a given irritation is subject to many degrees of variation throughout the animal kingdom. It may be difficult to determine how far circumstances of domestication affect the result, and it would need a greatly extended series of experiments before animals could be classified according to their liability in this respect; but we can say already that the differences are sufficiently obvious, both for different species and different individuals of the same species. From these experiments alone would it not have been suspected that in the great human family this same liability had gradations analogous to those seen in the lower animals?

The difference between the two chief combatants in this question is not a difference as to facts, hardly as to the bearing of facts. Dr. Fox admits that in phthisis differences of anatomical structure are insufficient to establish clinical varieties; that there is one common series of growths to be found in all; that it is not in themselves but in their deportment that the structural elements of phthisis are distinguishable; that, in a word, the anatomical classification fails. Dr. Bastian enforces the same truth, and especially charges pathologists to fling away the word "tubercle" which has been the source of so much embarrassment to his colleague, and to reconstruct their nomenclature upon a sound basis. Dr. Williams agrees with neither; he adopts indeed the prevailing opinions in so far as they affirm the development of miliary tubercles out of a pre-existing tissue; but for the rest he still insists upon that view of the pathology of infiltrating tubercle which he was amongst the first to expound, and which Dr. Fox not untruly describes as "for the moment abandoned." Dr. Williams's opinions upon phthisis are well known to all who take interest in the subject, and are entitled to all respect

from the eminence of their author, both as a pathologist and physician. He sees in tubercle an "exudation" of an evil type, the result of depraved nutrition, which by its presence injures the textures it ought to nourish, and by compressing the blood-vessels starves and wastes the surrounding parts. Dr. Williams regards the expression "growth" with peculiar scorn; to speak of fibroid indurations as connective tissue growths is with him to speak "high Dutch." It may be so. We must at least concede that his own nomenclature is open to no such objection. The terms Dr. Williams uses are a mere transcript of undoubted facts. He would speak of the process of healthy inflammation which results in the formation of healthy tissue as *euplasia*, while the contrary process, where the tendency of the sarcophytes is towards degeneration and decay, he would call *cacoplasia*. The terms are unobjectionable, yet the information they convey, if it were not in Greek, would seem to be superfluous.

It is time, however, that we should by way of conclusion endeavour to place before the reader that general view of the subject which best embodies established facts. The matter, as we have repeatedly said, rests partly upon hypothesis. Any arrangement of it now must be tentative and provisional. A plain statement, however, though likely to err, is better than one which takes refuge from error in mere vagueness. We shall endeavour in the space that remains to place together the practical conclusions which seem to be the most tenable.

Caseous softening may occur to any individual as a result of long-continued catarrhal proliferation whose products are not got rid of. Circumstances of the time, unfavorable hygienic surroundings, imperfect recovery from acute illness, temporary debility from whatever cause, may favour that ending to any alveolar catarrh. The event, therefore, of caseation is no proof of a special predisposition, although it makes one suspected. The abnormality of constitution which it indicates may be temporary and quite apart from any special dyscrasia.

Caseation once produced has amongst its histological elements, together with those products which are intra-alveolar and of directly inflammatory origin, a distinct product or growth within the alveolar walls of a lymphoid structure, resembling that of the grey granulation. Whether this product results from the irritation of the inflammatory contents, or they from it, may be regarded as uncertain; the connection between the two is at least a very close one. *But the presence of this adenoid material in the walls, its abundance or its scarceness, makes no difference at all in the endowments of the caseous spot.* Should the spot become a focus of contagion we have no cause to suppose that it acquires that property in virtue of this adenoid

growth. However the growth comes there is nothing specific in it, and nothing can be predicated from its presence or absence. "It is open to the gravest doubt," says Dr. Fox, "whether it is the caseous matter as such or the growth which is the source of the further infection." Others are of the same opinion.

Since, then, the individual who is the subject of these changes need have no special dyscrasia, his health may at any time mend, and the conditions of his nutrition alter; that improvement of his will probably find local expression in a fibroid metamorphosis which shall either lead to the gradual obsolescence of the affected part or else erect a tough fibroid wall around it, and so protect an already formed cavity from further spreading. Should no such change occur the caseation will not even then *necessarily* become the focus of tubercular infection. It will do so or not just in accordance with the circumstances, that is, with the diathesis of the individual. Every caseation gives ground for suspicion that the patient owns the diathesis (we call it for the moment "tubercular") in which caseation is known to be common. That suspicion is strengthened when the caseation breaks up into a cavity, and no sign is shown of any fibrous development or arrest. But it is still only a suspicion, the proof is afforded in some cases that it is an erroneous one.

A caseation arising, as it may do, in a person without the "tubercular" diathesis is a very imperfect means of infecting the adenoid tufts of the lung, is an insufficient cause, that is to say, of general tuberculisation. But a caseation arising *as it mostly does* in a person with this diathesis becomes an almost certain source of infection of these same bodies which, in his case, are waiting to respond to any such irritation. Thus there is nothing special in caseation, although particular persons are the most liable to it; nor is there anything in intimate constitution to distinguish one caseation from another. It is in the liability possessed by certain persons to suffer proliferation of their adenoid tufts under a certain irritation that the speciality consists.

The question will occur, Can a long abiding caseation at length infect so as to tuberculise an individual who, on first succumbing to his caseation, exhibited no proclivity of that sort; in other words, may the irritability, which is so markedly inherited, be occasionally acquired? The thing seems sufficiently probable, and the supposition is almost a necessary one in the light of clinical experience. At the same time unless we devise an arbitrary definition of the tubercular diathesis such cases must be always difficult to recognise.

Dr. Wilson Fox seeks to place phthisis upon some anatomical

basis. The necessity of attributing to precisely the same elements now a larger and now a smaller share in the process of infection, now a greater and now a less liability to suffer degenerative changes, would seem to render such a classification for ever impossible. Without attempting any rigid classification it seems to us that the full recognition of this inherent liability of which we have spoken, serves the best to link together in a consecutive order all the varieties of phthisis which it is necessary to recognise. Caseation will be at one end of the series, acute tuberculosis at the other; the two agencies, separable throughout, are yet seldom dissevered and each is interpreted by the other. Caseation, possible in any lungs, is for the unsusceptible class apt to take on the fibroid change and so to become obsolete; whether or no it is, for them, chiefly harmful on its own account, and hardly at all for what it produces since it exists in a soil which is uncongenial to its growth and powerless to propagate its evil effects. Acute tuberculosis, on the other hand, the inheritance of certain individuals, implies an abnormal susceptibility of portions of their lymphoid structures, rendering them ever ready to undergo hyperplasia, a susceptibility variously exhibited in different animals, different races, and perhaps at different ages.

Individuals possessing this susceptibility in an extreme degree are from their birth in circumstances of great jeopardy; mostly they are attacked young and die, the immediate or exciting causes of the fatal illness being trivial or obscure, or quite unseen. Children, therefore, exhibit the condition best, not because the lymphatic tissues are naturally irritable in early life, but because an extreme susceptibility renders lengthened existence almost an impossibility. Those who are susceptible in a less degree may survive longer or even escape altogether; their fate will depend upon the accident of incurring or escaping the requisite infection, the best recognised and the surest source of which is caseation.

The views of Dr. Bastian would seem to fall short of the truth in divorcing altogether these caseous changes from the special diathesis with which they so often concur. Caseation, as we have said, may by possibility be the product of the exceptional circumstances of the time apart from all constitutional causes. Such are the rare cases where the drain of the caseation itself kills, and the lung exhibits a softened mass with imperfect fibroid surroundings, abortive efforts towards cure, but without granulations; such again are the less rare cases where the fibroid change persists and the patient recovers. Nevertheless, in the event of caseation, and especially of caseation so advanced as to be recognisable as such at the bedside (which is the grand matter),

we cannot but see strong grounds for supposing the existence of the "tubercular" diathesis, that is, of abnormally susceptible adenoid tufts; we cannot but be sure that, with that diathesis, the caseation supplies a sufficient cause for their consecutive hyperplasia. Thus the source of danger in too many instances goes hand in hand with the liability to be overcome by it, and caseation is regarded as specific because it plays a special and uniform part in the class of subjects which it is most prone to attack.

Such appears to us to be a fair writing out of the facts in connection with this subject. It must await further elucidation and more scientific expression. There is wanting for phthisis, according to Dr. Sanderson, three things, a local irritation, a process of infection, and a constitutional predisposition. We have much to learn in reference to each of these factors, but are thoroughly persuaded of the virtual supremacy of the last.

It is to the credit of English pathology in dealing with this subject (and M. Lépine, though not without his theories, must be allowed his share in it), that hypothesis is for the most part carefully separated from demonstration. Yet assuredly conjecture has been neither idle nor timid elsewhere. Much of the pathology of lung disease has no other foundation than the assumption of what, in the nature of things, would be likely to befall an organ so constructed and so employed. The lung lends itself very readily to such treatment. With its constant liability to catarrh, what a world of morbid changes to be made out of retained secretion, unexpelled products inspissating, decaying, irritating, setting up proliferative and inflammatory changes. With its delicate and unprotected network of vessels what ample room for hæmorrhage, and the gradual caseation of a dried-up blood clot. It needs no more than familiarity with a somewhat uncouth phraseology to deduce all the varieties of phthisis out of the materials supplied by a common cold. How simple, yet how complete. The view commends itself to the common sense and common observation of every thinking man. As Dr. Powell quite naïvely says, the mystery to him is that phthisical lesions are not more intractable and destructive. It is in truth a subject for frequent remark, that the diseases the imagination can construct are strangely out of harmony with the pattern of nature. The subjects of chronic catarrh, or of yearly recurring bronchitis, those who from time to time have pulmonary hæmorrhage vicariously, or from valvular disease, all these are or should be on the high road to phthisis.

The investigation of those who have small confidence in

such a mode of argument—among whom Dr. Wilson Fox is conspicuous—has served to show that these seductive theories, which fail so signally in their clinical application, are but little supported by histological facts. Nevertheless, the hopeful doctrines of Niemeyer, which his admirable clearness of expression and obviously honest belief have contributed to render popular, are being pushed still further by his followers. Such views are not wholly harmful. In so far as they encourage a careful tending of the sick and the practice of those hygienic rules, which whether in health or sickness cannot fail to promote the well being of the individual, they would seem to be wholly good. And for the error that lurks in them we may safely trust the common observation of our race, the individual experience of too many of us to limit the signification of the words when consumption is said to be curable or preventible by medical means.

Bibliographical Record.

Guide to Trefriw and Spa (Conway).[1]—This little popular medical "guide" to a Welsh spa repeats the characters of similar productions published in the interests of other places where a mineral spring of some sort or other exists and visitors are desired to partake of its healing virtues. We are required to believe that the spa is unrivalled in more or fewer qualities, that it works marvels in making the sick sound, and that all its surroundings are romantic and beautiful. All these excellent qualities attach, we must believe, to the Trefriw spa, and after due allowance made for the *couleur de rose* influence of local association on the part of their expounder, sufficient ground will be left to warrant the invalid in making trial of the waters.

The spa belongs to the chalybeate class, containing "sulphate of iron and of alumina, with some sulphate of lime, magnesia and soda and a large quantity of silica and a little chloride of calcium." This account of their composition will serve to indicate the class of cases to be benefited by these waters, even without the knowledge of the details so precisely given by Dr. Hayward, and which intimate such recondite physiological information on his part, that they act "principally, directly, specifically on the pneumogastric and great sympathetic nerves which, when weakened and relaxed, they brace and invigorate." At all events, we, for our part, would encourage invalids wanting rest and change of air and scene to resort to the quiet charming valley of the Conway, even were it deprived of the accessory boon of its health-giving spring.

American Marine Hospital Service.[2]—This volume makes us acquainted with the praiseworthy fact that the United States Government has charged itself with the supervision and the provision in specially constructed hospitals of sick mercantile seamen employed on its coasts, rivers and lakes. Much of its cost is borne by the men themselves. An Act of Congress, in 1798,

[1] *Guide to Trefriw and the Vale of Conway Spa.* By John W. Hayward, M.D. Second Edition. London, 1872, pp. 64,

[2] *First Annual Report of the Supervising Surgeon of the Marine Hospital Service of the United States for the year* 1872. Washington, pp. 103.

levied a tax of 20 cents per month on seamen employed on American vessels engaged in the foreign and coasting trades, to be collected by the custom house officials. The fund thus raised was put under the charge of the President, who appointed directors of marine hospitals at such ports as he thought proper, charged with making suitable provision for the sick and disabled seamen there found.

The sum realised by this taxation was soon found inadequate to meet all the cases requiring assistance, and hence various restrictions, inflicting hardships upon many poor fellows, were from time to time proposed. Moreover, many places needing hospital accommodation could not get it, and in those professedly possessing it, the provision made was in many instances of the most unsatisfactory and wretched description. The representations that arose respecting this state of things induced Congress, in 1837, to appoint a Board of Army Medical Officers to select and purchase sites for hospitals, and money was set apart for their erection. Miserable blunders in selecting sites and in other particulars were at first made, and much money wasted. The particulars are stated in the book before us and each hospital briefly reported upon as to site, cost and efficiency. The summary shows that 32 in all have been established. "Two marine hospitals are unfinished; 2 have been abandoned; 1 was transferred to the War Department; 1 washed into the river, and 15 were sold." There are besides 39 hospitals, at 36 different ports, owned by private or corporate parties, and wholly or in part devoted to seamen, and at 34 smaller ports, extemporised hospitals—usually private dwellings—afford what relief is sought.

The Marine Hospital Service was reorganized by Congress in 1870, and the tax levied upon seamen advanced from 20 to 40 cents per month. A supervising surgeon of the entire service was at the same time appointed. "During the fiscal year 1872," 12,302 sick and disabled seamen received hospital relief, and 854 were treated as out-patients. Less than 4 per cent. of the number treated died.

Very brief notes of the surgical cases treated are appended; but they are too superficial to be of value. These are followed by tables classified so as to show the diseases and injuries of all those treated in the several hospitals during the year.

In conclusion, we have to thank Dr. J. M. Wordsworth, the supervising surgeon of the department, for this interesting volume kindly forwarded to us.

Thorowgood on Asthma.[1]—A notice of the first edition of this

[1] *Notes on Asthma, its Nature, Forms and Treatment.* By JOHN C. THOROWGOOD, M.D., &c. Second Edition. London, 1873, pp. 166.

small treatise on asthma appeared in this Review, in October, 1871; the one now before us is somewhat enlarged, but retains the same features as before and merits the same degree of commendation. That it has, as a brief and correct exposition of the pathology and approved modes of treatment, met the wants of practitioners, is made obvious by the demand for this new edition; and, unless Dr. Thorowgood can by patient study and extended experience add materially to the knowledge we have of asthma, we would advise him to restrain any uprising ambition he may feel to write a lengthy systematic treatise on the subject; for by keeping to the present character and dimensions he will command more success and a greater amount of usefulness by his literary labour.

Black on the Functional Diseases of the Renal, Urinary, and Reproductive Organs.[1]—We find it difficult to condense within a few paragraphs the impression left upon our minds by the perusal of Dr. Black's work; but we will introduce the author to our readers by allowing him to speak for himself. He has evidently a considerable acquaintance with medical literature, and he displays a remarkable fondness for somewhat recondite chemico-physiological speculations with reference to the phenomena of disease. He says—

"That in health the various excretions of the body are formed from the oxidation of effete tissue, and that the adequate performance of this, the histolytic, as well as the converse, the histogenetic formation of the living body, constitutes healthy existence; that health is represented by an unvarying composition of the various excreta; that certain of the secretions are formed from the nitrogenous elements of the body, viz. urea, carbonic acid, and ammonia; that these excretions are not formed directly by oxidation from the nitrogenous compounds, but by a series of chemical evolutions; that in health the intermediate products are fully oxidized in order to present to the emunctories their natural stimuli; that if the conversion is arrested, or from causes to which we have referred, rendered incomplete, intermediate compounds are the result; that these constitute the *materies morbi* of several diseases; the inferences—*that it is predicable of these diseases that their materies morbi is the result of deficient oxidation, and that their rational treatment consists in furthering the perfect metamorphosis of such compounds.*"

These views he submits fulfil the requirements of a sound theory. Then he goes on to say—

"No doubt the reduction of medical science, so-called, to general principles has its disadvantages. It tends to prevent, for instance, the unbridled evolution of the luxuriant imagination, to which, alone, so many practitioners of medicine are indebted for success.

[1] *On the Functional Diseases of the Renal, Urinary, and Reproductive Organs, with a General Review of Urinary Pathology.* By Dr. Campbell Black, M.D. Edin. London, 1872. 8vo, pp. 321.

What is medical science? A few, a very few ultimate facts; a chaos of isolated assertions and contradictions, innocent of the parentage of reason, but greedily accepted, according to the law of supply and demand, by a gullible public; ideas selected from the confusion of untutored brains (for that is of no matter, or rather it is an advantage), as fancy and the purpose to be subserved determine. . . But medicine does present a field for scientific investigation, a scope for genius. . . . Yes, there *is* such a theory as *medical science*, but there is likewise such a thing as *medical trade;* the few devotees of the former occupy the garrets of the profession; the many of the latter rollick away useless lives in affluence and luxuriance! Quackery abounds!"

As another specimen of Dr. Black's style, we take the following:

"Congestion of various organs, it may be submitted as a rule, is a condition usually seen by fools. At least, my experience has taught me to attach little importance to the discrimination of the frequent discoverer of 'congestions.' Now there can exist congestion of any organ, independent of static conditions on the one hand and inflammation on the other, I have never been able to determine; yet, like every other conceivable, and not a few inconceivable, diseases, congestion is frequently diagnosed by a process of reasoning to me equally unexplicable. A little humiliation, therefore, it may be alleged, becomes the writer whose mental grasp is of so mediocre an order."

After this onslaught on the doctrine of congestion, the author immediately proceeds to say—"But that congestion of the kidney does occur under particular circumstances, it would be folly to deny," &c. &c. So that, apparently, those who accept and those who reject the doctrine of congestion are equally liable to the imputation of folly.

Nearly one half of Dr. Black's volume is occupied with the pathology and treatment of spermatic incontinence—or, as he calls it, "spermorrhagia"—sterility in the female, and impotence in the male. We do not consider that any apology is needed for the space which he has devoted to these delicate and difficult subjects, nor do we complain of the manner in which he has dealt with them. We do not, however, find anything in his pathology or his practice requiring special notice.

Roberts on Urinary and Renal Diseases.[1]—We have pleasure in welcoming a new edition of Dr. Roberts's well-known and excellent practical treatise, which has evidently been carefully revised. Amongst the additions we find a very clear condensed account of that curious affection usually called "intermittent hæmaturia," but

[1] *A Practical Treatise on Urinary and Renal Diseases, including Urinary Deposits.* By William Roberts, M.D. Second Edition. London, 1872, pp. 621.

which Dr. Roberts designates "Paroxysmal Hæmatinuria." In discussing the etiology of Bright's disease the author believes that the abuse of spirituous liquors ranks high as a determining cause, and referring to Dr. Dickenson's elaborate attempt to prove that this etiological influence has been greatly over estimated, Dr. Roberts maintains that in a paper published in the 'British Medical Journal' for November 4th, 1871, he has shown conclusively that Dr. Dickenson's arguments are inadequate to shake the commonly received opinion that alcoholic excess is a fruitful cause of renal disease. A careful perusal of this new edition of Dr. Roberts's work enables us to express a confident opinion that while it makes no claim to be very original or profoundly pathological it will maintain its position as an excellent clinical and practical treatise on urinary and renal diseases.

Diphtheria in Italy.[1]—The Medico-Physical Society of Italy proposed in March, 1872, a series of questions for consideration by the members on the subject of diphtheria, which seems to have been lately prevailing as a severe epidemic in that country. The following were the questions thus submitted:—1. Whether diphtheria and croupous inflammation are two distinct morbid conditions, or are to be considered identical in their intrinsic nature? And even if they are distinct, may they be combined together? 2. Is there a true, primitive, laryngo-tracheal, localised croup? 3. Do the diffusion and propagation of diphtheria arise in consequence of contagious principles, fixed or volatile, or are they due principally to that combination of occult causes comprised under the name of epidemic influence? 4. Is diphtheria a disease primarily local, with a tendency to diffuse itself to the general organism, or is it *ab origine* the expression of a diathesis? 5. Are there any medicines which are prophylactic of diphtheria? 6. The curative treatment and more especially cauterisation. Should diphtheric croup be treated by tracheotomy?

These questions are all examined with very great care, not only by means of consulting the works of the most esteemed authorities, but also by collecting and comparing the results of the experience of the Italian physicians in the recent epidemic, and Dr. Faralli has combined in the present report the inquiries and the conclusions to which the questions have given rise.

In discussing the first question, namely, the identity or non-identity of diphtheria and croup, Dr. Faralli begins by giving an historical sketch of the disease which, under different names, has appeared as an epidemic in various countries and periods, and which

[1] 1. *Studi sulla Difterite.* (*Studies on Diphtheria.*) Rapporto del Dottore G. Faralli.

2. *Sul Ciclo Termico della Difterite.* (*On the Thermic Cycle of Diphtheria.*) Nota del Dott. G. Faralli.

is now generally recognised as diphtheria. Baillou in France, Heredia in Spain, Sgambati and Carnevale in Italy, Cortesius in Sicily, Home in Scotland, Bard in New York, have all described the disease, which, when breaking out as an epidemic at Tours, in France, attracted the attention of Bretonneau at the commencement of the present century, and became but too well known also in our own country in the years 1857, 1858 and 1859, and is still prevalent, though in a less degree, in all the countries of Europe and America, and, indeed, throughout the world. Dr. Faralli then carries on the history of the disease as it was rendered still more distinct by the researches and the writings of Trousseau, and he also discusses the question of its parasitic origin, as proposed and studied by Tigri, Letzerich, Oertel, Tommasi, and others. It is well known that Bretonneau and Trousseau regard diphtheria as an affection which may attack different parts of the body, almost always, however, beginning in the fauces, but assuming the name of croup when it attacks the larynx and trachea, and hence, in the opinion of those authors, tracheal diphtheria and croup with plastic exudation are identical. This is one of the questions examined by the members of the Italian Medico-Physical Society, and after regarding it in all its bearings, they came unanimously to the conclusion that "the greater part of the facts collected in the recent epidemic (in Italy) as well as those related of former epidemics, prove that the croupous and the diphtheric forms are associated together and succeed one another, so as to appear like phases and modifications as to time and situation of a single morbid process."

The next question, which is closely allied to the preceding one, is as to the existence of a primitive laryngo-tracheal croup. It is certain, says Dr. Faralli, that the greatest part of the cases of croup are not developed primarily in the larynx, but are the consequences of a pre-existing pharyngeal diphtheria, and to this category belong all the cases of epidemic croup. This view is supported by the history of the recent Italian epidemic, during which it was found that while diphtheria was prevailing the number of croup cases also increased, so that many physicians were compelled to regard them both as manifestations or different forms of the same affection. Still, it is admitted (as it was admitted by Bretonneau), that cases sometimes occur where the disease is primarily developed in the larynx independently of the pre-existence of pharyngeal diphtheria, and two cases of this kind were reported by two Italian physicians, Drs. Barbini and Bargioni. These cases, however, are very rare; nevertheless, as they have been observed, the society came to the conclusion that "there exists a true primitive laryngo-tracheal and localised croup."

The third question refers to the etiology of the disease and its mode of propagation or diffusion, and this involves the subject of its

contagious nature, which was maintained by Bretonneau and partially admitted by other authors. Facts in support of this view have been often adduced, and some more are added as the results of the recent observations made in Italy. But there are also a great number of instances where no communication by contagion can be traced, and many more where persons apparently fully exposed to the morbid influence have entirely escaped. From the whole of the recorded facts it would appear that a case of diphtheria, developed either spontaneously or by contagion, may become the centre of a small epidemic, limited to the persons immediately in contact with the patient, who are attacked in a relatively short space of time, without any further extension of the disease, unless the specific epidemic influence exists in the neighbourhood. The conclusion at which the society arrived on this question was a guarded one, to the effect "that diphtheria is a disease which is either spontaneously produced or developed in an epidemic manner, but may give rise to the generation of contagious principles."

The fourth question, relating to the pathology of diphtheria, or, in other words, to its proximate cause, is considered by the reporter, Dr. Faralli, the most difficult of all, and that which is most controverted by the medical profession, some regarding the affection as a general one, affecting the whole system, but attacking certain parts of the mucous membrane in preference to others, while another school considers it as a primarily local malady, with a tendency to generalise itself. Those who advocate the latter view point out that the local affection always constitutes the earliest symptom of the disease, and that the general phenomena are developed in the most severe cases, but may be wanting in the slight ones. Those who have thought they discovered parasitic organisms in the diphtheric membranes, adopt the view of the local origin of the malady, and they believe that these organisms penetrate into the circulation and thus poison the system by the diffusion of the morbific germs which at first are locally produced. On the other hand, those who maintain the theory of the general primitive infection attribute to the presence of parasites only a secondary degree of importance, and point to the epidemic nature of diphtheria, the violence of its general symptoms, and their concomitance or successive manifestation. Dr. Faralli, who has himself made some valuable experimental inquiries on the subject, reviews the opposing theories with great candour, but refers to his own experience as proving that the general phenomena of fever are far more marked than is generally supposed, being characterised by shivering, and accompanied by vomiting, convulsions, delirium, &c. He gives several tables, showing, from his own observations, the pulse and the temperature on different days, and different times of each day, during the progress of the disease. The supervention of paralysis and the occurrence of albuminuria

in diphtheria seem also to point to a general diathetic disturbance, but nevertheless the majority of the society declared in favour of the theory of "a primarily local affection, with a tendency to diffusion to the general system."

The fifth question, as to the prophylactic medicines to be employed in diphtheria, is not one of much practical importance as it is treated in the report, for it relates only to the use of one substance, namely, sulphur, and its employment is based upon the supposed efficacy of the drug in destroying the parasitical germs, which by some are considered to be the essence of the disease. But as there is no proof that sulphur exercises any such prophylactic power, the majority of the society arrived at a conclusion to that effect.

The curative treatment of diphtheria, as summarised by the Italian Society, will be found mentioned in our Materia Medica Report.

The *Note* by Dr. Faralli on the Thermic Cycle (*ciclo termico*) of diphtheria discusses a question on which a considerable difference of opinion has existed, namely, the value of the presence of fever in that disease. Some believe that diphtheria runs its course without any febrile symptoms, and others, on the contrary, maintain that they are never absent. The discrepancy arises from the difficulty of observing exactly the feverish process in children, and from the fact that the practitioner is seldom called in at the beginning of the illness. The observations made by Dr. Faralli himself during the recent epidemic in Italy, and founded on thermometrical observations, which he never omitted in doubtful cases, have convinced him that fever is a phenomenon which may be constantly observed in diphtheria. It is developed early, not unfrequently commencing with shivering, and is sometimes accompanied with sympathetic phenomena, such as vomiting, convulsions, &c., rising to an elevated degree in a few hours, even in slight cases, and decreasing gradually till complete apyrexia is reached about the third or fourth day. In cases of moderate severity the temperature again rises towards the fourth day, but without ever returning to the degree of temperature shown at first, and the exacerbation is owing to the appearance of fresh diphtheric patches on the part which had previously been healthy, or more frequently to the development of glandular enlargements as an effect of secondary infection. The consequences of this secondary infection are clearly observed in severe cases, which assume the form of so-called "infectious typhoid" (*infeziosa tifoidea*). Dr. Faralli gives the particulars, accurately recorded from day to day, of four cases, in which the temperature is carefully noted, together with the rate of the pulse, and the observations are made twice, and in many instances three times, in the same day. Three of the cases terminated favorably. The same series of phenomena, in connexion with fever, recorded in the four given cases, was observed

in the greater part of the numerous patients (upwards of sixty) visited by Dr. Faralli during the epidemic, and he thinks that the different course of the temperature, in different instances, probably indicates two forms of diphtheria, properly so called, namely, one slight, in which the disease is confined to the pharynx, and soon ceases; and another, in which the morbid product accumulated in the fauces is decomposed and gives rise to a secondary infection.

The Half-Yearly Medical Abstracts.[1]—These two well-approved half-yearly retrospects of the progress of medicine continue the even tenor of their way, contributing to no small extent to keep practitioners *au courant* with the researches, opinions and modes of practice as set forth in the numerous periodicals of the day, as well as in papers read before learned societies, and in books. The steadiness of demand for these abstracts is a healthy sign in the profession. Indeed, the demand has appeared, it would seem, so pronounced to writers and publishers that of late years there has been quite an outburst of periodical volumes of abstracts and reports, some of which in the presence of the old-established ones can make but indifferent apologies for their production. However, those now before us are of well-approved character and have made good their hold with the profession. That same character is fully sustained in these last-issued volumes.

University Oars.[2]—Dr. Morgan, in his Introduction, informs us that the inquiry which forms the chief subject of his book has been carried out by him during the last four years, though with more or less interruption. He himself is no mean authority on the topic of which he writes, for besides being physician to a large hospital, he has also been for many years a distinguished "oar," beginning his experience on the Severn at Shrewsbury, and maturing it at Oxford, where he was for three years captain of a boat which has often played a prominent part in the races.

The circumstance which gave occasion to the composition of the book was the correspondence published in a leading newspaper some few years since, and excited by a letter from the late Mr. Skey, who, it may be recollected, held the opinion that the university boat-races were injurious to those who took part in them, and appealed to his own experience in proof of his statement. In a leading article of the same paper it was remarked that no statistical inquiry had

[1] *The Retrospect of Medicine.* Edited by W. Braithwaite, M.D., and James Braithwaite, M.D. January to July. London, 1873.

The Half Yearly Abstract of the Medical Sciences. Edited by W. D. Stone, M.D. January to July. London, 1873.

[2] *University Oars; being a Critical Inquiry into the After Health of the Men who Rowed in the Oxford and Cambridge Boat Race from the Years* 1829 *to* 1869, *based on the Personal Experience of the Rowers themselves.* By John Ed. Morgan, M.D., M.A. Oxon., F.R.C.P. Pp. 397. London, 1873.

ever been instituted by the medical profession to determine the effect of boat-racing on the human constitution, and a suggestion was offered that some such inquiry should be made. At the period when the article was written (1867), twenty-four regular university boat-races had been rowed, and, allowing for those who may have rowed more than once, the number of persons who had taken part in the contests must have exceeded 100, besides the many hundreds of thousands who had taken part in college races. It was therefore represented as desirable to ascertain the proportion of the rowers who had died in the interval, or become disabled, and also to compare their mortality and illness with those of other men during the same period. The inquiry, thus suggested, has been taken up by Dr. Morgan, with whom it has evidently been a labour of love, as he himself states, and has been "prompted by a pure affection for rowing, and by a deep-rooted conviction that in these days of incessant mental tension and intellectual excitement of every kind we should not allow so manly and health-giving an exercise to be unjustly assailed" (Introduction, p. 15). In these feelings it is probable that all persons will sympathise, but it does not necessarily follow that those who object to the violent and paroxysmal efforts made at the annual boat-races are therefore opposed to the practice of rowing in general, which, as is well known, is followed by thousands of people, both for a livelihood and for amusement, without any ill-consequences whatever.

Dr. Morgan's self-imposed task, although a restricted one, is surrounded by great and insuperable difficulties. The elements necessary for his conclusions are, to a great extent, unattainable, and, where they are procurable, the results may often be fallacious. It is evident that the information required must in most cases be derived only from the persons concerned, and it is unnecessary to point out the unsatisfactory nature of such intelligence; and where it is obtained from others, what guarantee is there that it is either correct in fact or well founded as to opinion? Again, on the one hand, as to the rowers in the boat-races who survive, who is to determine whether they are really in such robust health as they themselves, or even their friends, may suppose? and, on the other hand, as to those who are dead, who shall determine whether the boat-races did or did not shorten their lives? It is quite probable that among the crews there may have been some who, although robust at the time of the race, were really the subjects of hereditary predisposition which would have developed itself afterwards, whether they had rowed in the race or not; but we may take it for granted that those who were actually labouring under any physical malady, or who showed a tendency towards it, would not be allowed to take part in the contests. The crews were and are, no doubt, selected from the best specimens of the youth of our two great universities,

and the training they undergo would probably improve rather than deteriorate their condition, so that any statistics collected together to show that the mortality of such subjects is not greater than that of other men of the same age would certainly not prove much.

But although Dr. Morgan's book is not of great value as a scientific work, it possesses a certain degree of interest for those immediately concerned in the boat-races. It appears that in the course of forty years, 294 different men have rowed in 26 contested races. Among these 294 men there were 39 deaths, or about 13 per cent. By comparing this death-rate with that of other persons, Dr. Morgan shows that it is decidedly favourable for those who have participated in the contests, and, when the special causes of death are considered, there is really hardly anything discovered to connect the fatal event with the boat-races. Out of the 39 deaths, 11 were from fever, 6 from accidents, others from cancer, calculus, erysipelas, Bright's disease, &c.; and, in short, only 9 died from consumption and other forms of chest-disease, and 3 from heart affections. Dr. Morgan employs almost a superfluity of argument to show that these results by no means favour the supposition that participation in the boat-races tends to shorten life, but he is not so successful in showing that it does not impair the health. The reports as to the health of the survivors must, as we have observed, be obtained from the subjects themselves, or their friends, and in either case the information can hardly be considered trustworthy, even when answers are given to inquiries, which was not always the case.

In many of the general remarks made by Dr. Morgan as to the beneficial effects of rowing and other athletic pursuits in developing the physical condition of the body we cordially agree, and we are not disposed to deny his statement that rowing developes all the muscles of the body, expands the circumference of the chest, and altogether contributes to improve the vigour of both mind and body. Nor are we inclined to undervalue the importance of this and other out-door amusements as a counter-attraction to the casino and the music-hall; and we are not prepared to dispute another suggestion offered by Dr. Morgan, that perhaps the great intellectual efforts made by some of our youth in the present day in competitive examinations may not always be followed by unmixed advantages in after life, either to the individuals or to the community.

Chauveau's Comparative Anatomy.—Comparative Anatomy in England, at least, has until recently been pursued rather on account of its intrinsic scientific interest, and the light that it was capable of throwing upon the intricacies and peculiarities of human structure

[1] *The Comparative Anatomy of the Domesticated Animals.* By A. Chauveau, Professor at the Lyons Veterinary School. Second edition, revised and enlarged by S. Arloing. Translated and edited by George Fleming, F.R.G.S., London, 1873. Pp. 957.

than for any fruit it was likely to bear from a utilitarian point of view. Veterinary surgery, until a comparatively very recent period, was chiefly in the hands of the farriers, and had but little claim to be characterised as an art, and none to be considered a science. The progress, however, which has been made in all natural sciences, and in none less than in comparative anatomy and biology, together with the increased actual value of all domesticated animals, the higher estimate which a growth of public morality has conferred upon all animal life, and lastly, the generally improved state of surgery and medicine, are causes which have combined to turn attention to the relief and treatment of the diseases and accidents to which our faithful friends and servants of the lower creation are liable. The foundation of colleges for public instruction in the veterinary art in our large cities, and the institution of establishments for the treatment of the diseases of animals, Quixotic though the latter may be, are as much peculiar to the nineteenth century as are railroads. It is true that there have not been wanting in every period of Christianity gentle souls who, like St. Francis of Assisi have recognised the great truth that

> "He prayeth best who loveth best,
> All things both great and small."

But we believe that if there be one thing which proves more prominently than another, that the present state of society is a progressive and not a retrogressive one, it is the increasingly high estimation in which animal life and animal suffering are held by all classes of the community.

Amongst the means to improve the veterinary art the first and most fruitful must be that of placing it on a firm basis of anatomy and physiology. Fortunately for veterinarians the study of comparative anatomy had preceded their special pursuits, and they found ready to hand the grand outlines of the structure of the mammalia which they had to treat. They have not been slow to avail themselves of existing materials, and passing from the general labours of the comparative anatomist into a more specialised field they have so cultivated hippotomy as a science, that, in the precision of its facts and minuteness of its details, it is but a little below anthropotomy itself. This work, however, has been chiefly performed on the Continent. In England, to quote the words of the translator's preface to the work before us, "The only text-book to which the student or practitioner of animal medicine could until recently refer, has been that on the 'Anatomy of the Horse,' written by the late distinguished army veterinary surgeon, William Percivall, more than forty years ago, a book which, though in every way creditable to its author, was notoriously incomplete, even as regards the only animal it treated of, and was without illustrations." In France

and Germany, on the contrary, numerous treatises and monographs upon the anatomy of the domestic animals have appeared, and large opportunities have been given for the pursuit of this study at such schools as those of Lyons and Toulouse. Mr. Fleming has earned the gratitude of the whole of his profession, by presenting to the veterinary surgeon and student, in an English dress, one of the best and most comprehensive of Continental text-books, enriched with additions which prove him to have been a conscientious student of the best writers on the 'Comparative Anatomy of the Mammalia.'

Continental writers have the faculty of self-assertion in a more highly developed state than is common amongst our own authors. We think M. Chauveau's an excellent book, but in awarding it this praise we only express the opinion of the author himself, as we gather it from the following amusing specimen of Gallican modesty:

"No effort has been spared to achieve exactitude—the primary desideratum in such a work as this; neither have evenings spent in bibliographic researches, nor fatigue in the dissecting room been considered. All published writings on animal organisation, general treatises, special manuals, monographs, and articles in periodicals have been read and interrogated. But we have more particularly sought for information from Nature—that certain and infallible guide, always wise, even in her diversities; we have consulted her scalpel in hand, with a perseverance that nothing could repel. Animals of every kind were had recourse to, and we have largely profited by the immense resources which our position as principal of anatomical teaching in the Imperial Veterinary School has placed at our disposal."

Certainly M. Chauveau's trumpet gives no uncertain sound. But we must allow something for a foreigner's mode of expression and for the tone of Continental literature, and proceed to glance at the contents and characters of his book.

The work is divided into nine books, the first of which treats of the locomotory apparatus under the heads of osteology, arthrology, and myology. The animals which are the subjects of description, are the dog, cat, pig, horse and ass, ox, sheep and goat, and a small space is also given to the anatomy of the domestic fowls. The book, however, is best described as a treatise on hippotomy with, in addition, a comparison of the structure, form, and relations of the organs in the other animals we have mentioned, and in man, with the equine.

From so large a mass of detailed description it would be impossible to select any portion which would fairly and adequately represent the whole. We must, therefore, content ourselves with a few general observations. In the exceedingly full descriptions of the bones of the horse, which are well illustrated with clear woodcuts,

the names of processes, &c., borrowed from anthropotomy, are chosen in preference to those which have been coined on the vertebral theory. This probably was to be expected in a French work, for as yet the more scientific nomenclature adopted in part by Flower, Mivart, and others, and introduced by Owen, has not come much into use on the Continent. The names of the vertebral system of nomenclature might, we think, have been added as synonyms with advantage to the English student. The fact is that some of the time-honoured appellations which are especially descriptive of parts in the case of man, are not at all suggestive of their homologues in the case of other vertebrates, and although use has familiarised the anthropotomist with them, they are likely to be rather hindrances than helps to the student of general comparative anatomy. It is probably reserved for a succeeding generation of anatomists to introduce a nomenclature which shall be at once simple, applicable to the whole vertebrate series, suggestive of no theory, unless that theory be universally received, and easily retained in the memory.

The nomenclature of myology likewise has its special difficulties. To give such names as deltoid, digastricus, &c., to muscles in animals in which they have not the implied form is absurd. M. Chauveau's nomenclature is adopted with alterations from Bourgelat's, but Girard's which gives to each muscle a name indicating its insertion is made to furnish synonyms. The confusion which prevails is certainly a hindrance to the young student, and we cannot but think that a nomenclature founded on one principle, either on the attachment or on the action of the muscles, should be brought as far as possible into general use. Until, however, a system can be agreed upon by anatomists, the student has a right to expect in his text-book a full synonymy such as M. Chauveau's work affords.

The author gives a somewhat new and full description of the ligamentous apparatus which plays so large a part in the support of the heavy head in large quadrupeds, and which is familiarly known as the ligamentum nuchæ. M. Chauveau describes it in the horse as the common super-spinous ligament, and divides it into a super-spinous dorso-lumbar ligament, and a super-spinous cervical ligament. The former, he says, commences on the sacral spine. It is composed of white fibrous tissue, and extends forward as far as the inferior third of the dorsal region where it insensibly assumes the texture (yellow elastic) of the cervical ligament with which it is continuous, and which is divided into a funicular and lamellar portion. In the ox the super-spinous-dorso-lumbar ligament is composed of yellow elastic tissue, and the cervical portion is very largely developed to support the great weight of the head. In the pig and cat, there is no cervical ligament, and in the dog it is reduced to a simple cord. There can be no doubt that the develop-

ment of the ligamentum nuchæ is as much in relation with the extent and character of movement as with the weight of the head. In the giraffe the ligamentum nuchæ extends back to the sacrum. In the mole it is ossified.

The work we are reviewing has no pretension to be anything more than a work on descriptive anatomy. The student will find in it but little reference to function, and less to the higher teachings of transcendental anatomical science. Although to the worker at comparative anatomy in its wide sense this may prove a disappointment, it, perhaps, is not without its advantage to those who are studying veterinary medicine and surgery, and who need to have their attention concentrated upon exact details. After all plain descriptive anatomy is the basis of all anatomical reasoning, and there is some reason for thinking that, if the details of the anatomy of all vertebrate animals had been worked out with the same minuteness and zeal as have been bestowed on that of man and the horse, we should have had less of rash speculation as to the origin of species than has characterised the science of the nineteenth century. It may prove that the more rigorously the anatomy of species is studied and compared, the less seductive will be found such theories as that of natural selection.

External differences may be the result of habit, climate, mode of life, or struggles for existence maintained through long periods of time, and transmitted by genetic law; but the same cannot be said of minute anatomical differences, the result of no appreciable causes acting on the superficies of the animal—peculiarities perhaps, for which it is impossible physiology in its present phase should assign a reason, but which we can only class as ultimate facts forming part and parcel of the intimate specific character of the animal. For instance, in the case of species so nearly allied as the horse and the ass, who would have expected that there would be a large and tolerably constant anastomosing branch between the basal and internal carotid arteries in the ass which M. Chauveau states is rarely present in the horse. For such a minute difference deep in the recesses of the organism, it is as difficult to assign a reason as for the large vaginal process which is a distinguishing character of the human cranium. Again taking two ruminants, the ox and sheep, why does the occipital artery form part of the rete mirabile in one and not in the other? The theories propagated in the present day seem powerless to explain these minute but real specific distinctions.

The arrangement of M. Chauveau's work is convenient. After the apparatus of locomotion follow sections on the digestive apparatus, the respiratory apparatus, the urinary apparatus, the circulatory apparatus, the apparatus of innervation, that of the special senses, the generative apparatus and embryology. The illustrations consist of 450 excellent woodcuts, many from the French

edition, and others introduced from various sources by Mr. Fleming. We have nothing but praise to bestow on the manner in which that gentleman has performed his work. His additions to the original text, placed between brackets, are useful and judicious. His translation, despite the retention of a few Gallicisms, is readable and clear.

Allingham—Diseases of Rectum.[1]—So recently as in October last we reviewed the first edition of this work by Mr. Allingham, in an article "On the Surgery of the Rectum;" it is therefore unnecessary on this occasion to do more than notify the appearance of a second edition, a circumstance sufficiently indicative that the treatise has been acceptable to the profession. "The additions," writes the author, "made in this new issue relate chiefly to the questions of treatment"—an announcement very welcome to practitioners generally, who are more apt to refer to books in elucidation of treatment, than to those of which scientific disquisitions and pathological minutiæ are the characteristics.

The author writes from large experience in diseases of the rectum; writes practically, and illustrates both teaching and practice by the histories of suitable cases. We will here make this one remark, that, notwithstanding the array of 4000 consecutive cases in the out-patient department of St. Mark's Hospital, and the fact that fistula was the malady treated in as many as 1208, whilst hæmorrhoids, both external and internal taken together, only numbered 965, we are not prepared to subscribe to the author's opinion that fistula is the most common lesion of the rectum; on the contrary, we believe that hæmorrhoids hold that position, and would account for the preponderance of fistula at St. Mark's Hospital, from the special character of that institution attracting those generally more troublesome cases, and from the fact that piles are borne with by a very much larger proportion of their sufferers without recourse to treatment, because their consequences do not make operations so necessary to comfort and cleanliness, and to remove pain.

On the Sulphites and Hyposulphites in the Treatment of Intermittent Fevers.[2]—The efficacy of the sulphites and hyposulphites in the treatment of intermittent fevers has never been generally admitted in British medical practice, and even in Italy, where the idea first originated, it would appear that the question is not yet definitively settled. The present memoir of Dr. Faralli is

[1] *Fistula, Hæmorrhoids, and other Diseases of the Rectum; their Diagnosis and Treatment.* By William Allingham, F.R.C.S. Second edition. London, 1873. Pp. 263.

[2] *Dei Solfiti ed Iposolfiti nella Cura delle Febbri Intermittenti.* Memoria del Dott. Giovanni Faralli. (*On the Sulphites and Hyposulphites in the Treatment of Intermittent Fevers.* A Memoir by Dr. Giovanni Faralli.) Milan, 1872. Pp. 128.

one out of several which were presented to a Medical Commission in Lombardy, who were intrusted with the charge of examining the works sent in with the object of proving or disproving the curative and prophylactic properties of the alkaline and earthy sulphites and hyposulphites in intermittent fevers, as compared with other remedies already known. Out of four memoirs sent in for competition that of Dr. Faralli was considered by the Commission as the most meritorious, and a sum of 1000 lire was awarded to the author as a premium of encouragement (*premio d'incoraggiamento*).

Dr. Faralli's work is divided into eight chapters, in which he successively considers the general nature of morbid fermentations; the etiology of malarious fevers; the treatment of these maladies by the cinchona preparations, and by arsenic and other substitutes; the use of the sulphites and hyposulphites in the treatment of intermittent fevers, and the antifermentative properties of these salts; the reducing powers of the sulphites and hyposulphites; the clinical experience of the treatment of intermittent fevers by those substances; and their comparative value contrasted with the cinchona preparations and with arsenic.

On the whole, Dr. Faralli's opinion in favour of the use of the sulphites and hyposulphites in intermittent fevers is not very strong, and the arguments he adduces in support of his somewhat negative conclusions are characterised by extensive knowledge and great critical acumen. The Commission, while awarding to his memoir the first rank, and to himself the premium above mentioned, appear to be not altogether satisfied with the results at which he has arrived, and they express their opinion that his essay is more full of criticism than of experiment, and that his views are based rather on theoretical arguments than on clinical observation. But be this as it may, they admit the great ability and research displayed in the memoir, and we willingly indorse this favorable judgment, the more especially as the value of the treatment by the sulphites and hyposulphites has never appeared to us to be based on very strong evidence.

Dr. Faralli discusses, in the first place, the question of morbid fermentations, and points out some of the difficulties of the investigation, inasmuch as we are ignorant of the manner in which miasmatic exhalations operate, whether by acting on the components of the blood, or by making use of the blood only as a vehicle; and then he observes that septic poisons, before altering the crasis of the blood, have a tendency to localise their action in certain organs, whereas the phenomena of fermentation, when once commenced, are carried out with the greatest rapidity. He then refers to the geography and etiology of intermittent fevers, and, with respect to their supposed zymotic nature, he remarks that if their presence were due to a ferment the patients ought never to experi-

ence relapses when removed from the marshy districts, whereas relapses do occur after such removal; and, on the other hand, there are relapses which do not occur for many years, although in the midst of miasmata; and again, if these fevers were caused by a ferment, they ought to be contagious, whereas they are not so. Then he proceeds to examine the action of the cinchona preparations, the efficacy of which is universally admitted both in the cure and prophylaxis of intermittent fevers, but the antizymotic properties of which he very much doubts. The power possessed by the sulphites of retarding fermentation is generally admitted, and Dr. Faralli himself found that (among other experiments) some urine mixed with sulphite of soda underwent no change for six days; while another portion of urine, left by itself, became turbid and developed torulæ after two or three days. But, he argues, although this and other experiments prove that the sulphites retard fermentation in certain cases, it is not allowable to affirm that the same effect is produced in all fermentations, and, in fact, some substances are destructive to the life of certain infusoria but favorable to the life of others. Thus the hydrochlorate of quinia prevents the formation of *paramecia*, but not of *vibriones*, and chloral prevents the formation of *vibriones*, but not of *mucedines*.

Passing on to the examination of clinical facts in support of the treatment by the sulphites, Dr. Faralli analyses the observations made upon this part of the subject by Cantani, Sestini, Mazzolini, Cavaleri, Hampton, and Turner; and he adds a few observations of his own, made on the use of sulphite of magnesia and hyposulphite of soda, but the results, on the whole, were not very striking.

The general conclusions at which Dr. Faralli arrives are thus summarised by himself, viz.—1. That the zymotic nature of intermittent fevers is not demonstrated. 2. That although the sulphites in many cases cure intermittent fevers, yet their action is not so rapid and constant as that of the preparations of cinchona. 3. That their mode of action seems to depend on their reducing properties (namely, by removing oxygen) rather than on their antifermentative power. 4. That the only result obtained from them (as is proved by a considerable number of facts) is their great activity in reducing abdominal swellings. 5. That their too long use produces a certain degree of anæmia, and thereby favours the development of paludal cachexia. 6. That their prophylactic power, which was once conceived to exist, is not supported by exact observations like that of the sulphate of quinia. 7. That in the treatment of intermittent fevers, the sulphites are less efficacious than cinchona and its compounds, and should only be used when the latter have failed. 8. That the arsenical preparations, to which it is rarely necessary to have

recourse in the treatment of miasmatic fevers, may still be employed with greater advantage than the sulphites for curing paludal cachexia. 9. That, therefore, of the three methods of treatment most commonly employed in periodical fevers, the sulphites and hyposulphites, being manifestly inferior as well in prophylaxis as in treatment, must be considered less efficacious than the preparations of arsenic.

Original Communications.

I.—On Anæsthesia, Hyperæsthesia, Psuedo-Æsthesia, chiefly as met with among the Insane.[1] By W. A. F. Browne, M.D., F.R.C.S.E., late Commissioner in Lunacy for Scotland.

The objects of this address are—(1) to impress upon you as forcibly as I can that asylums are rich storehouses of instruction, not merely as to the symptoms and treatment of mental diseases popularly so called, but of all, even the most microscopic neurosis, of every condition, in fact, connected with sensibility, motility, and mentalisation; (2) by a very humble epitome to show that we, the officers of such establishments, narrowly and carefully observe and utilise such phenomena, however minute and ephemeral; and (3) thus to expose and refute the cruel and callous calumnies that are from time to time directed against us as having deserted the mission of our profession, betrayed the objects and interests of science, and, in our disloyalty, as having degenerated into mere administrators, agriculturists, and house-stewards. In dealing with psychological subjects it is customary to direct attention chiefly to the cerebro-spinal axis, but I shall endeavour to reverse this course, and to lead you from the centre to the circumference. For our immediate purpose the nervous system may be regarded as a sheath enveloping the rest of the body, sentient, susceptible, vibrating to every impulse and impression from without, and conducting these with a marvellous rapidity, and, during health, with marvellous precision and accuracy, to a consciousness which constitutes us what we are both in identity and external relations. The velocity of the nerve-current is supposed by Helmholtz to be, in a frog, twenty-six yards in a second, which may be regarded as sluggish progress when compared with that of electricity, which travels 460,000,000 yards in the same time, but supposing that the course of external impressions, such as those of touch or hearing, are regulated by the same laws, the access of these to the sensorium is manifestly retarded in certain forms of dementia and stupidity, in which an external irritation or sound is

[1] The substance of a lecture delivered to the members of Professor Laycock's class of psychological medicine, on their clinical visit to the Crichton Institution for the Insane, Dumfries, 26th July, 1873.

not cognisable until many minutes, or even a longer period, has elapsed. An enquiry addressed to a dement or epileptic has often been answered many minutes after I had left their presence, although the attention had been roused and the question apparently understood. It was a magnificent, though confessedly imaginative, conception of Plato, that the world with all its order, and beauty, and life—that the universe itself—had been created and brought into contact with our minds, through the senses, for the special purpose of rousing, raising, and enriching that consciousness, and of elevating us from mere vegetative and animal existence to the influences of reason, conscience, and God-like truth.

It might appear fanciful to prosecute Plato's train of thought further by depicting what we must have been, had there been no external objects to excite, and no tissues to receive intimations from without, did not pathology afford approximate illustrations of such a condition. It is not necessary to advert here to such cases as that of the celebrated Laura Bridgeman, who was deaf, blind, and dumb, or of many idiots whom I have examined, who, deprived of every special sense except the exercise of an imperfect vision, were shut in, as it were, from all world communion, from all the pleasures which pour in through the media of the senses, and from the instruction of which they are likewise the channels. Duchesne has supplied us with examples more to the point:—1st. A hysterical girl, aged 21, presented anæsthesia of all surfaces, including the entrances to passages such as the mouth, nose, &c. The senses of smell and taste were destroyed. The insensibility included the subcutaneous tissues and muscles, however deeply seated, which could be grasped squeezed, struck without consciousness on the part of the patient. 2nd. A hysterical female, æt. 22, sustained loss of sensation over the whole body. This insensibility included painful impressions, electric irritation, &c., and extended to all tissues, muscles, and bones except one small spot on the left side of the thorax, which was painful on pressure, and where occasionally pain arose spontaneously.[1] Here, then, are two individuals to a great degree isolated and removed from the sphere of human sympathies and sufferings. Very early in my experience under this roof I was struck and perplexed by the immunity from pain and annoyance exhibited by patients presenting deep sloughing bed-sores, by their disregard and even jubilant denial of the existence of such indications of decay, and by the rapidity with which such local death took place. As a proof of the latter observation I can recall the history of a female dement who, although of advanced years, appeared in excellent health when attacked by slight cerebral congestion, who lied down quietly on a comfortable bed, and who, after preserving the same posture for

[1] 'Gazette des Hôpitaux' quoted in 'Anal. Med. Psych,' 3, i, 3rd series, pp. 276, 294.

eleven hours, presented a large bed-sore, in its first stage, on one of the nates, but was altogether ignorant of the cause of our solicitude. The greatest known amount of anæsthetic apathy is affirmed to exist in what is called Norwegian leprosy, where exquisite hyperæsthesia is followed by such insensibility to pain that dead fingers and other parts of the body may be cut off, and the bleeding surfaces plunged into boiling pitch without suffering. The disease is supposed to depend upon effusion into the base of the brain or around the spinal chord. My attention was specially arrested by the following circumstance:—A male, æt. 42, had been placed in one of those stuffed beds provided for the infirm, for epileptics, &c., and rested either upon a wool, a water, or an air bed. The night-watch noticed that he was in the habit, during the night, of rubbing the soft leather pads which formed the sides of his bed so constantly as to redden the tips of his fingers. We were not then so easily terrified by that ominous word, *restraint*, and this poor man's hands were enveloped in large, well-padded gloves. Notwithstanding this protection I was summoned early next morning, and found that by continued friction he had not only worn through the leather and stuffing of the gloves, but had rubbed off the skin, muscles, and tendons of the tips of three fingers of each hand, leaving a portion of the bone of each extreme phalanx denuded and exposed, the patient continuing as joyous and hopeful as before, and altogether unaware of the mutilation. This man laboured under what was then an unfrequent or unfamiliar disease, the general paralysis of the insane, a form of disease towards which my thoughts have ever since been drawn, originating many speculations as to whether the exemption of its victims from physical pain and of many of "the ills that flesh is heir to" may foster, or even suggest those feelings and delusions of self satisfaction, infallibility, and immortality by which the great majority of them are characterised.

Of 188 males at present in the S. C. A. four labour under this malady, in all of whom anæsthesia is more or less marked. The same symptom is observed in 1 case of locomotor ataxy, 1 of idiocy, 30 cases of dementia (acute, chronic, and senile), 3 cases of melancholia, total 39, in 139 patients of the other sex; loss or impairment of sensibility is detectible in 7 cases of dementia (acute, chronic, and senile), 4 cases of melancholia—total, 11. Michea tested 20 cases of lypemania, of which 14 were affected more or less with anæsthesia, and did not perceive tickling, pricking, or burning, unless they saw the instrument by which the disturbance was produced. Legrand de Saulle doubts whether this proposition can be received as the standard of the frequency of insensibility among the insane ('Ann. Med. Psych.,' vol. ii, third series, p. 249). The rarity or non-existence of general paralysis among females, long since noted by myself, still continues to be a remarkable feature in this

institution, and there is accordingly no opportunity for experimentation upon the point under discussion. It should be understood that the presence, sometimes even the amount, of anæsthesia in such cases has been ascertained, not rudely, but by the instruments invented by Weber, Laycock, Sieveking, &c., which are capable of measuring sensibility from the extent of one half line on the tip of the tongue to thirty lines inside the thigh, and that, as in cases of division of the cord, this deprivation is consistent with the retention of reflex action.

St. Bernard, one of the most intelligent and pious saints of the calendar, failed, when unwell, altogether to distinguish between sang cru, a dish of half raw blood, and butter, and between water and oil. You may have heard of the more modern instance of the humble general paralytic who believed that he was swallowing green turtle while making a hearty breakfast of oatmeal porridge, but while admitting that his mess very much resembled the ordinary meal of his countrymen, attributed to his critics the mistake as to the real nature of his food. Now, we have some striking illustrations of the enfeeblement or obliteration of the sense of taste, which bear closely upon the dispute as to whether this sense depends upon the glossal branch of the fifth pair or upon the glosso-pharyngeal. They likewise corroborate the theory that affections of the nerve of taste generally depend upon changes within the skull, and show that although suspicions as to the deleterious or adulterated character of food may depend in the insane upon subjective delusions, there are somewhat similar perversions connected with local conditions of the tongue, &c. Although justifying a gloomy prognosis, the deprivation may be congenital, as happened to the celebrated Miss Martineau, who had no sense of taste. She upon one occasion, however, felt the agreeable impression of roast mutton. You may have heard that there are, amongst the more degraded insane, grass-eaters, earth-eaters, garbage-eaters; that these preferences do not depend upon any morbid state of the surface of the tongue; that nausea cannot be produced by tickling of the fauces, and that different parts of the tongue may be insensible to their ordinary stimuli.

We have at present four ordure eaters. In one, a dement, the sense of touch and of pain are likewise impaired in the tongue; in another under great excitement common sensation and sensibility to pain of tongue are much lessened; the gustatory perception is also greatly blunted, especially of sweet substances; in a third, a dement, common sensation and sense of pain of tongue are greatly decreased; but although he cannot detect the impression produced by mustard, taste does not appear to be otherwise interfered with. In a fourth, also a dement, the sensibility of the tongue is impaired. It was curious that Majendie, notwithstanding his acuteness and vivisections, came to the conclusion that the olfactory nerve was not the organ of smell,

—which may be called the twin sense of taste,—although the absence of disease of this nerve within the cranium had been found accompanied by the non-development of the sense. In two of the patients just alluded to as devouring fæces the complicity of the nerve of smell in the anæsthesia of taste is exhibited; in both there is likewise obtuseness to mechanical impression and to pain in the nose, and one of them cannot detect the presence of a rose or of the fumes of turpentine. Wordsworth, during the whole of his life, except once when passing a garden parterre, suffered from this infirmity of the organ of smell. In certain patients suffering from general paralysis there is a palpable inability to distinguish between heat and cold when applied to the surface; and in others manifesting different forms of mental disease there exists an indifference to, or incapacity to feel, different degrees of temperature. Many now under treatment would expose the face, hands, &c., to the fire even until vesication be produced. Others, and these a large number, although shivering, cold, blue, never experience inconvenience nor approach the fire, and will remain nude and, if not prevented, exposed, during intense frost, when the skin is only a few degrees above 32, apparently in perfect comfort, and affirming that they are sufficiently warm. How far such insensibility may be associated with common anæsthesia it is not now expedient to inquire, but it has been invariably observed that the temperature of the body is with difficulty maintained during anæsthesia.

To connect these observations with our present topic, while there are several instances in relation to gustation and deglutition where the individuals do not know the difference between a spoonful of mince and a spoonful of porridge, but who feel sensitively a pinch of powdered mustard applied to the tongue; where there is no distinction experienced between solids and liquids, and where a potato would be swallowed with as little mastication as a glass of water, yet where mechanical and chemical irritation excite pain. There is a notable case in which a glass of boiling water would be taken with the same indifference as if it had been drawn from the well, but as such crucial, we might almost say excruciating, tests cannot be resorted to, it cannot be determined whether the sensation of burning be lost as well as the perception of the degree of temperature.

Of anæsthesia of the auditory nerve, except where such an effect follows scarlatina, otrrhœa, structural diseases of the ear or of the temporal bone, or non-development, or organic affections of the brain; or where it is presented as the dulness or deafness of decay or age; or accompanies mental abstraction, stupor, stupidity, we have little experience among the insane. Cases do, however, occur where, apart from such causes and concomitants, hebetude of this sense accompanies that of others; where patients fail to recognise musical notes, although they still hear other sounds, or, on the contrary,

where, although common conversation or sounds of a similar pitch cannot be followed or discerned, musical notes are still appreciated; and where that curious phenomenon, partial or complete surdity, is temporarily suspended during the impression of very loud and continuous noises on the tympanum. I am acquainted with a gentleman who, though nearly deaf at other times, hears with comparative distinctness during the beating of a drum. Such cases are not peculiar to the insane, nay, the employment of loud or acute reverberations has been recommended in ordinary practice as a cure or alleviation of such affections.

It is said that the power of detecting particular notes has been lost, a condition also paralleled by the extinction of particular tones of the voice, but I have met with no instance of the sort. I have watched a considerable number of examples of blindness, both from amaurosis and glaucoma, which were evidently the off-shoot or outcome of cerebral disease creeping along the optic nerve to the retina; but we more frequently encounter less marked or slighter anæsthesia of this sense. You are aware that the vision of the external world may be interrupted or partially shut out by mists or opalescence of the retina, involving one half of this structure, either vertically or horizontally, before it spreads to the whole, in the form of spots, streaks, discs, not depending upon the transparent tissues, and that these obscurations may be compatible with great pain, referred to the optic nerve, and with the perception of heat in the retina, when no ray of light appears to reach it. Partial insensibility of the optic nerve is met with in the imperfect appreciation of forms, and in the better known colour blindness. The sane share in this deprivation, and if Dr. George Wilson's statistics be correct, every seventeenth medical student of his time proved an illustration of this imperfection. The most recent and accurate observations upon this subject are to be found in a paper by Dr. McDowall,[1] who says that "all forms of brain disease are in a large proportion of cases accompanied by a more or less marked diminution of power to appreciate colour." "Of 324 women examined, 319 certainly perceived colours normally, but 9 (*sic*) were evidently more or less colour blind." Of 207 then examined, 13 were more or less colour blind. Phrenologists were formerly quizzed on referring the perception of colour to one of the convolutions of the frontal lobe; but if Helmholtz's observations be correct, we are called upon to regard particular parts of the retina as in some way connected with the perception of red, blue, and green. It will be readily understood how much the presence of such a defect must influence, or to use a justifiable play of words, must colour the hallucinations of the insane, or even where no

[1] 'Medical Reports West Riding Lunatic Asylum,' vol. iii, p. 129. A less delicate and extensive experiment was tried here, but of forty women tested all correctly distinguished red, blue, green, orange.

delusions exist, how much it must disturb their estimate and relations to surrounding objects.

The localisation of anæsthesia, although more frequently met with in the nerves of the special senses, is not confined to these. Portions of the skin or trunk are occasionally found to be altogether insensible to tactile impressions and to pain, while the adjoining surface is in a normal state. This has been observed chiefly as a result of lead or metallic poisoning. In a youth known to me patches resembling circles on the thorax appeared uninfluenced by pricking or pinching. This peculiarity was traced to large libations of champagne, which was then supposed to be sweetened by sugar of lead. Death, however, ensued, and, I suspect, from one of the neuroses. Somewhat similar limitations have been detected in Psoriasis, of which there are three examples in the asylum, and in other diseases of the skin.

You have already heard in these remarks of impairment of common sensation and of the sense of pain, and of the impossibility of distinguishing heat from cold; and of cases where, although a special sense be enfeebled, pain is acutely felt. Now, it becomes a question whether the perception of pleasure or pain be the same function, be identical with, or be merely an intense degree of, common sensation. Haller has been sneered at for conceiving that along the same nerve there were transmitted sensations of heat and cold, rough and polished, hard and soft, dry and moist surfaces; also weight, itching, tickling, pricking, and pain; an opinion which, if we accept the phenomena referable to reflex action, is either very generally held up to the present moment, or of which no satisfactory refutation has been advanced. Brown-Séquard and many of his countrymen do not concur in these views, but hold that these sensations, especially adverting to touch, pain, and the muscular sense, are different in nature as well as in their effect upon the sensorium, and that they are functions of different parts of the same nerve, physiologically, if not anatomically, separated. If the law of *isolated conduction*, as it has been called, be applied to such a separation, we can comprehend and admit that the various, and certainly widely distinct, impressions, confounded under the sense of touch, may pass contemporaneously along the same nerve, as Haller supposed, but along different portions or fibrils of the same nerve as we now suppose, but have not yet demonstrated. The frequent occurrence of complete or partial cutaneous anæsthesia in the insane while the sensibility to pain remains as in health or even exalted, or the reverse of these conditions, seems to settle this point, and to render it highly probable that the same law obtains in regard to temperature, and so on. You have likewise heard that individuals are incapable of noticing the difference between solids and liquids in the mouth, swallow stones, bones, and hard substances, amongst which I may enumerate, from my own experiences, stones, padlocks, buckles, spoons, knives,

glass, &c.; that others suffer severe injuries, involving the muscles, without evincing any proof of cognition or will. An idiot, having bored a hole in the middle of his cheek, enlarged it till it met the canthus of the lips. We have had rubbers, self-castigators, self-mutilators, one of whom removed the penis, testicles, &c.; another, who cut open the abdomen with a pair of scissors, acts which, however, may have been determined by the hyperæsthesia of heat, or by pseudo-æsthesia, complicated with the anæsthesia of pain. In cases of acute mania complete insensibility and invulnerability seem to exist. A gentleman, now in a padded room of this asylum, who has had repeated paroxysms, has told me that during these, his frightful struggles, for he is of herculean frame, his falls, injuries, contusions are never perceived, and leave no trace except the external cicatrices. Such impassiveness, almost always combined with cutaneous anæsthesia, is legitimately supposed to depend upon enfeeblement of the muscular sense, and of the functions of those sensory nerves distributed to muscular fibre, which intimate the performance of the commands of the will to the brain, from which they issued. In two patients, previously mentioned as displaying complete superficial anæsthesia, it is stated that in "one, feeble contraction of the muscles remained, that she could not stand or walk, and without the aid of the eye failed to direct irregular movements of arms, was unconscious of the act of contraction. When eyes were shut, all spontaneous movement impossible, but unconscious contraction of muscles is felt by others." In the second the contractions of the hand and arm were forcible, but unregulated; she could not distinguish a light from a heavy body; her limbs might be violently struck, might be moved, agitated, and let fall without her knowing. As she did not feel the resistance of the bed on which she rested, she felt alarm on awakening, until assured of her actual position by vision. When the eye was not directed to hands or arms she failed to shut or flex them, and if eye were withdrawn during the act, it was arrested tetanically. When commanded to make such movements, her will produced only slight irregular contractions, the limbs remaining unmoved, while she believed movement had taken place. On the hand being closed and the eye shut her will failed to reopen it, and force exerted by the bystanders required to effect this.[1]

This singular condition, which is altogether independent of paralysis or locomotor ataxy, though, perhaps, generically connected with the latter, has been described by a recent writer as muscular anæsthesia, and defined by him as "a loss of the feeling of muscular action, attended by irregularity, sluggishness, and diminished force of voluntary movement, but unattended by any necessary loss of cutaneous sensibility, or by distinct paralysis." This definition he subsequently enlarges by the addition of awkwardness and clumsiness

[1] 'Ann. Med. Psych.,' *ut supra.*

in performing certain voluntary movements, inability to regulate or pose the limbs without the assistance of such as are unaffected, and of the eye, and generally a want of co-ordinating power in muscular action. This condition is designated as the loss of muscular sense—"le sentiment d'activité musculaire;" "la conscience musculaire;" "le sens de la force."[1] A few weeks ago Dr. Shapter published a most interesting and instructive case of inco-ordination of the muscles of the left arm, with inability to pick up small objects, pose the fingers, or touch particular points with them, unpreceded or unaccompanied by paralysis, except a slight drag in left leg years ago, or by impaired perception of touch, heat, electric stimulus, &c.[2] Even within a few days we have a contribution upon the subject from Dr. Gairdner, Glasgow University, who describes a somewhat similar affection as a new form of disease, under the name of athetosis.[3] Those in charge of establishments for the care of mental disease are too conversant with such affections. Every modification of extraordinary and extravagant movement and attitude, either partially or entirely assumed, either without or in opposition to volition, may be witnessed in our wards, from the oscillations of an idiot, the gyrations of the ecstatic, the pronation or supination of dements, up to the jerking, leaping, gesticulating grimaces of choreaics and hysterics. Hammond mentions that in certain cases of sclerosis of the cord, where the mind remains healthy, the patient cannot stand or walk, but can maintain his balance, and so far command and direct the muscles of his legs when he runs or even rushes forward.

There is at present in the Institution a male long characterised by rapid movements, then by inability or unwillingness to sit, then by diminished precision in walking, then by requiring the assistance of a chair, or some other object, in moving along, then by falling when unsupported by the wall, when spoken to suddenly, or when shutting his eyes. He has long suffered from pain of the lower limbs; he is now generally confined to bed, probably by what may prove the second stage of sclerosis. He presents, among others, the following symptoms:—He does not know whether his legs are bent or extended, when standing sometimes falls backwards instead of sitting down, and makes various movements demanding contraction or extension, under the impression that these acts have been performed, although there has been no response to the will. He does not know when his feet are placed on wood, stone, or carpet, upon a smooth or a rough surface, yet is sensible to heat, cold, and to pain when pricked on the sole of the foot. There is another individual, a

[1] Dr. Russell Reynolds, 'A System of Medicine,' vol. ii, p. 328, art. "Muscular Anæsthesia," 1872.

[2] 'Medical Times and Gazette,' p. 595, June 7, 1873.

[3] 'Journal of Mental Science,' July, 1873.

female, who has passed through eight or ten years of violent chronic mania without paralysis, but cannot now stand or preserve her equilibrium without support. In all these examples of suspension of the muscular sense, which, too, are generally complicated with some degree of anæsthesia, there is apparently a marked interruption to the operation of the will on the muscular fibre. How far these may be connected we shall not decide, but it is certain that many of the grotesque and uncomfortable positions seen in the insane are attributable to the increased power of the will and to the duration of its mandates over the muscles, unknown in healthy men. I have seen a cataleptic maintain one or other of his limbs for hours in an awkward and painful position determined by the will of others, and supporting enormous weights, in one instance a table, a chair, and a stool. There is one male in the house now recovering who held his arm extended or raised until some one interfered, and another, once suicidal, and now silent and solitary, who when placed in a certain attitude, in that of military salute for instance, will remain motionless for hours. In these circumstances the abnormal muscular sense is obedient to volition in continuing contraction or extension, but dependent upon external force or the will of another to discontinue either the one or the other. The proposition of Cabanis that internal sensations or cœnesthesia were conveyed to the sensorium as intimations of the condition of the viscera, extended by the speculations of Gedry as to the four classes into which such intimations could be arranged, viz.—1, activity; 2, fatigue; 3, physical wants; 4, spontaneity, including pricking, creeping, shivering, heat; and lastly, modern discoveries and theories as to the functions and relations of the sympathetic system, have perhaps justified physiologists to treat of anæsthesia and hyperæsthesia of various organs, supplied with nerves chiefly from this source. It cannot, of course, be questioned that nutrition and various vital properties are influenced by vaso-motor, excito-motor nerves; nor that in the degraded habits of lunatics, when the healthy condition as well as the sensibility and motility of the bladder and rectum are implicated, the hypochondria plexus may have some concern, but when I find my discriminating friend, Dr. Bennett, declare that sympathetic nerves may become sensory under the pressure of cholic or during the passage of gall-stones, and that other authorities accept the cessation of pain in gangrene, or the hebetude of paralysis, as manifestations of anæsthesia of cœliac, &c., plexus, I demur. My doubt and difficulty arise not so much from the lack of all positive proof on the individual point, but upon the negative evidence, and upon the great law of the nervous system, that there can be no transference of the functions of one nerve to another, that a motor cannot become a sensory, that all Majendie's experiments go to prove that the office of a nerve once destroyed by

dividing it, and thus separating it from the cerebro-spinal centre, ever was subsequently exercised by any other nerve, even of the same class; and that, notwithstanding one or two obscure observations as to the portio dura becoming affected with neuralgia, pathology corroborates the view. This vicarious action is supposed to be seen in clairvoyance, when the hour may be determined by placing the watch on the epigastrium or nape of the neck, but of this there is no satisfactory evidence, and in the fabulous accomplishments of the blind, whose dexterity and penetration are certainly above praise. I may be received as somewhat of an authority upon the latter matter. I can write, could speedily attain a facility in reading, can ascertain the time on my watch, and above all, can shave with the celerity and safety of other days, and could learn to do all that other blind men have done, but assuredly do not see with my fingers. The solution of the mystery is to be found in the *law of educability* of touch, in common with the other special senses. Accordingly, I shall not introduce here any reference except to what is called paralysis of the vagus, and such sympathetic nerves as may affect the regions to which it is distributed, and this I do because Romberg formulizes and supports the theory, and because the theory serves to explain symptoms in the insane otherwise inexplicable.

You are aware that on dividing the vagus the number of respirations is diminished. Now, Romberg observed a similar diminution in a case of croup, that the stridulous sound was scarcely heard, that the apnœa occasioned no disturbance or distress, that the little boy lay quiet and still, and then died, and that subsequent to death the vagus was found on both sides surrounded by diseased glands. Now, it constantly occurs in asylums that in phthisical patients the nature and stage of their disease could not be ascertained except by the assistance of the stethoscope. The emaciation and the other rational signs may suggest a suspicion, but when there is neither cough, expectoration, pleuritic pain, nor dyspnœa, even suspicion might be lulled to sleep. Every superintendent has seen many of his charges hurry on towards the end, not merely buoyed up by the delusive hope concomitant with their malady, but presenting few of the more painful symptoms by which it is characterised. The cœnnesthesis or central monitor must here be at fault, in conjunction with anæsthesia of the vagus.

Anæsthesia not merely influences the aspect of other diseases and marks their progress, but it is influenced by other diseases and physical conditions. It may be endemic and intermittent, presenting, like mania itself, in the marshy lands along the Rhine, a quotidian or tertian type; it may become epidemic, as in the celebrated experience of the convulsionnaires of St. Medard, thousands of whom, of all ranks and constitutions, derived enjoyment rather than pain from the blows and injuries inflicted by mallets wielded by

professed athletes. Again, the same condition may be produced by stimulants and drugs. It follows nitrous oxide, chloroform, ether, opium, and alcohol in all its forms. The insane seem to be less affected by such agents than the sane; in other words, larger quantities are required to induce the physiological effects. In the use of anæsthetics it has been noted that while sensibility and consciousness are superseded, as in sleep, reflex actions continue, and that dreams of a vivid and ecstatic nature may take place. There is asserted to be another mode of inducing artificial anæsthesia. The experiments of Elliotson, and the operations of Esedale and Cloquet, might lead us to believe that a few passes of the hand, a look, or some unexplained relation between A and B, renders B insensible to pain. Without venturing upon any criticism of this matter, I introduce it merely in order to record that the insane appear to be altogether unaffected by such influences, if these exist.

Hyperæsthesia.—Neuralgia is popularly supposed to be the most common form under which hyperæsthesia is present. As I do not conceive pain to be a mere exaltation of touch, I am disposed to draw a distinction between what may be called the disease and the simply abnormal condition of the nerves. Indeed, I can call neuralgia into court in support of my opinion. An individual suffering exquisite anguish from neuralgia, whatever may be its origin, can perceive with perfect correctness cold, or heat, or pressure applied to the very spot affected, in addition to pain. It is, however, necessary to embrace this topic, as it is inextricably interwoven with true hyperæsthesia, and with many modifications of nervous unhealth. In the first place neuralgia, whether produced by malaria, and perhaps presenting a regular intermittent type; or by local injuries, wounds, or diseases; or arising from metallic poisoning, often induces or aggravates mental diseases. In the second place it is the precursor, concomitant, or consequence of such diseases, especially of general paralysis, epilepsy, and hysteria; and, in the third place, it may be the analogue or substitute for paroxysms of excitement. A melancholic under my care perfectly recovered after sixteen years of acute moral suffering, but was, during the remaining years of her life, a martyr to severe neuralgia, which could be roused by merely approaching the finger to the part. She ultimately died of embolism. I have known instances where suicide and insanity followed simple functional neuralgia. In the remissions of neuralgia, well illustrated in the alternations of pain in the heart and ulnar nerve in angina pectoris, a new paroxysm may be induced by the slightest tactile impression. This effect of common sensation is similar to what occurs when the system is under the influence of strychnia. In this and similar forms of neuralgia it has been observed that pain may pass from point to point and from nerve to nerve, which have no connection by anastomosis, or by community of

origin or function. I have now in my memory a case of general paralysis in the first stage, or what was then called the monomania of ambition, where there was intense suffering, and where the pain passed, not along the course of nerve trunks or twigs, but leaped from cavity to cavity, from limb to limb, and even from minute point to point. These phenomena are in virtue of what has been called the *law of eccentricity*. In many hysterical patients there may be detected weak or acutely sensible spots, the uneasiness being of a neuralgic character, and generally referable to points where nerves pass through bony canals or fibrous tissues. Like other impressionable persons, the insane are subject to sudden flushes of cold or heat, sometimes without, sometimes with, change of temperature.

There is an individual at present in seclusion whose head, while he labours under certain delusions, becomes hot, crimson, and painfully sensible. But there are more precise indications of hyperæsthesia. Many of the insane are intolerant of currents of air, or even of its agitation around them; they become nervous, excited, irritated, somewhat in the same manner as when the wind rushes against the face in neuralgia of the fifth pair, or into the lungs in angina pectoris. Many others recoil from the approach even of those they respect, and if touched by the finger start as if galvanized, although aware of what is about to take place. Epilepsy and hysteria have been induced by the contact of a finger of a bystander, taking the place, apparently, of an aura. The whole surface may share in this susceptibility, and in hemicrania and acute mania even the hair has stood erect, rigid, and intolerant of touch, as in the plica polonica. When sensation is both disturbed and intensified by the presence of the subcutaneous tubercle or other interstitial tumours, there are several features of peculiar interest. If the trunk of the nerve above the growth be pressed all pain ceases. There are, notwithstanding the structural change in the substance of the nerve, periods of complete remission, and, as is the case from the use of veratria, the pain is chiefly felt at the ends of the fingers, toes, &c., or at the extremities of the nerve, wherever they may be. I have known of great relief being obtained by the application of pressure or weight applied to the limbs of melancholics when writhing under the thrilling, throbbing, palpitating feelings which rush along their limbs; and the neuralgic convulsions of Dr. Laycock, and the aura and pricking in the limbs, whether depending upon tumours or superficial wounds, has been arrested by ligature. In a case of which I know the details the use of a tight garter below the knee was temporarily triumphant, but the ultimate issue was convulsive apoplexy and death. Kant proposed violent exercise as a remedy for bodily pain. This may be the source of the modern practice of recommending active exertion in the majority of cases of insanity, and especially in melancholia. Others have suggested that pain in a particular nerve might be relieved or

removed by concentrating the attention upon another remote and healthy nerve. This prescription is founded upon an erroneous interpretation of what is called the *Law of Reference*, whereby we intuitively refer to a particular point that sensation which is, and can be, present in consciousness alone. But this law has suggested the employment of blisters in order to withdraw the attention from mental suffering or delusion to a local painful sensation. When excision of a portion of a nerve is resorted to for such affections, the continuance of the calorific power of the surrounding tissues does not appear to be affected. Tumours in the brain, itself in healthy states insensible, are the source of frightful pain, the only form of hyperæsthesia of this organ known to us. Modern psychologists, however, have discovered or invented a metaphysical sensibility of the hemispheres. "Although the hemispherical ganglia are confessedly not sensitive to pain, still they have a sensibility of their own to ideas, and the sensibility which thus declares the manner of their affection is what we call emotional; and as there may be a hyperæsthesia or an anæsthesia of sense, so, also, there may be a hyperæsthesia or an anæsthesia of ideas." Is not this hypothesis equivalent to saying that an idea, say of an equilateral triangle, acts upon the vesicular neurine, as light upon the retina excites the feeling called pity, pride, or any other, and that these feelings are, or should be, regarded in the light of sensations? Is it not notorious that no class of ideas necessarily excite emotions, such as that of self-love; are not connected with ideas at all; that, unless there be a capacity for emotional excitement, no ideas, not even those supposed to stand in a certain objective relation to feelings such as benevolence, veneration, &c., produce any effect at all; and, lastly, were even all other objections futile, what ground have we for supposing any sensibility or excitability in the hemispheric cells at all? In fact, it might be as plausibly argued that the emotions excite the ideas, and that the fervour of a passion or appetite stimulated the hemispherical cells to present or reproduce the object by which it might be gratified. Hyperæsthesia of gustation is frequently met with in the insane, either alone or accompanied by irritability and uneasiness in the tactile nerve, or by vitiation of the secretions in the adjacent glands. Of the exquisite delicacy of taste attained by wine-testers and others we have no examples; indeed, this affection consists in perceiving particular sapors when no sapid body is in contact with the tongue. When hyperæsthesia of the olfactory nerve exists a centric origin is generally inferred, but certain instances of the inhalation of supposed odours, or, as generally happens, offensive effluvia, may depend upon the impressions made from diseased passages. True exaltation or education of the sense, such as is found in perfume-makers, is not known, although I have seen lunatics most powerfully affected by smells which could scarcely be detected by those around. The outer

world is more prolific in illustrations of the power and constitutional effects of smells. Erasmus experienced febrile symptoms on smelling fish; Voltaire narrates that an officer was thrown into convulsions and unconsciousness by the odour of pinks; to Orfila we are indebted for an account of the painter Vincent, who was seized with vertigo and syncope on smelling roses, and of a lady who, after smelling a decoction of linseed, was immediately attacked by swelling of the face, fainting, and loss of consciousness, which continued twenty-four hours. Can disagreeable and disgusting odours and sapors stand in the same relation to olfaction and gustation that pain does to ordinary sensation? The acoustic sense may become morbidly developed in hearing sounds which do not exist, in regarding as loud and painful what are really low and pleasurable impressions, in accepting as acute what is grave, and in detecting harmony which is inappreciable by others. The deaf are sometimes able to hear musical notes. Tinnitus aurium is believed to accompany hypertrophy of the heart, and explosive noises in the ear to prognosticate cerebral disease. It should be recollected, however, that it is impossible to determine upon what part of the organ of hearing these perversions may depend, and whether they may not be sensorial. Luminousness, whether general or diffused in concentric rings or flashes of light, as when constituting the aura of epileptics, form the first stage of a long series in the hyperæsthesia of vision. The impressions become first chromatic, then intensely clear and unbearable, as if every object were close to the eye and blazing under the rays of the sun or of a bright light. It has even been asserted, but without due caution, that when the retina is the seat of the affection the blood-vessels, and even blood-corpuscles, may be seen in sharp outline—in other words, the eye sees itself. By some modification of hyperæsthesia a lady well known to me, whenever the light of a lamp, candle, &c., was projected on the retina, saw three lamps, candles, &c. In glaucoma, lights assume different forms, generally circular, differing from that of the external object represented. The totally blind can recall not only the pictures of their former experience, but see brilliant spectra in the sphere of vision. It is obvious they could not succeed in their acts of recollection were the theory propounded by Sir D. Brewster correct, that where memory recalled former visual impressions the original pictures were projected anew upon the retina, and thus perceived. His speculations, however, lend some countenance to the power claimed by certain individuals of calling up objects beyond themselves, by Goëthe and Müller of creating or fashioning and then seeing them, by the former of having seen his own person—himself, in fact—approaching, and by the poor hypochondriac described by Gregory, who constantly saw a skeleton in accordance with physical laws, that is to say, if a certain or opaque body were, at a certain distance, interposed between him and the

thorax or pelvis he could not see these parts of his tormentor, but the head still grinned at him above the curtain. It would be absurd to question the dogma that space and our relations in it are determined by sight and touch, or to draw any bold conclusions as to the instrumentality of other physical agents from the facts that distance and breadth are intimated by the acuteness of sound, and that rays of light and heat give notions of extent when brought into contact with the surface of the body, and that the muscular sense is required to impress upon us the idea of resistance. But it has been held that those vertiginous feelings of loss of equilibrium, of falling to one side horizontally, vertically; the apparent rotation of surrounding objects; the oscillations, corruscations in dreaming, drunkenness, seasickness; sensations of increased or diminished size, are all due to hyperæsthesia of vision. It can be readily conceived, when such a man as the philosopher Saussure became a victim to such experiences, which, although knowing them to be unreal, compelled him to take down partitions, doors, &c., in his house, in order to secure free and safe transit for himself,—how vast is the field for speculation as to the conceptions and movements of the insane when in a state of excitement involving the external senses, and when we observe their mystification as to the qualities and position of surrounding objects, or watch their insecure step, their staggering, vacillating gait, even when no disease of motility can be detected. It is highly probable that there may be a regulating as well as a co-ordinating faculty connected with the muscular system, and long referred to the cerebellum. It is likewise probable that cœnæsthæsis, or the intimations of the existence and conditions of the viscera, which, Reil asserted, would create a consciousness of our personal organisation even although no sheath of sensory nerves covered the superficies; but, at present, although the door be ajar, and although a long vista can be seen beyond, we are not in a position to pronounce dogmatically upon the cause of the phenomena described. For the same reason, and because of the constant conjunction of sensory nerves and ganglia with sympathetic nerves, a few remarks will suffice to state the supposed existence of hyperæsthesia of the vagus, evidenced by tickling, cold and heat in the bronchi, the globus hystericus, bulimia, polydipsia, so characteristic of many forms of insanity; and hyperæsthesia of the cardiac nerves, solar, hypogastric, mesenteric, spermatic plexus, and so on, of which the only symptom is pain in a given region. This enumeration is submitted solely because neurologists have sanctioned such a classification of the painful affections of the thorax and abdomen. It is highly probable that some influence is exercised by what are called the excito-motor nerves over the expression of the emotions, by the excito-nutrient nerves as displayed in the emaciation and corpulency of different stages of mental disease, by the excito-secretory nerves as seen in the limpid urine of hysteria, the

oxaluria of hypochondriasis, melancholia, &c., and in the vitiated secretion of saliva pituita, &c., and by the vaso-motor nerves,—those valuable aids in difficult diagnosis, in the production of local congestions in epilepsy, &c.; but this influence is not peculiar to the bodily conditions existing during mental diseases, is only hypothetically explicable, and is chiefly supported on anatomical grounds, on the juxtaposition, or apparent connection, of different classes of nerves, a mode of reasoning as logical, in my estimate, as if we were to predicate the nature and effect of messages transmitted along telegraph wires because we can trace the direction, destination, &c., of these wires. There are some minor forms of hyperæsthesia, such as pruritus, formication, sensations of creeping, tingling, &c., which are frequently met with in the insane, which involve great distress, and originate various delusions, such as the presence of pediculi, animals under the skin, and so forth, which I introduce at this place principally because similar sensations are artificially produced, and, it is curious to note, by the very agents which are exhibted to allay more intense irritations of the same kind. Thus morphia is followed by itching, veratrine by tingling as well as numbness, and ergot of rye by creeping. Of the same number of patients as formerly specified, hyperæsthesia in different degrees may be traced in eight males, five of whom labour under chronic mania, two under acute mania, one under lypemania; in five females, of whom two are affected with chronic mania, two with lypemania, and in one the exaltation of sensation precedes an attack of epilepsy. This section of the inquiry has its ethical bearings. On visiting the insane you will meet with individuals who complain of great suffering in various parts of the body, inflicted by mesmerism (of this we have one notable example in the house), by galvanism, by supernatural agency; and others who allege that they have been struck, thrown down, maltreated, mutilated, even ravished (and of these we have likewise examples), by officers or companions, but under circumstances which they can neither describe nor establish, but of which, if labouring under pseudo-æsthesia of vision, they may conceive themselves to possess proof. Now, the interpretation is erroneous, the sensations and the sufferings may, in the majority of instances, be real, and it is of paramount importance to those who are accused of such assaults that they should be exonerated from such grave charges, and that those who are pained and terrified should be relieved.

Pseudo-æsthesia.—Esquirol and Brièrre de Boismont essayed the differentiation of illusions and hallucinations, or, in other words, perverted sensations, defining these as erroneous judgments as to the nature of the impression on the senses, not including deception or error in the senses themselves, holding that there was no external

object or actual impression in illusions while both exist in hallucinations.[1]

Other authorities have imagined that to be an illusion which is known and recognised as unreal, and that an hallucination which is believed in and acted upon. It is suspected that both of these distinctions are erroneous. The insane believe illusions as firmly as they believe hallucinations. Even sane men have reposed trust in illusions. Luther credited the choir of angels; Bernadotte the old woman in tatters; and in order to produce the spectrum of the sun seen by Newton it was previously necessary to gaze intently upon the great orb itself; and Pascal, in order to realise the great gulf on the brink of which he seemed to be, required to look to one side and over the arm of his chair. It is well known that pseudo-æsthesia often passes into or is conjoined with actual delusion in the insane, and that it is very difficult to separate in them what is objective from what is subjective in mentalisation. There is in every asylum a very large number of patients who are the sport of such errors, so great that the census would include one half of the community. There are with us individuals who are tormented by flies creeping over the skin; who see and sometimes pursue sheep; who are visited by angels and divine persons; who assume the name and dignity of kings and conquerors; but in place of giving our own experience of the proportions of such manifestations, we shall select, first, that of Thore and Aubanel. These psychologists noted in general delirium and maniacal excitement hallucinations, of hearing in 23 cases, of vision in 21, of taste in 5, of touch in 2, of smell in 1. But, unfortunately, the total number of persons examined is not given, and these statistics apply to the relative frequency with which the special senses are affected. Of these 52 cases 4 presented hallucinations both of hearing and sight, and another, in whom four senses were implicated. In partial delirium and monomania hallucinations were observed—of hearing in 25 cases, of vision in 14 cases, of taste in 5, of touch in 1.[2] From these facts it would appear that affections of hearing are most frequent, those of vision next in order, of taste more rare, and those of touch rarer still, and those of smell have not been detected. Secondly, that of Michéa, of 106 cases of hallucination examined by him, vision alone was affected in 24 cases, hearing in 27, touch in 4, taste in 2, smell in 1, whereas hearing and vision were both hallucinated in 27 cases, vision and touch in 6, hearing and touch in 1, vision and taste in 1, hearing and smell in 1, smell and touch in 1; 2nd, those of hearing, vision, and touch in 5 cases, of vision, smell, and touch in 1, of vision, hearing, and

[1] 'Des Hallucinations,' A. Brièrre de Boismont, p. 108; Esquirol, "Des Illusions," 'Maladies Mentales,' § i, p. 202.

[2] 'Recherches Statistiques sur l'Aliénation Mentale faites à l'Hôspice de Bicêtre.' Par MM. Aubanel et Thore, p. 98, 1841.

smell in 1, of vision, hearing, smell, and touch in 4.[1] As may be inferred from these numbers, the implication of more than two senses is comparatively infrequent, but this result is in great measure due to the difficulty attending the inquiry, and the reluctance of its subjects to confess the whole truth. There have been repeatedly patients here who experienced illusions of three senses, very generally of vision, hearing, touch, and many striking examples are on record. H. B—, engaged to translate a work into German, had delayed the execution of his plan, saw during the night an old man with white beard, &c., approach, who touched his right ear, and upbraided him with breach of contract. B— saw a phantom, but false angel, enter his room with a slow step, felt him grasp his hand firmly and warmly, heard him utter admonitory words, and then perceived a fœtid odour, when he abruptly left the apartment. I at one time attended a melancholic clergyman who was persecuted by delusions of gustation and olfaction into the belief of mercurial and syphilitic taint and into the commission of suicide. Ravaillac was prompted to the act of regicide, according to history, by hallucinations of four senses. At the present time I am consulted by an individual who hears profane and indecent proposals, who sees filmy and frightful figures, finds that her food has lost all its former flavour, is surrounded by the smell of a corpse, feels a mouse and lilliputian men and women running to and fro behind her left ear, and finally experiences the sensation of a solid body in her throat, which seems to interfere with her power to subdue such imaginary torments. She is certainly insane, but prosecutes her ordinary employment, and affords what is to me a unique instance of illusion of all the special senses and of the vagus or sympathetic. Many of the sensuous deceptions of the insane take their rise from sleep, from imperfect perception, and from concentration of the attention and interest on particular trains of thought or external objects. A patient of mine awoke from sleep with the conviction that he had seen "the valley of the shadow of death;" he awoke, and long remained insane, ever overshadowed by the horrors of his vision.

Müller could during the night, on closing his eyes, project landscapes on the wall of his darkened room, which did not follow the motions of the eyes, and he ascertained that this faculty was possessed by about two, three, or more per cent. of his students; and many of my charges have watched an endless succession of pictures pass before them during the night, to be afterwards haunted by them during the day, and ultimately to come to the conclusion that the whole was a chromatic phantasmagoria, devised by external agents.

The illusions of hearing, &c., which so often transform the hours of rest into scenes of misery, confusion, and fear, are often suggested

[1] 'Du Délire des Sensations,' par C. J. Michéa, pp. 269, 270. 1846.

by real noises, such as the sounds of a clock or the visits of a night-watch; and it is instructive to know that a similar connection has been shown by actual experiment to subsist in the sane and while consciousness is partially vigilant. Maury, after describing luminous-coloured, fantastic spectra seen between sleeping and waking, states that being himself subject to this semi-morbid condition, experienced, 1, when lips and nose were tickled by a feather, a dream that his face was smeared with pitch, which tore off the skin from the surrounding parts; 2, that vibrations of steel near to his ear were followed by dream of bells passing into the tocsin as pealed in 1848; 3, on inspiring eau de Cologne, dream followed of perfumer's shop, Cairo, the East, the shop of Jean Farina; 4, pricking of neck, followed by dream of blister, and physician who ordered it.[1]

We have accounts of the permanent appearance of angels to an artist accustomed to depict them. A friend of my own, after long microscopic examination of insects, could not get rid of coleoptera and lepidoptera. We had here the inventor and manufacturer of one of the golden pens formerly so fashionable, who for months saw myriads of golden flies floating about him, at which he used to clutch; and one shepherd at present pursues the images of the sheep once so familiar to his eye; indeed, so numerous are such cases as almost to justify the ancient philosophical fancy that films or phantasmata were incessantly thrown off by external bodies stamped upon the organs of the senses, and there remained as materials for memory. A similar connection has been traced in the illusions of the other senses. The ear, when all is silent, perceives sounds, voices, tunes, heard at remote times; even to the deaf are conjured up favourite melodies; and flavours and perfumes reach the sensorium without external impressions, but which have been dwelt upon in former years. A patient of my own was constantly enveloped in an atmosphere of chloroform, and another for long periods enjoyed an imaginary quid of tobacco. But sudden and powerful emotion as well as concentration is succeeded by illusion. I have a reference to a case where a youth, taking up an object in the dark, which proved to be a skull, became terrified and excited, and for ten days saw a luminous object constantly rotating in his sphere of vision before one eye. Moreover, it has been advanced that such visions may arise from certain parts of the retina coming into action, and then falling within the conception of the sensorium, or that they are excited by the transmission of images from the sensorium to the retina, somewhat according to Sir D. Brewster's theory. In this way it has been supposed the visions of ecstatics and religious enthusiasts may be explained, who thus saw out of themselves heavenly visitants upon whose traits they had long meditated.

[1] Maury, 'Sur le Sommeil et les Rêves.'

It is much more probable that these apparitions were nothing more than the reproduction by recollection of the pictures or sculptures by which such devotees were generally surrounded, and before which their devotions were paid. A similar solution may be formed for the combat scene in the sky during the crusades, for the luminous armies observed in march along the hills of Westmoreland previous to 1745, although conditions of the atmosphere may have aided in this deception, as in the case of the Giant of the Brocken. Into the inquiry concerning epidemic illusions of the other senses, such as the cries heard from the battle-field of Marathon, the wailing psalms from the hole of St. Patrick, this element cannot enter, nor can it elucidate the seas of blood which seemed to ebb and flow around many of the fanatics of the middle ages. But the crimson thus seen by these multitudes of madmen is not the only chromatic illusion which has been recorded. Men have seen every object around them green as the verdure of our spring, or red as with the roses of the current month; and unless medical annals have misled us, the jaundiced, both sane and insane, have gazed for months upon a yellow world. You are aware that the blind behold objects not merely in their natural forms but in their natural colours, and this whether their infirmity has followed atrophy of the optic nerves, disorganisation or extirpation of the whole eye, that colorisation in patches of clothes, walls, &c., is seen, with partial credence, after operations for cataract and glaucoma. In like manner the imagination of those of unsound mind; both paint objects that are colourless, misinterpret lights, shadows, see their friends draped in mourning when they may be gaily attired, and hold that they have changed the colour of the skin, their height, weight, &c. There is a case supplied by Dr. Dewar to Dr. Abercrombie of a blind woman who, whenever she went into the open air, was preceded by an aged female in a red cloak, carrying a staff with the beak of a raven as a head, but who never entered her house. There occur, from time to time, mental affections, as in chronic melancholia and the monomania of suspicion, where patients conceive that a veil, or mist, or mantle, intervenes between them and the solid earth, that "they see through a glass darkly," that they see like Nicolai men, parts of men, or animals, suspended in the sky or tumbling from it, processions winding serpent-like around them, the latter impressions being closely assimilated to the peculiarities of the Second-Sight of our Highlanders. In that prophetic vision the funereal or ominous assemblage of well-known personages pass within a certain distance of the seer, but through its links or members he perceives, and can likewise identify the real trees, houses, and inhabitants of the spot. Are such impressions, well authenticated and inexplicable as they are, reducible from the ranks of supernatural visitations to that of illusions? There are more repulsive forms of pseudo-æsthesia.

An Emperor of the West carried with him a morsel of horse-dung as an exquisite gratification to the sense of smell, and an old military man with whom I was acquainted chewed assafœtida as a *bon bouche*. Many of the insane revel in the most disgusting and loathsome effluvia. It is said that certain of the lower castes of Hindoos prefer carrion, and we have known lunatics, whose olfactory sense was perfect, retain their food for days beside them, or resort to rumination as a luxury to the palate. In the same category may be placed the hideous and disgusting words and suggestions which reach the ear unaided by pulses of the air. We had here, not long ago, a clergyman who, even at moments of the gravest character, heard opprobrious epithets addressed to himself, and resented them vindictively. Long series of faces and masks are seen by children in the dark, but in noon-day grotesque or jibbering phantoms obtrude upon the insane. We have had inmates who recoil from hobgoblins and harlequins, and one, an imbecile merry-andrew himself, who for many a sunshiny day attempted to dance down his own shadow. It has been long believed that pain could be created or aggravated by thinking of pain in reference to a particular point or organ, and sensibility is certainly exalted or rendered more cognisable by attention to the seat of suffering; the perception and study of symptoms but increases their intensity, affects the nutrition of the organ with which they are connected, creates the suspicion of mortal maladies, the craving for new remedies, and those false and fanciful interpretations of vague and imperfect, but what may be true impressions, which constitute the essence of hypochondriasis. It is highly probable that anomalaus, or painful sensations in the gullet, may have evolved to the morbid imagination the presence of worms creeping up the gullet and out from the nose, the intrusion of a bird within the thorax, the gambols of frogs or the struggles of contending armies in the stomach, of the mimicries of Punch and Judy, of the doings of judicial and monarchical personages in the same viscus; of the predaceous ravages of a rat in the intestines, of the stinging of a serpent in the rectum, and of the quickenings of a fœtus in utero, —of all of which we have examples at present. The effects of stimulants and anodynes in the production of illusions are well shown in the diminution of the size of objects which has been experienced after the inhalation of æther, in the rats, mice, grain, and other minute objects in delirium tremens, and in the glorious vividness and variety of the effects of Hachisch. The eater of this drug mingles sensuous with sensorial impressions, and is sensible in a brief space of exquisite and glowing pleasure, and of the immediate presence of fire, voices, and remembered landscapes.[1] You may have conceived, and rightly conceived, from these rather desultory remarks,

[1] Moreau, 'Du Hachisch et de l'Aliénat. Ment.,' p. 151.

that I am inclined to be an Iconoclast, that I attempt to break down the idols of the cave and of the theatre, perhaps, in order to set up images of my own for worship; that I wage war against the substitution of hypothesis for logical induction, of sesquipedalian phrases for simple facts, but I think that, in my scepticism or humility, as the case may be, I have adduced sufficient evidence to prove (1) that the mind may be studied at the surface through sensations, as well as metaphysically through emotions and ideas; (2) that every mental condition casts out from itself an indication or reflection on the external senses, which may so far reveal the character, though not the nature or amount, of departures from healthy action in the centres; (3) and that, in proportion as these indications, or reflections, or symptoms are studied, analysed, clsssified, we shall approach a broader and more strictly psychological basis for diagnosis and treatment of mental disease, and a more humane and philosophical interpretation of the crimes, the follies, the failings, even the foibles of our sane as well as of our insane brethren.[1]

[1] I would here express my obligations to Dr. Gilchrist, Dr. Anderson, and Mr. Malan, for important aid in the preparation of these observations.

II.—On Puerperal Convulsions.

By Edward Copeman, M.D., F.R.C.P., Senior Physician to the Norfolk and Norwich Hospital, &c.

This disease assumes a more unpromising appearance to an inexperienced practitioner than almost any other in the department of midwifery, the symptoms being so threatening, and the aspect of the patient so distressing, that recovery would seem almost impossible. Yet we find, by experience, that many cases get well, and that, in spite of the frightful disturbance to the nervous system and the apparent suffering of the patient, she is unconscious of what is passing, and, on recovery, has no recollection of what previously excited so much apprehension and alarm in the minds of her attendants and friends. As far as my own experience goes, there is, for the most part, an absence of any serious organic lesion, and even death may occur without a *post-mortem* examination revealing any very marked pathological condition. No doubt these circumstances render it a disease favorable for medical treatment, and the practical lesson to be learned from it is that we must not allow ourselves to be led into too great activity of treatment from the idea that, with symptoms apparently so severe, we not only have no time to lose, but are bound to make use of appliances of equal power and activity with the symptoms they are to combat. When we have acute inflammations to treat, which, if neglected, would rapidly run on to destroy the texture of an important organ, we cannot be too energetic in our endeavours to subdue them. But this does not apply in an equal degree to puerperal convulsions, for in these cases organic changes are of less frequent occurrence, however severe and threatening the external phenomena; and too great a reliance upon powerful remedies unsparingly employed may not unfrequently be found to complicate the disease and bring on difficulties of another description, not more under command nor less dangerous to the patient. For instance, there can be no doubt of the general propriety of venesection in this disease; but if carried to too great an extent, sudden exhaustion of a most alarming character may be produced, or we may have to encounter a state of mental derangement which, from its severity or long duration, might induce permanent mania or imbecility. This is exemplified in case No. 5.

It has been said that in most instances an observant practitioner will be able to detect symptoms in a patient, previous to delivery, indicative of a liability to convulsions during labour; and to some extent this is true, but however much prevention may be better than cure, I fear we shall rarely be able so to prejudge a case as to be sure that an attack would have occurred had not the precaution

we had taken been observed. One of the most common premonitory symptoms is headache, sometimes oppression of the stomach seems to be a cause; and when we know these to be present before labour we ought undoubtedly to correct them. But distress of mind and depression of spirits are more potent causes, and are often beyond the control of the medical adviser. Another very frequent cause appears to be a general congestion of the system, accompanied with albuminous urine, and this can but rarely be effectually relieved by anything short of delivery. So that, after all, we shall generally have to encounter the disease itself rather than be called upon to treat its premonitory symptoms; and our chief consolation will be that, however much appearances may contradict the assertion, time is generally afforded by the disease for the administration of suitable remedial means. *What these should be must be determined by the nature of each individual case.* But I would place in the foremost rank venesection; at all events, to the extent of relieving signs of external congestion when these are present. After venesection I believe opium to be the best remedy, and it is not unusual for patients to awake almost well after a long sleep. Neither the degree of stertor, nor the length of time a state of unconsciousness remains (unless unusually prolonged), are of so much importance in this as in many other diseases; and they are not indicative of great danger unless the respiration becomes very *slow* as well as loud, and we find it impossible to lessen the insensibility *in any degree* by attempting to rouse the patient. In some cases the inhalation of chloroform is very beneficial in subduing the convulsive movements and preventing exhaustion, and in others the only way of putting a stop to the convulsions is to put a termination as soon as possible to the labour.

The subject of puerperal convulsions is so extensive and of such great importance that it is impossible to deal with it satisfactorily in a paper of this kind. I shall, therefore, content myself with performing the pledge I made to give detailed histories of some cases I have met with in consultation, and with noticing a few direction-posts to mark which I consider the best roads to lead to success, leaving it to the acumen, judgment, and experience of each individual practitioner to determine how far he should continue on the main road, or when he should branch off into the by-ways he may meet with in his progress. To be enabled to distinguish clearly between the various kinds of puerperal convulsions, and at once decide upon the befitting treatment, is a most desirable qualification for an obstetric practitioner, for a mistake in the application of remedies in a disease of such severity might be attended with fatal consequences; and yet how difficult this must be when we consider that, *with a train of external phenomena very similar, almost identical, in appearance*, one case may demand venesection to as great an extent, perhaps, as almost any other

disease whatever, whilst another requires an exactly opposite method of treatment.

The first general division I should make would be under the heads of convulsions before labour and those which occur after labour; and I would lay it down as a *general* rule that convulsions before labour require bleeding, and those after labour do not; this, however, not without many exceptions, dependent upon the cause and nature of the symptoms. Convulsions during labour occupy a middle space, and must be treated according as they partake more or less of the other two divisions. In convulsions before labour we generally find the system in a state of plethora and congestion. They occur mostly in first labours, where the resistance to enlargement of the abdomen is greater than when children have been born before, and the pressure upon the organs generally more considerable. The blood-vessels are more active in pregnancy, and the return of the blood is retarded by the pressure exercised upon the venous system. The kidneys, like other organs, become congested, and the urine of pregnant women for the most part contains albumen. When there is no actual disease of the kidneys, this passes off soon after the relief from pressure afforded by the birth of the child, and the resolution of the uterus to its healthy size and weight. It is equally true that albuminuria may occur from other than vascular causes —from Bright's disease, from defective innervation, and other organic changes. But that vascular congestion is a very fruitful cause of it in pregnancy is proved, I think, by the great number of women who labour under it during pregnancy, and have nothing the matter with the kidneys at any other time.

In convulsions after labour many of the conditions are different, and the necessity for bleeding is much more doubtful, although sometimes, from a continuance of the plethoric condition of the system, it is absolutely required. After labour the patient may be much worn and exhausted from the sufferings she has undergone; she may possibly have lost a considerable quantity of blood; she may have been deprived for many hours of comfortable and refreshing sleep, and unable to take a sufficient quantity of food. Such a condition of general depression and fatigue does not usually demand bloodletting, and opium is the more suitable remedy. Convulsions dependent upon fear or alarm and of an hysterical or epileptiform nature do not require bleeding, but are more satisfactorily treated by opium or inhalations of chloroform. It is said by Dr. Carl Braun, a German physician, who has written very forcibly on eclampsia or uræmic intoxication, that chloroform inhalations are the best means of mitigating and bringing to an end uræmic convulsions, either during pregnancy, labour, or in the puerperal period.

Case 1.—The first case I relate was that of a strong plethoric woman, æt. 25 years, who had had an attack of convulsions at the

eighth month of pregnancy, with slight indications of commencing uterine action; and under the idea that labour would go on and be accompanied with convulsions, I was requested to visit her. She was largely bled and had a turpentine enema; and, contrary to expectation, she had no more attacks of convulsion, but went on to her full period and passed through her labour without any complication.

2. The next case exemplifies the value of bleeding, and shows to what a considerable extent it may be carried under some circumstances with safety to the patient. Mrs. —, æt. 28, subject to frequent headaches and confusion of thought at times for two or three years; during the last few weeks pain in the head has been more frequent and severe. Taken in labour with her fourth child early on the morning of February 21st, 18—. At 6 a.m. she had a convulsion, and her surgeon was sent for, the convulsions having recurred several times before he arrived. After bleeding her freely twice, her labour, which was not severe, terminated. When the child, a healthy living male, was born, she recovered consciousness so as to be able to speak to those around her; but she was confused, complained much of her head, and soon had another convulsion, for which Mr. — bled her again, and gave 8 grs. of calomel. She did not, however, now recover her senses, and as the convulsions continued I was requested to attend. She lived in the country, and I arrived at 11 a.m. Mr. — had bled her five times, and drawn a large quantity of blood; but the convulsions were frequent, and he was afraid to carry bleeding further. She was quite insensible, at times breathing with a fearful stertor. Body quite warm; pulse hard and strong; pupils contracted; frothing at the mouth; muscular tremor. In the night, before convulsions came on, she vomited a large quantity of green fluid. I advised another bleeding to ℥xij; an enema of castor oil and turpentine, mustard poultices to the legs and epigastrium, cold to the head, and hartshorn to the nostrils. After this there was an interval of an hour without a convulsion; then she had two more, and again she was bled to about a pint. We then gave ℥xxv each of Sp. Ammon. c. and Tr. Opii. The pulse became much softer, though more frequent; and the bleeding appeared at once to put a stop to the convulsion that was present when the blood began to flow; it lasted not a minute after the vein was opened. We also gave another turpentine injection with ʒss Tr. Opii in it; her lips had become pallid, but her skin was warm, and her breathing quiet and regular; she seemed to fall into a quiet sleep, in which state she remained until I left her, having had no convulsion for three hours. She occasionally moaned as if from after-pains; she could move her limbs, and opened her eyes occasionally, but could not be roused to speak. I left her with the hope that her present calm and quiet condition would terminate in a

return of consciousness, as her pulse did not at all falter and her respiration was tranquil. During her illness she was bled seven times, averaging from ten to sixteen ounces of blood each time; yet the pulse was regular, about 120, the lungs unembarrassed, the skin warm, and there was no approach to syncope. I was informed next day that she remained in the same tranquil state as when I left her until four o'clock this morning, when she awoke conscious and took some milk broth. The bowels had not acted, and she was still drowsy, having no recollection of her labour, and not believing the child she saw was her own. She was easily roused to answer questions, but then turned round and dozed again. On the 26th I heard she was going on favorably, had free action of the bowels, and took fluid nourishment without difficulty. On the 3rd of March all uncomfortable symptoms had left her, and she was rapidly getting well.

3. The next case shows the inapplicability of bleeding, and exhibits fully the injurious effects of depression of spirits, disappointment, and remorse.

I was summoned at 11 p.m. on the 4th of January, 1851, to the following remarkable and distressing case. The patient, æt. 23 years, was pregnant, and had taken lodgings for her confinement, which she was daily expecting. There was a good deal of mystery about her marriage, and it was doubtful whether or not she was married; at all events it was a secret, and she had been living with her mother until within a few days. She had been much depressed in spirits and full of grief the day before and this morning, in consequence of being disappointed of an interview which she expected. She was a fine, good-looking woman, in good general health, and respectably connected. After much fretting she suddenly threw herself upon a sofa at about 5 p.m., and could not be made to recognise anybody or to answer questions. She soon afterwards became convulsed, and breathed badly; but it was supposed to be an hysterical attack, and but little notice was taken of it. Then convulsions occurred again; she was twice very sick, and remained unconscious. At 9 p.m. her surgeon was called, and found her quite insensible, with stertorous breathing. Looking upon the case as a very serious one, he summoned me to his assistance at 11. She was then in profound apoplexy, feet cold, pulse small, unable to swallow, pupils dilated and fixed, and she could not be roused. A large mustard poultice was applied to the epigastric region, and an enema of castor oil and turpentine administered. The breathing now became more stertorous at intervals, and on removing the mustard plaster, we observed that contractions of the uterus corresponded with those intervals. Hoping, therefore, that although the apoplectic attack preceded the commencement of labour pains, it might depend somewhat upon uterine irritation, a few ounces of

blood were taken from the arm, and the uterus was examined *per vaginam*, when we discovered a moist relaxed state of the genitals, the cervix uteri almost obliterated, and the os just beginning to open. The head presented, and was forced down a little during the pains. After waiting some time, during which the breathing became more impeded and difficult, exciting the greatest apprehension about her life, we ascertained by the stethoscope that the child was alive, and determined to effect delivery as soon as possible—not only for the purpose of saving the child, but also of giving the mother the chance, bad as it was, of having the uterus emptied. With the concurrence of my colleague, I therefore gradually dilated the os uteri, introduced my hand, turned the child and effected delivery. The child was stillborn; but after attempting for three quarters of an hour to restore animation, respiration was at last established, and the child, a female, saved. No loss attended the delivery. The child was born at about 3.15 a.m. on the 5th. The mother continued to get worse, the stertor being loud, constant, irregular; and she had not been conscious since the commencement of the attack at 5 the previous evening. Soon the stertor was accompanied with a peculiar convulsive noise, which I have never heard but as the harbinger of death; but as her pulse was strong, we tried, as a last resource, a second bleeding, during which the pulse became freer, the hands and cheeks warmer, and the heart more vigorous; but no impression was made upon the sensorium, and no relief given to respiration; very soon after, about 4 o'clock, she ceased to breathe, the heart acting well and the pulse beating distinctly more than a quarter of an hour afterwards; then the beats became imperceptible, her face turned cold, and every sign of life disappeared. I regret very much that a post-mortem examination was not permitted.

Case 4.—A strong, healthy looking unmarried woman, 19 years of age, was confined at 6 o'clock on the morning of the 27th of May, 1852. Primipara. Labour natural, and not severe. Was attacked with convulsions at noon, which recurred at short intervals. Between the first two or three attacks she regained consciousness, but not afterwards, and the intervals became of shorter duration. I was called to her at midnight, and whilst there she had two severe convulsions in a quarter of an hour. I persuaded Mr. — to bleed her, for her pulse was strong, her countenance flushed, and her arms and hands red and hot. She was bled to a pint and a half, after which she breathed more easily, and had not another convulsion for an hour; she appeared also to be a little more conscious. She had two more convulsions between this and 4 in the morning, after which they ceased. I heard next day she was going on well, and she soon recovered. Before I saw this patient Mr. — had given two doses of opium, and expressed great

doubt as to the beneficial effects of bleeding in convulsions after labour. My own impression was that the bleeding had much to do with the recovery, considering the manifest signs of cerebral congestion and general plethora under which she laboured.

Case 5.—Mrs. —, æt. 24 years, a highly sensitive lady of refined mind and not very strong constitution, was taken in labour with her first child on the 21st of April, 1853. Breech presentation. Had been in good health during pregnancy until the last week or two, when she complained of headaches, but did not think it worth while to consult her medical man. Indeed she had scarcely had an interview with him until he was summoned to her in labour, from a dislike to enter into conversation with him relative to her pregnancy. She was over-sensitive about such matters, and of a nervous temperament; but was generally cheerful, and accustomed to substantial diet. In the evening a severe convulsion came on, with extreme congestion of the head, evidenced by blood escaping from the mouth and nose. She was then largely bled. After a while another fit came on, and she was bled again. Then another fit and a third bleeding; about this period her delivery was with some difficulty accomplished, which, it was hoped, would put an end to the convulsions. But she remained in a semi-conscious state, and after the placenta came away she became again convulsed; and this time leeches were applied to the temples, the hair was cut off, cold applied to the head, ten grains of calomel administered, and a turpentine enema. Notwithstanding all this she had another convulsion at three o'clock in the morning, became totally unconscious, had scarcely a pulse, countenance bloodless, and was apparently in a dying condition. Her bowels had been relieved by the injection. It was clear that no further depletion was admissible, and the only thing was to give her some mild nourishment, and await the result. Mustard poultices and warm flannels were applied to the feet and legs. Three hours passed without a convulsion, and hope revived; but at 7.30 she had another, though less severe; and, in despair, I was summoned by telegraph. I arrived at 2 p.m. on the 22nd, and found her sleeping; but on awaking, she noticed that I was a stranger, and her face became flushed. She then recognised her husband and her surgeon, answered me several questions, said she had no pain in her head or elsewhere, and could tell me what o'clock it was by my watch. Pulse 150, small; tongue bloody and very much bitten; had passed urine twice. We left the room as soon as possible, and she went to sleep again. There had been no convulsions since 7.30 a.m. I waited until 5.30 p.m., and still no convulsion; and when I took my leave her pulse was a little more restored and she was quite conscious. I advised but little interference, except giving nourishment; and, in case of a return of convulsion, an enema containing ʒj of laudanum in gruel. This

patient went on well, without any return of convulsion or unconsciousness, until Tuesday, the 26th, when she became hysterical on first being able to realise the loss of her child, which was stillborn. After the hysterical fit she became somewhat wild and soon quite maniacal; this continued several days, accompanied with great exhaustion and feeble pulse: on the morning of the 30th she became so alarmingly exhausted that I was again summoned, her surgeon fearing, however, that she would die before I could arrive. But at 3 p.m. I found she had taken milk with egg freely, and her pulse had rallied a little. She had been taking also Acet. Morph. gr. j in solution every two hours, and the bowels had been relieved. The lochial discharge had gone on the usual time, and had now ceased; but the nurse said there had been a disagreeable smell, and that the urine was offensive also. On my arrival her pulse was 150, and she was quite deranged; skin warm; face a little flushed; the pulse was strong enough if it had been less frequent; but during her fits of raving it became weaker, and during her short sleeps slower and stronger. Urine passed freely in the bed. Under the influence of the morphia she dozed occasionally, but always awoke as deranged as ever. I stayed with her all night, and by giving her an opiate enema and frequent doses of acetate of morphia to the extent of eleven grains during the night, kept her more quiet than she had been the two previous nights. She had several attacks of opisthotonos in the night; in the morning we gave an enema of gruel and castor oil, which brought away some fæcal matter and flatus; but the irritation of it seemed to increase the delirium, and she was exceedingly wild all the morning, with such an unfavorable expression of countenance that I feared there was but little chance of recovery. In the night I had combined $\frac{1}{8}$ grain Antim. Tart. with several of the doses of morphia, but no sickness nor nausea resulted from it; at about noon, however, she perspired very freely, without losing warmth in the body or limbs. At about 2 p.m., finding her still raving and refusing all nourishment, spitting it out of her mouth, and behaving with all the cunning of a maniac, I administered some chloroform on a piece of linen several times, and it seemed to quiet her; we then left the room, and she soon fell asleep for an hour and a half, during which time her pulse was of fair strength and 120. She awoke just before I was obliged to leave, shook hands with me, and showed me her tongue; but soon again looked wild, ceased to take notice of questions, but was not so vociferous as on former occasions. The chloroform had certainly improved her condition; I did not use enough to produce sleep, but she had sufficient to quiet the nervous system and allow the morphia to produce more effect. We agreed, in case of a return of the mania, to resort to it again in preference to increasing the quantity of morphia. Shortly after I left the patient again became very much excited. In the evening she had gr. j

doses of the morphia every hour, and about 1 o'clock she went to sleep and was perfectly quiet until 8 or 9 in the morning. Mr. — still continued to give her the drops, and when he saw her at 1.30 to-day (May 2nd), she was in a sound sleep and had slept an hour and a half earlier in the morning. Pulse varying from 140 to 120; on the 3rd, much the same. In the morning diarrhœa came on violently for a short time, but has ceased, and there has been little raving for many hours. Still under the influence of strong and oft-repeated doses of morphia. Early on the morning of the 4th the surgeon informed me of the attack of diarrhœa, but thought it had done no harm; he said she slept three hours in the night, but was as violent as ever in the morning. On the 7th he wrote as follows: "Mrs. — is certainly better; she is generally calm, and much more rational; sleeps about three hours each night, and dozes the rest of the night, with about the same period of rest during the day. Pulse continues very quick; bowels open once or twice daily; tongue dryish, but not much heat of skin. The right arm is very much inflamed from the wrist to the shoulder, I suppose from her being held when so very violent. I took her some flowers this morning; she was much pleased, said, "How lovely! how beautiful!" and told me all their names correctly. Complained of her arm, and asked for some lotion for it; fell asleep whilst I was sponging it. I left off the morphia in the large repeated doses on Tuesday, giving only one dose of it at night, and she takes a mixture containing Liq. Ammon. Acet., Tr. Hyos., and Mist. Camph. On the 16th I received a very favorable account from the husband, stating that her mental powers were perfectly restored, and that she was calm and cheerful. Her chief trouble now, he says, is a considerable eruption of the skin, and a great discharge (purulent) from her arms and shoulders, which have been lanced. I suppose the case altogether, even as far as you witnessed it, was one of a thousand in severity. After repeated formation and discharge of abscesses in various parts of the limbs, this patient ultimately recovered. She has had several children since without any casualty whatever.

Case 6.—Mrs. B—, of delicate constitution, was pregnant with her first child, and said to be within six weeks of her confinement; was attacked at 2 a.m. on the 22nd of August, 1854, with pain in the head and sickness, accompanied with a feeling as if she should lose her senses. A few hours afterwards she had a convulsion, and this having recurred several times, her surgeon was sent for. The symptoms continued with increased severity in spite of free relief from the bowels by enema, and I was summoned at 11 p.m. She was insensible, suffering from eclampsia, with no consciousness in the intervals, and the fits recurring frequently. Pulse rapid and small, pupils not dilated, stertor, frothing at the mouth, abdomen tense, cervix uteri gone, but os scarcely at all open. No power to

swallow. The convulsions were so severe and frequent that we thought it desirable to bring on labour, and I managed to insinuate my finger into the os and rupture the membranes, after which the abdomen became more flaccid, but the convulsions did not abate. I observed that the fits recurred every eight or ten minutes, and the stertor was frightful; and at the height of the convulsions the lips became swollen and purplish, though in the intervals the external signs of congestion were not extreme, and the conjunctivæ were not injected. She was bled to eight or ten ounces, and then made to inhale chloroform in small quantities, by placing it on lint under her nostrils. By these means the fits abated in frequency, and she had only two in an hour, with an interval of half an hour between them. I had several times attempted to dilate the uterus; and at about 3.40 in the morning of the 23rd, finding the os fully dilated and the child's head in the pelvis, I applied the vectis and quickly delivered her of a dead child; the placenta followed immediately, and the uterus firmly contracted to very small dimensions. Fits of convulsion, however, continued, with intervals of a quarter of an hour or twenty minutes; the pulse became more resisting, the skin warmer, and another cupful of blood was allowed to flow A blister had been applied to the nucha and turpentine to the sternum. After an hour I left her for a time, the fits recurring with as much severity as before, bloody froth issuing from her mouth. The pulse was of sufficient strength; there was no uterine loss; nothing could be given by the mouth. 10 a.m.—Still quite unconscious; respiration hurried and blowing, but *without stertor*. Pulse very rapid and indistinct; skin hot; a convulsive fit about every half-hour. 2 p.m.—Has never been conscious since I first visited her. Pulse 140, not very weak. Skin hot. Urine to be drawn off and examined. Enema terebinth. 9 p.m.—No convulsion since last visit. Injection not returned. Pulse 108; respiration quiet, less rapid, and without stertor or blowing. Still unconscious, but yet in a more favorable state than at last visit. Has been made to swallow a few spoonsful of fluid. Appears to feel pain from the blister when her head is moved, and the nurse says she has once pronounced a word. Has had two four-grain doses of calomel put into her mouth, and they were probably swallowed. For the first time I am inclined to think she may be restored to consciousness and rally from this fearful attack. All active interference to be avoided at present.

24th.—10 a.m. No convulsions during the night, and she has been able to swallow some gruel. Is writhing about with pain, apparently from uterine contraction, and there appears a gleam of returning consciousness. Pulse 108. Bowels fully relieved; motions dark and passed involuntarily. Urine contains a considerable quantity of albumen. To have ℥x Tr. Opii at once and

another dose in about two hours if required. 10.30 p.m.—Took only one dose of the opiate, and has slept comfortably since. Bowels still acting involuntarily. Pulse 84. Respiration natural. Uterus less rigidly contracted and lochia increased. She awoke during my visit and put her tongue out on my asking her to do so. To be kept perfectly quiet.

25th.—10 a.m. Found her awake, with her eyes open, face slightly flushed, with an expression of semi-consciousness; pulse 120. She had a copous relief from the bowels voluntarily; had undergone too much excitement from having seen several relations. Upon the whole she was better. 10 p.m.—Found her sitting up in bed, looking wildly about her. Pulse 120, and of less strength. Skin cool, no pain. Felt thirsty; we gave her some porter and an opiate, which she drank, taking the cup in her own hand.

26th.—Still only partially conscious, and has occasionally what may be termed a suppressed convulsion, when she says she must die. To take ℥x Liq. Cinchon. in milk three times a day.

29th.—Much the same except that she has longer lucid intervals. Has a wild demented look, and raved a little in the night. Bowels open. Pulse 108. Goes on with the porter and opiate and Liq. Cinchon.

30th.—More conscious to-day, but very artful. Pulse under 100. I need not go into any more detail; she gradually improved, both in mind and body, from day to day; and when I chanced to call upon her early in October she was in all respects quite recovered. She had no recollection, however, of the most serious part of her case, nor did she remember any of the circumstances attending the birth of her child.

Case 7 represents the value of bleeding for convulsions occurring some time before labour, the labour afterwards taking place without complication. One such case I have before related.

Mrs. H—, living in the country, of middle age, had convulsions at the eighth month of pregnancy, preceded by headache, but no perceptible uterine action going on. The convulsion was sufficiently severe to induce the surgeon to send for me to help him. On my arrival I found he had bled the patient twice, and the convulsions had ceased for about two hours. Next day she was going on well, not having had any return of convulsions. In about three weeksshe was confined and had a good labour without complication.

Case 8 was one of hysterical convulsions, but of considerable severity. A delicate young lady, under twenty years of age, married about a year since, was taken in labour with her first child on the evening of Saturday, Sept. 11th, 1858. Pains continued at intervals all night, and her surgeon was in attendance the whole of the next day, during which he frequently administered chloroform in

small quantities to allay extreme pain and irritability of the uterus, which made examinations distressing to her. The effect was beneficial, and all went on well until the evening, when she became restless and fatigued, with a pulse above 100, and pains insufficient to move the head, which had been in the same position for several hours, not impacted, but arrested by a narrow outlet, an only partially dilated perineum and diminished muscular power. I was summoned at about 6.30 on Sunday, the 12th, and consented to Mr. M.'s suggestion, that assistance should be rendered by the vectis. In a few pains I was enabled to deliver her of a living female infant without injury to mother or child, and we left her at night in a favorable state. She went on very well until about 4 p.m. on the 14th, when, owing to some little disappointment, she had an hysterical fit; this was quickly followed by another, and then she had several distinct puerperal convulsions. Pulse not more than 80. The abdomen seemed exquisitely tender on the slightest touch, but there was a plentiful supply of milk, healthy and free lochial discharge, and no fever. We ordered a warm-water enema, turpentine and laudanum stupes to the abdomen, and thirty drops of laudanum in water, to be repeated if necessary. On seeing her again at midnight she had had some comfortable sleep; the child had been put to the breast. Pulse quiet, skin warm. A little moaning, but no incoherence. Anodyne to be repeated if necessary.

15th.—Good night and quite sensible this morning. Had nursed the baby without inconvenience. Pulse 84. No relief from the bowels. To have an enema. At noon she was seized with another convulsion, but it soon passed off; in the evening she had another, but at night she was comfortable and collected. The bowels acted satisfactorily after the injection. From this time she went on well and recovered.

CASE 9.—In April, 1859, I attended a case of puerperal apoplexy with Mr. Master, of which I am sorry to say I have no record, except that it was a Mrs. Knight, forty-five years of age, primipara, that she was delivered with the vectis, and recovered.

CASE 10.—This was the case of a surgeon's wife, living near the sea, many miles from Norwich, who was attended by her husband and a medical friend of great experience and ability living within a short distance. The lady was twenty-three years of age, and had previously very good health. She had two children, and both labours were difficult on account of narrowness of the pelvis. She was confined with twins on Thursday morning, the 19th of September, 1861. She had been very unwell during the latter part of her pregnancy, the uterus being very much distended, and its sympathetic effects severe. The uterus was of a peculiar shape, being most enlarged longitudinally from cervix to fundus, and she suffered

very much from dropsy of the lower half of the body, the œdema being greater in amount than is usually the case simply from pressure. The urine was albuminous. In this labour the first child presented naturally, but the passage of the head through the pelvis was as usual difficult; hæmorrhage occurred afterwards sufficient to make it desirable to complete delivery as soon as possible; and the second child, which presented with a shoulder, and from the state of the cuticle had been dead some time, was delivered by turning. The placenta soon followed, and the uterus contracted quite firmly. There was no more hæmorrhage, nor had the loss been either severe or lasting. When the membranes belonging to the first child (living) ruptured, it was observed that the liq. amnii, which escaped in great quantity, had a very peculiar urinous smell. All went on well until the afternoon, when a frightful convulsion occurred, lasting some time, but giving way at last to mustard poultices to the feet, cold to the head, &c. She had also five grs. of calomel. She appeared to be going on well all Friday and Saturday; but on Sunday, without any apparent cause, convulsions came on again and continued almost without intermission for several hours, so as to leave scarcely any hope of recovery. I should mention that, after the first convulsion, although she seemed better in most respects, she found she could not see; and this symptom, as well as a certain degree of bewilderment, remained in some degree until the convulsions returned on Sunday. On Monday, the 23rd, I was summoned a distance of forty miles to see her, and remained with her from 2.30 to 6 p.m., during which time she had no convulsion, but looked wild, and was semi-unconscious, her sight being still imperfect but pupils not dilated. She kept moving about in her bed slowly and deliberately, slept but little, looked pale and ghastly, had a feeble pulse at 90, and turned cold on the least exposure. There had been no sickness nor any convulsion since the night before, neither had the bowels been relieved since taking ten grs. of calomel on Sunday. She ate and drank greedily whatever was put to her lips. Lochia scanty but healthy; uterus all right; complained of pain in her back. It was difficult to get her to answer questions, and she had almost a demented look. I thought her in great danger from exhaustion, and gave her a little brandy with milk, she having had no stimulant before. We then agreed to give her an enema of turpentine and castor oil, after which she had a good relief from the bowels, some of it being lumpy. She had passed water very freely the day before, and the œdema was almost gone. She was a good deal exhausted by the disturbance of having her bowels relieved and her linen changed, but her pulse rallied after more nourishment. We gave ℥xx. Tr. Opii in brandy and water, to be repeated at intervals if required. She had no convulsion up to the time of my leaving; and I hoped, if her system were nou-

rished, and her nervous system kept quiet by opiates, she might yet recover.

On the 26th I received a report to the effect that no more convulsions had occurred. "The first dose of laudanum had very little effect; the second, given in two hours, made her rather wild and confused for some time, after which she slept for half an hour. She had two more doses yesterday, and had some sleep, but not calm rest. I was afraid, last night, she would become maniacal, as she shrieked very violently, and was rambling and delirious. However, the violence has passed off to-day since 2 a.m." Next day she was better. On the 27th the husband writes—"Last night, at 7 p.m., she took ℥xlv Tr. Opii, and slept four or five hours. She appears to be quite herself to-day; takes food well, and talks very reasonably and sensibly." Next day the discharge was a little offensive, and puerperal mischief was apprehended. On the 4th of October Mr. R. writes—"You will be sorry to find that our case is not yet *well out of the wood.* Up to yesterday morning she appeared to be doing well, and yesterday ate two mutton chops for her dinner with keen relish; but in the evening she suddenly became very desponding, and then wildly maniacal, in which state she continued all night. When I saw her at 7 a.m. she was in a perfect state of religious frenzy—took me for the devil, and required two or three people to keep her in bed. Some opium and 4 grs. of morphia had been given in the night, but failed in procuring sleep or the slightest remission of the violent ravings. Finding things in this desperate state, I forthwith caused her to inhale a drachm of chloroform, and succeeded in sending her into a most tranquil sleep, and left her sleeping peacefully at 8.30 a.m."

Next day the report was as follows: "We gave her, last evening, from 7.30 p.m. to 3.30 a.m., four ℥xl doses Tr. Opii, when she got about three hours' sleep, and I then gave her ℥xl more. She has been sleeping a good deal to-day; when she awakes we force down beef tea, &c. She will take nothing voluntarily, as, it being a case of religious mania, she fancies it a great sin." On the 7th the husband writes—"Mrs. B. has been very quiet since the night before last, although she has not had very much sound sleep. We trust she is doing well, although she still has delusions. I am much obliged for your suggestions, but hope the stage of violence is past. To support her with good nutritious diet, and remove every cause of excitement, and attend to the state of the bowels, appears to be the course now to pursue; at the same time giving her a full opiate at bedtime, to get sleep, if possible. What do you think of this plan?" On the 11th he says—"Since I last wrote, Mrs. B. has been progressing very favorably. She now talks rationally, and I think I may say all her odd ideas have disappeared. She is more hearty than I have ever known her, and begins to look quite herself again. She

cannot, however, bear much talking. Thinking also fatigues her, and her memory is deficient." On the 21st I heard she was going on exceedingly well; had been downstairs the last two days, and was rapidly gaining strength. She had a complete recovery.

Case 11.—On the 15th of ——, 1863, I was summoned by Mr. Payne to a case of puerperal convulsions of some severity, coming on before labour. He bled her freely before I arrived, and the convulsions did not recur after the bleeding. Labour took place quickly and naturally in the course of the night, and she had a good recovery.

Case 12.—The following very serious case assumed the form principally of apoplexy, and was altogether so complicated and severe, that her surgeon said after her recovery that he should never run the risk of encountering the same difficulties by attending her again. The patient was a stout, fat, healthy woman, only 19 years of age, in labour with her first child. She had taken a hearty supper the night before, and awoke early in the morning with headache and vomiting, supposed to be the effect of her heavy evening meal; but she soon became convulsed and unconscious, and her surgeon was called to her at about 7 a.m. on Sunday, June 21st, 1863. From this time till my arrival at 3 p.m. the convulsions had returned with increased force and frequency, so as to leave scarcely a quarter of an hour's interval, attended with intense congestion of the face and head during the fits, and entire loss of consciousness even in the intervals. Her tongue was very much bitten; her pulse was full; no decided signs of labour had taken place, but there were occasionally indistinct uterine contractions. Mr. C— had opened a vein in the right arm, and applied leeches to the temples, but could get but very little blood. When I saw her at 3 p.m. she was still unconscious, having severe convulsions frequently; and I opened a vein in the other arm, which bled well, and we took away about four pints of blood. This lessened the appearance of congestion, and increased the intervals between the fits, but she remained quite insensible. I found the uterus contracted during the fits, and on examining ascertained that the os was sufficiently patulous to admit the finger, and the head was pressing upon it. To relieve uterine distension I ruptured the membranes, and after awhile I could dilate the uterus sufficiently to admit Mr. C.'s narrow and almost straight vectis; but it had not a sufficient curve to do any good with the head so high up. When my own vectis arrived I had dilated the uterus just enough to admit with some difficulty its wider blade, and I found I could get a very firm, steady purchase upon the head. But the natural efforts had ceased, and the os uteri presented a rim so firm and undilatable, that I could not get the head through it. The condition of the patient was becoming so unfavorable that there was no

time for delay; the stertor was dreadful, and we feared irreparable injury to the brain. We therefore determined to divide the hard rim of os uteri, which I did with a guarded bistoury, and then got the head lower into the pelvis. Still the resistance remained so great that we thought it right to lessen the head, as there was every reason to think the child was dead, and the mother seemed to be dying. So, without removing the vectis, I perforated the cranium, and after some difficulty got the child away. There was no hæmorrhage; the uterus contracted, but not sufficiently to expel the placenta, which I therefore removed. Even after delivery, a very difficult operation, occupying from first to last about four hours, she remained unconscious, but she had not a sinking pulse; the respiration was much less laboured than before the bleeding, and the outward signs of congestion were much less manifest. We tried to rouse her by applying hartshorn to the nostrils, and this she felt, because she turned her head from side to side to avoid it. I felt much inclined to draw a little more blood, but finally we determined to let her remain perfectly quiet, and see what nature would do for her. She remained unconscious all night and all the next day, and her surgeon thought there was no chance of recovery, but applied a blister to the nape of the neck. Two days after this she opened her eyes, looked about the room and talked wildly, giving her friends the impression she was dying; but it turned out to be instead the dawn of returning consciousness. Her tongue was so sore from being bitten during the convulsions that it was most difficult to give her any nourishment; but we got down a little broth and milk. The lochia flowed naturally; she passed water without difficulty, and there was no swelling, laceration, nor local inflammation in the vagina. Each day she improved; and on Sunday, July 5th, exactly a fortnight from the day of her confinement, I called upon her and found she had been downstairs for an hour in the morning, and, with the exception of a little faintness, was free from illness. The lochia ceased the day before; there was no secretion of milk; she had no recollection of her labour or that she had seen me before; her mind was still a little uncertain about some things she half remembered, but it was clear she had escaped from the great dangers she had so recently encountered, almost without either general or local injury to her future health and comfort.

CASE 13.—Mrs. T—, æt. 35, a clergyman's wife, living within a few miles of Norwich, was attacked on the 26th of August, 1865, with giddiness and difficulty of speech whilst at dinner, but managed to get upstairs with but little assistance; soon after which she was seized with a convulsion. She was in daily expectation of being confined with her eighth child, indeed had rather exceeded her expected time for delivery; and had undergone some unusual excite-

ment from the occurrence of two birthdays in her family a few days ago, as well as previously some mental depression, in consequence of the death of a relation. Soon she had a second convulsion, and Mr. G., who was in attendance, wished me to be summoned. I arrived at about 1.30 on the morning of Sunday, the 27th, and found she had had four attacks of convulsion, in the last of which the child was born. It appeared that once before, in her second confinement (the child being very large), she had a slight convulsion; and once or twice since then had had occasionally a little difficulty in giving expression to her thoughts; but generally she enjoyed good health, was clear and well informed, fond of music, lived well, &c. There was nothing peculiar with respect to the delivery, and after it Mr. G. gave her ℥xl Tr. Opii, ℥x more in two hours, and the same dose again at 7. I was in the house all night, but as there was no return of convulsion I did not see her, and left at about 6 a.m. At noon I was again sent for, in consequence of a return of convulsions. I arrived at 1.30, and in an hour she had another convulsion of an epileptic character, but not severe, and we gave a turpentine enema. This appeared to excite uterine contraction, and at 3 p.m. she had another convulsion of a more severe character, the uterus being very hard and contracted during the fit, but relaxing, though not dilating, as it went off. We then gave ℥xl Tr. Opii in starch by the bowel; neither injection returned; at 3.40 she had another fit, and another at 5.30, each accompanied with firm uterine contraction; we then applied fomentations of turpentine and opium to the abdomen. At 7 p.m. she had another convulsion, and soon after we drew off 1½ pint of urine by the catheter. At 8 p.m. we gave ʒss Tr. Opii, and half an hour afterwards she had a slight convulsion, followed by another slight one at 10.20, after which she slept. In the intervals she was sensible to a varying extent, and took some beef tea; her pulse during the whole time was steady at about 80, but not strong, even during a convulsion giving no intimation that bleeding was admissible or necessary. She had free lochial discharge, but not amounting to hæmorrhage, and as she was always accustomed to good living and more than an average amount of stimulant, we thought it right not to lower her by bleeding. She passed a good night; and at 6 a.m. on the 28th there was no return of convulsions. Catheter again used. Took ʒss Tr. Opii. At 11 no return of convulsions. Skin perspiring; pulse quiet; partially sensible, but dull, as if under the influence of the narcotic. She remained tolerably sensible all day, her bowels acted twice in the afternoon, and there was no return of convulsions until midnight. No opiate since early in the morning. At midnight she had another fit, and she had ʒss Tr. Opii as soon as it was over. At 1.45 a.m. on the 29th she had a slight convulsion, and the catheter was used. At 4.40 another fit. Enema opiat. At 6.30 another convulsion and enema repeated.

At 8 in the morning she had a very strong convulsion, in which her tongue was bitten. At 8.45 she took ♏xl Tr. Opii. At 9.10 another convulsion, and at 11 another, but she was sensible in the intervals, and took beef tea occasionally. I saw her at 11.30 a.m., and we repeated the opiate fomentations. I found the breasts were beginning to secrete milk, and uterine contractions still preceded and accompanied each convulsion. At 12.15 she had another convulsion, and as soon as it began I made her inhale chloroform, which seemed to cut it short, for there were no muscular contractions after it had been inhaled for a few seconds. I used about 2 drs., and gave it very cautiously; but it soon had considerable effect on the pulse, which became very feeble and extremely rapid. At 1.30 we gave her a glass of sherry, and at 2.15 a little champagne with ʒss Tr. Opii. At 3 p.m. champagne again, and at 4 some beef tea. At 5 more champagne, the pulse still remaining at 150, or more, the beats of the heart jarring the whole body, but not interfering with respiration. I should have mentioned that after giving chloroform by inhalation we applied some on lint over the contracted uterus, which in a few minutes caused considerable redness upon the surface as far as the lint extended, about 3 in. square. When I left in the evening there had been no return of convulsion, and she had not been sick. The pulse gradually lessened in frequency. At 6.10 she took beef tea. Pulse 140; skin moist; no convulsion, but dull of comprehension. At 7.40 she had ♏x Tr. Opii in champagne. At 8.40 beef tea. At 10 champagne; pulse 86. 12, beef tea, and slept a good deal in the night. On the 30th, at 1. 30 a.m., she had ♏x Tr. Opii with champagne, and took beef tea and champagne alternately every two hours during the night. At 5 pulse quiet, and no flushing of the face. At 8.45 she had 2 oz. champagne; pulse 80. 10, beef tea; bowels not open, but passed urine; lochia pale and offensive, but free. Wine and beef tea alternately every three hours. Vagina washed out with Condy's solution diluted. She had several comfortable naps during the day and night, and at 5 a.m, on the 31st., she had a rhubarb and potash draught. At my visit I found her with a quiet pulse, and she said she enjoyed what she took. Lochia more healthy. At 11 a.m. she had an enema, after which her bowels acted twice slightly. She saw her baby and husband for the first time and bore it well, but remembered nothing of the birth of her child.

Sept. 1st.—Child put to the breast, and no convulsion followed. Took another rhubarb draught at 7 a.m., and had an enema at 10.30, after which she had two plentiful reliefs. Pulse 84. Lochia healthy, more coloured. At about 4 p.m. she became rather excited, and the pulse rose to 120; but, after taking ♏xx Tr. Opii in champagne, it soon came down to 80, and she had a good night. Next morning (2nd) she took a grain of jalapine, followed by two good reliefs.

Under the idea that the wine excited her the day before, we substituted invalid porter. On Sunday, the 3rd, I found her going on well, with a quiet pulse (82), an almost healed tongue, and gradual restoration of intellect. Milk secreted. Baby nursed without inconvenience. She went on well till Friday, the 8th, when I was hastily summoned, late at night, on account of an attack of pleurisy, which had attacked her right side a few hours before. Turpentine had been applied, with partial relief; but the pain soon returned; respiration was so difficult that, shortly before I arrived, several leeches had been applied. I found very distinct pleuritic friction, and some small crepitation in the lung, and recommended more leeches. Several doses of calomel and opium had also been given, which we continued every four hours. When I saw her the next evening she breathed much better, and had scarcely any pain; but there was still very audible pleuritic rubbing, and I recommended a blister to the right side of the chest. Next day she was so much better that we discontinued the calomel and opium, especially as the bowels seemed to be a little irritable. On the 12th I saw her again. She was more cheerful, and began to realise things which she could not before remember. Enjoyed her food. Pulse 84. Bowels well relieved, and I thought her going on quite favourably. I should mention that, on Sunday, the 10th, she became irritable, feverish, and excited in her mind towards evening, and this state was followed by an eruption on the skin, from head to foot, and then the febrile action and excitement ceased. The eruption was in distinct spots, all over the body, similar to urticaria, but it was difficult to know what caused it. The weather was very hot, and she had, several times, profuse perspirations; could it have been of the nature of sudamina? At all events, it seemed to give her relief rather than otherwise, and was fading when I saw her on the 12th, although it had not quite disappeared. I had no occasion to see this patient again; she quite recovered, and has had children since.

Case 14.—Mrs. R—, æt. 28, wife of a soldier now occupying the office of superintendent of postal telegraph in this district. Has had five children before, and health generally pretty good. During the present pregnancy she has suffered much from sickness, debility, and lowness of spirits, and has had a presentiment that she should not recover from her confinement. She was taken in labour on the 16th of April, 1873, and confined at four o'clock in the morning on the 17th, after a natural labour. Soon after delivery she was seized with a very severe convulsion, attended with unconsciousness; and, scarcely without intermission, convulsions of a very severe character continued up to the time of my seeing her late in the evening of the 18th. I was away from home when first sent for, in the evening, and when I visited her, at about 10 p.m., I found two medical men who had been consulted (one of whom

attended her in her labour), had just left, supposing her to be in a hopeless condition, and not likely to live throughout the night. I found her quite unconscious, as she had been for many hours, and seized with convulsions, one after another, as rapidly as could be. Countenance very suffused; lungs filled with mucus, causing most laboured and noisy respiration; pulse rapid and small, skin perspiring freely; sphincters relaxed. She had scarcely been conscious for five minutes from the beginning, and my first impression was that she was actually in a dying state. Knowing, however, that in this disease even the most threatening symptoms are sometimes recovered from, I applied strong smelling salts to the nostrils, to see if I could rouse the brain at all, and thus relieve the congestion of the lungs, arising from loss of nervous influence; and I found that it did produce a little sensation, and a corresponding lessening of the stertor and abundant mucous râles. But convulsions occurred immediately, and I then sent for chloroform, believing that a diminution of such a profound stertor by stimulating the nostrils, rendered it probable that the stertor did not depend upon actual extravasation of blood in the brain, and that there might be a chance. Soon after this I made her inhale a little choloroform, and to my delight it evidently diminished the tendency to convulsion, and I left directions to have the inhalation repeated whenever a convulsion appeared to be coming on. On the following morning (19th) I found her still alive, but quite unconscious, and I could not satisfy myself whether or not she had hemiplegia of the right side. She had had only one convulsion in the night, and had a slight one during my visit this morning, which was stopped at once by a little chloroform. It was perfectly useless to attempt to give her either medicine or food, as she could not be made to swallow. The bowels had acted, and urine had been passed, both involuntarily in the bed, and I could not get any urine for analysis. On visiting her again, late in the evening, I found she had had no convulsion since ten in the morning, and her respiration was much freer and clearer. She was restless, moving her limbs about, showing they were not paralysed; I was enabled to get her to swallow some milk and wine and water, and although still unconscious, she seemed, by her actions, to enjoy the fluid as if thirsty.

On the morning of the 20th the pulse was 120, and the respiration was quite calm. Her countenance was improved, and she had spoken once or twice as if she recognised people in the room. Bowels freely relieved in the night, and plenty of urine passed, but both evacuations had taken place involuntarily. I should have mentioned that turpentine stupes were applied yesterday and continued to-day. She had no convulsion in the night, slept quietly, and swallowed fluids occasionally.

I visited her again on the morning of the 21st, and was told she

had a slight fit last night, stopped by chloroform. In other respects the same as yesterday. On the 22nd she was a little more conscious, and took more nourishment. Pulse 120. Respiration natural. No more convulsion. I paid her daily visits for a few days longer, during which time she gradually improved, and there was no return of convulsions. I can now report her as quite recovered.

I have thus performed the promise of relating the particulars of cases of puerperal convulsions, fourteen in number, occurring in my own consultation practice, of which only one proved fatal. A careful study of them may be useful both to young and old practitioners, and I think they exemplify all the principal phases of the disease as well as the proper modes of treatment. The results, at all events, have been satisfactory, and are encouraging to those who may be called upon to treat this serious and *alarming* disease.

III.—On the Morbid Histology of the Brain and Spinal Cord, as observed in the Insane.

By J. Batty Tuke, M.D., F.R.C.P.E., Visiting Physician, Saughton Hall Institution for the Insane; late Medical Superintendent of the Fife and Kinross District Asylum.

(*Continued from* p. 209.)

Part III.

The histological changes which have been observed specially affecting the grey matter of the hemispheres in my ninety-three autopsies have been—

1. A general degeneration of the four outer layers.
2. Granular and pigmentary degeneration of the nerve-cells.
3. Atrophy of nerve-cells.
4. Enlargement of nerve-cells.
5. Limited yellow softenings.
6. Limited atrophies.

In many cases of general paresis and of old-standing chronic mania the pia mater was found adherent to the brain substance to such a degree, that when the membrane was attempted to be stripped off by means of forceps the nerve tissue was torn away with it. Wherever this was the case the naked eye could, on section, detect a marked difference in the outer layers of the cortical substance—they were paler, softer, and separated from the subjacent layers by a white line. This lesion was best marked at the vertex, extending backwards to the occipital convolutions. When portions were submitted to the microscope in a fresh state a clear granular plasm was seen, with very few fibres (sometimes none), no cells or nuclei. Prepared sections showed a thickened condition of the pia mater, between which and the outer layer broken-up blood deposits existed in considerable quantities. This layer (taking the number of normal laminæ as seven) appeared indurated, thicker, and more fibrous-looking than in health, and was colourable by carmine. The three subjacent layers were represented by a clear, finely granular material, by which they were fused together, causing obliteration of their normal characteristics. The cells were absent; a few irregularly distributed fibres might here and there be seen; colouring matters were not absorbed; in fact, the normal appearances of the brain elements were entirely destroyed, with the exception of the larger capillaries, the coats of which were thickened, kinked, and covered with the various deposits already described.

The internal layers have not been observed to be affected by this lesion; on the contrary, where it existed in the outer laminæ, the cells of the inner were frequently very well defined, and even en-

larged. There was, however, no well-marked line of demarcation, the normal and affected tissues gradually merging into each other in the white line spoken of above.

In one case of cerebro-spinal meningitis, in which the inner table of the calvarium, the membranes, and the four outer layers of the grey matter were most severely affected, the latter, which to the naked eye presented a soft, greenish, pultaceous mass, in microscopic sections appeared as a clear, lymph-like, greenish stratum, in which was imbedded disintegrated nerve tissue, granular matter, compound corpuscles, and colloid bodies.

Granular and pigmentary degeneration of the nerve-cells has been constantly present in cases of senile insanity and of old-standing cases of dementia. In general paresis the granulations were more generally pigmented than in any other form of insanity, but it appeared to me that there was no difference, except in colour, between the merely granular and the deeply pigmented varieties of this degeneration. When the affected cell was examined in the recent state it was seen covered with a mass of fine, highly refracting material, which strongly suggested a fatty origin. The most careful application, however, of fat tests failed to produce any change in its condition or general appearance. The contour of the cell was blunted, the angles were ill-defined, and the poles were invisible. Most frequently the granulation was seen commencing at the periphery of the cell, more rarely in the neighbourhood of the nucleus and gradually extending outwards. Under either of these conditions the nucleus was the last part to become implicated; in fact, I believe that occasionally it was left free by the complete degeneration of the surrounding cell structure. In the extreme stage the cells were represented by mere masses of this fatty-looking material, retaining in some degree their original shape, but eventually breaking up into a confused *débris*. It is to this *débris* that is due the appearance of so-called free oil-globules in the brain. I have constantly noticed that specimens, the cells of which have undergone complete degeneration, present also scattered refracting bodies much resembling oil, but which, like the cell granulation, are unamenable to the proper tests. Such bodies are not to be found when the cells are tolerably healthy.

In prepared sections the fuscous or pigmentary degeneration was often well seen affecting the cells of the two inner layers of the convolutions of the superior surface of the brain and those of the corpora striata. In the latter position the whole cell was seen to be of a rusty brown colour, covered with fine molecules of a darker shade, its angles obtuse, and its poles destroyed; the nucleus was barely discernible. Although by no means confined to general paresis, fuscous degeneration of the cells has been found in its extreme condition much more freqently in its victims than in those of any of the

other varieties of insanity; indeed, it may be said that in no other form of disease has it been so completely and thoroughly pronounced, especially in the corpora striata. The cells of Purkinje have never been observed in my series of cases in any way affected by disease. In cases of chronic mania of long standing the large cells of the deep layers were not unfrequently noted as tinged more or less deeply by pigmentary deposit.

Atrophy of the cells was always well marked in the subjects of senile insanity, and in those who had for long been deeply demented. This condition of atrophy is, according to my experience, confined to the smaller cells of the outer layers of grey matter, the large cells of the subjacent layers being unaffected by it; on the contrary, often presenting themselves with peculiarly distinct definition. The atrophied cells, although well defined, were shrivelled in appearance. They were shrunk to a fourth of their natural size, surrounded by a clear, somewhat triangular, space, and they were readily and highly colourable by carmine. Their poles could generally be traced for a short distance, the nucleolus showed as a black spot, their position was normal, and they were not affected by any deposit.

The naked eye will frequently detect broad pale or white lines in the grey matter of the brains of the chronic insane, different in character to the natural variations of colour of the various layers. Microscopic sections of such specimens show a marked diminution of the normal number, occasionally complete absence, of cells in the bands, their position being marked by clear triangular spaces filled with a homogeneous plasm, the cells having become completely atrophied.

The large cells of the inner layers were occasionally found *enlarged*. This was described by Dr. Rutherford and myself in a paper read before the British Association in 1871. Very frequently, where the cells of the five outer layers were affected by degeneration, those of the inner layers were found dilated, puffed out, as it were, so as to render them rounder and larger than in health. The nucleus and nucleolus were well pronounced, and no evidence of disease further than the enlargement could be detected.

A marked difference in the behaviour of the cells of the medulla oblongata and spinal cord of various subjects has been observed on sections being submitted to colouring agents. In two cases of severe epilepsy the cells were most wonderfully demonstrated by the same process as has been adopted in all my sections. They appeared larger than in ordinary cases, their poles extended from them far beyond the field of the microscope, and the colouring matter was very readily absorbed. If the term were admissible, I should say that the cells presented evidences of hypertrophy. In no other cases have I succeeded in obtaining such brilliant demonstrations, and I believe that it is due to some structural and chemical change of the organs.

The microscopic appearances presented in vertical sections of the limited *yellow softenings*, which occurred in 10 per cent. of my subjects in the grey matter of the superior convolutions, were ragged fibres extending from the base of the ulcer (as it may be called), aggregations of colloid bodies, compound granular corpuscles, and blood deposits. These erosions had seldom worked deeper than the thickness of the grey matter. In one case a large number of these yellow softenings were found in the orbital convolutions of both hemispheres (besides many others in the upper surface of the brain), below and around the olfactory bulbs. Attention is drawn to this case, as the patient had been the subject for many months of persistent hallucinations of smell—everything had smelt badly, he had refused his food because it stank, and the air and water were alike disgusting. He died eventually from gangrene of the lungs. (*Vid.* 'Fife and Kinross Report for 1871-2.')

Local atrophies of the convolutions have been observed in 10 per cent. of my cases. These lesions have been chiefly found on the superior aspect of the brain. On section a simple absence of grey matter was found; sometimes a thin layer of indurated cortical substance could be traced, presenting no normal appearances, the subjacent white matter being unaffected.

The behaviour of *nervè fibre* in recent specimens varied considerably in different subjects; gentle pressure in one case causing no greater amount of ampullation than is normal, whilst in another the fibre was easily broken up or rendered monilliform; this occurred in œdematous subjects, and in those in which there was a tendency to *ramollisement*. Thin sections of the convolutions of the senile insane generally showed a tendency to thickening of the fibres, which appeared coarse and stringy, and were very amenable to carmine. Where this state existed there was also a diminution of the amount of neuroglia.

In three cases of epilepsy transverse sections of the medulla showed a decided increase of the white substance of Schwann. The *amyloid bodies* of Virchow were observed in numerous instances, especially in cases of epilepsy, general paresis, and chronic mania. In the first-named class of cases they were met with in immense numbers in the anterior median fissure of the medulla oblongata and on its periphery, bound down by the pia mater; they were also seen in its substance. In general paralytics amyloid bodies in considerable aggregations were observed below the ependyma of the ventricles, and in chronic maniacs on the surface of the superior convolutions, between it and the pia mater, and in the substance. Contrary to the observations of many, I have invariably failed to obtain any reaction by the application of iodine, nor can I feel certain of the existence of concentric rings.

Only one case of *tumour* occurred in this series of 93 autopsies. It was a small glioma in the centre of the centrum ovale.

General conclusions.—The blood-vessels are, without doubt, the structures of the encephalon most uniformly affected by disease. As far as my experience goes they never present themselves in the brains of the chronic insane in a perfectly healthy condition. Arguing from the fact that the morbid deposits above described are found modified only in degree on the cerebral vessels of subjects who have died in the delirium of fever, alcoholismus, insolation, and other rapidly induced forms of insanity, we are led to the belief that in at least the majority of insane cases the blood-vessels are the first structures to show evidences of morbid change. The word majority is used advisedly, for I believe classes of insanity exist in which the primary and efficient disease must be sought for in the nervous elements.

If the question of the position, nay, the existence, of cerebral lymphatics were settled, the morbific influence of modified blood-supply would assume a more important pathological position than at present can be ascribed to it. According to Eberle, His has abandoned his theory of perivascular lymph spaces; on the other hand, Obersteiner asserts their existence, and further believes in communications between them and the cells by means of "spur-like" processes. He figures a space around the cells which he has injected from the canal. If this observation were fully established it would afford a clue to the rationale of the induction of mania and other psychical symptoms, as a consequence of hyperæmia and congestion. Let us admit its accuracy for the sake of argument. We have a somewhat rigid canal, in which is contained a vessel and a lymphatic, the latter surrounding the former and in direct communication with the cells. It must be presumed that in health the vessel and the lymphatic are alike patent; when, from whatever cause, hyperæmia and congestion occurs, the dilatable vessel, being overcharged with blood, must press against the surrounding tube or canal, occluding it and thereby preventing the return to the lymphatic system of unassimilated and waste matters. At the same time the cells, stimulated to over-action by the increased blood-supply, cannot rid themselves of the products of over-action, as the orifices of their spur-like processes are occluded. In such a state of matters it is not difficult to understand that their functions must be modified or perverted—that they must be poisoned, as it were—by the presence of morbific matters, and that their delicate structures must be temporarily or permanently injured, according to the persistence of the congestion of the vessels. Atrophy and fuscous or granular degeneration would be readily explainable on this hypothesis. I have frequently met with clear spaces surrounding the affected cells in the brains of the chronic insane, but have no

reason to believe that they are in connection with the vascular system. As has been already said, further anatomical demonstration of Obersteiner's theory is only needed to give a clue to the causation of many psychical disturbances. It is unnecessary to again consider the objections to the supposition of the existence of perivascular lymph spaces, as they have been recounted in the earlier part of this paper; I would only add, on the one hand, that the system of demonstration by means of forcible injection possesses many elements of fallacy; on the other, many good observers think that argument by exclusion tends to support the theory.

I revert to the so-called hyaline-fibroid membrane, in order to discuss shortly its pathological and anatomical significance. The hyaline-fibroid membrane is held by certain eminent observers to be a peculiar characteristic material, the product of a specific morbid process—arterio-capillary fibrosis—of which the contracting, gouty, or cirrhotic form of Bright's disease is simply a concomitant. It is believed by others to have no actual existence. The former proposition cannot hold good in the face of the fact that its presence is constantly demonstrable in the subjects of general paresis, epileptic insanity, and chronic mania, in which no disease of the kidneys exists. Although it always accompanies an hypertrophied condition of the vascular coats, it is as frequently met with in cases where they are normal. As to the latter proposition, I will undertake to demonstrate its existence, without the use of chemical reagents, in the subjects of any of the above-named diseases, not only on the vessels of the pia mater, but, what is of equal importance, on the deeper vessels of the brain. In a series of preparations in my possession, made from the same subject, the thickened pia mater can be seen entering with the vessels, gradually losing its fibrous character as it reaches the deeper parts, until it shows itself as a simple hyaline membrane, surrounding them as a sheath; it can also be seen in various vascular tracts, from which the other coats of the vessels have been removed, as a very fine, somewhat puckered, translucent membrane, non-fibrous and non-fenestrated, filling up the tracts from end to end and from side to side. This series of preparations only confirms the statement of Rindfleisch, that, although the arteries of the brain are usually said to enter naked, it is inexcusable for any one familiar with the morbid anatomy of that organ to overlook the sheath of connective tissue, which, however slender, surrounds its arteries. ('Pathological Histology,' vol. ii, New Sydenham Society.) I reiterate the opinion that this "sheath of connective tissue" is merely the extension inwards of the tomentum cerebri, and that it must be reckoned as the anatomical sheath of the cerebral vessels. It is difficult of demonstration in health on account of its gossamer texture; but when we are assisted by the action of disease in this particular condition, as we have been in the

demonstration of other organs, the difficulty is overcome. The "hyaline-fibroid" membrane of the pia mater is simply its clouded and thickened portions investing and supporting its vessels, which from propinquity to the vessels are most liable to suffer in structure from the products of hyperæmia. Treatment with reagents brings these thickened investing portions more plainly into view by clearing up the general opacity of the superficial parts, and by endosmosis between the vessel and its membrane differentiating between them.

The various degenerations of the neuroglia must exercise a most important influence on the induction and maintenance of psychical aberration. The questions of its anatomical characters and of its nature being that of connective tissue or nervous matter may be said to be quite undetermined, as the opinions of many of our best observers are diametrically opposed to each other. If it be a mere packing material, its increase or diminution in quantity or quality must produce changes in the position and consistence of nerve-fibres and nerve-cells, and alterations in their electrical conditions; if it be a nerve-tissue, morbid processes in it must act directly. I may say that, judging from observations on its behaviour in various forms of disease, my opinion inclines towards regarding it as connective tissue.

In three cases, all of which resembled each other in duration, history and symptoms, colloid degeneration was found as the leading pathological appearance in fresh specimens and prepared sections. The early stages of these cases were characterised by sudden and deep dementia; there was only occasional and slight excitement, and no melancholy; the patients were at first merely apathetic, then became stupid, childish, fatuous and helpless, passing motions and urine where they sat or stood; the face became heavy, immobile and expressionless; speech was slow and hesitating; reflex action was rapidly impaired; a great degree of anæsthesia existed; in two hæmatoma auris appeared. They all grew fat and were utterly useless. All these symptoms developed themselves within three months, and death took place at eight, twelve and fourteen months respectively, in two from apoplexy on the superior surface of the brain, and in the third from gangrene of the lungs. These were all cases of that profound and rapid imbecility of mind and body which points directly to the diagnosis of "organic brain disease." On post-mortem examination the amount of colloid degeneration in the grey and white matter of all the convolutions of the superior surface of the brain was found to be very extensive; it was also well marked in the corpora striata, and patches of grey degeneration were found in the medulla oblongata. In the two cases which had terminated most rapidly, and in both of which hæmatoma auris had been observed, sections of the convolutions presented thoroughly the sago-pudding appearance described above. I believe, from these and other cases,

that colloid degeneration is a primary pathological condition of the brain, and that its leading symptoms are those recounted as existing in the three typical cases; that it occurs in other forms of insanity, but whether as a primary or secondary lesion it is as yet impossible to say; and that it is very closely allied to, if not identical with, the lesions of the spinal cord which have been demonstrated in tetanus, locomotor ataxia, and other forms of paralysis.

No data exist by which we can determine the period of incidence of degenerations of the ganglionic cells. Their primary affection is merely a matter of theory; that they become diseased in consequence of over-due or impaired nutrition, from toxic agency or from the atrophy of disuse, seems to stand on firmer ground. But even on this point our pathology is vague and hypothetical.

It must be remembered that as yet pathological brain-research has only extended to the subjects of chronic insanity, that it has been desultory, as might be expected from the difficulties of manipulation and its novelty, that the question is only now opening itself, that its real value has yet to be eliminated. Up to the present time the somatic nature of the diseases comprised under the generic term insanity has alone been demonstrated, the actual significance of the manifold lesions is as yet obscure. The sequence of their incidence is unknown, their localisation is imperfect. It will take many years of many lives to construct a true pathology of insanity, that term being used in its widest sense. Correlation of symptoms with morbid appearances from the earliest stages to the latest, of the observations of the physiological clinicist with those of the pathological histologist, must in time result in the elucidation of the materies morbi and the seats of the various forms of insanity.

The prospect is by no means unhopeful, even to the present generation of inquirers, when we reflect on the vast strides that have been made during the last few years in the acquirement of positive knowledge in the various departments of experimental physiology, experimental pathology, and minute anatomy. With the brilliant results of Ferrier's experiments before us, we may hope soon to arrive at an accurate knowledge of the localisation of cerebral function, and the establishment of a practical system of psychology, without which a practical system of cerebral pathology is impossible. When the centres of function are determined there will yet be left behind the question of the several actions of the several components. Here pathology may give a helping hand to physiology in determining the sequence of incidence of lesions. The delicate touch of disease may supersede the coarser hand of the scientific artificer.

Admitting fully the weakness of the existing basis of argument, I cannot refrain from predicting that the nosology of neuroses will eventually be established on the primary morbid conditions of the component structures of the centric organs.

Chronicle of Medical Science.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.,
Member of the Royal College of Physicians, Physician to the Bloomsbury Dispensary, London.

On the Nature of some Acrid Bodies. By Professor Buchheim.—Professor Buchheim has made some chemical investigations as to the nature of the acrid principles of many well-known irritating substances, and the results are very interesting, both in a physiological and a therapeutical point of view. From *Euphorbium* he has obtained, by a rather complicated process, a substance called *Euphorbon*—a kind of acrid resinous body which causes great irritation when applied on delicate parts of the skin, or on the mucous membrane. On further examination Dr. Buchheim found that this matter, when dissolved in spirit in the presence of potash, became converted into *Euphorbic acid*, and as this change appears to be effected only by the addition of the elements of water, he regards the *euphorbon* as the anhydride form of euphorbic acid. The latter substance is not readily formed in euphorbium, but appears to he produced by long keeping of the tincture of euphorbium. *Mezereon bark* also contains an acrid resin, which causes great irritation, but is likewise changed by the presence of a solution of potash into an acid, the *mezereous acid*, of which the resin is the anhydride form. The same fact is observed in the case of *podophyllin*, which yields an acrid resin, alike convertible into an acid, the *podophyllic acid*. *Elaterium jalap*, and *pulsatilla* (*Anemone pratensis*) exhibit the same peculiarities in their active principles, so that Dr. Buchheim arrives at the general conclusion that the active constituents of the above-mentioned substances are *anhydrides*, namely, substances which are in themselves neutral, but are converted by water into acids, which last possess very little activity or, at least, differ but slightly from that of the original substances; while the anhydrides, on the contrary, exhibit very intense action on the constituents of the body. From experiments made on other substances, Dr. Buchheim also arrives at some novel results, and in reference particularly to the active principles of *senna*, *rhubarb*, and *rhamnus*, he thinks that those principles are really complicated acids, which in the free state are only sparingly soluble in water, but with dilute mineral acids behave as glycosides, and with calcium and magnesium form compounds

very soluble in water, but insoluble in spirit of wine. The members of the *aloetic* series appear to be neutral bodies.—*Schmidt's Jahrbücher der Gesammten Medicin*, Jan. 20, 1863.

On the Arrest of Destruction of the Lung in Chronic Phthisis by the Inhalation of the Vapours of Oxygenated Essences. By Dr. Jules Chéron, Physician to the Hôpital St. Lazare, Paris.—In consequence of the great success obtained by the use of powdered camphor, which is the type of the oxygenated essences, in the treatment of hospital gangrene, Dr. Chéron employed many other similar essences in cases of phagedæna at St. Lazare, and published the results in 1871; and he has more recently employed the same agents with the view of promoting cicatrization of the lung cavities in chronic phthisis. In order to ascertain whether the vapours of the oxygenated essences possessed the same properties as the powders, Dr. Chéron caused a current of air charged with these vapours to be directed on some obstinate ulcerations, and he found that the ulcerative process was arrested and cicatrization effected. He has since made numerous observations on hospital patients during a period of eighteen months, and he summarises his results in the following manner:—The vapours of the oxygenized essences possess, in common with the same substances in the form of powder, the property of arresting phagedenic ulcerations and promoting cicatrization, although the same effect is not produced by the non-oxygenated essences, such as turpentine. Pulmonary cavities in cases of phthisis, when treated by the inhalation of these vapours, cicatrize in a great number of cases, and in a relatively short time. All the oxygenated essences may be employed with a chance of success, and Dr. Chéron has used the oxygenated essences of Japan camphor, of camomile, of cedar, and of eucalyptus, but the preference must be given to the oxygenated essence of the *Laurus camphora* and that of the cedar, the agreeable smell of which latter causes it to be well borne by the patients. The plan is most successful in the chronic forms of phthisis, and it effects an improvement in the expectoration, the dyspnœa, and the cough; the appetite returns, the strength is restored, the hectic fever is diminished, and the weight of the patient is increased; and Dr. Chéron declares that in a great number of cases he has found all the symptoms disappear, and the health completely re-established. As the free vapours of the oxygenated essences have too weak a tension when only diffused in the atmosphere of an apartment, Dr. Chéron has devised an inhaling apparatus which he figures and describes.—*Gazette Hebdomadaire*, December 30, 1872.

On the Use of Carbazotate of Ammonia (Picrate of Ammonia) as a Substitute for Sulphate of Quinine. By Dr. Dujardin-Beaumetz, of Paris.—Dr. Dujardin-Beaumetz has previously published a memoir on the use of picrate of ammonia in the treatment of intermittent fever, and his subsequent researches have partially confirmed his previous views. The picrate has been long employed in medicine, having been used in France, in 1830, as a febrifuge by Braconnot, and subsequently both in France and Africa. The salt has

a red colour, is perfectly crystallized, not at all explosive, and when heated in the air burns slowly, like a resin, with a very smoky flame. Dr. Dujardin-Beaumetz has used it in the form of pills, each containing one centigramme. In his former paper, the author determined that the carbazotate of ammonia was very efficacious in intermittent fever; that the suppression of the paroxysms might be obtained by the employment of two to four centigrammes in twenty-four hours; that in this dose the drug never produced any bad effect, and appeared better tolerated than the sulphate of quinia; that it was very similar in its physiological action to the last-named salt, and that it might be used as a substitute for it in a great number of cases. These conclusions have not been invalidated by the later researches of Dr. Dujardin-Beaumetz, and a fresh series of observations have been made on patients, chiefly at the Hospital of La Pitié, and seven cases are recorded in which the effects were watched. In four of them the carbozotate alone effected a cure of the intermittent fever, but, nevertheless, it cannot be said to be entirely curative, for in one case, which was at first cured, there was a return of the symptoms, for which sulphate of quinia was eventually and successfully administered. In the three other cases the carbazotate was ineffectual. The author, however, does not think that the employment of the salt should therefore be discontinued, for the results only show that it is inferior in its anti-periodic properties to the sulphate of quinia. He believes it is proved that the carbazotate has an evident and manifest action in the treatment of paroxysmal fevers, and that its value should be further tested, more especially when the price of sulphate of quinia is compared with that of the carbazotate of ammonia.—*Bulletin Général de Thérapeutique*, Nov. 15, 1872.

On Hyoscyamia and its Action in some Spasmodic and Convulsive Diseases. By Dr. Oulmont, of the Hôtel Dieu, Paris.—In a memoir read at the Académie de Médecine, Dr. Oulmont has communicated his researches on the properties and effects of henbane and its alkaloid (hyoscyamia), and the result has been that, although he denies the utility of this drug in a certain number of cases, he has obtained satisfactory effects in the treatment of some nervous affections. He has not employed henbane itself, because the drug is so variable in its qualities, owing to a number of accidental circumstances, that it cannot be relied upon for any uniform action, but he has always used the alkaloid, which has a fixed composition, and represents all the active principles of the plant. Hyoscyamia was discovered in 1820, by Brandes, and a formula for its preparation was given in 1833, by Geiger and Hesse. When dry it has no smell, but in the moist state and when pure it has a disagreeable odour, something like that of tobacco. It has been administered to patients in doses of one milligramme ($\frac{1}{1000}$ of a gramme, a gramme being about 15 grains), and in France it is found in commerce as a brownish liquid, of a syrupy consistence, and called the *hyoscyamine of Merck*. It has also been used in hypodermic injections in the form of watery solutions.

Dr. Oulmont, before instituting any clinical researches, made experiments on the lower animals, and he found that hyoscyamia diminished the capillary circulation, when given in a small dose, and paralysed it in a large one, and that it had no action on the nervous system of the life of relation, or, in other words, that it did not act directly on the sensory nerves. On the human subject Dr. Oulmont has employed hyoscyamia chiefly in various kinds of tremor (as mercurial tremor), in locomotor ataxy, and in tetanus. The general results are, that hyoscyamia may be given in gradually increasing doses, from 2 milligrammes to 10 or 12 milligrammes a day, and may be continued even if a few symptoms of poisoning supervene, but if these symptoms become serious, the use of the alkaloid must be suspended. In man, hyoscyamia acts as a narcotic, relieving pain and neuralgia in particular, but it is inferior in this respect to opium and belladonna. It is very efficacious in mercurial tremor, in senile trembling, and in paralysis agitans; it has no influence over locomotor ataxy, but appears to be somewhat beneficial in traumatic tetanus.—*Bulletin Général de Thérapeutique*, December 15, 1872.

On the Use of Mustard in Leaves, and its Mode of Employment. By Dr. Astier.—This new form of sinapism is distinguished by the certainty of its operation and the convenience of its employment, and its action is rapid as well as certain. But the mustard of commerce is often adulterated either with the *Sinapis arvensis*, or with colza or linseed, and moreover it is often impaired by long keeping. The mustard powder employed by M. Rigollot for the preparation of his mustard leaves is made from the mustard flour of Alsace, partly freed from its integument and its fixed oil, which latter, although quite inert as to its sinapising properties, may cause a spontaneous alteration in the powder, and thus diminish its activity. The mustard leaves supersede the necessity of employing mustard itself for sinapisms, and they thus obviate the necessity of guarding against adulterations in that substance when used for medical purposes. But it is found that water above the temperature of 65° C., and also alcohol at any temperature, coagulate the *myrosine*, one of the constituents of mustard, and thus prevent the formation of the rubefacient volatile oil, and it was, therefore, desirable to find for the solvent of the mustard a liquid which contains neither water nor alcohol, nor any fatty or resinous matter. Water would have developed the volatile oil, alcohol would have coagulated the myrosine, and fatty or resinous bodies would have prevented the mustard from being impregnated with water at the moment of application. Only one substance has succeeded as a solvent, namely, caoutchouc dissolved in sulphide of carbon or a volatile oil. After the operation of solution the solvent evaporates and leaves the powder entangled in the imperceptible meshes of a network of fibres of caoutchouc, adhering to the paper and permeable to water, like the warp of a tissue. For children and delicate females M. Rigollot has invented some mild leaves (*feuilles dulcifiées*), formed of a mixture of black and white mustard. This mild revulsive acts slowly and gradually, and requires twenty minutes

to produce its *maximum* effect.—*Le Mouvement Médical*, November 30, 1872.

On the Use of Strychnia in Amaurosis. By Dr. J. J. Chisolm, Professor of Ophthalmic and Aural Surgery in the University of Maryland.— In this communication Dr. Chisolm records a case which not only shows the beneficial effects of strychnia in the treatment of amaurosis, but exhibits a remarkable tolerance, on the part of the patient, of this potent alkaloid. The case was that of a man, aged 40, who had been under medical treatment for four years for progressive optic nerve atrophy, both eyes being equally defective. It appears that he had been in the habit of indulging freely in tobacco and whiskey, but although he afterwards abstained from both, and entered upon a long course of medical treatment, he lost rather than gained ground. When he came under Dr. Chisolm's care, it was found by ophthalmoscopic examination that the retina was apparently healthy, but the optic nerve-discs were white. Although the patient stated that he had already tried strychnia, and had derived no benefit from its use, Dr. Chisolm determined to persevere in it, and bring about its stimulating effects without delay. He began with one-thirtieth of a grain of the sulphate of strychnia, but this dose was only the minimum, and as it was well borne, it was increased by small additions from day to day, until the largest amount was reached which the system would tolerate. The successive increase was $\frac{1}{24}$, $\frac{1}{20}$, $\frac{1}{15}$, $\frac{1}{10}$, $\frac{1}{8}$, $\frac{1}{7}$, $\frac{1}{6}$, and $\frac{1}{5}$ of a grain, and the interval of time required by the system to tolerate the largest dose, from the commencement of the treatment, covered the space of two weeks. These doses were given three times a day until one tenth of a grain was reached. The constitutional effects of excitement of the nerve-centres and stiffening of the leg and spine muscles were kept under control by modifying the dose, but a marked improvement in vision was manifested even within two weeks. The patient returned to his distant home, but was instructed to keep up the use of the remedy in full doses. An appreciable stimulation was daily experienced from one half of a grain a day, continued for over five weeks, and the improvement in vision, although slow, was steady. The toleration of the alkaloid was now, however, so well established that the constitutional effects were no longer produced, and the improvement in vision seemed to be arrested, but the dose was again increased with the results of renewing the constitutional symptoms and further improving the sight. Eventually the vision was permanently improved, and in general health the patient was stronger and more vigorous than he had been for many years.—*New York Medical Journal*, February, 1873.

On the Antiphlogistic Treatment of Disease. By Dr. E. Bouchut.—Dr. Bouchut, so well known as the Physician to the Hôpital des Enfants Malades in Paris, and who writes from long experience, remarks, in commencing this paper, that too many physicians confound what is new with what is good, and he says that of some

recent methods of treatment what is new is not good, and what is good is not new. For some years, he goes on to observe, the antiphlogistic treatment has undergone some important modifications, leading to the abandonment of measures of great importance in favour of others which are of no value. Tartar emetic and bleeding, which, with opium, were considered by Hufeland as the bases of all therapeutics, are scarcely employed at present, the first from the fear of symptoms which are only imaginary, and the second because the existing school of practitioners do not know how to use the lancet. Dr. Bouchut says that there are now young physicians who, after five or six years' study, have never bled or seen any one bled. In recent works on the antiphlogistic treatment, emetics and bleeding are set aside in favour of digitalis, veratria, quinia, repeated cold baths, &c., and Dr. Bouchut asks whether the change is good or bad? He then speaks in favour of bleeding and leeches, the antipyretic effect of which is proved by their lowering the temperature from one to three degrees in a few hours, and diminishing the frequency of the pulse. He asserts that when they are employed at the commencement of some acute inflammations, they arrest the development of the disease—they, as it were, strangle it (*on jugule*), and prevent the formation of interstitial exudations, or promote their absorption. No other method of treatment, he affirms, produces similar results, and, supposing the malady to be unattended with putrid or septic phenomena, no remedy is so efficacious. Tartar emetic produces a similar effect, both by the gastric and intestinal evacuations it produces, and by the well-known contra-stimulant action it possesses. Like bleeding, it modifies the febrile temperature, and this is its most obvious action. The temperature falls from two to three degrees, and nothing is more certain and rapid than this antipyretic action. It is given in small and repeated doses as a contra-stimulant, and then its action is less considerable than when given in doses to cause vomiting, but in either case its effects are indisputable. When given so as to produce nausea and vomiting, it has a considerable effect on the pulse, but its action is different to that of bleeding. The arterial tension is increased, and the arteries are contracted so as to embarrass the peripheric circulation, which produces paleness and even tegumentary cyanosis.

As it is thus shown that tartar emetic and bleeding are powerful and rapid antiphlogistics, what remedies are to be compared to them in the present day, and what substitutes are to be employed in their place? Digitalis, veratria, quinia, bryony, and cold baths administered to fever patients are the antiphlogistic measures now recommended, and Dr. Bouchut admits that they are not altogether to be despised, but that, when a rapid action is required to prevent the development of an inflammation, they are insufficient. They cannot, he thinks, be employed as general and systematic remedies, and can only be serviceable in carrying out special indications. They are notably antipyretic, they abate fever, and lower the temperature from one to three degrees. In this respect their action is rapid enough, but they do not so quickly diminish the arterial tension, and

do not reduce the pulse till the end of from two to three days: They never produce the antiphlogistic effect of tartar emetic or bleeding. They have never been known to *strangle* a disease, they can only pretend to impede the progress of an inflammation. Dr. Bouchut's own experience has shown him that their inferiority as abortives is real, and he has never been able to obtain, by their aid, the same success at the beginning of pneumonia, rheumatism, or typhoid fever, as with tartar emetic or bleeding. With digitalis and its preparations administered in typhoid fever, acute pneumonia, and articular rheumatism, in suitable and not poisonous doses, it requires two or three days to lower the frequency of the pulse in a notable manner, and to diminish the febrile heat. The sulphate of quinia and veratria, both of which exert such a tranquillising action on the velocity of the pulse, have, nevertheless, no immediate action on fever when they are employed in non-poisonous doses, and they require at least two days to make their influence perceptible. On the other hand, their antiphlogistic effect is, in some diseases, very considerable, and in acute articular rheumatism, without cardiac complication, they are, according to Dr. Bouchut, true specifics. As to cold baths of an hour's duration, repeated several times a day, as advised by Liebermeister in typhoid fever, Dr. Bouchut entirely disapproves the plan, which appears to him to be madness, and he thinks the same of the method of cold immersion adopted by Jürgensen, of Kiel, in pneumonia. "Heaven protect me," he says, "from going to Kiel and having an attack of pneumonia sprinkled with cold water!" Cold, he goes on to observe, is a well-known antipyretic agent, employed in France for the last twenty years. Cold lotions on the body of a fever patient in his bed, or a rapid affusion, are daily employed in certain eruptive and other fevers, and there are cases where the febrile heat of the skin is so intense that this plan alone can save the lives of the patients, but he strongly objects to plunging cases of adynamic fever in a cold bath several times a day. The experiments of this kind which he has seen appear to him to be inhuman, a patient being taken three times a day out of his bed by the help of the attendants, plunged into the water, held there, and then carried back after being wiped; all this, he says, causes shocks and painful sufferings, which render this mode of treatment almost impracticable on a large scale. He admits that if success justifies the treatment, it ought to be adopted, notwithstanding its cruelty; but he maintains that it is not superior in efficacy to the use of cold lotions employed while the patient is in bed. Cold, therefore, is not to be used, according to Dr. Bouchut, as an antiphlogistic remedy, but only to diminish fever and to lower the temperature one to three degrees; the effect is only temporary, as is known, but as soon as the thermometer rises again another washing causes it again to fall, and thus the feverish condition is successfully opposed. Dr. Bouchut, in concluding his paper, observes that it is quite justifiable to add the antiphlogistic and antipyretic measures to which he refers, to those which have been long in use, but they ought not to cause physicians to forget the old plans of treat-

ment, the advantages of which are indisputable.—*Bulletin Général de Thérapeutique*, April 15, 1873.

On Method in Therapeutics. By Professor Hirtz.—Professor Hirtz observes that in the present epoch of analysis and demonstration it is especially necessary to define the method which ought to preside over therapeutical experiment, and also the direction it ought to follow, the results to which it should tend, and the criterion which ought to judge those results. The Professor formulates his opinions in certain propositions, of which the following is an abstract. He begins by remarking that disease ought not to be considered either as an independent being or as a concrete body, but as a physiological act which has deviated from its normal type. By the light of the erroneous views thus condemned, a disease is still regarded as a whole, and treatment is directed against it, while in reality one or other of its elements only is attacked. So again among the anatomo-physiologists of the beginning of the present century, the idea of *lesion* prevailed, such lesion being, in fact, only the product of the disease, and not its essence. For instance, is pneumonia or typhoid fever attacked as a whole by the treatment adopted? Not at all, says Professor Hirtz, but the remedial measures are directed either to the temperature, or the fever, or the vascular tension, or the state of the patient's strength, &c. Then again, he continues, the value of a remedy must be determined not from the disease considered as a whole, nor from the bare and accidental fact of cure or failure, but from the physiological action of the medicine on certain organs and functions, or from its clinical influence on certain morbid actions. A disease spontaneously curable may disappear without, or in spite of, treatment, and disease necessarily fatal may defy the most heroic remedy; thus, a hundred cases of small-pox will recover under the use of gum-water or liquorice, and ten cases of pneumonia in a state of suppuration will sink, although treated by digitalis or any other powerful antipyretic drug. The remedy deserves no credit in the one case supposed, and no blame in the other. Professor Hirtz then goes on to argue that to form a scientific system of therapeutics it is necessary to know thoroughly the physiology of the disease, and also the physiology of the medicine, in order to control the one by the other; but he admits that here the problem is very difficult, for it is necessary to deduce the physiology of the disease from all the symptoms, and among them to distinguish those which are primitive and hold under their control the accessory or secondary phenomena. — *Bulletin Général de Thérapeutique*, March 30, 1873.

On the Treatment of Diphtheria in Italy.—In a Report on this subject presented to the *Società Medico-Fisica* of Italy, it is admitted that there is no specific for diphtheria as a constitutional affection, but the local treatment of the affected parts is generally practised, and is, to a certain extent, successful. Those Italian physicians, who regard diphtheria as due to the presence of parasitic organisms,

rely upon remedies which have the reputation of being parasiticide, but all are agreed that topical applications to the throat in the early stages of the disease are very valuable, and should never be neglected. Various remedies of this kind, viz. caustics, astringents, and solvents, have been recommended; among the first are hydrochloric acid and nitrate of silver, among the astringents are alum and tannic acid, and among the solvents may be reckoned lactic acid, chlorate of potash, acetic acid, or even common vineger, gastric juice, and lime-water. From a consideration of all the facts communicated, and after a long discussion as to the value of different remedies, the Society came to a unanimous resolution to condemn the abstraction of blood in any case ; to admit the utility of local treatment with the object of modifying the false membrane or promoting its removal, and to sanction the use of remedies for improving the condition of the diseased surfaces, but without admitting the necessity of cauterizing the parts, a plan which seems to have failed in the hands of most practitioners.

After all remedies, both local and general, have failed, the grave question then arises whether an artificial opening should not be made for the admission of air into the windpipe, or in other words, what is the evidence in favour of tracheotomy in laryngo-tracheal diphtheria? And here again great differences of opinion exist. The Italian Report traces the history of this operation from its suggestion by Asclepiades to its actual performance in London in the year 1772, by John Andrée, and to its revival by Bretonneau and Trousseau in more recent times. The results of the operation as performed in our own day and in England are, as is well known, somewhat unsatisfactory, but in France and Germany the proceeding has been to a certain extent successful. Trousseau states that he has performed the operation more than 200 times, with more than fifty recoveries, but in other hands the success has not been so great. The Società Medico-Fisica, having taken the whole subject into consideration, have resolved that tracheotomy should not altogether be condemned, and have arrived at the following conclusion by a majority, namely, that "while admitting the necessity of the operation in cases of primitive laryngeal croup, it should not be performed in cases which are consecutive to pharyngeal diphtheria, except when the symptoms of laryngeal obstruction immediately threaten life, and there are no well-marked indications of general infection."—*Studi sulla Difterite. Rapporto del Dottor Faralli.*

REPORT ON SURGERY.

By Henry A. Reeves, F.R.C.S.E.,
Assistant-Surgeon to the London Hospital, and Surgeon to the East London Hospital for Children and Dispensary for Women.

Surgery of Children.

On Retro-pharyngeal Abscess in Infants.—By Professor H. Abelin, of Stockholm ('Nord. Med. Ark.,' III, 4, No. 24; 'Schmidt's Jahrbücher,' Band 154.—W. Berger gives an abstract in 'Schmidt's Jahrb.' of Professor Abelin's paper on this subject.

To those interested in children the professor's remarks will be instructive as well as important; we, therefore, make no apology for condensing the paper. Abelin says that retro-pharyngeal abscesses occur not rarely in earliest infancy, but they are often overlooked. Medical literature is poor in records of cases in the old hand-books; they are either not at all or only briefly touched on, and yet these abscesses deserve attention so much the more as a favorable issue depends mainly on an early diagnosis and treatment.

Of the two forms of this disease, viz. that due to inflammation and necrosis of the vertebræ and their cartilaginous or ligamentous apparatus, or that caused by inflammation of the cellular tissue between the posterior wall of the pharynx and the vertebral column, it is the latter which occurs in children under varying forms as regards size and extension. It is either limited to a small circumference at the posterior pharyngeal wall, or it spreads downwards along and on both sides of the œsophagus. It sometimes breaks through externally and with adenitis is secondary to the acute exanthemata, especially to scarlet fever. Abelin is undecided as to whether the disease affects scrofulous children by preference, as it might be difficult to determine during the first few months whether the child be scrofulous, as this disease commonly shows itself in a definite form at a later period. The symptoms vary in intensity according to the acuteness or chronicity of the affection. The phenomena which more particularly accompany the malady are, dysphagia, dyspnœa, hiccups, whistling inspiration, and pain on moving the head, and these vary in different cases. The diagnosis is not easy at the early stages of the disorder, and it is only when the abscess, by its increasing size, causes the symptoms above enumerated, and when fluctuation can be felt that one becomes certainly aware of the nature of the affection.

With regard to spontaneous rupture of the abscess, Abelin believes such a result to be extremely rare. It is frequently mistaken for croup, laryngitis, œdema of the glottis, and other laryngeal affections.

The prognosis is favorable if the disease be detected early and appropriate treatment adopted, but if left to itself death is unavoidable, unless spontaneous evacuation occurs. Even in the latter case death by suffocation may ensue by the abscess bursting into the

air-passages. Secondary pulmonary lesions sometimes complicate the cases and serve to mask the diagnosis. In opening the abscess it is necessary to do so carefully, in order to prevent the pus passing into the larynx. Abelin had a death from this cause. A trocar and canula should be rapidly plunged into it with the head held forward.

Abelin communicates the following three cases; the first was observed by Dr. Wettergren, and is in many respects interesting:—A girl, when only a few weeks old, showed a marked peculiarity of deglutition; there was a great flow of saliva, stiffness of the neck and head, and during the first three months the child was constantly restless and screaming. Coughing and diarrhœa supervened after four months. The voice and cough were hoarse, and the inspiration, especially during sleep, was whistling. The exploration of the pharynx and the examination of the chest gave no results. The child became gradually more feverish and restless, and suddenly died, about four and a half months after the first appearance of the disease. At the autopsy an abscess was found extending from the second cervical to the fifth dorsal vertebra, and containing healthy pus. The walls were pretty firm, and were bounded in front and on the sides by the pre-vertebral fascia, and behind by the anterior longitudinal ligament. The vertebræ presented nothing morbid either in the periosteum, bones, or ligaments; neither in the cavity of the abscess nor in the walls of the same were any elements of perished glands met with. The pharynx and the œsophagus presented a perfectly healthy aspect. The lungs were œdematous, emphysematous, and evidences of acute catarrh were found in them as well as in the bowels.

The second case occurred in the practice of Dr. Leverton, who consulted Professor Abelin. The subject of it was a girl, æt. 2. The symptoms were somewhat similar to those of the last case, but differed in the fact that there was otorrhoa, and a small glandular tumour was found at the angle of the jaw, and on examining the pharynx another tumour was found at the corresponding region. The tumour extended on the right side from the ear downwards, beneath the lower jaw. On examining by the mouth Abelin felt with difficulty a tumour to the right of the median line, which fluctuated slightly. As the external tumour showed this fluctuation plainly he introduced a bistoury, and gave vent to a considerable amount of pus, and the dysphagia and dyspnœa diminished.

The improvement which followed the discharge of pus from the external tumour did not continue; the symptoms became aggravated, and suffocation was threatened, and as fluctuation was now perceptible in the internal tumour, a trocar was thrust into it with considerable relief, but towards evening they again returned, and were only relieved by a fit of coughing which occurred, and caused a free gush of pus; the child was quite well in a few months.

As opposed to the chronic course with remissions, as mentioned in the first case, there is shown in the present case the increase of the symptoms in direct proportion to the increase of the tumour, the violence of the phenomena, and the facility of diagnosis. But that

still greater differences in relation to the course and symptoms in apparently similar pathologico-anatomical conditions may occur is proved by the following two cases, which Dr. Abelin had previously communicated to the 'Nord. Med. Ark.,' No. 9, p. 24, 1870.

A child, three months old, became affected on 18th November, 1866, with catarrhal phenomena, which constantly increased. On the 27th the neck appeared tumefied on the left side, immediately beneath the jaw. There appeared at the same time a peculiar, suffocating cough; respiration was very difficult; the face appeared cyanotic; frothy saliva accumulated on lips; and the child appeared very restless and anxious. Physical examination of lungs showed nothing morbid. A fluctuating tumour could not be proved by examination of pharynx, but this examination could only be effected with difficulty, as it produced suffocating paroxysms. The next day there was a slight improvement; on 29th December (probably it means November) the child died of asphyxia. The treatment consisted only of application of warm poultices and warm inhalations. Although the examination during life gave no positive ground for diagnosis, Dr. Abelin, from previous experience, concluded this was a case of retro-pharyngeal abscess. Enlargement of the bronchial glands may simulate this disease, but the other symptoms were against this explanation. To mistake it for pertussis, which in infants is not easily distinguishable from retro-pharyngeal abscess, was impossible in this case. The diagnosis was confirmed at the autopsy. There was found at the posterior wall of the pharynx a well-marked cavity the size of a chestnut, the surroundings of which were infiltrated with pus and appeared œdematous to a considerable distance. That the examination of the pharynx during life yielded no results need occasion no surprise, as Professor Oedmaussen was unable to feel the abscess from the buccal cavity even on the cadaver.

The following case is quite different as regards symptoms and the course of the disease, in which, during life, no diagnosis could be made. An infant six weeks old was admitted with severe gastro-intestinal catarrh, which defied all treatment. On November 20th there was observed tumefaction of the cervical glands and furuncles of the trunk. On December 12th an incision was made into a gland on the right side, which showed distinct fluctuation, and a considerable quantity of pus was discharged. Subsequently violent cough, dyspnœa with fever and emaciation, and double pneumonia supervened, and a constant exacerbation of all the symptoms, especially of the cough and dyspnœa, and the infant died in the night of the 10th December. At the post-mortem was found, close under the angle of the lower jaw, a tumour partly covered with bluish-red skin, on which were a couple of apertures leading into a cavity of the size of a hen's egg, partly filled with pus. The neighbouring glands were enlarged, partly only hypertrophic, and partly indurated and filled with pus. The abscess extended from the large vessels of the neck almost to the basilar process, from which it was only separated by a thin stratum of hardened connective tissue. At its

upper part it did not pass over the middle line of spinal column, but lower down it crossed it for two or three inches. The pathologico-anatomical agreement in both these was only apparent, as on close examination there were considerable differences. Whilst in the latter case the glands formed the starting-point of the disease, in the first case the connective tissue and muscles were first attacked. In all the cases of retro-pharyngeal abscess observed by Dr. A. he has found that the symptoms in this disease are very varying—a circumstance which gives rise to great difficulties in diagnosis, so that this disease may be easily overlooked; whilst, as Dr. A. considers, it occurs more frequently in children than is usually believed.

In conclusion Dr. A. communicates another case, which only came to his knowledge when he had finished his paper.

A boy sixteen months old was received in the polyclinic of the General Orphan Asylum, affected with pneumonia at apex of right lung, intestinal catarrh, tumefaction of glands of left side of neck. On October 2nd all the symptoms except the gland tumefaction had improved. On October 21st the fluctuating glands were opened; a few days after fever, hoarseness, cough with dyspnœa, restlessness, pain on moving head, and violent pain on deglutition supervened. On November 1st unmistakable phenomena of retro-pharyngal abscess showed themselves; the pharynx was reddened; the posterior wall projected in a sacculated form, and fluctuation was distinct. Evacuation of the pus caused much improvement in the symptoms. On the evening of November 2nd the dyspnœa became more severe and increased very much, in consequence of cessation in the flow of pus. On re-establishing its flow all the symptoms much improved. On November 3rd the discharge had ceased, and examination led to the belief that the abscess had healed; but on the 4th the preceding phenomena had returned and soon reached the same degree as before the operation; and on examination it was found that the abscess was again quite full. A larger incision was now made, and about fifteen grammes of pus were discharged, after which there was again instant diminution of all the symptoms, and no sensitiveness of pharynx. All danger seemed past; but on November 6th the child again became worse, and the abscess appears to have burrowed downwards; an incision was made as deep as possible, but the strength of the child was almost gone, and despite the fact that deglutition after the last operation was quite free, the child died on November 11th in a state of general collapse.

Schmitz on Idiopathic Retro-pharyngeal Abscess in the first two Years of Life ('Jahrbuch für Kinderheilk,' vi, 3, 1873).—In the short space of three years Dr. Schmitz has observed no less than sixteen cases of this affection (this disease appears to be far more common in Germany, Norway and Denmark than it is in England. During the last four years we have seen but six cases, and these in the practice of one of the largest general hospitals, and at an institution specially devoted to the diseases of children in the most thickly populated and poorest part of London, where most of the conditions which help to bring about this affection are generally found combined,

viz. deficient hygienic arrangements, insufficient food and clothing, and an inherited debilitated condition of system. This greater frequency may perhaps be attributed to the greater cold of those countries, but on the other hand one would think that the unstableness of our climate, added to the favouring conditions just mentioned, would suffice to bring this disease more frequently under our notice). He says that none of them were due to, or associated with, disease of the vertebra, and for this reason he terms them *idiopathic*. In his opinion it is inflammation of the post-pharyngeal lymphatic glands which is the cause of the affection, and these glands are constantly present up to the third year. In seven cases the abscess projected below the jaw and under the sterno-mastoid. Thirteen of the cases were under a year old, the rest less than two. The symptoms in Schmitz' cases are very similar to those witnessed by Abelin and by us, and may be taken as typical. There is dyspnœa, often urgent, respiration goes on through the half-open mouth and is accompanied by a snoring noise. This respiratory noise resembles that in cases of enlarged tonsils, but is not like that observed in croup. The head is thrown back and the neck stretched out. There is fulness and tumefaction at the angle of lower jaw. Dysphagia is commonly present, and liquids return through the nose and mouth. It is not easy to examine the throat, for when the tongue is depressed the child retches and chokes and the fauces are filled with fluids or mucus. Sometimes a projection can be seen on the posterior wall of the pharynx, but examination with the finger is much more satisfactory, as by it the position, size, and fluctuation of the swelling can be ascertained. In all cases of dyspnœa in infants arising without obvious cause post-pharyngeal abscess should be suspected and inquired into. Thirteen of Dr. S.'s cases quite recovered, two died and one was lost sight of. One died from maternal neglect and marasmus, and one, suddenly, from œdema of the glottis. In the latter case the abscess was opened. Dr. S. says that resolution never occurs and that spontaneous bursting of the abscess is commonly fatal from the pus entering the larynx.

The proper treatment is to open the abscess with a bistoury, and the opening should be either in the pharynx or neck, or both, according to circumstances, but an incision in the neck should be avoided, if possible. In seven cases Dr. S. made the incision in the pharynx only, in three in the neck alone, and five times in both places. He uses a guarded pharyngotome, and as the knife enters the abscess the left forefinger depresses the epiglottis and closes the larynx. Two of our cases were treated by incisions, one by the pharyngeal incision alone, one by an additional free incision in the neck, one was tapped by the aspirator, and in one the pus was withdrawn with a long trocar and cannula and some diluted Condy's fluid injected. These cases all recovered, but one case which was obviously due to caries of upper cervical vertebræ and which was under the care of a colleague was removed from the hospital by the mother and subsequently died. In another case under our own care the affection was *chronic*. There was a somewhat tense fluc-

tuating swelling behind the posterior pharyngeal wall, the local symptoms of this disease were present in a modified degree, there was no constitutional disturbance, and the tumour did not appear to be due to cervical caries, or any growth congenital or other. Nothing operative was done as nothing seemed necessary, and the child was sent into the country. A child was admitted into the London hospital about three months since. There was redness and a large tense swelling on the right side of neck in front of sterno-mastoid; dyspnœa was urgent and accompanied by stridor. Some fulness could be detected by faucial examination. The temperature was high, there being great fever and general disturbance. The house-surgeon, Mr. Widdas, made an incision in the neck and gave exit to a large quantity of fœtid pus. This temporarily improved the general condition and relieved the dyspnœa, but in a few hours the respiration again became difficult, and as the symptoms pointed to laryngeal spasm and obstruction, trancheotomy was performed. The operation was rendered difficult because the trachea was pushed to the left side. This proceeding gave further temporary relief, but the next day the child died. At the post-mortem a large abscess cavity was found extending from about the second to the sixth cervical vertebra; it was much larger on the right side, but passed behind the trachea and encroached on the left side. The periosteum and anterior common ligament was gone in parts and the bone exposed and eroded in patches. This, however, was caused by the pressure of the abscess and was not the cause of it, as the bones were not carious, and the joints were intact. The disease commenced in the lymphatic glands on the right side of and behind pharynx. The right recurrent laryngeal nerve was found imbedded in an enlarged and inflamed gland, explaining the spasm which was superadded to the obstruction by pressure. The mucous lining of the larynx and trachea were much congested. There was tubercle in the intestines, lungs, and pia mater, and a large quantity of fluid in the ventricles. Another interesting and important point noted was that, although the tracheotomy tube had only been in about two days, its lower end had caused ulceration and perforation of anterior wall of trachea.

We have notes of three cases of this disease occurring in adults who were under the care of Mr. Shaw and Mr. Moore, at the Middlesex Hospital; two were due to caries and one idiopathic. It would seem that the *idiopathic* form is much more common; in fact, almost confined to children, and that pharyngeal abscess due to cervical caries is uncommon in them, but is a commoner cause in the fewer cases occurring in adolescents and adults. From our own experience, and from the perusal of these and other cases, it may be said that the diagnosis is not usually difficult, and that the line of treatment is sufficiently clear. The indications are, thorough evacuation of the abscess and prevention of the reaccumulation of pus. This is well done, and the risk of the matter running into the larynx or of the tongue or other parts being injured is prevented by tapping, with a long trocar and canula, through which the cavity may be

washed out with some disinfectant. Some lint should be placed in the opening and withdrawn, and replaced twice in twenty-four hours.

Medicines are not needed—in fact, cannot usually be swallowed; but the strength must be supported by nutritious and stimulating enemata till food can be taken. Above all, evacuate abscess early and freely, and prevent reaccumulation of matter. The child should be held by an assistant, and the gag employed if necessary. A very serviceable one, in such cases, would be Smith's or Wood's, by which the tongue would be kept out of the way, and the tumour well seen, explored, and tapped. Æther or chloroform is of great service, as it is difficult to examine or in any way manipulate the throats of children without.

On Serous Cysts in the Neck. By Dr. Burow ("Arch. f. Klin, Chir.,' xii, 3, p. 976) and Dr. A. Trendelenburg (ibid., xiii, p. 404. 'Schmidt's Jahrb.,' B. 155, p. 56).—Among the tumours occurring in the neck cystic tumours present, according to Dr. Burow, the greatest varieties. The knowledge of them forty years ago was very imperfect, as they were not distinguished if they presented their common character of a sac filled with a firm-feeling substance. Brünninghausen, Maunoir, Wernher, Fleury, Marchessaux, and Gurlt, were the first who threw some light on this subject. At present we distinguish the following varieties of cysts in the neck:—1. Ranula. 2. Cystic goitre. 3. Hygroma cystic-colli congenitum (according to Wernher). 4. Hygroma of bursæ mucosæ. 5. Hydrocele colli. 6. Atheroma. 7. Dermoid cysts. 8. Echinococcus cysts outside the thyroid gland. (We may add, 9. Echinococcus cysts in the celullar tissue of the neck.)

The hygroma cystic-colli congenitum first described by Wernher is characterised by tumefaction in the lateral region of the neck greatly varying in size. A bilateral tumour may cover the whole neck, raise the tongue as in ranula, or it may hang down towards the chest. Its chief character is its composition of many cysts, which adjoin without intercommunication, and may reach backwards to the vertebral column. A spontaneous cure sometimes occurs. Extirpation of the tumour is to be rejected. Repeated puncturing or incision at intervals are to be commended; the prognosis is, nevertheless, very difficult. Burow applied drainage in one case. The patient, æt. 11 months, had a congenital tumour the size of an apple, which occupied the whole left side of the neck, and extended from the lower jaw to the clavicle. The third rib formed the lower limit, and it reached considerably backwards. A trocar was thrust in, and a drainage tube of 4 mm. introduced through the canula. A violent reaction ensued, and the tumour slowly diminished. Dr. Burow heard of the case two years after, and the child was then in a healthy condition.

Hygromata occurs at the central line of the neck as degenerations of the ante-thyroid, thyro-hyoid, or supra-hyoid bursa. The cure is best effected by puncturing followed by injections, by drainage, or, if they are superficially situated, by removal of the anterior wall,

followed by cauterisation of the interior of the cyst. Sometimes they break spontaneously, leaving fistular canals difficult of cure.

Hydrocele of the neck is a non-congenital cystic tumour, which occurs on one side of the neck, between the angle of the lower jaw and the mastoid process on the one hand, and the larynx and sterno-mastoid on the other. Burow had observed four cases, two on the right and two on the left side of the neck. The wall is not thick, it is united with or embedded in the adjacent parts. As regards treatment, a violent reaction sometimes takes place after puncturing and subsequent stimulating injections. Incisions have not proved sufficient. Total extirpation has resulted very unfavorably (Bardeleben, Schuh). Burow has, nevertheless, among his four cases, twice performed this operation with the best success.

Case 1. A woman, æt. 40, had a tumour in front of the sterno-cleido-mastoid, which was of the size of an apple, and commenced forming five years ago. During the operation Burow isolated the sac with his finger from the large vessels of the neck beneath, and removed it entire. Rapid cure.

Case 2. A man, æt. 36, had a tumour which commenced close to the lobule of the ear, and projected before the inferior margin of the lower jaw. An incision was made parallel to the sterno-mastoid, and Burow reached the cyst by cautious dissection. The anterior wall being adherent, two incisions were made large enough for the introduction of the finger to effect traction on the cyst. This procedure proving unsuccessful, Burow was obliged to leave the posterior smaller half, which was adherent to the large vessels of the neck. The wound diminished and granulated well, when the patient was dismissed.

Case 3. The patient, æt. 37, had a tumour of the size of a medium apple, of two years' standing. Fluctuation was so indistinct that exploratory punctures had to be made. In this case the adhesions could also be detached with the finger, and the whole tumour removed. Above and behind the adhesions were tied and cut off. Strong reaction and a rapid cure followed.

Case 4. The patient, a lad, æt. 17, had a tumour the size of an apple, between the angle of inferior maxilla and the mastoid process. An incision was made parallel to the sterno-mastoid, through which the cyst, which adhered throughout, was removed. When Burow incised it he found in it free, floating cysts, of which he removed as many as possible, and a cure was effected in three weeks. A cyst, the size of a pigeon's egg, which was removed, contained an echinococcus.

Of these last-mentioned growths Gurlt could only collect three cases; these occurred in the practices of Cloquet, Dixon, and Langenbeck. In Langenbeck's case only was a cure effected. Two other cases have been published, one by Stanley ('Med. Times and Gaz.,' Aug. 14, 1852) and one by O'Beirne ('Dublin Journ.,' Sept., 1834), so that, including that of Burow, six cases are at present known.

Trendelenburg publishes from Langenbeck's clinic four cases of

congenital cysts in the neck, in which injections of Tr. Iodid. were successfully employed.

Case 1. The patient, æt. 2½ years, was born with a tumour of the size of a walnut, which gradually attained the dimensions of a child's head, and extended backwards to the vertebral column, upwards, close to the mastoid process, and downwards beyond the clavicle. Puncturing and injection of two grammes of Tr. Iodid. caused slight fever, and the anterior part of the tumour diminished. A second injection was made, causing violent fever. The cysts became ichorous, and the child died from collapse seven weeks after the first injection.

Case 2. Infant, æt. nine days, had a tumour the size of an orange, in the left supra-clavicular fossa, reaching the anterior margin of the sterno-mastoid in front upwards close to the mastoid process, and extending almost to the vertebra behind. It fluctuated distinctly. A cure was effected after puncturing and three injections of iodine.

Case 3. Patient, one day old, had a tumour the size of a china orange, between the lower jaw and the clavicle, which was tapped and injected with one grain of tincture of iodine. This proceeding was repeated after the lapse of eight days, and was followed by a cure four weeks after the first injection.

Case 4. A child, nearly 2 years old, was born with a tumour the size of a nut, on the nape of the neck, which was at first situated at the level of the last cervical vertebra, but had now attained the size of an orange, and could not be diminished by pressure. It was punctured and injected, and this treatment was repeated at intervals of eight to fourteen days. Four weeks after the first injection there remained only a gibbous, disc-shaped, fibrous mass.

Cases of cysts in the nape have hitherto only been observed in the fœtus; still the normal relations of the spinous processes, the impossibility of reducing the tumour by pressure, and the rapid healing, render the diagnosis certain. These cases prove, at all events, the possibility of a certain cure of congenital neck cysts by the injection of tincture of iodine.

Myxomatous Coccygeal Cyst cured by Ligature of Tumour.—Dr. Jayakar, of Ahmedabad, relates the following interesting case in 'Med. Times and Gaz.,' 1872. M—, a healthy-looking infant, æt. about eight months, had a cystic-looking growth, situated about half an inch behind the anus, and just over the lower end of the coccyx. It was about the size of a medium-sized apple, firm and rather hard above, and soft and cystic in its lower part. It was first observed as a small growth a few days after the child's birth, and then kept on increasing in size. There was some amount of pain on squeezing the tumour; but it was so slight that its communication with the spinal cord may have been fairly disputed.

On exploring the lower part of the tumour with a grooved needle a viscid, transparent, mucous-like substance escaped; it was sticky in its feel, and heat seemed to have no effect on it. The tumour was tapped, and about four ounces of similar, sticky, viscid fluid exuded. The fluid being removed, the nature of the harder and firmer part of the tumour rendered the diagnosis of the case easy. The lower

lagging portion of the tumour soon filled up again, and she was admitted into the hospital. The tumour having rather an undefined base, no active surgical interference was thought advisable, and a thick, strong ligature was, therefore, passed through it, and tied on either side. Within twenty-four hours the ligature seeming to give rise to convulsions, was loosened, and again an attempt was made gradually to tighten it with success. At the end of three weeks the tumour had mostly separated, and a few touches of the knife completed its final removal from the body, leaving a red granulating surface to heal gradually.

There are few recorded cases of myxomatous tumours, either owing to the rarity of such growths, or their being easily mistaken for colloid and similar tumours of a malignant nature. The physical condition and general structure may often lead to a wrong diagnosis; but the total absence of any signs of malignancy, together with a microscopical examination of the contents of the growth, would soon decide the question as to its nature.

The upper part of this tumour, or rather the undefined pedicle described in the case, was solid, and, on making a section, the same glairy, viscid fluid as in the lower part oozed out. Microscopically it was found to be mainly composed of fibro-cellular tissue, with fat-cells and oil-globules scattered in places, showing that already degenerative changes had commenced. The lower or cystic portion of the growth was divided into small compartments by bands of fibro-cellular tissue extending in all directions, with vessels ramifying over the greater part of its inner surface. The viscid, glairy fluid, under the microscope, presented numerous large rounded cells, and some oval, with thready branches shooting forth from all sides of the cells, and freely anastomosing with each other.

Cases of congenital tumours, whether cystic or solid, are not very infrequent, and many examples may be found recorded in British and foreign medical literature. Mr. Holmes, in his 'Surgical Diseases of Children;' Mr. Bryant, in his 'Practice of Surgery,' and Mr. Thomas Smith, in the second volume of 'St. Bartholomew's Hospital Reports,' mention several cases; and at the recent meeting of the British Medical Association, in London, we exhibited some drawings of cases of congenital, sacral, and coccygeal tumours, and Mr. Tay exhibited one of hydrocele of the neck, which spontaneously decreased, and also a drawing and microscope sketches of a tumour from the right arm of a male infant, æt. 9 months, near the axilla. Drawings of cystic tumours of neck and sacral region were also shown by Mr. F. Beach from the museum of the Hospital for Sick Children, and a large congenital tumour in subcutaneous tissue of leg of child, partly erectile, partly fibrous, was lent from the London Hospital Museum.

The microscopic examination in Mr. Tay's case of solid tumour revealed some points of resemblance to Dr. Jayakar's, but, microscopically, it differed in that there were no cysts. It was of the consistence of udder, and had a quite homogeneous section resembling *blanc mange*. Microscopically its structure was very simple,

it being hypertrophy of the connective-tissue elements of the subcutaneous fatty layers, and allied to the solid portions of the cystic tumours of the neck and other parts ('Path. Trans.,' vol. xxii, p. 250). In an average specimen of the structure of the whole tumour the areolar appearance was shown to be produced by numbers of connective-tissue corpuscles, whose processes join one another.

In our two cases of congenital cystic tumours of the neck one was treated with setons and one was tapped and injected; they both recovered after somewhat considerable constitutional disturbance.

A Jewish girl, æt. 20 months, was brought to the London Hospital among our out-patients, she had an elastic tumour the size of a large orange on the right side of the sacrum and coccyx, which could be plainly felt with the finger in the rectum; fluctuation was indistinct, the cyst being tense. It was opened from without, and several ounces of a straw-coloured, rather viscid fluid, were evacuated, or rather spurted out. The cavity was syringed out with carbolic acid lotion and plugged with lint; it inflamed, suppurated, and gradually contracted, and was cured.

Simultaneous Dislocation of both ends of the Clavicle.—Dr. Stanley Haynes communicates the following interesting case to the 'British Medical Journal,' 1872. In our last report we related a case which occurred in the practice of a French surgeon, and in the person of a young woman. Dr. Haynes' patient was a girl æt. 10, rapidly growing, of lax tissues, and of a consumptive family, but who had always had good health. While washing the back of her neck with her left hand, she felt something give way in the shoulder of the same side. Dr. Haynes found dislocation forward of the sternal end of the clavicle, and partial luxation upward of the acromial one. There was very little pain. Both extremities of the bone were easily replaced by drawing the shoulder backward and downward, but the double deformity was reproduced immediately the shoulder was liberated. A pad was applied under a figure-of-8 bandage over the sternal end, and the arm was placed in a sling as a temporary measure. Dr. Haynes subsequently tried a strap with a truss, pad and sling, and then a modified hernia truss, the anterior pad of which kept the sternal luxation reduced in all positions of the arm, while the band passed under the left axilla, and the other pad lay between the right scapula and the spine; but it could not be kept in place.

It will be noted that this is not a case of *complete* dislocation of the clavicle, this accident being a very rare one. It is interesting as occurring at so early a period of life, and shows that the clavicle may in common with other bones, as the humerus, inferior maxilla, metacarpals, especially that of the thumb, be partially or totally dislocated in individuals of lax tissues and ligaments. It is not very improbable that in such cases there may be an imperfect development of the joint surfaces.

Tetanus Neonatorum cured with Chloral Hydrate, by Dr. Franz Auchenthaler ('Jahrb. f. Kinderheilk.,' N. F. iv, 2, p. 218).—An

infant, æt. 14 days, who had previously been well, was affected on May 20th with trismus ; the attacks were repeated, and became more and more frequent. The otherwise healthy infant showed in different places, especially on the forehead, united miliary vesicles, moderate sized pupils, which reacted well to light, swelling of the nasal mucous membrane, with somewhat increased secretion, and slight injection of the navel. Pulse 158. Respiration weak, 56, but quiet and regular. Nothing abnormal in the head, chest, and abdomen. The attack began with restlessness, cries, cyanosis, knitting of the brows and firm closure of the eyelids, expansion and contraction of the alæ nasi, pursing of the mouth and blowing out of the cheeks. The masseters were hard and the jaw firmly closed; the abdominal muscles were tense, the vertebral column bent forward, the arms crossed over the chest with the thumbs infolded and the fist closed; the legs were extended and the tendons stretched. The whole body of the child was completely stiff; the heart's action was very irregular, and the respiration was carried on by fits and starts. The attacks occurred very frequently and lasted a long time—almost an hour.

During the attacks the child was given a powder of 0·06 grammes of chloral hydrate, with gum Arabic dissolved in the mother's milk, which was administered through the nose. Shortly after the attacks ceased, but returned in an hour, and again another in one and a half hour, but was much weaker, and after the third dose left off. There was no attack during the night, and those occurring the next day were very weak and of short duration. The attacks became gradually more infrequent, and ceased entirely on the tenth day of the disease, and did not recur during the next fourteen days. During the six days' stay of the child in hospital it had altogether twenty-seven seizures, and twenty-five doses of chloral; an increase of the dose was not necessary, as the effect was sufficient. Subsequent effects of the chloral were not observable during the whole treatment, with the exception of enteritis, which, according to Dr. Auchenthaler, must be ascribed to another cause. The temperature was not increased even during the attacks.

Numerous remedies have been tried in the treatment of this terrible disease, but most of them have fallen justly into desuetude. Calabar bean, subcutaneous morphia injections, spinal warm and cold bags, chloroform, and chloral hydrate, with the avoidance of all things likely to induce the attacks, and the removal of any source of irritation, seem to be the medicaments most serviceable and the indications on which to act.

REPORT ON OBSTETRICS, AND THE DISEASES OF CHILDREN.

By W. S. Playfair, M.D., F.R.C.P.,

Professor of Obstetric Medicine in King's College, Physician for the Diseases of Women and Children to King's College Hospital, Examiner in Midwifery, &c., to the Royal College of Physicians.

1. The Non-Pregnant State.

1. *Remarks on some points in the Clinical Pathology of Uterine Disease.* By Dr. J. Braxton Hicks.
2. *On Prolapsus of the Womb.* By Wm. Goodell, M.D.
3. *On Vaginal Urethrocele.* By Dr. Gillette.
4. *Connection between Menstruation and Smallpox.* By Dr. Otto Obermeier.
5. *On Vaginismus from Lead Poisoning.* By Dr. Neftel.
6. *On the Treatment of Vesico-Vaginal Fistula when the Urethra, Neck, and Floor of the Bladder have been destroyed.* By Dr. C. Kidd.
7. *On Retro-Uterine Hæmatocele.* By Professor Dolbeau.
8. *On Retro-Uterine Hæmatocele.* By Dr. Weber.
9. *On the Pathology of Areolar Hyperplasia and Sclerosis Uteri.* By Dr. Skene.
10. *On Ovariotomy.* By J. Marion Sims.
11. *On the Effects of Quinine on the Uterus.* By Dr. F. K. Baily.
12. *On the Physiological Results of the Removal of both Ovaries.* By Dr. Koeberle.
13. *On the Orifices of the Unimpregnated Uterus and their Surgical Treatment.* By Dr. J. Matthews Duncan.
14. *On Mechanical Dilation of the Cervix Uteri.* By Dr. J. Matthews Duncan.
15. *On a Case of Complete Extirpation of the Uterus in Inversion.* By Dr. Martin Barber.
16. *On Endo-metritis.* By Lombe Atthill, M.D.
17. *On Junod's Boots in some forms of Amenorrhea.* By Elizabeth Garrett Anderson, M.D.
18. *On the Causation of Acquired Flexions of the Uterus.* By Dr. Squarey.
19. *On a Case of Secondary Syphilis, communicated from an Infant to its Nurse.* By Dr. Macdonald.
20. *Case of Fibroid Polypus, complicated with complete Inversion.* By Mr. A. J. Field.
21. *On Latent Gonorrhœa in the Female Sex.* By Dr. Angus Mac-Donald.
22. *On Manual Rectal Palpation of the Pelvis and Abdominal Organs.* By Dr. Simon.
23. *On Movable Kidney in Women.* By Dr. Howitz.

1. In these papers the author discusses those diseases of the uterus which are usually classed as inflammatory, and maintains

that they depend very much on some local nervous irritation for their character. He points out that the pain of epithelioma, and many other uterine disorders, such as retroversion, with engorgement and abrasion of the cervix, depend on peripheral irritation of the nerves of the os and cervix. He also argues that pelvic cellulitis from traumatic causes, such as the passing of a sound, may be explained in the same way, the irritation on the terminal extremities of the nerve-fibres producing reflex irritation or exaltation of their action, and by this means the vaso-motor nerves of the ovarian vessels become affected, so as to cause a stasis of their contents, or, in other words, a congestion of the veins, in consequence of which exudation of serum, or perhaps of blood, takes place in the broad ligaments.—*Obst. Journ.*, June, 1873.

2. In a series of clinical lectures on this subject the author attempts to explain that hypertrophic elongation of the supra-vaginal portion of the cervix, so often met with in old-standing cases of prolapsus, is in no sense a true growth, but is a secondary accident, the result of elongation consequent on the traction exerted by a primary prolapse of the vagina and bladder. This is, he thinks, specially apt to occur where the womb is ductile, either from congestion, or through the arrest of post-partum involution; so that it yields more readily to traction. In the treatment he rejects the very difficult and dangerous operation of the removal of a portion of the cervix above the vagina as unnecessary, and is content with removing a portion of the infra-vaginal cervix. It almost always succeeds in curing the disease, and its success is, he holds, an unfailing proof of the correctness of his theory.—*Philadelphia Med. Times.*

3. The author relates an interesting example of this condition, in which he found a partial dilatation of the lower portion of the urethra, forming a projection on the anterior wall of the vagina, in which urine collected, and from which it was expelled on slight exertion. This he eventually treated by the excision of a flap of the substance of the vaginal wall, forming the dilated pouch, and bringing the edges together with sutures; and this operation proved effectual in relieving the distressing symptoms.—*L'Union Médical*, April, 1872.

4. Dr. Obermeier comes to the following conclusions:—1st. Menstruation generally coincides with the first period of smallpox (in three fourths of the cases), and comes on generally at the time of the eruption. 2nd. The disorders of menstruation are not so frequent as is generally stated (only one fourth of the cases); the supervention of the disease, especially the eruptive period, hastens the appearance of the menses, whilst their retardation is exceptional, as also their absence or suppression; and it is very seldom that smallpox brings on real hemorrhage. 3rd. It is most common to observe the coincidence of normal regular menstruation with the first period of smallpox (in more than one half of the cases), and the fact probably depends on some physiological modification of the period of incubation. 4th. The pathological influence of smallpox on men-

struation depends less on febrile irritation, as suggested by Peroud, than on the morbid process itself (eruption). 5th. The menstrual flux which comes on after the disease is generally weak and retarded. —*Virchow's Arch.*, Sept. 1, 1873.

5. The author describes a case of vaginismus from lead poisoning, which had come under his observation, in addition to three others already published, in which it resulted from the use of cosmetics containing lead. The present case was that of a young actress, and was traced to the same cause. There was also saturnine colic. The face and extremities were cold; the contractions of the heart feeble, only 44 in the minute. The issue of this case is not given, but in those previously referred to the cure of the plumbism cured the vaginismus.—*Brown-Séquard's Arch. of Pract. Med.*, March, 1873.

6. Dr. Kidd relates a severe case of this kind, in which he effected a cure by closing the whole vagina, leaving only a small aperture anteriorly to serve as a urethra. This he effected by paring the mucous membrane of the inner surface of the labia and posterior wall of the vagina. He then placed a No. 10 catheter close up under the arch of the pubes, and then brought the pared surfaces into contact by means of four deep-quilted sutures and some superficial wire sutures, just as in an operation for ruptured perinæum. After removal of the sutures perfect union was found to have taken place. A spring pad like a truss, invented by M. Trélat, of Paris, was then fitted on to the orifice of the urethra, and the woman was then able to retain the urine perfectly in any position. The fact that the patient is unsexed by this operation is, he thinks, a secondary consideration in such a state of things.—*Dub. Med. Jour.*, April, 1873.

7. In a lecture on this subject the author explains the fact that the hemorrhage in some cases of retro-uterine hematocele is very dangerous, and the reverse in others, by the supposition that in the latter it is the result of a pelvic peritonitis of an hæmorrhagic form. From some cause or other a pelvic peritonitis is produced by it, numerous adhesions are formed, shutting off the general peritoneal cavity. In these new blood-vessels are formed, and it is from the rupture of these that the hæmorrhage is produced. Analogous instances of hemorrhage are found in meningeal hemorrhage, in cases of arachnitis, and in scrotal hematocele. A similar doctrine was advanced by Virchow, in 1862, whose claims to priority the author contests.—*Med. Times and Gaz.*, March, 1873.

8. The author concludes, from an analysis of twenty-three cases observed by him, that this condition occurred most frequently in young women, disposed to anæmia, whose work was laborious. In six out of the twenty-three immoderate sexual indulgence was the partly known, partly conjectural, cause, in four the immediate occasion of the disorder.

The use of the sewing machine, according to the author, plays an important part in the production of hematocele. The right ovary was the source of the hemorrhage in eighteen cases. In respect to the prognosis, as regards life, the author considers it very favorable, as none of his cases were fatal, a result which he ascribes to his

method of treatment—ice bladder, perchloride of iron internally, and avoidance of puncture. Ten cases completely recovered. In three the decomposed extravasation burst through the rectum.—*Berl. Klin. Wochensch.*, No. 1, 1873.

9. The author applies this term to cases in which, as he thinks, enlargement of the uterine walls, both of the body and cervix, arises from simple hyperæmia, which is the only lesion or tissue change present. He includes only those cases in which there is simply *excess of the connective tissue*, *i. e.*, when the whole increase of the size of the parts is due to excessive growth of that element of the structure. As a rule it does not extend to the uterine walls, but is limited to the cervix. The consistence as well as the quantity of the tissue is changed, the part affected being denser and less elastic than usual. The lesion is a permanent one, there being no tendency to restoration of the normal tissue; on the contrary, there is a lowered vitality, with a proneness to degenerative changes, such as epithelioma.

A committee of the New York Obstetrical Society reports on this paper to the effect that there is no evidence that simple hyperæmia, unassociated with inflammatory changes, ever give rise to growth of tissue, and that the balance of modern pathological opinion points in the opposite direction.—*New York Jour. of Obst.*, Nov. 1872.

10. In this paper Dr. Marion Sims contends that far too much attention has been paid to the pedicle in the study of ovariotomy, and that the importance of septicæmia as a cause of death after operation has not been sufficiently recognised. He believes that a far larger proportion of deaths may be traced to this source than is generally believed, and in support of this view he analyses the symptoms in Spencer Wells' fatal cases with the view of proving that in many of these, in which death was attributed to other causes, septicæmia really existed. He proposes to establish drainage of the peritoneal cavity through Douglas's space as a rule of practice in every case. By this means any offensive secretions will at once flow away, and antiseptic injections can be used at will. Nor does the passage of the tube at the time of the operation at all increase the difficulty and danger of the operation. Several illustrative cases, in which the method was tried and acted well, are recorded. The whole paper is highly suggestive, and well worthy of careful study.—*New York Medical Journal*, Dec., 1872.

11. The author gives the results of his experience in a malarious district. He found that abortion and miscarriage were exceedingly common in women suffering from intermittent fever, unless anteperiodic treatment could be carried out. In all such cases he recommends that quinine should be given promptly and freely. He has also given it in labour with beneficial results. He found that labour was often ushered in with a chill, and the depressing effects of the malarial poison would render the pains feeble and without expulsive power. At this juncture a full dose of quinine would equalise the circulation and render contractions more forcible.—*Philadelphia Med. and Surg. Reporter*, Nov., 1872.

(Dr. Monteverdi has recently published a paper on quinine as an oxytoxic (vide 'Reports on Obstetrics,' Jan., 1873). Possibly these observations of Dr. Bailey's may explain its supposed oxytoxic properties, as it is not improbable that the author's patients were suffering from the effects of the malarial poisoning, especially as experiments on healthy women have not corroborated Dr. Monteverdi's views.—*W. S. P.*)

12. Dr. Koeberle states that in women in whom he has removed both ovaries comparatively little change has taken place. The genital organs remain excitable. The character becomes gentle and less irascible. The breasts are not at all atrophied. The women have no tendency to excessive corpulence unless there has been a predisposition to obesity before the operation. There has been no alteration in the growth of the hair. The quality and pitch of the voice have not been modified. Perfect health has been the rule; but the catamenia have never reappeared.—*Montpellier Medical*, Jan., 1873.

13. The author details his views on this subject, in continuation of a former paper. He believes that an acquired stricture of the cervical canal, using the term in its surgical sense, is a condition never met with. In spasmodic dysmenorrhœa the passage of a bougie, however small, through the internal os uteri, causes great pain. In such cases the internal os is occasionally small, sometimes transmitting an ordinary uterine probe with a little difficulty; and its dilation must be by very slow degrees, if it is to be done by a gentle or slight force, without causing any split or laceration. The internal os does not, as a rule, recontract after the dilatation.

The pain is spasmodic, occurring before, during, or after the monthly flow. There is extreme sensitiveness of the internal os, and of the mucous membrane of the body of the uterus. It is quite possible that an inflammatory condition of the mucous membrane may exist, but it has not been demonstrated. Dr. Duncan conceives that the spasmodic pains may be analogous to strangury or tenesmus in catarrh of the bladder or bowels. As regards surgical treatment Dr. Duncan maintains that cutting operations and dilatations with tents are dangerous, and that the dilatation is best effected by the daily passing of graduated metallic bougies, as in stricture of the urethra. He finds that strikingly good results are often obtained from this method, which he very rarely uses in the virgin.—*Brit. Med. Journ.*, March, 1873.

14. The author replies to the criticisms which have been made on his former paper, in which he recommends graduated metallic bougies as the best method of dilating the cervix. He maintains that the force required to pass them, being only exerted for a short time, is less hurtful than even the more prolonged action of the sponge or laminaria tent. The latter, he contends, have very frequently been followed by severe inflammation, and even death; and he ends by saying, "The treatment of spasmodic dysmenorrhœa, by mechanical means, appears to me, at present, to be best effected by the use of a graduated series of metallic bougies, applied

as I have elsewhere described—never at any time allowed to remain in the uterus above a few minutes.—*Brit. Med. Journ.*, Jan., 1873.

15. In this case complete inversion of the uterus occurred after the second labour, which was very rapid, of a patient twenty-five years of age. Reduction was impossible. Constant hemorrhages occurred, which reduced the patient to a state of extreme anæmia. Chaissaignac's écraseur was first employed, the operation being completed by ligature. Slight peritonitis followed, but the patient made a good recovery and left the hospital on the twenty-second day after operation.—*Gaz. Méd. de Paris*, No. 4, 1873.

16. The author contributes a valuable paper on this subject. He fully recognises the impossibility of effectually curing cases of this kind, except by appropriate intra-uterine medication. The practice he recommends is the free cauterization of the cavity of the uterus with the fuming nitric acid, a method he has constantly adopted, and which has, in no single case, been followed by bad results. He generally commences by local depletion of blood; then he applies his remedy through an instrument devised by him, called the intra-uterine speculum, the cervix having first been dilated with sea-tangle tents. In the discussion which followed the reading of the paper it was stated by Dr. Churchill and others, that they had constantly adopted the practice with much benefit to their patients. —*Dublin Journ. of Med. Science*, May, 1873.

(Dr. Atthill's experience, corroborated as it is by other distinguished Dublin physicians, conclusively proves the safety of intra-uterine applications in suitable cases. The risk of this plan seems to be not the strength of the application used, but the preliminary dilatation with tangle tents, which is recommended. The dilatation of the cervix is in itself a serious operation, not unfrequently followed by formidable consequences, and it is an expedient not to be lightly adopted.—*W. S. P.*)

17. The author has tried the remedy with success in the case of acute menstrual suppression. Essentially it is dry cupping on a large scale. The leg is enclosed in an air-tight case, and the air is exhausted over it. The result is that for a time the capillaries and vessels of the leg enlarge, and the circulation in other parts is proportionately relieved. It has the great merit of not interfering with other treatment, of being quite painless, and so easily regulated as to be practically harmless, and of being applicable to young girls.

This treatment was tried in two cases in which the suppression depended on a violent chill during the menstrual period. In both the usual remedies had been tried without success, and in both the flow returned after two applications of the apparatus for about forty minutes at a time.—*Practitioner*, Dec., 1872.

18. Dr. Squarey attempts to explain the production of ante- and retroflexion, according to the position of the uterus in the pelvis at the time the displacing force came into action. "When an anteflexion is produced the uterus is, at the time the cause came into action, situated high up in the pelvis, occupying its normal position, the axis being forward, so that any force striking it from above

would impinge on the *posterior* and upper surface of the fundus, and so force it forwards, and produce anteflexion; when a retroflexion is produced, the uterus has sunk more or less deeply in the pelvis, with the axis directed more or less backwards, and in this case any force striking it from above would impinge on the *anterior* and upper surface of the fundus, and so force it backwards and produce retroflexion." With regard to the pathology of flexion Dr. Squarey is of opinion that in flexions suddenly produced in previously healthy women, as by falls, &c., the maintenance in the abnormal position is effected by some rupture of the uterine fibres, accompanied by more or less extensive effusion of blood into the uterine tissue, thus completely destroying the elasticity of the uterine wall, and its power of righting itself.—*Obst. Trans.*, Dec., 1872.

19. Dr. Macdonald relates an interesting example of this kind. The child of a mother known to have syphilis, communicated by the father, showed marked symptoms of hereditary syphilis a month after birth. The child was nursed by its grandmother, a perfectly healthy woman, of fifty-nine years of age. This woman contracted distinct secondary symptoms, with the evidence of a forming sore, the first evidence of her illness being sore throat, followed by the specific eruption.—*Edin. Med. Journ.*, July, 1873.

20. Mr. Field relates an interesting case in which a fibroid tumour, weighing 20 oz., was attached to the fundus of a completely inverted uterus. From the history of the case it appears that the inversion must have occurred acutely at the fundus, following down the intra-uterine polypus, as it were, expelled by uterine contractions. The tumour was removed by the écraseur. Subsequently, various attempts were made to reduce the inversion, at first by continuous pressure with a caoutchouc bag, afterwards by manipulation of the uterus. These were eventually successful, but not until pressure had been kept up for a period of twenty days.—*Bartholomew's Hospital Reports*, 1872.

21. In an elaborate paper on this subject the author discusses the views on this subject enunciated by Dr. Emil Noeggerath of New York, who maintained that the wife of every man who at any former period of his life had been affected with gonorrhœa, even though the cure had been apparently perfect, was affected, with very few exceptions, with latent gonorrhœa, which sooner or later brings its existence into view by establishing some form of uterine disease, often of great gravity. Without going as far as Dr. Noeggerath, the author thinks it certain that even an apparently cured gonorrhœa in the husband may prove a complication fraught with extreme danger to the female, and, when pregnancy occurs, one which is apt to give rise to dangerous and even fatal forms of puerperal fever. He thinks these cases prove that if a man marries with the slightest shade of a gleet he exposes his wife to great misery throughout her menstrual life, as well as to great risk of her life should she become pregnant. —*Edinburgh Medical Journal*, Jan., 1873.

22. The author describes his method of exploring the pelvis,

The patient is placed on the back, in the lithotomy position. Chloroform, to complete insensibility, is administered; and then first two, and subsequently three and four fingers, are passed gradually into the rectum by a rotatory movement. Bimanual manipulation is enjoined, the other hand being pressed over the abdomen. Tearing of the sphincter seldom occurs if care be taken, and the operator's hand be not too large; some temporary, but no permanent, inconvenience may follow.

The examination is useful for all cases of disease of the pelvic organs, especially the uterus, ovaries, and broad ligaments, as also for tumours in the lower half of the abdomen, and is likely to prove useful in midwifery cases.—*Goschen's Deutschr. Klin.*, 1872.

23. The author has detected movable kidney in twenty-seven cases, which did not give rise to any great inconvenience. The author believes that pregnancy has much to do in its production. It acts, he believes, by distending the abdominal walls, and thus depriving the kidney of the support it receives from the pressure of the kidney against it. Moreover, the renal vessels are enlarged during pregnancy, and thus become incapable of performing their function of holding the kidney in its place. The pressure and traction on the ureter, in consequence of the growth of the uterus, may also have some influence in producing the displacement.—*Hospitals Tidende*, April, 1873.

II.—Pregnancy.

1. *On the Structure of the Human Placenta.* By Professor Turner.
2. *On an Ovular Cyst successfully Removed by Abdominal Section.* By Dr. J. Lloyd Roberts.
3. *On the Influence of Changes in the Position of the Uterus in producing Sterility.* By Dr. Biegel.
4. *On Placental Syphilis.* By Dr. Franckel.
5. *On Determination of the Life or Death of the Fœtus.* By Dr. Cohnstein.
6. *On Tuberculosis and Pregnancy.* By Dr. Lebert.
7. *On the Diagnosis of Early Pregnancy.* By Dr. Rasch.
8. *On Chronic Endometritis as a Cause of Abortion in Displaced Uteri.* By Dr. Slavjansky.
9. *On a Case of Uncontrollable Vomiting, in which Abortion was practised with a Fatal Result.* By Dr. Nitta.
10. *On the Proper Management of the Placenta in Cases in which Gastrotomy is performed in Extra-uterine Fœtation.* By Dr. Barnes.
11. *On a successful Case of Gastrotomy in Intra-Uterine Fœtation.* By Dr. Lawson Tait.
12. *On Abortion as the result of Perimetritic Adhesions.* By Dr. Reece.
13. *On the Fatty State of the Liver in Women during Lactation.* By M. de Sinely.

1. In this valuable paper, Professor Turner (reprint from 'Journal of Anatomy and Physiology,' vol. vii) enters at length

into the disputed points of the anatomy of the placenta. His observations are arranged in the following sections: (1) The relations of the maternal blood-vessels to the placenta; (2) the arrangement and structure of the decidua serotina; (3) the minute structure of the villi of the chorion. He specially discusses, under the first head, Braxton Hicks's recent paper in the 'Obstetrical Transactions,' in which it was contended that the so-called sinus system of Hunter does not exist, and that the maternal blood does not pass into the interior of the placenta. Experiments were made on this point by injecting the substances of the placenta, as well as by passing the injection, which consisted of gelatine tinted with carmine, through the uterine arteries and veins. In all the specimens the injections flowed with great ease into the placenta, which swelled up and became tense, as if it passed into a system of pre-existing freely intercommunicating spaces readily and naturally admitting of its diffusion, and not as if it were extravasated into spaces artificially produced by the gentle pressure to which the injection was subjected. When the free edge of the placenta was raised, and the organ carefully separated from the uterus, the injected utero-placental sinuses, with distinct though delicate walls, were torn across, and the injection which they contained was seen to be continuous with that within the intra-placental spaces; at the margin of the placenta also a direct continuity was traced between the injection in the so-called circular sinus and that within the placenta. On examining sections of placentæ hardened in spirit the injection was found to be diffused with great regularity through each cotyledon, so as to surround not only the stems of the villi, but the multitude of bud-like offshoots which branched off from them. Each villus was, as it were, immersed in a bath of coloured gelatine, which occupied the place of the mother's blood, and gave a vivid conception of the mode in which that fluid during life laves the villi of the chorion in which the fœtal capillaries are contained.

The conclusion that the inter-placental spaces exist naturally in the placenta was strengthened by the fact that on examination with the higher powers of the microscope, crowds of red, and some white, blood-corpuscles were seen to occupy the inter-villal spaces imbedded in the coloured gelatine. These, it is contended, were too numerous and too universally found to admit of their presence being explained by the accidental rupture of fœtal vessels as Hicks supposes.

Professor Turner then proceeds to discuss the theory of Weber, Reid, and Goodsir, that the maternal blood-vessels are prolonged into the placenta, and that their lining membrane is reflected on to the chorionic villi, so as not only to ensheath and separate them from the mother's blood, but to form a limiting membrane for the spaces or passages through which the blood circulates. It is concluded that the decidua serotina does not form that complete and continuous membranous septum between the uterus and placenta that has been described by many obstetricians, but that its continuity is in various localities interrupted so as to allow of the penetration of many of the placental villi into the utero-placental vessels.

By a simple experiment the direct continuity of the inter-villal spaces with the canal of the sinuses was shown. An injecting pipe was introduced into one of the cotyledons situated near the edge of a separated placenta, in which the marginal sinus was uninjured, and coloured gelatine was very gently propelled into the inter-villal spaces. The cotyledon swelled up, and the injection flowed both into the marginal sinus at the edge of the placenta and into the sinuses situated in the decidual dissepiments which divided the injected cotyledon from those adjacent to it.

Professor Turner concludes that these observations place the question of the existence of an intra-placental circulation of maternal blood in the category of established and demonstrated facts.

2. Dr. D. Lloyd Roberts relates the particulars of an interesting case in which he operated on July 30, 1872. The tumour had existed for ten years, and had been tapped in 1867 and 1870. On abdominal section a cyst composed of thin homogeneous fibrous membrane, highly vascular, was removed. It contained fourteen pints of clear fluid, feebly albuminous. The tumour had no connection whatever with the uterus, ovaries, or their appendages, which seemed perfectly normal. The author looks upon this as having been originally a non-fecundated ovule, which, having escaped from the Graafian follicle, had been unable to engage in the fimbriated extremity of the Fallopian tube, and had dropped into the peritoneal cavity, becoming from the first adherent to some part entirely distinct from the internal generative organs. Instead of rupture of the wall, and absorption of the contents of the ovule, retrograde changes appear to have subsequently taken place in its interior; the fluid having assumed a dropsical character, the walls, *pari passu* with the increase of fluid, becoming more condensed and of a tougher consistence. In consequence of the absence of a fecundating influence, the nutritive power has been expended in the enormous development of the cell wall and the exudation of fluid.

The author finds an analogy to this theory in the development of the fecundated ovule in cases of extra-uterine peritoneal pregnancy.

3. The author gives the following conclusions as the result of his observations:—1. Changes in the position of the uterus, whether versions or flexions, play an important part in the production of sterility. 2. These conditions of the uterus do not, however, *per se*, produce sterility, which depends upon the extent to which they cause closure of the uterine canal, and impede or entirely prevent the passage of the spermatozoa. 3. Their influence is exclusively mechanical, in so far as they hinder the contact of the semen with the ovum. 4. From this it follows that the removal of the malposition, where it is possible, is the only rational method of curing the sterility.— *Wien. Med. Wochensch.*, No. 12, 1873.

4. The author, in a most interesting and exhaustive paper, illustrated by some well-executed plates, and recording in detail the particulars of more than twenty cases, arrives at the following conclusions:— 1. The placenta may become syphilitic, and there are certain characteristic indications of this. 2. The syphilitic placenta occurs only

in hereditary or congenital syphilis in the fœtus. 3. The seat of the disease is different, according as the mother remains healthy and the syphilitic virus is communicated directly from the father to the ovum by means of the spermatozoa, or according as the mother is diseased. In the former case the affected fœtal villi of the placenta degenerate through cellular granulation, with consecutive obliteration and atrophy of the vessels, complicated frequently by marked proliferation and condensation of the epithelial covering of the villi. 4. In the second case, when the mother is syphilitic, the three following conditions are possible:—(a) The mother, through the act of impregnation, is simultaneously affected with syphilis with the fœtus; diffused syphilis of the placental villi may then develop itself, but primary infection of the maternal parts—endometritis placentaris—is not excluded. (b) The mother has been infected with syphilis before or shortly after conception. The placenta may remain normal, or become diseased under the form of endometritis placentaris gummosa, or (according to Virchow), in a narrower sense, endometritis decidualis. (c) The mother becomes infected only during the latter months of pregnancy (seventh to tenth month). It then generally happens that, in case the father was healthy at the time of impregnation, the fœtus as well as the placenta are exempt from the alterations described above. 5. The infection of the fœtus by passing through the genital canal of the mother is rare, and not yet satisfactorily proved.—*Arch. für Gyn.*, April, 1873.

5. The author states that information as regards the life or death of the fœtus during pregnancy, but especially during parturition, is often of the greatest importance; and where hearing the fœtal heart and feeling the fœtal movements fail or are uncertain, ascertaining the temperature *in utero* will often very materially assist, if not decide us in determining the question. The temperature of the fœtus *in utero* is higher than the maternal temperature; and experience proves that the careful introduction of the thermometer into the uterine cavity, between the membranes and the wall of the uterus is unattended by harm.

We have thus a ready mode of settling the question when it is otherwise doubtful.—*Arch. für Gyn.*, 4, 1872.

6. Hy. Wm. Lebert records the reciprocal influence of these two conditions in thirty-three cases. The influence of pregnancy and parturition upon tuberculosis shows itself most often between the ages of twenty and thirty years, especially between twenty-five and thirty, and next between thirty and forty years. It is exceptional that women who have previously had tubercle resist the influence of repeated pregnancies; their infants are generally feeble and tuberculous.

Pregnancy generally moderates advanced phthisis, but the initiatory phases of tuberculosis follow their course without being moderated during the term of pregnancy.

Abortion, pregnancy, and parturition accelerate in three fourths of the cases the progress of phthisis. Pregnancy has no notable

influence upon the localisation and the form of tuberculosis. The injurious influence of parturition shows itself especially in the cases where pregnancy had favoured the development of tubercle. Women affected with tuberculosis have little milk; they cannot generally nurse; the children are weak, scrofulous, and later, tuberculous.—*Arch. für Gyn.*, iv, 3, 1872.

7. The author draws attention to a new sign of pregnancy in the first three months, which, when observed in conjunction with mammary changes, he considers to be of great value. It consists in the detection of fluctuation in the enlarged uterus, which can easily be made out by bi-manual examination, and which makes us to differentiate *en toto* the uterine enlargement from that produced by tumours or other causes. The best way to feel it was to introduce two fingers into the vagina, while the other hand steadied the womb through the abdominal walls.—*Brit. Med. Jour.*, Aug., 1873.

8. The author explains the frequency of abortions in connection with flexions of the uterus by the theory that they give rise to such alterations in the circulation in the decidua as to produce changes in it which eventually lead to abortion. He gives the details of two cases in which the aborted ovum, in cases of flexion, was very carefully examined. In both changes were observed which led to the diagnosis of chronic inflammation of the decidua. The author believes that this is not an uncommon cause of such miscarriages.—*Edin. Med. Jour.*, Aug., 1873.

9. The author relates an example of one of these unusual cases. The patient was only about six weeks advanced in pregnancy, but from the very commencement of gestation the sickness had been incessant, and the patient was reduced to the last stage of depression. Eventually abortion was produced by means of a laminaria bougie passed into the uterus. Immediately after the ovum was thrown off the vomitings ceased, and the patient was able to take food, but died after the lapse of forty-eight hours from peritonitis.—*Union Méd.*, Dec., 1872.

10. An interesting discussion on this subject took place at the Obstetrical Society, in reference to a paper read by Dr. Meadows. Dr. Barnes's note is written with the view of insisting on the necessity of leaving the placenta intact. Its adhesions and vascular communications are so common and complicated that any attempt at removal is followed by severe and unmanageable hæmorrhage. It is, therefore, suggested as a rule of practice, sanctioned by the experience of all who have practised the operation, that the placenta should be left untouched and the fœtus alone removed.—*Obst. Trans.*, December, 1872.

11. The patient was 27 years of age. She had arrived at term and the child had died in July. The operation was performed in July. No difficulty was experienced in removing the body.

The edge of the wound in the sac was stitched to the edge of the peritoneal wound by a continuous suture, the peritoneal cavity being thus completely closed. The upper half of the wound was closed. A syphon drainage tube was inserted deeply into the

cavity of the pelvis, and the lobule was syringed out every eight hours with a solution of sulphite of soda. The placenta was left untouched, though the cord was removed. The patient made an excellent recovery.

The author insists on the importance of closing the peritoneal cavity, and of leaving the parietal wound open, so as to admit of drainage of the sac and the injection of antiseptics. The operation thus performed has, he says, no greater risks than ovariotomy, and it is certainly preferable to leaving the patient to take her chance of expelling the misplaced fœtus by suppuration.—*Medic. and Chirurg. Soc.*, Feb. 1873.

12. The author relates an interesting case in which abortion resulted from this cause, the adhesions being the result of inflammation following a former pregnancy. The abortion occurred in the fifth month, the liquor amnii first escaping, and then decomposing portions of fœtal structure after the lapse of some days. On examination, the cervix was found to be forming fluid by old-standing adhesions.—*Gaz. Med. de Strasburg*, Jan. 1873.

13. The author states that the fats constantly met with by him during lactation presented a particular disposition, being situated in the rows of cells surrounding the central veins, sometimes gaining the middle parts, and even, though rarely, the cells of the periphery. This localisation of the fats appears to be more limited in women, and in the bitch, than in the herbivora. This disposition is the reverse of that which the author has observed in fatty degenerations and infiltrations of the liver, where the course of the process is from the periphery to the centre of the lobule.—*Comptes Rendus de l'Académie des Sciences.*

Note.—The remainder of the abstract is unavoidably omitted for want of space.

BOOKS, PAMPHLETS, &c., RECEIVED FOR REVIEW.

A System of Midwifery, including the Diseases of Pregnancy and the Puerperal State. By William Leishman, M.D., &c. Glasgow, James Maclehose. 1873. pp. 835.

A Report on the Cerebral Affections of Infancy, with a few Comments and practical Remarks. By Edward Copeman, M.D., &c. Norwich, Fletcher and Co. 1873. pp. 211.

On Marienbad Spa, and the Diseases curable by its Waters and Baths. By A. Victor Jagielski, M.D. London, Trübner and Co. 1873. pp. 185.

A Treatise on the Pneumatic Aspiration of Morbid Fluids. By Dr. George Dieulafoy. London, Smith, Elder, and Co. 1873. pp. 392.

The Retrospect of Medicine. Edited by W. Braithwaite, M.D., and James Braithwaite, M.D. Vol. LXVII, January to June, 1873. London, Simpkin, Marshall, and Co. pp. 408.

Elephantiasis Græcorum, or True Leprosy. By Robert Liveing, M.D. The Gulstonian Lectures for 1873. Revised and Enlarged. London, Longmans and Co. 1873. pp. 150.

Army Medical Department Reports for the year 1871. Vol. XIII. London, 1873. pp. 453.

Clinical and Pathological Observations in India. By J. Fayrer, C.S.I., M.D. London, Churchills. 1873. pp. 648.

Old Medicine and New. By E. W. Lane, M.D. London, Churchills. 1873. pp. 72.

The Mineral Springs of Wiesbaden, and their effect in Disease. By Henry Roth, M.D. Mayence, 1870. pp. 162.

Essays on Diseases of Children. By

W. H. Day, M.D. London, Churchills. 1873. pp. 190.

On Megrim, Sick-Headache, and some Allied Disorders. A contribution to the Pathology of Nerve-Storms. By Edward Liveing, M.D. London, Churchills. 1873. pp. 512, with tables.

A Manual of Midwifery, including the Pathology of Pregnancy and the Puerperal State. By Dr. Karl Schroeder. Translated into English from the Third German Edition by Charles H. Carter, M.D. With 26 engravings. London, Churchills. 1873. pp. 388.

Contributions to Practical Surgery. By G. W. Norris, M.D. Philadelphia, Lindsay and Blakiston. 1873. pp. 318.

Thoughts, Philosophical and Medical, selected from the Works of Francis Bacon. By John Dowson, M.D. London, Lewis. 1873. pp. 80.

Fractures of the Elbow-joint. An Essay by Walter Ela. Cambridge, Welch and Co. 1873. pp. 57.

The West Riding Lunatic Asylum Medical Reports. Edited by J. Crichton Browne, M.D. Vol. III. London, Smith, Elder, and Co. 1873. pp. 349.

On the Curability of Cancer and its Medical Treatment with Surgical Operation. By Dr. G. Von Schmitt. London, Simpkin, Marshall, & Co. 1873. pp. 92.

Insanity in its Relations to Crime. A Text and a Commentary. By W. A. Hammond, M.D. New York, Appleton and Co. 1873. pp. 77.

The Microscopic Structure and Mode of Formation of Urinary Calculi. By H. V. Carter, M.D. London, Churchills. 1873. pp. 50.

The Half-Yearly Abstract of the Medical Sciences. Edited by W. D. Stone, M.D. Vol. LVII, January to June, 1873. London, Churchills. pp. 372.

Tent Life and English Gipsies in Norway. By Hubert Smith. London, H. S. King and Co. 1873. pp. 540.

Lectures on Diseases and Injuries of the Ear, delivered at St. George's Hospital. By W. B. Dalby, F.R.C.S., &c. London, Churchills. 1873. pp. 221.

Body and Mind: an Inquiry into their Connection and Mental Influence, specially in reference to Mental Disorders. An enlarged and revised Edition, to which are added Psychological Essays. By Henry Maudsley, M.D. London, Macmillan and Co. 1873. pp. 342.

A Treatise on the Continued Fevers of Great Britain. By Charles Murchison, M.D. Second Edition. London, Longmans and Co. 1873. pp. 729.

On the Operative Surgery of the Foot and Ankle-joint. By Henry Hancock, F.R.C.S. London, Churchills. 1873. pp. 476.

Twenty-seventh Report of the Commissioners in Lunacy to the Lord Chancellor. 1873. pp. 316.

Fifteenth Annual Report of the General Board of Commissioners in Lunacy for Scotland. 1873. pp. 312.

A Biennial Retrospect of Medicine, Surgery, and their Allied Sciences for 1871-72. Edited for the New Sydenham Society. London, 1873. pp. 542.

Éléments de Chirurgie Clinique comprenant le Diagnostic Chirurgical, les operations, &c. Par J. C. Felix Guyon. Avec 63 figures intercalées dans le Texte. Paris, J. B. Baillière et fils. 1873. pp. 672.

Nouveau Dictionnaire de Médecine et de Chirurgie pratiques, illustré de figures intercalées dans le Texte. Tome XVII. Gros—Hu. Paris, J. B. Baillière et fils. 1873. pp. 798.

La Variole au point de vue epidemiologique et prophylactique. Par Leon Colin. Paris, J. B. Baillière et fils. 1873. pp. 159.

Traité des Sections Nerveuses physiologic pathologique, indications; procédés opératoires. Par E. Létiévant. Paris, J. B. Baillière et fils. 1873. pp. 548.

Leçons sur la Physiologie normale et pathologique du Système nerveux. Par le Dr. Poincaré. Tome I. Paris, J. B. Baillière et fils. 1873. pp. 395.

De l'Inflammation et de la Circulation. Par le Prof. M. Schiff. Paris. J. B. Baillière et fils. 1873. pp. 96.

Quarantaines. Par le Dr. Leon Colin. (Reprinted from the Dictionnaire Encyclopédique des Sciences Médicales). Paris, Masson. 1873. pp. 170.

Pamphlets.

Remarks on New and Old Medicine. By Surgeon-Major W. J. Moore. (Reprinted from 'Indian Medical Gazette.')

Surgical Cases in Relation to Temperature. By Joseph Bell. Edinburgh. 1873.

On the Connection of Bright's Disease with Changes in the Vascular System. With illustrations from the Sphygmograph. By A. L. Galabin, M.D. London, 1873.

General Conclusions as to the Nature of Yellow Fever. By Joseph Jones, M.D. New York, 1873.

A Reply to Dr. H. C. Wood's Review of the Medical Testimony in a Trial for Alleged Poisoning. By P. C. Williams, M.D. Louisville, 1873.

An Examination of Prof. Reese's Review of the Trial for Poisoning. By P. C. Williams, M.D. Baltimore, 1873.

Black Vomit of Yellow Fever. By Joseph Jones, M.D. New York, 1873.

European Child-Life in Bengal. By J. Fayrer, C.S.I., M.D. Churchills. 1873.

The Power of Mind over Matter. By Andrew Ross, M.D. Sydney, 1872.

Jottings on Vitality, or the Physical Basis of Life. By Andrew Ross, M.D. Sydney, 1872.

Typhoid Fever, its Pathology and Treatment. By Andrew Ross, M.D. Sydney, 1873.

On the Physiological Action of Light. By James Dewar and John G. McKendrick, M.D.

Medical Missions. A Paper by C. Valentine, M.D., read at the Allhabad Missionary Conference.

Asylum Notes. By Dr. Yellowlees, Medical Superintendent of the Glamorgan Asylum. 1873.

On the Origin of Cancer. By Alexander Ogston, M.D. (Reprint.) 1873.

Journal of the Scottish Meteorological Society. With Tables for the half-year ending 31st December, 1872. Blackwood and Sons. 1873.

Report of the Sanitary State of the Hackney District for the year 1872. By John W. Tripe, M.D. 1873.

The Reign of Terror. A Letter to the Right Hon. W. E. Gladstone. By G. Crayon.

Observations upon the Treatment of Yellow Fever. By Joseph Jones, M.D. Louisville, 1873.

Snake-bite Curable and Hydrophobia Preventible. By Donald Butter, M.D. London, 1873.

Life and Mind—their Unity and Materiality. By Robert Lewins, M.D. Lewes, 1873.

Thoughts on Nursing. A Paper by E. H. Sieveking, M.D. London, 1873.

A New Operation for Anchylosis of the Elbow-joint, resulting from Fracture. By P. H. Watson, M.D., &c. Edinburgh, 1873.

Some Conclusions in Regard to General Paresis. By H. R. Bigelow. Boston, 1873.

Remarks on the State Medicine Qualification. By H. W. Rumsey, M.D. (Reprint.) 1873.

Reports of the County Asylums of Derby, Glamorgan, Belfast.

Report of the Hospital for the Insane, Gladesville, New South Wales, 1873. Ditto of the State Lunatic Asylum, Utica, New York.

Weekly and Quarterly Returns of the Births and Deaths from the Registrar-General.

Des Asiles d'Aliénés et des Gheels au point de Vue Morale et Économique. Par J. Parigot, M.D. 1873.

Journals.

The Dublin Journal of Medical Science. July to October, 1873.

The Edinburgh Medical Journal. July to October, 1873.

The Journal of Mental Science. July, 1873.

The Birmingham Medical Review; a Quarterly Journal of the Medical Sciences. July, 1873.

The Journal of Anatomy and Physiology. By Drs. Humphry and Turner. Second Series. No. XII. June, 1873.

Food, Water, and Air, in Relation to the Public Health. July to September, 1873.

The Indian Medical Gazette (Monthly). May to August, 1873.

The Food Journal. July to September, 1873.

The Lancet, Medical Times and Gazette Medical Press, Medical Record, and British Medical Journal (Weekly).

The Monthly Mirror. Edited by Robert A. Gunn. New York, August, 1873.

The American Journal of the Medical Sciences. July, 1873.

The Sanitarian; a Monthly Journal. New York, June to August, 1873.

New York Medical Journal. June to September, 1873.

Archives of Scientific and Practical Medicine. Edited by Drs. Brown-Séquard and Seguin. No. 4. 1873.

The Western Lancet; a Monthly Journal. San Francisco. June to September, 1873.

Canada Medical and Surgical Journal. Edited by Dr. G. E. Fenwick. Montreal, June and July, 1873.

The Medical Examiner; a Semi-Monthly Journal. Chicago. July, 1873.

Archives Générales de Médecine. July to October, 1873.

Bulletin Général de Thérapeutique. July to September, 1873.

Gazette de Hebdomadaire. July to October, 1873.

Le Mouvement Médicale. July to October, 1873.

Revue des Sciences Médicales en France, et à l'étranger. (Quarterly.) Tome II. No. 1. July, 1873.

L'Union Médicale. June, 1873.

L'Imparziale Giornale pratico di Medicina. Firenza. June, 1873.

Revista Odontalgica. Periodico Mensual. Madrid. No. 8. July, 1873.

O Correio Medico de Lisboa. May to June, 1873.

Archiv für Pathologische Anatomie und Physiologie. Von R. Virchow. June, July, and August, 1873.

Deutsches Archiv für Klinische Medicin. July, 1873.

Schmidt's Jahrbücher der In- und Auslandischen Gesammten Medecin. June and July, 1873.

Monatsschrift für Ohrenheilkunde. June to August, 1873.

Medicinische Jahrbücher. Redigirt von Stricker. Vienna, 1873.

INDEX TO VOL LII.

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

	PAGE
Abelin on retro-pharyngeal abscess	502
Abortion consequent on endometritis	525
Abortion from perimetritic adhesions	521
Abortion practised for uncontrollable vomiting	525
Abscess, retro-pharyngeal	502
Abstracts, half-yearly	430
Acrid bodies, nature of	493
Air and rain	1
Alcoholic poisoning and testamentary capacity	257
Allbutt's review of Duchenne on electricity	319
Allingham on disease of rectum	437
Amaurosis, strychnia in	496
Ambulances in the army	104
American, marine hospitals	422
Anæsthesia in the insane	441
Anatomical variations	436
Anatomy and Physiology, Journal of, by Humphry	164
Anatomy, comparative, by Chauveau	432
Ancylostomum disease	252
Anderson on Junod's boots in amenorrhœa	519
Aneurisms of cerebral arteries	249
Antagonistic agents to phosphorus poisoning.	256
Antiphlogistic treatment	497
Antozone and ozone	158
Aphasia and disease of anterior cerebral lobes	223
Archives of scientific medicine	150
Armand's treatise on climatology	365
Army hygiene in France	92
Arteries, changes in, from Bright's disease	133
Arterio-capillary fibrosis	134
Asthma, Thorowgood on	423
Astier on uses of mustard	496
Atthill on endometritis	519
Auscultatory sounds, theory of	29
Beale, on disease germs	269
Bergeon on the theory of murmurs	13
Beri-beri, characters of	370
Bigel on sterility	521
Billroth's surgical pathology	167
Bioplastic theory of fever	278
'Birmingham Medical Review'	148
Bluck on renal diseases	424
Blood-changes in relapsing fever	246
Boeck on properties of syphilitic poison	224
Bone-disease in horses	84
Books received for review	266, 526
Bouchut on antiphlogistic treatment	497
Boudet on respiratory murmurs	13
Brain, colloid degeneration of	208
Brain, morbid histology of	199, 485
Brain, sclerosis of	200
Bree on Darwinism	305
Bright's disease, pathology of	132
Browne on nerve phenomena of insane	441
Brown-Séquard's 'Archives of Medicine'	150
Buchheim on acrid bodies	493
Burow on serous cysts	508
Cardiac murmurs, theory of	13
Case of choking by a probang-sponge	239
Caseation, results of	417
——— and tubercle	410
Caseous pneumonia, Lépine on	410
Catalogue, teratological, of the College of Surgeons	163
Catheterisation of ear	344

	PAGE
Causes of epidemics	269
Cerebral aneurisms	249
——— lesion without aphasia	223
Cerebria, Elam on	165
Chapman's neuro-dynamic medicine	138
Chauveau on vascular murmurs	13
Chauveau's comparative anatomy	432
Chemistry of climate	4
Chance on army mortality	92
Chéron on value of oxygenated essences in phthisis	494
Children, surgery of	502
Chisolm on strychnia in amaurosis	496
Cholera in India	365, 376
Chloral in tetanus	512
Christiania, epidemic of measles in	242
——— lying-in hospital	219
Chronicle of medical science	216
Clavicle, dislocation of both ends	512
Climatology	1
———, Armand on	365
Cohnstein on signs of life of fœtus	524
Colloid degeneration of brain	208
Comparative anatomy, by Chauveau	432
Contagion hypotheses	275
Controversy on pathology of Bright's disease	132
Convulsive diseases, hyoscyamia in	495
Convulsions, puerperal	464
Cooper's dictionary of surgery	39
Copeman on puerperal convulsions	464
Copenhagen Lying-in hospital statistics	235
Correio-medico de Lisboa	154
Corrosive sublimate injections in syphilis	224
Cysticercus in neck	508
Dalby's lectures on diseases of the ear	339, 353
Darwin's hypotheses examined	305
Delivery, artificial, lecture on	234
Denmark, factory laws	242
Dental caries, pathology of	37
——— surgery	32
De Sinely on fatty liver during lactation	521
Dietaries, requisites of	65
Diabetes, treated by glycerine	238
Diagnosis of ear diseases	342
Dilatation of cervix uteri	518
Diphtheroid fever	251
Diphtheria in Italy	426
———, thermic cycle of	429
——— treatment of	500
Diseases of the ear	339
——— of the stomach, by Wilson Fox	166
Dislocation of both ends of clavicle	512
Drewitt, on inflammation	46
Dublin Obstetrical Society's proceedings	170
Duchenne on electricity	319
Duncan, Mathews, on dilatation of cervix uteri	518
Ear, anatomy of	339
——— diseases of	339
—, parasite fungi of	221
Elam on cerebria	169
Electricity, Duchenne on	319
——— in cerebral palsies	328
El Amfiteatro Anatomico Español, new journal	154
Emotion, in its effects on life and health	120
Empyema, treatment on	237
Endo-metritis, Atthill on	519
———, and abortion	525
English factory legislation	162
Epidemics, causes of	269
Epidemic measles in Copenhagen	240
Essences, oxygenated, in phthisis	494
Evolution, theory of	309
Extra-uterine fœtation	394
Factory legislation	162
Factory laws of Denmark	242
Factors in the etiology of fever	269
Fallacies of Darwinism	305
Faradism, Duchenne on	322
Faralli on diphtheria	426
——— the sulphites	437
Fatigue, law of, Houghton on	364
Fatty heart, pathology of	247
Fatty liver during lactation	521
Farery on epidemics and puerperal infection	219
Febris diphtheroides	251
Fever-contagion theories	275
——— incubation of	295
——— of diphtheria	429
——— protection from	301
——— relapsing, blood-lesion in	246
Fevers, intermittent, use of the sulphites	437
Fibroid polypus, with inversion	520
Fibrosis, arterio-capillary	134
Field on fibroid polypus	520
Fleming's translation of Chauveau's comparative anatomy	432
Flexions of uterus	519
Fœtus, determination of the life or death of	524
Fœtation, extra-uterine	394
Fœtation, intra-uterine, gastrotomy in	521

PAGE

Food, digestibility of . . . 61
—— Letheby's lectures on . . 57
—— water, and air, by Hassall . 149
Foods, relative value of various . 57
Forensic medicine, report on . 257
Fort Leon le, military surgery . 92
Fox on ozone and antozone . . 158
Fox, Wilson, on tubercle . . 413
———— stomach diseases . 166
Fractures in horses . . . 82
French review of the medical sciences 152
Frey on the microscope . . 156

Gastrotomy, case of . . . 521
Gastrotomy in extra-uterine fœtation 395
Glosso-labial paralysis . . . 332
Glycerine in diabetes . . . 238
Gonorrhœa, latent . . . 520
Goodell on uterine prolapse . . 515
Grimshaw on fever in Dublin . 269
Gruber's handbook of diseases of the ear 339
'Guide to Trefriw Spa' . . . 422
Gull, Sir W., and Dr. Sutton, on pathology of Bright's disease . 132

Hackley's translation of 'Billroth's Surgical Pathology' . . 167
Hæmatocele, retro-uterine . . 516
'Handbook of Medical Information' 160
'Handbook of Therapeutics,' by Ringer 165
Hassall's 'Food, Water, and Air' . 149
Haughton's 'Animal Mechanics' . 354
Hayward's 'Guide to Trefriw Spa' 422
Heart, fatty 247
Heart-muscles, mechanism of . 362
Heiberg on foreign hospitals . 217
Hemiplegia, rarer forms of . . 248
Hicks (B.) on uterine disease . 514
Histology, morbid, of brain . . 485
'Horace, Epistles of,' by Wood . 171
Horses, contagious disease of generative organs of . . . 91
Horses, diseases of . . . 80
Hospitals in Germany and France 217
Hospitals, temporary, in war . 107
'Humphry and Turner's Journal of Physiology' . . . 164
'Hunter's (W.) Lectures' . . 210
Hut hospitals in armies . . 108
Hydrocotarmin, toxicology of . 256
Hydropathy, Lane on . . . 384
Hygiene, military, in France . 92
Hygiene of the wounded in war . 104
Hygienic requisites for an army . 90
Hyoscyamia, action of . . . 495

PAGE

Hyperæsthesia of insane . . 441
Hysterical oliguria and anuria . 250
Hysterical paralysis . . . 335

Imagination, influence of, on body 112
Incubation of fever . . . 295
'Indian Commissioners' Report' . 365
Inflammation, theories of . . 46
Inflammation and tubercle . . 406
Inflammation, microscopic appearances in 243
Influence of mind on body . . 110
Insane, morbid anatomy of brain of the 199
Insane, nerve phenomena in . 441
Intermittents, notes on . . 375
Irritability, muscular, after death 259
Ischmia, hysterical . . . 250
Italian medical journal, 'Lo Sperimentale' 153

Jayakar on cocygeal cysts . . 510
Jewish leprosy 194
Johnson, G., on pathology of Wright's disease . . . 132
'Journal of Anatomy and Physiology' 164

Keratitis, neuralgic . . . 238
Kidd, on vesico-vaginal fistula . 516
Kidney, movable, in women . . 521

Lamarck's teachings . . . 305
Lane's edition of 'Cooper's Dictionary of Surgery' . . . 39
Lane on old medicine and new . 381
Larvæ in nasal passages . . 368
Lebert, tuberculosis and pregnancy 524
Legouest on army surgery . . 92
Leishman's system of midwifery . 389
Leper-houses of Damascus . . 185
Leprosy, cases of 180
——— forms of 179
——— history and pathology of . 173
——— morbid anatomy of . . 178
——— notes on 187
——— specific character of. . 193
——— treatment of . . . 196
Letheby's lectures on food . . 57
Liebig's extract of meat, value of . 69
Light, sanitary influence of . . 262
Limbs, mechanism of the . . 360
Locomotor ataxy 336
'Lo Sperimentale' 153
Lowrie's teratological catalogue of the College of Surgeons . . 163
Liston on auscultation . . . 13
Lying-in hospital, hygiene . . 219

PAGE

Lying-in hospital of Copenhagen statistics 235
Lyons on relapsing or famine fever 269

Macdonald on latent gonorrhœa in females 520
Madness, lectures on . . . 401
Madras, sanitary report. . . 365
Mankind, their origin and destiny. 155
Marine hospital service of America 422
Massage, its application . . 223
Material of fever . . . 272
Materia medica, report on . . 493
Measles, epidemic of, in Copenhagen 240
Meat, characteristics of good . 71
Mechanics, animal, principles of . 354
Mechanism of parturition . . 359
Medical department, army report . 365
——— information, handbook of . 160
——— periodicals, recent . . 14
——— Record 150
——— service in French army . 94
Medicine, archives of, by Brown-Séquard 150
———, old and new . . . 381
———, report on . . . 243
———, Scandinavian, report on 216
Memoir on leprosy in Syria . . 17
Menstruation and smallpox . . 515
Mental faculties in relation to bodily functions . . . 116
Method in therapeutics . . 500
Microscopic appearances in inflammation 243
Microscope, Frey on the . . 156
Midwifery, Leishman on . . 389
Military and Naval Reports . . 365
Mind and body 110
Moore's, J. W., report on Scandinavian medicine . . . 216
Morbid histology of brain and spinal cord 199
Morgan on University oars . . 430
Mortality of troops in India . . 367
Murchison on incubation of fever . 269
Murmurs, cardiac, physiology of . 13
———, varied as to quality . 21
———, respiratory, physiology of 13
———, vascular, physiology of 13
Muscular fibre, mechanism of . 355
——— exertion, measurable by carbonic acid eliminated . . 63
——— irritability after death . 259
Musical murmur, theory of . . 23
Mustard, uses of 496
Myo-sclerotic palsy . . . 335
Myxomatous coccygeal cyst . . 510
Naval and military reports . . 365

PAGE

Neuralgia and kindred diseases . 138
Neuralgic keratitis . . . 238
Neuro-dynamic medicine . . 138
Nerve disorders of the insane, Browne on 441

Obermeier on menstruation in smallpox 515
Obstetrics, report on . . . 514
Odontomes, nature of . . . 35
Oesterlen on pestilences . . 269
Oldham on malaria . . . 269
Organic germ theory of fever . 277
Origin and destiny of mankind . 155
Original communications . . 173
Original notes of Dr. W. Hunter's lectures 210
Otology, Journals of . . . 339
Oulmont on action of hyoscyamia 495
Ovaries, effects of removal of both 518
Ovariotomy 517
Ovula cyst, removal of . . . 521
Oxygenated vapours in phthisis . 494
Ozone and antozone . . . 158

Pain, theory of 141
Paralysis, use of electricity in . 324
Parasitic fungi of ear . . . 221
Parkin on epidemiology . . 269
Parturition, mechanical . . 359
Passions, influence of the, on body 114
Pathology of Bright's disease . 132
Pathology of diphtheria . . 426
Pathology of leprosy . . . 174
Periodicals, recent . . . 148
Pelvis, structure of . . . 390
Periodicity of fever . . . 300
Pettenkofer, on etiology of typhus 269
Phosphorus poisoning, antagonistic agents to 256
Phthisis, oxygenated vapours in . 494
Phthisis, forms of . . . 414
Physical theory of cardiac and other murmurs. . . . 13
Placenta, management of, in gastrotomy 525
Placenta, structure of . . . 521
Placental syphilis 521
Pneumonia and tubercle . . 404
Playfair, report on obstetrics . 514
Poisoning by forms of zinc oxide . 254
Plover on Factory Legislation . 162
Pregnancy, early signs of . . 525
Portuguese medical journal . . 154
Principle of least action . . 362
Principles of animal mechanics . 354
Proceedings of the Dublin Obstetrical Society 170
Progress of veterinary surgery . 72

PAGE

Pseudo-æsthesia of insane . . 441
Puerperal convulsions, Copeman on 464
——— infection, precautions against 219
Pus-cells, origin of . . . 243

Quinine as an oxytoxic . . . 518
——— in abortion with intermittents 517

Rasch, signs of early pregnancy . 525
Recent medical periodicals . . 148
Record, medical, periodical . . 150
Rectal exploration of pelvis . . 520
Rectum, diseases of . . . 437
Reece on abortion . . . 521
Reeves, report on surgery . . 502
Relapsing fever, changes in blood in 246
Renal diseases, Black on . . 424
Report of army medical department 365
Report on health of navy . . 365
——— on hygiene . . . 262
——— on midwifery . . . 514
——— on obstetrics . . . 514
——— on pathology and medicine 243
——— on Scandinavian medicine . 216
——— on surgery . . . 502
——— on toxicology, forensic medicine and hygiene . . . 254
Reproductive organs, diseases of . 424
Respiratory murmurs, theory of . 13
——— sounds, nature of . 27
Retro-pharyngeal abscess . . 502
Retro-uterine hæmatocele . . 516
Révue des sciences médicales . 152
Rheumatismal tetanus . . . 378
Richardson's, B. W., report on toxicology 254
Rindfleisch's pathological histology 169
Ringer's handbook of therapeutics 165
Roberts on removal of ovular cyst 521
——— on urinary diseases . 425
Rowing and health . . . 430

Sanitarian, new monthly journal . 151
Sanitary influence of light . . 262
——— reports of India . . 365
Scandinavian medicine, report on . 216
Schmitz on retro-pharyngeal abscess 505
Sclerosis, disseminated, of brain . 203
——— general, of brain . . 200
——— miliary, of brain . . 204
——— uterine 516
Semple, report on Materia Medica 493
Septicæmia, experiments on . . 245
Serous cysts in neck . . . 508

PAGE

Shampooing, uses of . . . 231
Sheppard on madness . . . 401
Simon on exploration of pelvis per rectum 520
Sims, Marion, on ovariotomy . 517
Smallpox and menstruation . . 515
Smallpox mortality as influenced by vaccination 264
Skene on uterine sclerosis . . 517
Smith, Alex., on fever and cholera 269
Smith, Angus., on air and rain . 1
Spanish new medical journal . 154
Special creation hypotheses . . 309
Spinal cord, histology of . . 485
Squarey on uterine flexions . . 519
Sphygmographic lines in mitral contraction 248
Sterility connected with uterine position 521
Stomach, diseases of, by Wilson Fox 166
Strangles in horses . . . 80
Stricker's Histology . . . 169
Strychnia in amaurosis . . 496
Sulphites in fever . . . 437
Surgeons, College of, teratological catalogue 163
Surgery, dental 32
Surgery, dictionary of, by Cooper. 39
Surgery of armies . . . 92
——— of children . . . 502
——— report on . . . 502
——— veterinary . . . 71
Surgical pathology and therapeutics, by Billroth . . . 167
Sydenham Society's publications . 169
Symptomatology of fever . . 292
Symptoms of beri-beri . . . 370
Syphilis, communication of, by suckling 520
Syphilis, placental . . . 521
Syphilis treated by subcutaneous injections of mercury . . 224
Syphilitic poison, properties of . 224

Tait, Lawson, case of gastrotomy . 521
Temperature in tuberculosis 236, 253
Teratological catalogue . . 163
Tetanic rheumatic state . . 378
Tetanus neonatorum, chloral in . 512
Testamentary capacity and poisoning from uræmia and alcohol . 257
Tetanoid pseudo-paraplegia . . 248
Thermic cycle of diphtheria . . 429
'Therapeutics, Handbook of' . 16
——— method in . . 500
——— report on . . 493
Thorowgood on 'Asthma' . . 423
Tomes's system of dental surgery 32

	PAGE
Toxicology of hydrocotarmia	256
———, report on	254
Transport of sick by railway	107
Treatment of diphtheria	500
Trefriw Spa, Conway	422
Trendelenburg on serous cysts	509
Tropical chlorosis	252
Tropic hyperemia	252
Trousseau's clinical medicine	169
Tubercle, artificial production of	416
'———,' discussion on	405
Tubercle, nature of	405
Tuberculosis, acute	415
——— and pregnancy	524
Tuke, Batty, histology of brain	485
——— on morbid histology of brain and cord	199
Tuke, Hack, Dr., on influence of mind and body	110
Turnbull's manual of diseases of the ear	339
Turner on structure of placenta	521
Tympanum, inflammation of	347
University oars	430
Uremic poisoning and testamentary capacity	257
Urethrocele, vaginal	515
Urinary diseases, Roberts on	425
Uterus, extirpation of	519
Uterine diseases, Hicks on	514
Uterine flexions	519
——— passages, stricture of	518
——— position and sterility	521
——— sclerosis	516
——— prolapse	515
Vaccination, influence of in small-pox	264
Vaginal urethroscele	515
Vaginismus from lead-poison	516
Value attached to Liebig's extract of meat	69
Vascular murmurs, theory of	13
Veine fluid, its nature	15
Vesico-vaginal fistula, treatment of	516
Veterinary surgery, teaching of	75
——— surgery	71
Waring's hospital prayer book	172
Webb's report on pathology and medicine	243
Weber on uterine sclerosis	6
Williams on principles and practice of veterinary surgery	71
Wood's epistles of Horace	171
Wortabet on leprosy in Syria	173
Wounded in war, case of	104
Wunderlich on diagnosis of typhus	269
Zinc oxide fumes, as a poison	254
Zymotic theory of fever	276

END OF VOL LII.

PRINTED BY J. E. ADLARD, BARTHOLOMEW CLOSE.