TRANSACTIONS

OF THE

ACADEMY OF MEDICINE IN IRELAND.

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IN IRELAND.

VOL. IV.

EDITED BY

WILLIAM THOMSON, M.A., F.R.C.S.,

GENERAL SECRETARY;

SURGEON TO THE RICHMOND HOSPITAL, DUBLIN.

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HONORARY FELLOWS.

BILLROTH, PROFESSOR T., Vienna.
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*FLINT, PROFESSOR AUSTIN, New York.
HUTCHINSON, Mr. JONATHAN, F.R.S., London.
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PASTEUR, PROFESSOR, Paris.
SCHRŒDER, PROFESSOR, Berlin.
SIMON, Mr. J., F.R.S., London.
VIRCHOW, PROFESSOR, Berlin.
VON RECKLINGHAUSEN, PROFESSOR, Strasburg.

* Dead.

FELLOWS.

[*The figures appended to Names denote the number of Communications.*]

ABRAHAM, PHINEAS S., F.R.C.S., B.Sc., 40 Peter's Park, Elgin-avenue, London.

ASHE, ST. GEORGE, L.R.C.S., Medical Officer, No. 2 South Dispensary District, 55 Lower Baggot-street.

ATTHILL, LOMBE, M.D. F.K.Q.C.P., late Master Rotunda Lying-in Hospital, 94 Merrion-square, W. [1]

AUCHINLECK, HUGH ALEXANDER, F.R.C.S., Lecturer on Forensic Medicine Carmichael School, 35 York-street, Dublin.

BAKER, ARTHUR WYNDOWE WILLERT, M.B., Ch.M., Surgeon to the Dental Hospital of Ireland, Dental Surgeon to St. Mark's Ophthalmic Hospital, 18 Lower Fitzwilliam-street, Dublin. [1]

BALL, CHARLES BENT, M.D., F.R.C.S., Surgeon Sir P. Dun's Hospital and Maison de Santé, 16 Lower Fitzwilliam-street, Dublin. [1]

BANKS, JOHN T., M.D., F.K.Q.C.P., Physician Richmond, Whitworth, and Hardwicke Hospitals, Regius Professor of Physic, Univ. Dub., Physician in Ordinary to Her Majesty the Queen in Ireland, 11 Merrion-square, East, Dublin.

BARTON, JOHN KELLOCK, M.D., F.R.C.S., Surgeon to the Adelaide Hospital, 24 Lower Fitzwilliam-street, Dublin. [1]

BARTON, JOHN, M.D., Demonstrator of Anatomy, Carmichael College of Medicine, 26 Upper Merrion-street, Dublin.

BAXTER, PATRICK CHARLES, M.B., F.R.C.S., Surgeon St. Joseph's Hospital for Sick Children, Demonstrator of Anatomy, Ledwich School, 66 Lower Baggot-street, Dublin.

BEANEY, Hon. JAMES GEORGE, M.D., F.R.C S. Edin., Surgeon Melbourne Hospital, 154 Collins-street, Melbourne.

BEATTY, JOSEPH, F.R.C.S., Surgeon Monkstown Hospital, 93 Upper George's-street, Kingstown.

BEATTY, WALLACE, M.B., Assistant Physician Adelaide Hospital, Medical Officer Throat and Ear Hospital, 21 Lower Leeson-street, Dublin. [3]

BENNETT, EDWARD HALLARAN, M.D., F.R.C.S., Surgeon to Sir P. Dun's Hospital, Professor of Surgery Trinity College, 26 Lower Fitzwilliam-street, Dublin. [3]

BENSON, ARTHUR HENRY, F.R.C.S., Ophthalmic Surgeon City of Dublin Hospital, Assistant Surgeon St. Mark's Ophthalmic Hospital, Lecturer on Ophthalmic and Aural Surgery Ledwich School, 42 Fitzwilliam-square, W., Dublin. [1]

BENSON, J. HAWTREY. M.D., F.K.Q.C.P., Physician City of Dublin Hospital, 57 Fitzwilliam-square, N., Dublin.

BERNARD, WALTER, F.K.Q.C.P., 14 Queen-street, Londonderry.

BIGGER, S. LENOX L., F.R.C.S., 8 Harcourt-street, Dublin.

BOYD, MICHAEL AUSTIN, F.R.C.S., Physician Mater Misericordiæ Hospital, 30 Merrion-square, North, Dublin. [2]

BRADY, JAMES, M.B., L.K.Q.C.P., 63 Harcourt-street, Dublin.

BROOKS, H. ST. JOHN, M.B., Demonstrator of Anatomy, Trinity College, 22 York-street, Dublin. [2]

BROOMFIELD, HUMPHREY JOHN, F.R.C.S., Demonstrator of Anatomy Carmichael College, 30 York-street, Dublin.

BROWNE, J. WALTON, A.B., M.D., Surgeon to the Royal Hospital, 10 College-square, North, Belfast.

BROWNE, ROBERT, F.R.C.S., Physician Maison de Santé, Large House, Rathmines-road, Dublin.

BURGESS, JOHN J., L.R.C.S., Assistant Surgeon to the Richmond Hospital, 116 James's-street, Dublin.

BURKE, JOHN RICHARD, M.D., Deputy Inspector-General, Hospitals and Fleets, R.N., Assistant Medical Superintendent, Criminal Lunatic Asylum, Dundrum.

BYRNE, JOHN AUGUSTUS, L.R.C.S., M.B., Gynæcologist to St. Vincent's Hospital, Professor of Medicine and Diseases of Women, Catholic University, 21 Merrion-square, North.

CAMERON, SIR CHARLES A., M.D., F.R.C.S., Professor of Chemistry Royal College of Surgeons, Superintendent Medical Officer of Health, City and County Analyst, Dublin, 15 Pembroke-road, Dublin.

CHANCE, ARTHUR, L.R.C.S., L.K.Q.C.P., Surgeon to Mater Misericordiæ Hospital, 15 Westland-row.

CHAPMAN, JOHN HENRY, F.K.Q.C.P., Physician to Hospital for Incurables, Medical Officer Donnybrook Dispensary, South Dublin Union, 122 Pembroke-road, Dublin.

COLLES, WILLIAM, M.D., F.R.C.S., Surgeon-in-Ordinary to the Queen in Ireland, Regius Professor of Surgery Univ. Dub., Surgeon Dr. Steevens' Hospital, 21 Stephen's-green, N., Dublin.

COOPER, AUSTIN U., L.R.C.S., British Hospital, Buenos Ayres.

COPPINGER, CHARLES, F.R.C.S., Surgeon to the Mater Misericordiæ Hospital, 11 Upper Merrion-street, Dublin.

CORBETT, DANIEL, M.R.C.S., Surgeon-Dentist in Ordinary to the Lord Lieutenant, 12 Clare-street, Dublin.

CORBETT, DANIEL, Jun., F.R.C.S., Surgeon Dental Hospital, Ireland, and Mater Misericordiæ Hospital, 12 Clare-street, Dublin.

COWEN, GEORGE, M.B., 78 Pembroke-road, Dublin.

COX, MICHAEL F., M.K.Q.C.P., Physician St. Vincent's Hospital.

CORLEY, ANTHONY HAGARTY, M.D., and Vice-President and Fellow R.C.S., Surgeon to Richmond, Whitworth, and Hardwicke Hospitals, Lecturer on Surgery Carmichael School of Medicine, 30 Lower Baggot-street, Dublin.

COSGRAVE, E. MACDOWEL, M.D., Physician to Whitworth Hospital, Drumcondra, Demonstrator of Anatomy Ledwich School, 24 Gardiner's-place, Dublin.

CRANNY, JOHN JOSEPH, M.D., F.R.C.S., Surgeon to Jervis-street Hospital, 17 Merrion-square, N., Dublin.

CROLY, HENRY GRAY, F.R.C.S., Surgeon City of Dublin Hospital, 7 Merrion-square, N., Dublin.

CROLY, ALBERT, L.R.C.S., Medical Officer Rathfarnham and Whitechurch Dispensary, Greenfield House, Rathfarnham, Co. Dublin.

CRONYN, JOHN G., L.R.C.S., L.K.Q.C.P., Medical Officer South Dublin Union Workhouse, 4 Clare-street, Dublin.

CRUISE, FRANCIS RICHARD, M.D., F. & Pres. K.Q.C.P., Consulting Physician Mater Misericordiæ Hospital, 93 Merrion-square, W., Dublin. [1]

CUNNINGHAM, D. J., F.R.C.S., F.R.S. Ed., Professor of Anatomy, University of Dublin, 69 Harcourt-street.

DENHAM, J. KNOX, L.K.Q.C.P., Medical Officer to Donnybrook Dispensary, 25 Northumberland-road, Dublin.

DIGGES, WILLIAM HENRY, L.R.C.P. Edin., 3 Lower Sackville-street, Dublin.

DILL, ROBERT F., M.D., Professor of Midwifery Queen's College, 3 Fisherwick-place, Belfast. [1]

DONNELLY, THOMAS, M.B., Medical Officer No. 1 East Dispensary District, North Dublin Union, 67 Amiens-street, Dublin.

DOYLE, JOSEPH PATRICK, L.K.Q.C.P., 72 Lower Mount-street, Dublin.

DUFFEY, GEORGE FREDERICK, M.D., F.K.Q.C.P., Physician City of Dublin Hospital, Lecturer on Materia Medica Carmichael College, 30 Fitzwilliam-place, Dublin. [1]

DWYER, F. CONWAY, M.B., Surgeon Jervis-street Hospital.

ELLIS, GEORGE, F.R.C.S., M.B., 91 Lower Leeson-street, Dublin.

EUSTACE, JOHN, M.D., L.R.C.S., Resident Physician Hampstead, Highfield, and Elmhurst, Lunatic Asylum, Drumcondra, Co. Dublin.

EVANS, WILLIAM R., M.D., L.R.C.S., 29 Molesworth-street, Dublin.

FINNY, JOHN MAGEE, M.D., F.K.Q.C.P., Physician Sir P. Dun's Hospital, King's Professor of Practice of Medicine, School of Physic, 19 Lower Baggot-street, Dublin. [3]

FOTTRELL, WM. JOSEPH, L.R.C.S., Resident Surgeon Jervis-street Hospital.

FITZGERALD, CHARLES E., M.D., Surgeon-Oculist in Ordinary to the Queen in Ireland, Surgeon to National Eye and Ear Infirmary, 27 Upper Merrion-street, Dublin.

FITZGIBBON, HENRY, F.R.C.S., Surgeon City of Dublin Hospital, 18 Lower Fitzwilliam-street, Dublin. [1]

FITZPATRICK, THOMAS, M.D., 31 Lower Baggot-street, Dublin.

FLEMING, R. H., M.B., Assistant Physician Rotunda Hospital, Dublin.

Fletcher, Robert Vicars, F.R.C.S., Resident Medical Superintendent District Lunatic Asylum, Ballinasloe, Co. Galway.

Flinn, David Edgar,. F.R.C.S., Surgeon St. Michael's Hospital, Kingstown, Mount Clarence, Kingstown. [1]

Foot, Arthur Wynne, M.D., F.K.Q.C.P., Physician Meath Hospital, Professor of Medicine Royal College of Surgeons, 49 Lower Leeson-st., Dublin. [2]

Foy, George Mahood, F.R.C.S., Surgeon Whitworth Hospital, Drumcondra, 80 Lower Gardiner-street, Dublin.

Franks, Kendal, M.D., F.R.C.S., Surgeon to Adelaide Hospital, Surgeon Throat and Ear Hospital, 69 Fitzwilliam-square, Dublin. [2]

Frazer, William, F.R.C.S., 20 Harcourt-street, Dublin.

Freeman, William Henry, L.R.C.P. Lond., F.R.C.S., Surgeon Royal United Hospital, Bath, 24 Circus, Bath.

Garland, John, L.R.C.S., 23 Arran-quay.

Gogarty, Henry Joseph Kelly, F.R.C.S., 5 Rutland-square, E., Dublin.

Gordon, Samuel, M.D., F.K.Q.C.P., Physician to the Richmond, Whitworth, and Hardwicke Hospitals, 13 Hume-street, Dublin.

Greene, Thomas Wm. Nassau, L.R.C.S., Monte Video, South America.

Grimshaw, Thomas Wrigley, M.D., F.K.Q.C.P., Registrar-General for Ireland, Priorsland, Carrickmines, Co. Dublin.

Hamilton, Edward, M.D., F.R.C.S., Surgeon Steevens' Hospital, Professor of Surgery Royal College of Surgeons, 120 Stephen's-green, W., Dublin.

Hamilton, J. Butler, M.D., Brigade-Surgeon A.M.D., Lucknow.

Harley, Robert William, L.R.C.S., 21 Pembroke-road, Dublin.

Haughton, Samuel (Clk.), M.D., F.K.Q.C.P., F.R.S., School of Physic, Trinity College, Dublin.

Hayes, Patrick Joseph, F.R.C.S. Edin., Surgeon Mater Misericordiæ Hospital, Lecturer on Surgery in the Catholic University School of Medicine, 18 Merrion-square, Dublin. [1]

Hayes, Richard Atkinson, M.D., F.R.C.S., Physician Steevens' Hospital, Physician for Diseases of Throat, National Eye and Ear Infirmary, 32 Merrion-square, South, Dublin. [1]

Head, Henry Haswell, M.D., F.K.Q.C.P., Physician to the Adelaide Hospital, 7 Fitzwilliam-square, Dublin.

Hearn, Richard Thomas, M.B., L.R.C.S., Medical Officer Rathmines Dispensary, South Dublin Union, 7 Belgrave-square, East, Rathmines, Dublin.

Hemphill, William D., M.D., F.R.C.S., Surgeon Co. Tipperary Gaol, South Riding, Visiting Physician Clonmel District Lunatic Asylum, Oakville, Clonmel, Co. Tipperary.

Henry, Richard, M.D., L.R.C.S., late Assistant Physician Rotunda Hospital, Dundrum.

Hepburn, William Joseph, F.R.C.S. Edin., Surgeon Meath Hospital and Co. Dublin Infirmary, 31 Upper Merrion-street, Dublin.

Heuston, Francis Tydd, F.R.C.S., Lecturer on Anatomy Carmichael College of Medicine and Surgery, 21 Harcourt-street, Dublin. [1]

Horne, Andrew John, L.K.Q.C.P., late Assistant Physician Rotunda Hospital, 24 Harcourt-street, Dublin.

Jacob, David, M.D., F.R.C.S., Surgeon Queen's County Infirmary, Visiting Physician Maryborough District Lunatic Asylum, Surgeon Queen's County Prison, Maryborough, Queen's Co.

Jennings, Ulick A., Brigade Surgeon, M.S., Retired, 5 Lower Fitzwilliam-street.

Jex-Blake, Sophia, M.D., M.K.Q.C.P., Lecturer on Hygiene London School of Medicine for Women, Bruntsfield Lodge, Whitehouse Loan, Edinburgh.

Jones, Henry Macnaughton, M.D., F.R.C.S., 141 Harley-st., London.

Kelly, J. Bellew, M.D., F.R.C.S., 27 Lawrence-street, Drogheda.

Kennedy, Henry, M.B., F.K.Q.C.P., Physician Whitworth Hospital, Drumcondra, and Simpson's Hospital, 3 Rutland-square, Dublin.

Kennedy, Hugh, L.R.C.S., 1 Gardiner's-place, Dublin.

Kenny, Joseph E., M.P., L.R.C.P. Edin., Medical Officer Dublin Union Workhouse, 15 Rutland-square, Dublin.

Kidd, Fred. W., L.R.C.S., Assistant-Master Coombe Hospital, 43 Harcourt-street, Dublin.

Kidd, George Hugh, F.R.C.S., M.D., late Master and Obstetric Surgeon Coombe Lying-in Hospital, Consulting Obstetric Surgeon to the House of Industry Hospitals, 30 Merrion-square, South, Dublin.

Kinkead, Richard John, M.D., L.R.C.S., Lecturer on Medical Jurisprudence, Professor of Obstetric Medicine, Queen's College, Galway, West House, Galway.

Kilkelly, Charles J., F.R.C.S., Deputy Surgeon-General, Indian Army, 12 Upper Fitzwilliam-street, Dublin.

Kirkpatrick, John Rutherford, F.K.Q.C.P., Professor of Midwifery, T.C.D., 4 Upper Merrion-street, Dublin.

Knapton, George, L.R.C.P. Ed., 11 Hogton-street, Southport.

Knight, Charles Frederick, M.D., Demonstrator of Anatomy Ledwich School, Physician Mercer's Hospital, 82 Harcourt-street, Dublin.

Knott, John Freeman, F.R.C.S., Demonstrator of Anatomy, School of Surgery, 34 York-street, Dublin.

Kough, Edward, M.A., M.B., Rhosygar, Monkstown, Co. Dublin.

Lane, John Lilly, M.B., late Assistant Physician Rotunda Hospital, 37 Lower Baggot-street, Dublin.

Lapper, Edwin, L.K.Q.C.P., Lecturer on Chemistry Ledwich School of Medicine, 2 Knapton-villas, Kingstown.

Ledwich, Edward L'E., L.R.C.S., Lecturer on Anatomy, and on Practical Physiology and Institutes of Medicine, Ledwich School of Medicine, Harcourt-street, Dublin.

b

Lentaigne, John Vincent, F.R.C.S., Surgeon Mater Misericordiæ Hospital, Demonstrator of Anatomy Carmichael College, 29 Westland-row, Dublin.

Little, James, M.D., F.K.Q.C.P., Physician Adelaide Hospital, 14 Stephen's-green, North, Dublin. [2]

Little, Thomas Evelyn, M.D., L.R.C.S., University Anatomist, and Surgeon to Sir Patrick Dun's Hospital, 42 Great Brunswick-street, Dublin.

Lush, William Vaudrey, M.D., M.R.C.P., F.R.C.S. Eng., Physician to the Dorset County Hospital, 12 Frederick-place, Weymouth.

Macan, Arthur V., M.B., M.A.O., F.K.Q.C.P., Master Rotunda Hospital, Dublin.

M'Ardle, John Stephen, L.R.C.S., Surgeon St. Vincent's and the Mullen Convalescent Home, 7 Merrion-street, Upper, Dublin.

MacCormac, Sir William, M.A., F.R.C.S., Surgeon to St. Thomas's Hospital, 13 Harley-street, London.

M'Cullagh, John, L.R.C.S., Demonstrator of Anatomy, Catholic University, Ireland, 68 Mountjoy-square, West, Dublin.

McDonnell, John, M.D., F.R.C.S., 32 Upper Fitzwilliam-street, Dublin.

McDonnell, Robert, M.D., Surgeon Steevens' Hospital, Surgeon Jervis-street Hospital, 89 Merrion-square, West, Dublin.

M'Keown, William A., M.D., Surgeon Eye and Ear Infirmary, 20 College-square, East, Belfast. [1]

M'Laren, Agnes, M.D., L.K.Q.C.P., Physician Canongate Medical Mission Dispensary, 26 York-place, Edinburgh.

M'Mordie, Wm. K., M.D., Physician Samaritan Hospital for Women and Children, Belfast.

MacSwiney, Stephen Myles, M.D., F.K.Q.C.P., Physician Jervis-street Hospital, Professor of Medical Jurisprudence Catholic University, 9 Upper Merrion-street, Dublin.

M'Veagh, John Francis, M.D., Physician to Reformatory High Park, and Children's Hospital, 1 Rutland-square, East, Dublin.

Madden, Thomas More, M.D., F.R.C.S., M.K.Q.C.P., Obstetric Physician Mater Misericordiæ Hospital, Physician to St. Joseph's Hospital for Sick Children, 55 Merrion-square, South, Dublin. [3]

Manly, B. C., L.R.C.S., Aberdeen House, Clyde-road, Dublin.

Mapother, Edward Dillon, M.D., F.R.C.S., Consulting Surgeon St. Vincent's Hospital, Consulting Medical Officer of Health City of Dublin, Professor of Physiology Royal College of Surgeons, 6 Merrion-square, North, Dublin.

Marquees, Laurence, L.R.C.S., Assistant-Surgeon Civil Hospital, Hong Kong, Pereira, Macao, China.

Martin, James, F.R.C.S., Medical Officer Portlaw Dispensary, Carrick-on-Suir Union, Medical Officer Portlaw Fever Hospital, Portlaw, Co. Waterford. [1]

Martin, William James, M.D., F.R.C.P. Edin., Physician Jervis-street Hospital and St. Joseph's Infirmary for Children, 17 Harcourt-street, Dublin.

Mason, Samuel Robert, M.B., F.R.C.S., Master Coombe Hospital, Lecturer on Midwifery Ledwich School, 92 Harcourt-street, Dublin.

Mason, Thomas Peter, M.B., F.R.C.S., Physician Mercer's Hospital, Lecturer on Anatomy Ledwich School, 74 Harcourt-street, Dublin.

Maxwell, Patrick W., M.B., Ophthalmic Surgeon Steevens' Hospital, Assistant Surgeon National Eye and Ear Infirmary, 16 Warmington-place.

Meldon, Austin, F.R.C.S., Surgeon Jervis-street Hospital, 15 Merrion-square, North, Dublin.

Middleton, William Henry, L.R.C.S., Surgeon Westmeath Co. Infirmary, Mullingar, Co. Westmeath.

Minchin, Humphry, M.B., F.R.C.S., Medical Officer North Dublin Union Workhouse, Surgeon City of Dublin Prisons, 56 Lower Dominick-street, Dublin.

Molony, John, M.K.Q.C.P., Master St. Patrick's Hospital for Lunatics, Dublin.

Montgomery, Robert, M.R.C.S. Eng., 4 Gardiner's-row, Dublin.

Montgomery, Alexander Nixon, M.K.Q.C.P., Secretary Vaccination Department, Local Government Board, 45 Upper Sackville-street, Dublin.

Moore, Charles Frederick, M.D., F.R.C.S., Medical Officer No. 3 Dispensary, South Dublin Union, 10 Upper Merrion-street, Dublin. [1]

Moore, John William, M.D., F.K.Q.C.P., Physician Meath Hospital, Physician Cork-street Fever Hospital, 40 Fitzwilliam-square, West, Dublin. [2]

Moore, Robert Henry, F.R.C.S., Surgeon-Dentist-in-Ordinary to His Excellency the Lord Lieutenant, 29 Upper Merrion-street, Dublin.

Moore, William, M.D., F.K.Q.C.P., Physician to Her Majesty the Queen and to Sir Patrick Dun's Hospital, 67 Fitzwilliam-square, North, Dublin.

Murphy, John, L.R.C.S., Assistant Physician Mater Misericordiæ Hospital, 14 Gardiner's-place, Dublin.

Murphy, John Joseph, L.R.C.P. Ed., 18 Harcourt-street, Dublin.

Neary, Bernard John, L.K.Q.C.P., Medical Officer Howth and Baldoyle Dispensaries, North Dublin Union, Howth, Dublin.

Neville, William Cox, M.D., M.A.O., late Assistant Physician Coombe Lying-in Hospital, 71 Lower Baggot-street, Dublin. [2]

Newell, F. T. Porter, L.R.C.S., Resident Surgeon Meath Hospital.

Nixon, Christopher J., M.D., F.K.Q.C.P., Physician to Mater Misericordiæ Hospital, Lecturer on Anatomy and Physiology Catholic University, 2 Merrion-square, N., Dublin. [2]

Nixon, Frederick Alcock, F.R.C.S., Surgeon Mercer's Hospital, 33 Harcourt-street, Dublin.

Nolan, Andrew O'Kelly, F.R.C.S., Gort, Co. Galway.

Norman, Connolly, F.R.C.S., Medical Superintendent Richmond District Lunatic Asylum, Dublin.

Norton, John J., L.K.Q.C.P., Emly, Tipperary.

NUGENT, GUY PERCIVAL L'ESTRANGE, M.B., Physician Richmond, Whitworth, and Hardwicke Hospitals, Demonstrator of Anatomy School of Physic, Trinity College, 4 Upper Mount-street, Dublin.

O'CARROLL, JOSEPH FRANCIS, M.B., L.R.C.S., Assistant Physician Richmond, Whitworth, and Hardwicke Hospitals, 77 Harcourt-street, Dublin.

O'GRADY, EDWARD STAMER, F.R.C.S., Surgeon Mercer's Hospital, 33 Merrion-square, North, Dublin.

O'KEEFE, PATRICK, M.D., Resident Medical Officer Mountjoy Prison, Dublin.

OLPHERTS, J. WYBRANTS, M.D., Medical Officer Downpatrick Dispensary District, The Villas, Downpatrick.

O NEILL, WILLIAM, M.D., M.R.C.P. Lond., Physician Lincoln Lunatic Hospital, Silver-street, Lincoln.

ORMSBY, LAMBERT HEPENSTAL, F.R.C.S., Surgeon Meath Hospital, Surgeon National Orthopædic and Children's Hospital, 4 Merrion-square, West, Dublin.

O'SULLIVAN, STEPHEN, M.D., F.R.C.S., Professor of Surgery, Queen's College, Cork, Surgeon North Infirmary, 6 Camden-place, Cork.

OULTON, HENRY W., M.D., L.R.C.S., 6 North Frederick-street.

PALMER, JOSEPH MANSERGH, F.R.C.S., Surgeon Armagh County Infirmary, Co. Infirmary, Armagh.

PATTON, ALEXANDER, M.B., Resident Physician Farnham House and Maryville Private Lunatic Asylum, Farnham House, Finglas, Dublin.

PEARSALL, WILLIAM BOOTH, F.R.C.S., 13 Upper Merrion-street, Dublin.

PECHEY, MARY EDITH, M.D., L.K.Q.C.P., Cumballa Hill, Bombay.

PETIT, JOSEPH, L.K.Q.C.P., Medical Superintendent District Lunatic Asylum, Sligo.

POOLE, J. SEALEY, M.D., late Assistant-Master Coombe Lying-in Hospital, Dublin, 24 College-gardens, Belfast.

POLLOCK, JAMES FERRIER, M.D., F.K.Q.C.P., Medical Officer Blackrock Dispensary, Rathdown Union, and Meath Industrial Schools, Avoca House, Blackrock.

POPE, FREDERICK ALEXANDER, M.B., Surgeon Dublin Throat and Ear Hospital, 63 Pembroke-road, Dublin.

PORTER, SIR GEORGE HORNIDGE, M.D., F.R.C.S., Surgeon-in-Ordinary to the Queen in Ireland, Surgeon Meath Hospital, 3 Merrion-square, Nth., Dublin.

POTTER, HRNRY, F.R.C.S., Brigade Surgeon Indian Army, 33 Belgrave-square, Rathmines, Dublin.

POWELL, GEORGE DENNISTON, M.D., 76 Upper Leeson-street, Dublin.

PRATT, JOSEPH DALLAS, M.B., Medical Registrar, City of Dublin Hospital, 25 Lower Fitzwillian-street, Dublin.

PURCELL, THOMAS, M.K.Q.C.P., Medical Officer and Medical Officer of Health, No. 1 South City Dispensary, South Dublin Union, 71 Harcourt-street, Dublin.

PUREFOY, RICHARD DANCER, F.R.C.S., Obstetrical Surgeon Adelaide Hospital, 13 Merrion-square, Dublin.

PURSER, JOHN MALET, M.D., F.K.Q.C.P., Professor of Institutes of Medicine in the School of Physic, Physician Sir Patrick Dun's Hospital, 3 Wilton-terrace, Dublin. [2]

QUINLAN, FRANCIS JOHN BOXWELL, M.D., F.K.Q.C.P., Physician St. Vincent's Hospital, Professor of Materia Medica and Therapeutics Catholic University, 29 Lower Fitzwilliam-street, Dublin.

REDMOND, JOSEPH MICHAEL, F.K.Q.C.P., Physician to Mater Misericordiæ Hospital, Lecturer on the Practice of Medicine and Pathology in the Ledwich School of Medicine, 8 Clare-street, Dublin.

ROE, WILLIAM, M.D., F.R.C.S., Obstetric Physician National Lying-in Hospital, Professor of Midwifery Royal College of Surgeons, 13 Lower Fitzwilliam-street, Dublin.

ROWLES, GEORGE, L.K.Q.C.P., 21 Richmond-hill, Rathmines.

SCOTT, CHARLES MASON, L.R.C.S., Rockingham, Kingstown, Co. Dublin.

SCOTT, JOHN ALFRED, L.K.Q.C.P., Lecturer on Physiology, Carmichael College, 25 Idrone-terrace, Blackrock, Dublin.

SCOTT, WILLIAM, M.D.. F.R.C.P., Edin., Physician to Aughnacloy Hospital, Aughnacloy, Co. Tyrone.

SHANNON, PETER, M.D., F.R.C.S., 91 Stephen's-green, South, Dublin.

SMITH, WALTER GEORGE, M.D., F.K.Q.C.P., King's Professor of Materia Medica School of Physic, and Physician to Sir Patrick Dun's Hospital, 34 Lower Baggot-street, Dublin. [2]

SMYLY, PHILIP CRAMPTON, M.D., F.R.C.S., Surgeon Meath Hospital, Surgeon to the Hospital for Throat and Ear Diseases, 4 Merrion-square, North, Dublin.

SMYLY, WILLIAM JOSIAH, M.D., F.R.C.S., Obstetric Physician City of Dublin Hospital, 35 Upper Fitzwilliam-street, Dublin. [2]

STACK, RICHARD THEODORE, D.M.D., Harvard, F.R.C.S., Dental Surgeon Adelaide Hospital, Surgeon to Dental Hospital of Ireland, 10 Westland-row, Dublin.

STOKER, WILLIAM THORNLEY, M.D., F.R.C.S., Surgeon Richmond Hospital, Surgeon Swift's Hospital for Lunatics, Professor of Anatomy Royal College of Surgeons, Ireland, 16 Harcourt-street, Dublin. [2]

STOKES, SIR WILLIAM, President and Fellow R.C.S., Surgeon Richmond Surgical Hospital, Professor of Surgery Royal College of Surgeons, 5 Merrion-square, North, Dublin. [1]

STORY, JOHN BENJAMIN, M.B., F.R.C.S., Surgeon St. Mark's Ophthalmic Hospital, 24 Lower Baggot-street, Dublin. [1]

SWAN, ROBERT LAFAYETTE, F.R.C.S., Surgeon Dublin Orthopædic Hospital, 31 Molesworth-street, Dublin.

SWANZY, HENRY ROSBOROUGH, M.B., F.R.C.S., Surgeon National Eye and Ear Infirmary, Dublin, Ophthalmic Surgeon Adelaide Hospital, 23 Merrion-square, North, Dublin.

Tate, Davis D., M.D., Resident Medical Officer, North Dublin Union.

Tait, Lawson, F.R.C.S., England, Surgeon Birmingham and Midland Hospital for Women, 7 The Crescent, Birmingham.

Thomson, William, F.R.C.S., Surgeon Richmond Hospital, 34 Harcourt-street, Dublin.

Torney, Thomas, M.D., 8 Blackhall-street, Dublin.

Tobin, Richard Francis, F.R.C.S., Surgeon St. Vincent's Hospital, Stephen's-green, Dublin. [1]

Truell, H. P., M.B., J.P., Clonmannon, Ashford, Co. Wicklow.

Tweedy, Henry Colpoys, M.D., F.R.C.S, Physician Steevens' Hospital, 2 Gardiner's-row, Dublin. [1]

Usher, Isaac William, L.R.C.S., Medical Officer Dundrum No. 1 Dispensary, Rathdown Union, Tudor House, Dundrum, Co. Dublin.

Ward, Montgomery Albert, M.D., F.R.C.S., Surgeon to Mercer's Hospital, Physician to Maison de Santé, 9 Rathmines-road, Co. Dublin.

Wharton, James H., M.B., F.R.C.S., Surgeon Meath Hospital, Hospital for Incurables, 28 Upper Merrion-street, Dublin.

Wheeler, William Ireland, M.D., F.R.C.S., Surgeon City of Dublin Hospital, 27 Lower Fitzwilliam-street, Dublin. [1]

White, Esmonde, Surgeon-Major, M.S., 30 Northumberland-road, Dublin.

Williams, David Mark, L.K.Q.C.P., M.R.C.S.E., Physician Liverpool Hospital for Consumption and Diseases of the Chest, 63 Shaw-street, Liverpool.

Wright, Edward Perceval, M.D., F.R.C.S., Professor of Botany and Keeper of Herbarium Dublin University, Kilrock House, and Howth, 5 Trinity College, Dublin.

Wright, W. McDowel Aikin, M.B., L.R.C.S., Medical Officer Killiney Dispensary, Rathdown Union, 6 Ulverton-place, Dalkey, Co. Dublin.

Yourelle, M. J., M.K.Q.C.P., 42 Morehampton-road.

MEMBERS.

Atock, H., Royal Canal House, Broadstone.

Bewley, Henry, M.B., 31 Trinity College, Dublin.

Boyce, Jos. W., M.B., Medical Officer Stillorgan Dispensary District, Co. Dublin.

Davys, Frank J., F.R.C.S., Medical Officer Swords and Donabate Dispensary and Constabulary, Swords, Co. Dublin.

Delahoyde, J. O'Connell, L.R.C.S., Medical Officer No. 2 District, North Dublin Union, 55 Rutland-square, Dublin.

Dillon, Paul Robert, 3 Eccles-street.

Eccles, Cuthbert, L.R.C.S., St. Cuthbert's Rectory, Drogheda.

Falkiner, Ninian M., M.B., Lecturer on Chemistry Carmichael School of Medicine, 45 Lower Dominick-street, Dublin.

Ferguson, H. L., F.R.C.S., New Zealand, care of M. Ferguson, Esq., 81 James's-street, Dublin.

Fetherston, R. H., 9 Gladstone-terrace, The Meadows, Edinburgh.

Gibbs, Richard, L.R.C.S., Medical Officer Coolock Dispensary District, Winstonville, Fairview, Co. Dublin.

Gordon, S. T., M.B., The Depot, Phœnix Park, Dublin.

Hadden, David R. H., L.R.C.S., 8 Castlewood-avenue.

Harvey, Joshua R., M.D., 62 Lower Baggot-street, Dublin.

Kenna, Denis P., L.R.C.S., 114 Great Britain-street.

Kennedy, J. M. Prior, L.R.C.S., Richmond Hospital.

Long, P. W., M.D., Assistant Medical Officer, Dublin Metropolitan Police, 118 Stephen's-green, Dublin.

M'Dermott, J., L.R.C.S., Kingstown.

M'Dowell, Francis Victor, M.B., F.R.C.S., Surgeon Baltinglass Union Infirmary, St. Kevin's, Baltinglass.

Morrogh, George, M.D., United Service Club, Dublin.

Myles, Wm. Zachary, Assistant Medical Superintendent, Richmond Lunatic Asylum, Dublin.

Myles, Thomas W., L.R.C.P. Ed., Resident Apothecary, House of Industry Hospitals, Dublin.

O'Flaherty, Richard George, 101 Upper George-street, Kingstown.

O'Neill, E. J., 6 Cavendish-row.

Owens, Sir George B., M.D., 126 Lower Baggot-street, Dublin.

Ridley, George P., 14 Upper Leeson-street.

Robinson, Chas., F.R.C.S., Lecturer on Anatomy, Ledwich School, 35 Harcourt-street, Dublin.

Rutherford, Wm., M.D., Visiting Physician District Lunatic Asylum, Ballinasloe.

Scott, J. H., L.R.C.S., Surgeon Adelaide Hospital, 89 Harcourt-street, Dublin.

Shaw, James, 93 Talbot-street.

Speedy, Albert O., L.R.C.P. Ed., Medical Officer, No. 3 Dispensary District, North Dublin Union, 28 North Frederick-street, Dublin.

Strahan, Michael, L.R.C.S., Medical Officer, No. 2 North City Dispensary District, Dublin.

Taylor, Alex., M.D., L.K.Q.C.P., 3 Botanic-view, Glasnevin.

White, Wm. Dudley, L.R.C.S., Medical Officer, No. 3 Dispensary District, North Dublin Union, 51 Rutland-square, Dublin.

STUDENT ASSOCIATES.

Barker, W., 6 Gardiner's-row.

Byrne, Louis A., 36 Carlisle-terrace, North Circular-road.

Delacherois, E. B., 26 Stephen's-green.

Frazer, Ed. K., 16 Richmond-hill.

Goulding, Henry B., 82 Harcourt-street.

Hill, J. R., 15 Lower Gloucester-street.

Jackson, R. W., 17 Trinity College.

Nickson, Wilfrid, Weston, Booterstown.

Norton, J., 33 Rutland-square.

Sproule, A. E., 23 Avoca-road.

Taylor, Wm. J., Harryville, Eglinton-road.

Wynne, Wm., Meath Hospital.

RULES.

1. The name shall be, "Academy of Medicine in Ireland."

Constitution.

2. The Academy shall consist of Fellows, Honorary Fellows, Members, and Student Associates.

Management.

3. The affairs shall be managed by a Council, consisting of the President, the four Presidents of Sections, the General Secretary, the Treasurer, four Secretaries of Sections, and eight Councillors, being two representatives from each Sectional Council.

Meetings.

4. The Meetings shall be General and Ordinary.

Publication of "Transactions."

5. The "Transactions" shall be published by the Council, subject to the provisions hereinafter contained.

Original Fellows and Members.

6. All the Members of the present Societies (Surgical, Medical, Obstetrical, and Pathological) shall be Original Fellows or Members, without entrance fee, on payment of the annual subscription on or before 31st December, 1882.[a]

Fellows.

7. Fellows of the King and Queen's College of Physicians in Ireland, and of the Royal College of Surgeons in Ireland, shall be admitted, without ballot, on payment of the entrance fee and the subscription for the current year. All others, being registered Medical Practitioners not directly or indirectly engaged in the sale of drugs, shall be proposed by two Fellows, and elected by ballot by the Council.

8. Candidates shall be proposed at one Meeting of the Council, and balloted for at the next; one black bean in four to reject.

Privileges of Fellows.

9. Fellows only shall be eligible for office in the Academy. They shall have the privilege of attending all Meetings of the Academy, of making Communications, and of voting and speaking at such meetings. They shall also receive a copy of the "Transactions."

[a] Those who have paid a Life Subscription to any of the above Societies will be admitted to the privilege of Fellows on payment of Member's subscription.

10. These privileges shall not be exercised by any Fellow in arrear with his subscription.

Honorary Fellows.

11. Honorary Fellows, limited in number to 25, may be nominated by the Council, and elected, on motion at a General Meeting of the Academy by a majority of at least two-thirds of those present and voting.

Members.

12. Any Registered Medical Practitioner may be elected as a Member, the election to be conducted in the same manner as that of Fellows.

Privileges of Members.

13. Members shall have the privilege of attending the Ordinary Meetings of the Academy, of making Communications, and of taking part in debate. They can purchase the "Transactions" at cost price.

Student Associates.

14. Registered Medical Students, of the third or subsequent years, may be elected as Student Associates in the same manner as the Members.

15. Student Associates shall have the privilege of attending the ordinary Meetings of the Academy.

Annual Subscription.

16. Fellows shall pay £2 2s., and Members £1 1s. Student Associates shall pay 5s. The Subscription shall become due on the 1st of October in each year, and if the Subscription be not paid on or before the first Meeting in February, the defaulter shall cease to belong to the Academy, unless the delay shall be accounted for to the satisfaction of the Council. No Fellow shall vote at the Annual General Meeting who has not paid his subscription for the year. Medical Officers of the Army and Navy, and registered Medical Practitioners not residing within 15 miles of Dublin, are eligible as Fellows of the Academy on payment of the entrance fee, and an annual Subscription of £1 1s.

Entrance Fee.

17. After admission of Original Fellows, all Fellows shall pay an entrance fee of £1 1s.

Council.

18. The Council shall meet on the first Wednesday in the month throughout the Session, or oftener should they see occasion ; five to form a quorum.

19. Notice of all Extraordinary Meetings shall be transmitted by the Secretary to every Member of the Council. The President or any five Members of Council may call an Extraordinary Meeting of the Council. The Council shall determine questions by vote, or division if demanded, the President having a casting vote only. Any regulation of the Council shall have the force of a law, until submitted to the next General Meeting. The Council shall have the power of filling up any vacancies which may occur in the list of Officers of the Academy, except that of President, before the Annual General Meeting.

Sectional Councils.

20. There shall be four Sectional Councils elected by the Annual General Meeting in October, termed respectively—the Surgical, the Medical, the Obstetrical, and the Pathological, Council.

21. No Fellow shall be eligible as a candidate for election on more than one Sectional Council.

22. Each Sectional Council shall consist of the President of the Section and ten Members, one of whom shall act as Secretary to the Section.

Meetings of Sectional Councils.

23. Each Sectional Council shall meet on a fixed day at least one week before the Ordinary Meeting of their Section, three to form a quorum.

Powers.

24. Each Sectional Council shall have the power of making any such arrangements as it thinks necessary to carry on the work of the Ordinary Meetings which are under its charge, provided that such arrangements do not interfere with the general laws of the Academy; and any Rules laid down by such Council shall have the force of laws at the Ordinary Meetings under its charge, until submitted to the General Council.

25. Each Sectional Council shall have the power of filling up any vacancies that may occur among its Members until the Annual General Meeting.

Committee of Reference.

26. The Council shall appoint a Committee of Reference, to report upon morbid growths and other specimens exhibited before the Academy; of this Committee the Exhibitor shall, for the occasion, be a Member.

Officers.

27. A President to be elected by the Annual General Meeting in October, and to hold office for three years.

28. The Presidents of the Colleges of Physicians and Surgeons for the time being shall be the Presidents of the Medical and Surgical Sections. The Presidents of the Obstetrical and Pathological Sections shall be elected by the Fellows at the Annual General Meeting in October.

29. (1.) One General Secretary to be elected at the Annual General Meeting.

(2.) It is expedient that a fixed salary (of one hundred guineas) shall be paid yearly to the general Secretary in consideration of the fact that the editing of the "Transactions" is part of his duties.

30. One General Treasurer to be elected at the Annual General Meeting.

31. One Honorary Secretary for Foreign Correspondence to be appointed by the Council.

32. The Councillors for each Section to be elected at the Annual General Meeting in October. Each Sectional Council shall elect two Members to act on the General Council.

33. Two Members in each Sectional Council shall retire annually, and be ineligible for re-election for one year.

34. Four Secretaries, one for each section, to be appointed by the Sectional Councils.

35. That, at all elections after the year 1882, each Fellow desirous of serving on a Sectional Council shall, at least one fortnight before the Annual General Meeting, forward an application to the General Secretary to enter his name on the list of candidates for the office of Councillor of a Section.

36. That all elections shall be by ballot.

Duties of Officers.

37. *The President* shall preside at the Annual and Special General Meetings and at General Council Meetings. In the absence of the President the senior Member of Council then present shall preside.

38. *The Presidents of Sections* shall preside at the Ordinary Meetings of the Academy, and shall also preside at the Sectional Council Meetings. In the absence of the President, the senior Member of the Sectional Council present shall preside.

39. *The General Secretary* shall attend all General Meetings of the Academy and General Council. He shall take minutes of such meetings, to be read at the following meeting.

40. He shall receive and have charge of all papers intended for publication in the "Transactions" of the Academy, after they have been handed over to him by the Secretaries of the several Sections.

41. He shall, on receiving notice from the Secretary of a Section, send out to all the Members notices of the title or titles of the paper or papers for the next Ordinary Meeting, with the name or names of the authors, and, so far as possible, of the subjects for Exhibition, with the names of the Exhibitors.

42. He shall arrange for the Exhibition of specimens and the reading of papers, which are forwarded to the Academy by those who are absent, or are not members.

43. The Treasurer shall receive all moneys, and lodge the same in bank to the account of the Academy, and all cheques shall be signed by the Treasurer and one other Councillor.

44. The Accounts shall be audited by two Fellows, not Members of Council, to be appointed by the President at some meeting previous to the Annual Meeting.

Duties of Secretaries of Sections.

45. To attend the Meetings of the Council of the Section and the Ordinary Meetings of the Academy, under the management of said Council, and to take minutes at such meetings, to be read at the next following meeting of that section.

46. To keep such papers as the Sectional Councils deem worthy of publication, for the purpose of handing them over to the General Secretary.

47. To inform the Secretary of the Committee of Reference of any specimens referred to that Committee, and to transfer the specimens to that Secretary.

48. To give notice to the General Secretary, one week previous to the meeting, of the titles of papers for the evening, the names of the authors, and, so far as possible, the objects for Exhibition, with the names of Exhibitors, so that the General Secretary may inform the Members.

Meetings.

49. The Annual General Meeting to take place on the last Friday in October, for the election of Officers and Members of Council, and for the general business of the Academy.

50. Due notice of the meeting shall be given by the Secretary to all members one fortnight previously, and the names of Candidates proposed for office must be in the hands of the Secretary one week before the meeting.

51. No motion involving a change of these Rules shall be brought before this meeting except one week's notice thereof shall have been given by the Secretary to each Member.

52. The President may, and shall, on receiving a requisition signed by seven Fellows, at any time, on giving one week's notice, forthwith summon a Special General Meeting, for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting, and at such meeting no other business can be transacted.

Ordinary Meetings.

53. The communications to be submitted to the Ordinary Meetings shall be grouped under the following heads :—Medical, Surgical, Pathological, and Obstetrical ; and the conduct of such meetings shall be in the hands of the several Sectional Councils, each Sectional Council to have the management of the Ordinary Meeting in regular rotation ; the Council of the Pathological Section to have charge of the first meeting in each Session ; that of the Surgical of the second ; that of the Medical of the third ; and that of the Obstetrical of the fourth ; and so on until the end of the Session.[a]

54. The Ordinary Meetings shall be held on every Friday evening, from the first Friday in November until the last Friday in May, inclusive, at eight o'clock, except during the Christmas and Easter recesses.

55. All Fellows, Members, and Student Associates attending the meetings, shall write their names in the attendance book.

56. Any Fellow or Member may introduce two Visitors by cards obtained from the Sectional Secretaries.

57. Officers of the Army or Navy Medical Departments shall, on presenting their cards, be admitted to the Ordinary Meetings of the Academy.

58. No communication shall exceed twenty minutes in its delivery, nor any speech thereon ten minutes, except by permission of the Chairman. No one shall speak twice upon the same communication, except the author, who has the right of reply.

Sub-Sections.

59. A Sub-section in Anatomy and Physiology, under the direction of the Surgical and Medical Sections, and a Sub-section in State Medicine, under the direction of the Medical Section, shall meet during the session, as may be arranged by the Councils of each Section.

[a] NOTE.—With the permission of the Colleges, the Sectional Meetings in Surgery and Pathology will be held in the Royal College of Surgeons; those in Medicine and Obstetrics in the King and Queen's College of Physicians.

60. The Sub-sections shall have each a Chairman and Committee of six Members, of whom one shall act as Secretary.

Ordinary Meetings.—Order of Business.

61. (1.) Chair to be taken at 8.30 p.m.

(2.) Chairman to read list of specimens, &c., exhibited by card, together with the names of the Exhibitors.

(3.) No Pathological Specimen shall be exhibited at any section other than the Pathological and Obstetrical, except by card. This Exhibition shall not exclude any subsequent communication regarding it at the Pathological Section.

(4.) There shall be no Exhibition of Specimens by card in the Obstetrical or Pathological Sections.

(5.) Any member shall have liberty to exhibit any recent specimen at any of the meetings of the Obstetrical Section, provided it illustrates any question in gynæcology.

(6.) At the meetings of the Obstetrical Section recent specimens may be exhibited, and the President shall invite discussion thereon, provided that such exhibition of specimens or discussion, if any, thereon, must terminate at 9 o'clock, p.m., but that, if necessary, they may be resumed after the papers for the evening have been read and discussed.

(7.) Chairman to ask if any member has any observations to make or motion to propose relative to any living specimen on the List of Exhibition.

(8.) Chairman to call upon the author of the first paper on the list to read his paper.

(9.) Chairman to call upon members to discuss the paper, or, at his discretion, to take any other paper or papers on the list relating to the subject, and have the discussion subsequently on all such papers collectively.

(10.) When the last paper has been discussed, the Chairman to ask if any member desires to speak upon any of the specimens exhibited by card.

(11.) After the discussion upon any specimen, the Exhibitor has the right of reply.

Regulations regarding the Exhibition of Specimens by Card.

62. (1.) Any member may exhibit by card at any Ordinary Meeting, except at the meeting of the Pathological and Obstetrical Sections. At the meetings of the Pathological all specimens must be presented and described *viva voce*, and debate may be invited thereon.

(2.) Notice shall, if possible, be given to the General Secretary, or the Secretary of the Section, on or before the previous Ordinary Meeting.

(3.) Specimens must be in the room at 7.45 on the night of Exhibition.

(4.) Specimens for Exhibition by card shall be open for inspection at 8 p.m.

(5.) A card, containing all particulars for publication, shall be placed with the Specimen. Cards for this purpose are to be obtained from the Secretary.

(6.) The Exhibitor should be present, and he shall furnish further details if asked for.

(7.) Every Exhibitor shall submit the Specimen or Specimens on view to the Committee of Reference, if the meeting so decide.

Exhibition of Pathological Specimens.

63. No lengthened reference to treatment shall be allowed upon any Specimen, except by the express permission of the Chairman.

Bye-laws concerning "Transactions."

64. The "Transactions" shall consist of such Communications made to the Academy by or through Fellows or Members as may be deemed by the General Council suitable for publication; also, of discussions of importance or interest arising out of such Communications.

65. All Communications accepted by the Academy become the property of the Academy, but authors may also print their Communications in any publication in addition to the "Transactions." Papers shall be handed to the Secretary of the Section immediately after they have been read.

66. The "Transactions" for the year shall be presented to all Fellows of the Academy who have paid their Annual Subscriptions.

67. The "Transactions" may be purchased by Members at cost price.

68. That the Publication Committee of each Section do meet not later than the Tuesday after each meeting of the Section, for the purpose of abstracting the proceedings—the abstract to be placed in the printer's hands on same evening, and forwarded to the editors of medical journals not later than Thursday afternoon.

69. That contributors of papers be requested to send their papers to the Academy printer early enough to allow of their being put in type before the meeting, and read in proof; that when one sheet of the "Transactions" has been completed it be printed off and laid aside, until the termination of the session, and then bound up in the volume of "Transactions."

70. That on the evening of the day of meeting of the Sectional Council, when the papers for the next meeting have been decided upon, a circular be sent to each contributor informing him :—

(1.) That he is expected to be ready or else take his place at the bottom of the list.

(2.) That he must have an abstract ready with his paper, otherwise he will be noted in the published proceedings in such form as the Publication Committee think fit.

(3.) The General Council is empowered to defray the expenses in whole or in part of any illustrations which it may consider advantageous to the elucidation of the papers published by the Academy.

71. That an abstract (prepared by the author) of each communication made at the Academy, along with a report of the discussions thereon, shall be furnished to the editors of such medical journals as may desire to publish them, and that the authors of such communications shall be empowered to publish their papers *in extenso* in any periodical or periodicals they may think fit, such communications also to appear in the "Transactions," provided the Council consider them worthy of insertion.

Expulsion of Fellow or Member.

72. Expulsion of a Fellow or Member can only take place at a General Meeting of the Academy, on the motion of the Council, if two-thirds of the Members present shall vote for the same by ballot. Of such ballot the Council must give at least fourteen days' notice in writing to every Fellow of the Academy.

New Laws.

73. New Laws, or alterations in existing Laws, can only be proposed at the Annual General Meeting in October. Any Fellow proposing such alteration shall give notice to the General Secretary at least ten days before the General Meeting in October.

ANNUAL REPORT.

THE General Council report with pleasure the continued success of the Academy, which now enters upon the fourth year of its existence. The meetings of the various Sections and Sub-Sections have taken place regularly, and in some instances it has been found necessary to hold special meetings in order to continue discussions on subjects of importance. The Fellows, Members, and Student Associates, number 211, 35, and 24 respectively, as compared with 208, 27, and 24 in 1883–4.

The Council have to announce that with the close of this academic year they lose the services of DR. BANKS, the President, whose term of office terminates. They are sure they only express the feelings of the Fellows and Members when they say that the Academy was honoured by the presidency of a physician so distinguished and popular.

At a Special General Meeting held on the 17th January, 1885, the Council submitted the names of ten members of the Profession whom they recommended for the Honorary Fellowship. These were all elected. The number to be admitted to this distinction is limited to twenty-five; and the Council now beg to nominate six well-known scientific workers whom they also recommend to the Academy for this honour.

The Treasurer's Account shows a satisfactory balance to the credit of the Academy. The Council have previously referred to the very kind assistance which the King and

Queen's College of Physicians and the Royal College of Surgeons have given by grants of £25 each; but the Council, having regard to the prosperous condition of the Academy, have thought it right not to seek any repetition of these payments.

In accordance with a resolution passed at the last General Meeting in reference to illustrations in the "Transactions," the Council considered this subject, and felt justified in paying in full for the illustrations required for the last volume issued by the Academy. They have also ordered 350 copies of this year's "Transactions," with a view to their more liberal distribution to libraries and journals.

During the visit of their Royal Highnesses the Prince and Princess of Wales, an address was presented by the Council to the Royal Visitors, and was graciously received.

W. THOMSON, M.A., F.R.C.S.,
General Secretary.

TREASURER'S REPORT.

RECEIPTS.

	£	s.	d.
To Balance in Bank, October, 1884	305	2	9
,, Fellows' Subscriptions	453	12	0
,, Members' do.	36	15	0
,, Students' do.	6	0	0
,, Royal College of Surgeons	25	0	0
,, Sale of Tickets	1	10	6
Total Receipts	£828	0	3

EXPENDITURE.

	£	s.	d.	£	s.	d.
Secretary's Salary				105	0	0
Printing "Transactions"				151	11	0
Illustrations				20	7	6
Reporters				50	0	0
Sundries—Servants' Wages	£25	0	0			
Tea, Coffee, Cakes, &c.	64	14	6			
Printing Circulars, &c.	26	4	2			
				115	18	8
Expenditure				442	17	2
Balance in Bank				385	3	1
Total				£828	0	3

We have examined the Treasurer's Statement of the Accounts of the Academy of Medicine for the year ending September 30th, 1885, and we find it correct.

C. J. NIXON,
CHARLES COPPINGER, } *Auditors.*

TRANSACTIONS

OF THE

ACADEMY OF MEDICINE IN IRELAND.

MEDICAL SECTION.

A CASE OF TRUE RELAPSE IN ENTERIC FEVER.

By J. W. MOORE, M.D., M.Ch., Univ. Dubl.; F.K.Q.C.P.;
Physician to the Meath Hospital and County of Dublin Infirmary.

[Read in the Medical Section, Friday, November 20, 1885.]

Among the many causes of renewed pyrexia, or feverishness, in the later stages of enteric fever, or in convalescence from this disease, true relapse necessarily occupies a foremost place. And this arises, not so much from any increased danger to the patient's life—which is theoretical rather than founded on fact—as from the comparative infrequency of the occurrence of true relapse in this form of continued fever.

By "true relapse" I understand a second attack, in which the characteristic phenomena of enteric fever present themselves in sufficient number to establish the diagnosis of the disease—for example, enlargement of the spleen, abdominal tenderness, ochrey diarrhœa, and rose spots; or epistaxis, feverishness with evening exacerbations, abdominal tenderness and tympanites, or any other grouping of the symptoms of this fever met with in practice; the fact being admitted that a perfectly typical case of primary enteric fever, showing *all* the characters of the disease, does not often come under observation, even in the wards of a large epidemic hospital. "By a relapse of enteric fever," writes Murchison,[a] "is understood a second evolution of the specific febrile process, after convalescence from the first attack is fairly established. Relapses must not be confounded with the *recrudescences* which are common during the stage of ulceration."

Without further preface, I will detail the salient points in a case which falls within the limits of the foregoing definition :—

Case.—On Saturday, January 24, 1885, Mrs. Mary B., aged twenty, a domestic servant, was admitted into Cork-street Fever Hospital, under my care, at the request of Dr. S. M. MacSwiney, F.K.Q.C.P., who had been called to see her when she was already several days ill, and who recognised her ailment as enteric fever. She had been married not long previously, but menstruation was regular up to the time of her illness, and there was no reason to believe that she was pregnant.

When I visited the hospital on the morning of the 25th I found that Mrs. B. had complained of weakness and chilliness nine days before. Diarrhœa soon set in, for which she was treated by a respectable general practitioner, who was at the disadvantage of seeing the patient up and dressed, and so failed to recognise the true nature of the case. At last she took to bed, and was seen by Dr. MacSwiney, who advised her removal to hospital.

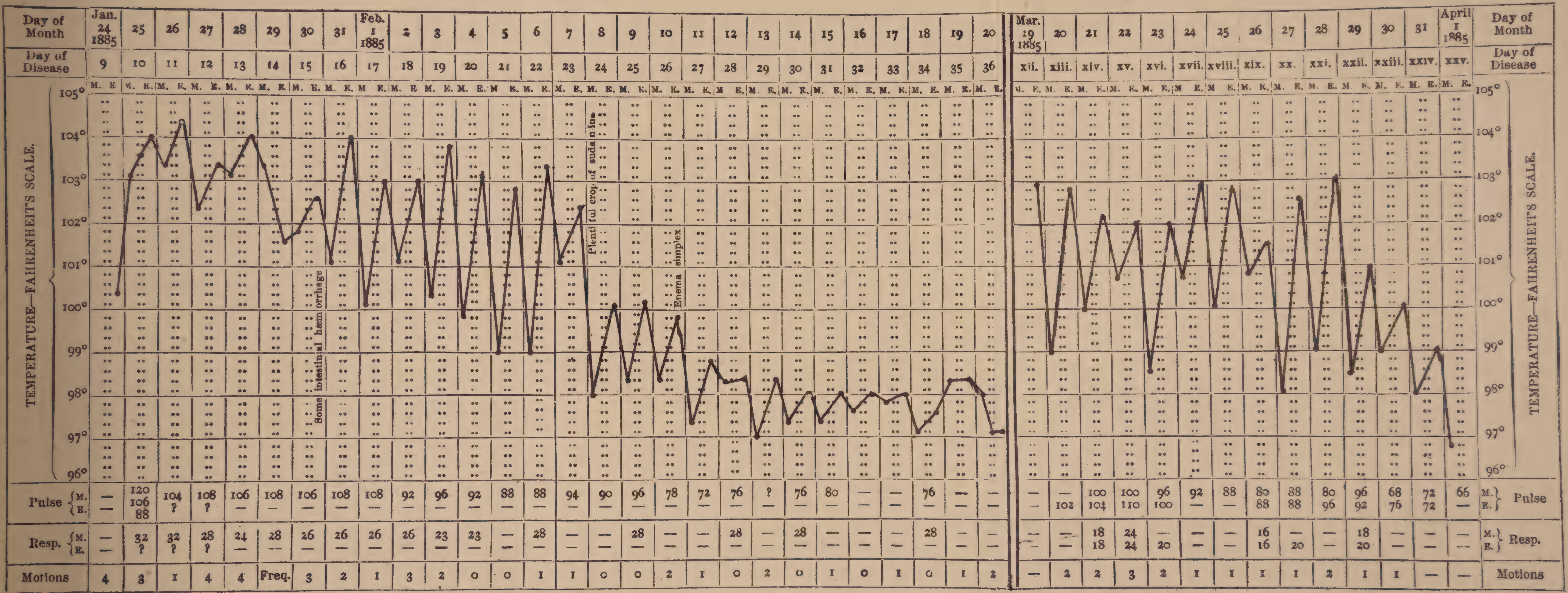
At my first visit, her pulse was 120, respirations 32, and temperature (10 a.m.) 103·2°. The area of splenic dulness was enlarged. There was a good deal of tympanites, and some tenderness on pressure existed in the iliac fossæ. She was passing from four to six yellowish fluid motions in the twenty-four hours. There was insomnia, and her mental state was unsatisfactory—nervous and excited. Crepitating râles were audible over the back of both lungs. Several typical rose-spots were detected on the trunk, and also some taches bleuâtres. A glycerine and laudanum poultice was applied to the abdomen. The back of the chest was dry-cupped. She was ordered ice, milk and arrowroot, and 6 ozs. of port wine. Chalk mixture, with chlorodyne and compound tincture of chloroform, was prescribed, as well as a full dose of Dover's powder at bedtime. Next day the chest was poulticed with linseed meal sprinkled with powdered camphor, and on the 27th 3-grain doses

[a] The Continued Fevers of Great Britain. Third edition. Edited by W. Cayley, M.D., F.R.C.P. London: Longmans, Green & Co. 1884. Page 552.

CHART OF TEMPERATURE, &c.

Name, Mrs. Mary B.; *Disease*, Enteric Fever, with relapse after 24 days interval; *Result*, Recovery.

I. PRIMARY FEVER. II. RELAPSE.



of quinine were given thrice a day. On the 28th turpentine fomentations were applied to both chest and abdomen, and on the 29th a further increase of diarrhœa required the administration of an enema containing 12 minims of tincture of opium in two ounces of mucilage of starch.

On January 30, the 15th day of the fever, the patient appeared to be going on well—P. 106, R. 26, T. 101·8° in the forenoon, rising only to 102·6° in the evening. At night some intestinal hæmorrhage occurred. Next morning a poultice of crushed ice was applied to the abdomen, and three grains of ergotin were ordered to be injected hypodermically should hæmorrhage recur. This did not happen, and the patient began to make excellent way; her pulse and respirations fell and diarrhœa ceased, giving place to obstinate constipation—a point of some moment in the light of the subsequent history of the case. Reference to the clinical chart will show that from the 16th to the 23rd days of the fever inclusive the evening exacerbations of pyrexia were very marked, the temperature being from 3° to 4° higher than in the forenoons.

On the morning of February 8 (24th day) the body was covered with a plentiful crop of sudamina, but the temperature was only 98°. The following evening it did not exceed 100·1°, and 48 hours afterwards it became permanently normal.

Mrs. B. remained in hospital until February 28, when she was sent to the Convalescent Home, Lynden, Blackrock, this being the 18th day of complete apyrexia and the 44th day from the commencement of her illness. During the last three weeks of her stay in hospital the bowels were obstinate, and had to be kept in order by the almost periodic use of simple enemata or the occasional administration of castor-oil in small and repeated doses.

During the first few days of her stay at the Convalescent Home, Mrs. B. made satisfactory progress. Her stomach then became irritable, she lost her appetite, began to cough, and felt hot and cold by turns. She came into Dublin, and in the course of a few days called to see me. I found her tongue furred and her pulse quick. There were few, if any, physical signs of chest affection, but she looked pale and thin, like one who would readily run into acute consumption. Acting under my advice, she went home to bed, and I visited her on Thursday, March 19, the twelfth day from the appearance of symptoms of her second illness. Her pulse at 5 p.m. was 108, and her temperature was 103°.

Next day she was again admitted to Cork-street Hospital, under my care. The morning temperature was only 99·1°, but in the evening the thermometer marked 102·8° in the axilla. There was some increased splenic dulness on percussion, and a few rose spots were found on the chest and abdomen. On this and the three following days there was a tendency to diarrhœa—two or three yellow soft motions occurring each day. Afterwards the bowels acted regularly once a day. The course of this second fever is sufficiently well shown on the accompanying chart. It will be seen that there was a well-marked remittent tendency in the temperature range—the morning readings were moderate or low, while the evening ones were between 102° and 103°. On the 20th day, although the pulse remained steady at 88, the temperature rose from 98·0° in the forenoon to 102·6° in the evening. A somewhat similar rise next day was succeeded by a rapid defervescence, which was completed in three days, and so more completely resembled a crisis than is usual in enteric fever. At this time, also, the pulse fell below normal, beating from 66 to 76 times a minute.

It is only necessary to add that Mrs. B. made a quick and satisfactory recovery; all chest symptoms subsided, her appetite returned, and she speedily regained her wonted health and strength. On the 31st of March (the 24th day of her second illness) she was transferred to the care of my friend, Mr. J. M. Redmond, F.K.Q.C.P., who then succeeded me as Physician to Cork-street (Fever) Hospital, and on the 15th of April she left hospital, finally convalescent. During the last ten days of her stay in hospital there was again some little trouble from constipation.

The foregoing case suggests some interesting points for discussion. In the first place, there seems little reason to doubt that it affords an example of true relapse in enteric fever, as distinguished from a secondary symptomatic pyrexia occurring in convalescence from that disease, and due to one or other of the recognised causes of such a sequela—septicæmia, enteritis, pneumonia, acute tuberculosis, and so on. In the case before us, there was a long period of apyrexia—twenty-four days—between the two fevers, the second of which, no less than the first, presented a grouping of symptoms sufficient to warrant the diagnosis of enteric fever. "It is on the presence of the eruption, and on the absence of any local inflammation to account for the pyrexia, that the diagnosis of a true relapse must be based."—(Murchison).

This being so, the case demands attention because of the rarity of relapse even in this form of continued fever. Murchison[a] states that, during seven years (1862–68), relapses were observed in 80 of 2,591 cases in the London Fever Hospital—that is, in 3 per cent. Griesinger noted them in 6 per cent. of 463 cases at Zurich; Human in 8 per cent. of 548 cases at Leipzig; and Maclagan in 13 (10 per cent.) of 128 cases at Dundee. Nevertheless, as compared with typhus, in which true relapse is one of the rarest events in clinical observation, these are all relatively high percentages. Out of 18,268 cases of typhus reported at the London Fever Hospital during 23 years, there is *only one instance* of a true relapse, although in several instances a genuine has been preceded by an abortive attack.[b]

Again, it is worth noting that the relapse ran a milder and slightly shorter course than the preliminary attack. The first fever terminated on the twenty-seventh day. Then came an apyrexial period of 24 days—from the 11th of February to the 7th of March—and this was lastly followed by a fever which ran a twenty-four days' course. All this is in accordance with clinical experience. Thus, Murchison[c] says that the duration of the second attack is usually, but not necessarily, shorter and milder than that of the first. Of 24 cases collected from various sources by Michel, the mean duration of the first attack was 27 days; of the intermission, 11 days (shortest 2 and longest 31 days); and of the relapse—shortest, 16 days; longest, 30. In 53 cases which came under Murchison's own observation, and which he has tabulated, the average duration was—of the first attack, 27·0 days; of the intermission, 11·76 days; and of the relapse, 16·4 days. By the way, of these 53 patients only 5 died in the relapse, the mortality being at the rate of 9·4 per cent. compared with 17·26 per cent. in primary attacks in 5,911 cases admitted into the London Fever

[a] Loc. cit., page 552.
[b] Loc. cit. page 189.
[c] Loc. cit., page 553.

Hospital. From this it would appear that a relapse is only about half as dangerous to life as a first attack.

The last point to be considered is the probable ætiology of relapse in enteric fever. In 1846 Hamernjk put forward the view that relapse was due to reabsorption of the "typhous material" thrown off by the patient's own bowel.[a] Dr. T. J. Maclagan adopts this view, believing that the surviving healthy glands become inoculated by the sloughs thrown off from those first affected. In accordance with this theory, Maclagan further holds that relapses are met with only when there has been constipation in convalescence. This condition was certainly present in Mrs. B.'s case, and we are justified in assuming that the non-elimination of the fever poison, owing to constipation, was at all events one factor, and that an important one, in the causation of the second attack.

It may be objected that to adopt this view in its entirety is to pledge ourselves to the exploded theory of the local origin of the phenomena of enteric fever—is to revert to Broussais' doctrine, according to which the pyrexia of the disease is the result of a local inflammation of the intestinal glands. But this is not stated. What we hold is that, as a result of constipation, the fever poison finds no exit from the system *viâ* the bowels, it is then absorbed by the glands which have hitherto escaped the characteristic secondary inflammation of the disease, and passing into the blood once more sets up an essential, not a symptomatic, fever.

One other theory of relapse may be drawn from the analogy of true relapsing, or spirillum, fever. In the third volume of the *Irish Hospital Gazette* (April 1, 1875, page 105), Dr. Gerald F. Yeo quotes from the *Centralblatt für klinische Medicin* an observation of Dr. M. Laptachinski, that with each attack of pyrexia in spirillum fever a discharge of the spleen contents into the blood takes place. From observation of the epidemic of relapsing fever of 1847–49 in Ireland, the late Dr. Alfred Hudson was led to believe in the frequent co-existence of the poison of endemic

[a] Murchison, loc. cit., page 555; and Hamernjk, Prag. Vierteljahrssch. 1846. X. I.(Zur Pathologie und Diagnose des Typhus).

typhoid with epidemic relapsing fever—the one disease replacing the other in the same patient.

He also noticed the influence upon the blood of the reabsorption into the circulation of a quantity of depraved blood laid up, as it were, in the congested spleen, and commingling with the circulating mass after crisis, in the purifying effect of which it had not shared. He adds:—"I shall be mistaken if future observations do not prove this to be an important element, not only in the production of relapse, *but also in determining the enteric lesions*, which occur during the second and third attacks, in this form of fever."[a] Remembering how constantly the spleen is enlarged and congested in enteric fever, we may perhaps apply to this disease also the ingenious pathological hypothesis which was first advanced, as we have seen, by Dr. Hudson, in explanation of the relapses in spirillum fever.

[a] Lectures on the Study of Fever. Second Edition. 1867. Pages 147 and 287.

PARTIAL EMBOLISM OF THE INFERIOR DIVISION OF THE CENTRAL ARTERY OF THE RETINA, ASSOCIATED WITH REPEATED PREVIOUS ATTACKS OF CHOREA.

By ARTHUR H. BENSON, F.R.C.S.;
Assistant-Surgeon to St. Mark's Ophthalmic Hospital.

[Read in the Medical Section, Friday, December 18, 1885.]

The retina offers many advantages not elsewhere obtainable for the observation of pathological conditions during life; and from the changes there observed may be at least conjectured the changes that take place in other somewhat similar structures, more especially the nerve centres.

In the retina we can accurately watch the day-to-day changes that occur, and note the correspondence of objective changes with subjective sensations and physical signs. In no other part of the body can this be done so well, for not only have we the processes directly and clearly under observation through the transparent media, but we have them considerably magnified. The details of the fundus oculi are viewed under a magnification of about 15 diameters. Thus, most minute changes can be noted with certainty. The retina and optic nerve are, moreover, closely allied in structure to the nervous centres, brain and spinal cord, and they are so intimately associated with the intra-cranial viscera that much may be learned of the intra-cranial conditions by observation of the pathological processes within the eye. It is on this account that I venture to bring this case before the Medical Section :—

Case.—James J.,[a] aged twenty-one, house painter, applied at the out-patient department of St. Mark's Ophthalmic Hospital on October 15th, 1885 (S. M. O. H. Disp., 2,987). He stated that he

[a] The patient was exhibited.

had always been shortsighted; that on the previous evening, at seven o'clock, while sitting reading, after an ordinary easy day's work, and while feeling perfectly well, the sight of the right eye rapidly but gradually failed, so that within a few seconds he became totally blind of that eye. He could not even see the light with it, whilst the sight of the left remained perfect. He had no premonitory symptoms whatever, nor any abnormal sensations accompanying the loss of vision. In about three minutes the sight began to clear from below upwards, and gradually improved till, in about fifteen minutes, the Field of Vision had reached the horizontal line. There it ceased abruptly, and has since remained. The chart taken with the perimeter fifteen hours after the attack shows absolute loss of the upper half of the Field, the boundary of the sentient half being a straight horizontal line coinciding with the horizontal meridian of the chart, except, perhaps, at the fixation point, which appears to include rather more than half of the macula. No headache, giddiness, sickness of stomach, or other symptom either preceded, accompanied, or succeeded the loss or the return of the sight.

The history the patient gave of his previous illnesses is interesting.

1. He had childish illnesses of various kinds. He had measles when three years old, followed by whooping-cough. At nine years of age he had scarlatina, and about twelve he had small-pox lightly.

2. Four and a half years ago—*i.e.*, in 1881—at the age of seventeen, he had rheumatic fever, and was under the care of Dr. Bennett, of Sandymount. This lasted ten weeks. His heart was not, at that time, affected, as far as he could tell. He says that Dr. Bennett considered that lead might have had something to do with the rheumatic fever.

3. Three and a half years ago—June, 1882—he had an attack of left-sided chorea in the Meath Hospital, under Dr. Foot, which was severe for about seven or eight weeks. Dr. Foot has kindly placed the notes of that attack at my disposal. He says :—"There is no cardiac murmur at present (June, 1882). The twitchings had been coming on for six weeks; however, he worked up to the beginning of the week in which he was admitted. The jerkings began in the fingers of the left hand; then spread up to the shoulder; then went to the lower extremities, beginning at the hip; lastly, they affected the face—left side—principally the muscles about the mouth and eye. He also felt his neck twitching. He

cannot hold anything in his left hand;" but he did not get quite well; and—

4. Two years ago (1883) he had another similar attack of chorea in the Edinburgh Infirmary, and was treated by a physician whose name he forgets. This lasted about two months, and was, he says, more general, affecting both sides.

5. Last year (1884) he had again another attack of chorea, and was treated by Dr. Quinan. This attack lasted about two months. Each attack was less severe than the previous one. He has since had no return.

Condition on Admission.—R., with -6 D. V. $= \frac{6}{18}$; tension normal; pupil active. L., with -7 D. V. $= \frac{6}{6}$; tension normal; pupil active, but more responsive than in L. This was fifteen hours after the embolism occurred. The ophthalmoscope showed the media to be clear. The upper part of the right disc was fairly normal. There was a myopic crescent and a large deep physiological cup, in which the vein was pulsating; its lower half was hazy and whitish, so that its margin could not be determined. The retina was divided horizontally by a sharp line of demarcation into a normal superior portion and an œdematous inferior portion. This line ran from about the middle of the disc towards the yellow-spot region, passing close to, but just below, the macula. It extended well out beyond the yellow spot, and also to the nasal side of the disc, following, with very great accuracy, the horizontal line. This line was best marked, as was also the œdema of the retina, in that part which lay between the disc and the yellow spot. The retina below the boundary line was of a whitish-gray colour (œdema), fading gradually into tolerably normal-looking retina towards the periphery. On the œdematous portions the smaller vessels stood out with undue distinctness. The macula presented the typical "cherry-red spot," which was horizontally oval; and surrounding it, in the more normal part of the retina, was a most distinct halo. The upper half of the retina exhibited, to a marked degree, the "shot silk" phenomena, but was otherwise perfectly normal. The vessels were all well filled with blood, though those supplying the inferior half of the retina were not quite as roundly filled as those running upwards, but pressure produced pulsation in the arteries of the affected part. Pulsation and emptying were more easily produced in the vessels of the affected than of the healthy retina. There were, however, no attenuated or empty vessels such as are usually seen in cases of embolism of the central artery. There were no hæmorrhages anywhere visible. There was total loss of sensation in the lower half of the retina; even a strong beam of light projected upon that part of the retina was not perceived, whilst the upper half possessed acute vision.

The diagnosis of embolism of the inferior division of the central artery of the retina was made. The urine and heart were each carefully examined at the time and found healthy; and on two subsequent occasions Dr. Hawtrey Benson kindly examined his heart and failed to discover any pathological condition whatever.

In a few days' time the œdema of the retina had markedly diminished. One small flame-shaped hæmorrhage appeared in the retina to the lower outer side of the disc. This was soon absorbed, and no fresh extravasation occurred. The vessels, in a few days, showed a decided tendency to diminish in size, and to exhibit the appearances of thickened coats.

The subjective conditions remained unchanged, but each day the objective appearances varied in proportion as the œdema was absorbed and the vessels shrank, till in a month's time the retina looked practically normal, except for the vessels on its lower half, which were shrunken to a great degree, and had thickened coats. One or two small brilliant yellow spots of fatty degeneration, which were noted in the diseased retina near the yellow spot, had disappeared. The halo round the disc was still visible, and a faint milkiness of the retina, which had been œdematous, still served to distinguish it from the sound part. Vision remained unchanged, and tension was normal. When the circulation had been re-established, the vessels, more especially the veins, of the affected portion of the retina were smaller on the disc than a short distance away from it.

It is now (Dec. 18th) more than two months since the accident, and the retinal changes above mentioned are only visible to a very slight degree, but the vessels, both veins and arteries, are more attenuated, and vision has in no way changed. Were it not for the history, the ophthalmoscopic appearances might be difficult to account for, the only subjective symptom being the horizontal right hemianopsia.

One or two points in this case admit of discussion. Was there any ætiological connection between the embolism, the three attacks of chorea, and the previous attack of rheumatic fever? Acute rheumatism is a common antecedent of chorea; and those who believe in the embolic theory of chorea find in the cardiac complications of rheumatism an explanation of the choreic attack. To such also the sequence would be renderèd complete when the individual got embolism of the central artery of the retina; and further probability is given in this case by the fact that it was left-sided hemichorea that he suffered from (*i.e.*, right side of brain), whilst the embolism lodged in the right retina. Cases of this kind are, however, extremely rare. Mr. Swanzy, in the R. L. O. H. Reports,[a] has recorded a case where a girl, aged ten, got chorea, and, at the same time, facial paralysis and embolism of the left central retinal artery. In this case, as in mine, nothing pathological could be discovered in the heart.

In my case the sudden and total blindness of one eye, followed by the recovery, in a few minutes, of half the Field of Vision and permanent loss of the other half, would seem to imply that the embolus, in the first instance, blocked the central artery at or before its bifurcation, and was dislodged from that situation and washed into the inferior division of the vessel before any permanent injury was done to the retina. That this general ischæmia should occur with even a small embolic mass is quite reasonable, as any sudden obstruction, though only partial, would be likely to produce such a diminution of the tension of the blood in the retinal vessels that their coats would spasmodically contract and, aided by the intra-ocular pressure on the vessels, serve to empty them almost to obliteration of their lumen. The mass when dislodged was carried into the inferior division, leaving the circulation free in the superior, and vision in it returned.

The early re-establishment of the circulation in the lower half of the retina, without the restoration of vision, and the subsequent contraction of the vessels and atrophy of the retina, may be explained, as has been done in an able article on the subject by Schnabel and Sachs (*Archives of Ophthalmology*, Vol. XIV., p. 262), by assuming that an embolus of irregular shape only partially filled the lumen of the branch into which it drifted. The spasmodic contraction of the arteries, aided by the intra-ocular pressure, were

[a] Sept., 1875.

at first sufficient to complete the arrest of the circulation produced by the partial embolus, but that presently the spasm passing off permitted the blood to flow, though slowly, through the vessels, which were thus again filled, but at a lower tension than the normal.

It will be remembered that, as regards its nervous connections, the retina is divided by a vertical line running through the yellow spot and separating it into lateral halves; and hemianopsia from post-ocular trouble is usually lateral, right or left. But as regards its blood-supply it is divided horizontally by a line also passing through the yellow spot; consequently hemianopsia from vascular causes is usually horizontal, superior or inferior. That the refilling of the vessels in these cases is not due to a regurgitation of venous blood is proved in many instances by actual observation of the direction in which the current flows. I remember seeing one case of recent embolism in which the blood column in the retinal veins was divided into segments, each separated by an apparently empty space. With each beat of the heart these segments gave a jerk forward, towards the disc, and so showed that the circulation, though impeded, was not totally stagnant or reversed in direction. This beading of the blood in the veins only lasted for some hours. When the case was seen next day there was an uninterrupted column of blood in all the vessels. The subsequent contraction of the vessels has been ascribed to secondary changes of a sub-inflammatory nature occuring in their walls, and caused by the partial stagnation of the blood-flow.

Schnabel and Sachs have had the rare opportunity of verifying, by *post-mortem* examination, the diagnosis in a case very like the one I have just described, and I may be permitted to quote their description of the conditions as seen by them :—

"In the central artery of the intra-ocular part of the left nerve, just at the lamina cribrosa, was an embolus, whose long diameter was about twice the transverse diameter of the artery. It was partly hyaline, partly finely granular, and contained no cellular elements.

"It stained an even blue with hæmatoxylin. One part of the upper surface of the embolus had become attached to the wall of the vessel against which it rested; another part projected into the lumen of the artery surrounded by blood. From that part of the arterial wall on which the embolus rested, the endothelium grew into the recesses existing between the upper and lower ends of the plug and the arterial walls, and was continued over the upper surface of the plug in a double or triple layer of cells, covering the embolus, and there protecting it from direct contact with the blood.

"The passage for the blood alongside the embolus measured at its widest part about one-third of the diameter of the artery. It was full of normal blood corpuscles, which seemed to form a continuous layer with those above and below the embolus. The two outer arterial coats presented no anomaly in the central arteries. Behind the embolus the lumen of the artery was normal.

"In that part of the lower main branch of the central artery which passed over the papilla was another embolus, which on section was of the shape of a long quadrangle, whose length was 0·16–0·20 mm., and its width 0·025–0.050 mm. Its relations to the lumen of the artery and to its endothelium were similar to the one seen in the trunk of the central artery."[a]

If it be true, as Schnabel and Sachs have demonstrated, that "partial emboli" occur in the retina, it is certain that such also occur in other organs, and give rise to symptoms of permanent or temporary importance according to the delicacy of the part obstructed. A very few hours' deprivation of blood is sufficient to destroy the function of the retina; but it may be without blood for a certain time and yet recover perfectly, as in the case observed by Wood White (*Ophth. Rev.*, Vol. I., 1882). Portions of the nervous centres may, in the same manner, suffer temporary deprivation of blood without permanent loss of function; and cases of vomiting, headache, giddiness, &c., may in some cases be most easily accounted for by this hypothesis. In this connection it may be mentioned that vomiting and headache are not infrequently associated with the occurrence of retinal embolus.

I have not attempted to discuss the points of diagnosis between embolism and thrombosis of the retinal artery. The subject has recently been treated in full by Priestly Smith (*Ophth. Rev.*, Vol. III., p. 1, 1884); and my case admits of no doubt.

[a] Loc. cit., page 277.

ON THE THERAPEUTIC USES OF THE DIGESTIVE FERMENTS.

By J. M. PURSER, M.D.;

Professor of Institutes of Medicine, Trinity College; Physician to Sir Patrick Dun's Hospital.

[Read in the Medical Section, January 29th, 1886.]

If we may judge from the number of advertisements with which the journals are filled, and by the number of testimonials from chemists and practitioners which accompany these advertisements, there are few kinds of medicine more widely used at the present time than are the digestive ferments.

If we neglect the somewhat doubtful lactic acid ferment of the stomach, we know five digestive ferments:—1. A diastatic ferment found in the saliva and also in the pancreatic fluid; 2. A milk-curdling ferment found in the stomach and also in the pancreas; 3. A ferment which decomposes neutral fats, and which is found in the pancreas, and possibly in small quantity in the stomach; 4. A ferment which splits cane sugar into glycose and levulose, and which changes maltose into glycose—found only in the small intestine; and 5. A ferment, or, more properly, ferments, which convert proteid substances into peptone and other products. These ferments, which differ very much in their action, and in the conditions under which they act, are found in the stomach as pepsin and in the pancreas as trypsin.

Now, as therapeutic agents there are some of these which we may eliminate, since their action is apparently unimportant for the process of digestion. We do not know why milk is curdled in the stomach, and I believe rennet is never given as medicine. The decomposition of neutral fats undoubtedly takes place in the intestine, but we have every reason to believe that this occurs only to a very limited extent, and that the absorption of neutral fats is effected for the most part without their having undergone any chemical change. Moreover, the fat-decomposing ferment of the pancreas is the most delicate of all the ferments, and even if it could be separated and administered, it would be immediately destroyed by the acid of the stomach, as it cannot resist the action of an acid medium.

Thirdly, the inversive ferment of the small intestine is not much used in medicine, and we do not know to what extent in normal digestion splitting of the sugars of the saccharose group takes place. We have thus remaining only two ferments—the diastatic and the proteolytic.

The diastatic ferment is given either as preparations of malt or as pancreatic extracts, and there is no doubt that many of these substances do contain a ferment which changes starch to dextrin and sugar. But what is the value of this? In the body the transformation of starch takes place in the mouth and for a short time in the stomach. Then the action is stopped by the increasing acidity of the gastric juice and the salivary diastase is destroyed, not merely suspended in its action. In the intestine the action is again resumed and completed by the more powerful diastatic ferment of the pancreas, and usually there is very little unchanged starch to be found in the fæces. Now, when diastatic ferment is given by the mouth it can only supplement the action of the saliva, for, before it can reach the intestine, it is destroyed by the acid of the stomach. The action of the saliva on starch is far less important than that of the pancreas, and, while after a starchy meal much unchanged starch is always found in the stomach, scarcely any is to be found in the intestine. In many of the lower animals who eat their food raw, the saliva has scarcely any digestive action, and the starch is digested altogether by the pancreas. Hence the useful effect of diastase given by the mouth must be insignificant, even supposing that the preparation is good, and that the ferment in question is wanting in the saliva. But is this ever the case? I do not remember to have heard of an instance in which the physician found this to be so before he prescribed maltin or pancreas preparations; and I believe that the absence of ptyalin from the secretion of the mouth will be found to be one of the rarest circumstances. I must therefore conclude that as regards diastatic ferments their administration as such is useless and irrational.

We now come to the proteolytic ferments, which are in many respects the most important of all. First, as regards the pancreas ferment or trypsin—it is generally stated that this ferment is destroyed by acid of the strength of that of the gastric juice, and there can be no doubt that this is true of many pancreas extracts. This being so, it is evident that it is useless to administer trypsin as a drug, for its activity will be destroyed if it be given while the stomach is digesting and contains acid, while if it be given during fasting, when the stomach contents are not acid, it is incredible that it could pass through the stomach and lie in wait in the duodenum until the next meal is taken, and, having undergone stomach digestion, has passed through the pylorus.

I have, however, found the statement that the proteolytic ferment of the pancreas is destroyed by acid to be not invariably true. I have lately worked with an example of the liquor pancreaticus made by Mottershead of Manchester, a very active preparation. This is acid, but what the nature of the acid is I cannot say. The liquor has a vinous smell, and the acid is possibly only that of the wine with which the preparation seems to be made. I have found that when the liquor was mixed with about five times its bulk of 0·2 per cent. hydrochloric acid, and kept for twenty-four hours at 100° F. in the water bath, then neutralised and added to 1 per cent. solution of carbonate of sodium, it dissolved fibrin readily, although not so rapidly as before acidification. This solution was not due to putrefaction, as it took place when thymol was added to the digestive mixture, and when this lay for many days in the water bath it developed no putrefactive smell. Another statement which has been made by Kühne I have been also unable to confirm. He says that in a mixture of pepsin and trypsin the latter is destroyed, while the former is uninjured, and he suggests that in the body the bile plays an important part in precipitating and killing the pepsin before it can reach the pancreatic fluid. In order to test this I mixed equal volumes of Mottershead's liquor pancreaticus and liquor pepticus together, carefully neutralised the mixture with sodium carbonate, and left it for twenty-four hours in the water bath at 100°–104° F. I then divided the mixture into two equal parts; to one I added 0·2 per cent. HCl, to the other 1 per cent. Na_2CO_3, and placed fibrin in each. The alkaline fluid digested the fibrin readily; the acid even after several days showed no signs of digestion. Here it would appear that the pepsin was destroyed, not the trypsin. Although I have repeated these experiments with constant result, I cannot affirm more than that what I have described took place with the particular material with which I worked, and I do not wish to throw any doubt on the contrary results got by other observers. To what the discrepancy is due, must be a matter for further investigation.

Still, even admitting that trypsin is not absolutely destroyed in the stomach, I think there is but little encouragement for its administration as a drug. Mixed up with the acid chyme such part of it as escaped absorption in the stomach would be precipitated by the bile along with the pepsin when the contents of the stomach passed into the duodenum, and even if it were again dissolved it would be so enfeebled in its action as to be of little use in digestion. But beyond this, is there any evidence that this ferment is wanting in the pancreatic fluid? Has any attempt ever been made to show this, or is there any known way by which it may be determined? It is more than probable that so long as pancreatic fluid continues to flow into the intestine it contains trypsin in far better condition than this ferment would be after having undergone stomach digestion. I would therefore condemn the administration of trypsin as I have done that of diastase.

We finally come to pepsin, and here, if anywhere, we have the ferment whose administration rests on a scientific basis. This ferment is easily prepared and kept, as it keeps well in an acid solution which is not prone to decompose. It can be introduced into the stomach without having undergone injury by the action of any other gland, and we can time its administration so as to introduce it when it is required. Is its administration, then, founded on sound principles? I believe not. In the first place, pepsin is very rarely absent from the gastric secretion.[a] I have repeatedly examined matters ejected from the stomach. They were sometimes alkaline, or fœtid—on one occasion smelling strongly of ammonia, but always when they were filtered and acidified they digested fibrin or albumen readily. It is, therefore, not to the want of pepsin that the failure of stomach digestion is usually due, but to some failure in the conditions under which only the pepsin will act. I do not deny, however, that there are cases in which pespin is absent, and that it may be possible to effect an artificial digestion in the stomach, but is that an experiment which any chemist would like to undertake?

If the condition of the stomach is so seriously affected as to cause a cessation in the secretion of the pepsin, it is most likely that the viscus is not a very favourable receptacle in which to carry on an artificial digestion. The matters introduced are withdrawn from observation and removed from control; we have no power of keeping them from hurtful mixture or of regulating the conditions so that they may be favourable to the process. In short, it seems to me that the stomach is about the worst place possible in which to perform a prolonged and delicate chemical experiment. Since, then, pepsin is very rarely absent from the stomach, and since, even in those cases where it is wanting, the conditions for its action are not favourable, I think that pepsin, like the other ferments, should be banished from the list of drugs employed for administration as internal medicine.

In what I have said I have supposed that the ferments in question are genuine, and capable, under favourable circumstances, of producing their digestive actions on the different alimentary substances. The pepsin preparations are usually good. As I have said, it is easy to make a good stomach extract. Many of the diastatic and pancreatic preparations are utterly worthless, while others are very good.

I suppose, too, that the different ferments are given with some regard to common sense, and not all mixed together, regardless of

[a] This is in accordance with the large experience of Dr. C. A. Ewald. Berliner klinische Wochenschrift. 1886. P. 51.

whether they are mutually antagonistic, or whether they act in acid or alkaline solution. But even so, and considered as a question of physiological chemistry, I hold that these substances are not suitable for internal administration.

Of course it will be urged that everyone has seen cases in which the use of these ferments has been attended with marked success. To this I answer—firstly, that the treatment is never confined to the administration of the ferments, but that dietary and other precautions are adopted, to which, more than to the medicine, the improvement of the patient is due. Secondly, the fact that a patient takes a drug and gets better is no proof that the drug is the direct cause of the improvement. How many patients are cured yearly by homœopathic globules in the thirtieth dilution? How many patients are relieved of the most severe symptoms by having bits of metal fastened on their limbs, by breathing through a solenoid, or by being put to bed with a horseshoe magnet? And yet does anyone really believe that these cures are produced by the treatment directly, and not through the mental effect the impressiveness of the treatment produces? When a patient gets a bottle of one of the digestive ferments he finds around it a sheaf of testimonials from the most eminent practitioners, and on the label of the bottle is a farrago of nonsense, which passes with him for science. He is told how these ferments (which, as every physiologist knows, have never been isolated or weighed) are here combined in the exact proportions in which they are found in the stomach; that he has here the means of digesting everything that can be swallowed; that he has in his hands a "medical certainty" which will cure all diseases the human frame is liable to. Is it any wonder that he feels comforted both in mind and body? Besides, these drugs have a great advantage over many others—they can do no harm. As placebos, then, I do not object to them, but do not let the medical man suppose that he is walking in the paths of physiological science when he prescribes them.

Are the ferments, then, of no use to the physician? On the contrary, I believe them to be of great utility. The attempt to perform an artificial digestion in the living stomach I have deprecated; but the artificial digestion of food in clean vessels outside the body and the administration of the digested material is a mode of treatment scientific in itself, and one which is attended with the very best results. Here we have no interference of one ferment with another—no uncertainty of whether the ferment will find in the body the suitable conditions for its action. The whole process is carried on under our eyes and in circumstances which are under our control. This use of the ferments inaugurated by Liebig, and so much extended in late years, chiefly through the labours of Sir William Roberts, is, I believe, the true one, and destined to replace altogether their internal administration as drugs.

ON SOME CASES OF PNEUMONIA.

By JAMES MARTIN, F.R.C.S.;
Medical Officer, Portlaw Dispensary District and Fever Hospital.

[Read in the Medical Section, February 26, 1886.]

I ADDRESS you this evening on the subject of pneumonia, not with a view to introducing any new theory as to the pathology of the disease or to propose any new plan of treatment, but to try to aid in the settlement of the now vexed question as to whether it is propagated by infection or not.

Occurring so frequently as it does in this variable climate, a disease so complete in all its courses, capable of being thoroughly watched and observed through all its phases by means of the stethoscope and thermometer, there is no disease with the life-history of which we ought to be so well acquainted as that of pneumonia; and I do not think I err in saying that there were few points on which the mind of the profession, some time ago, was more decided than in looking on it as being a non-infectious disease. However, of late doubts have arisen upon the subject; yet, if we remember how different a view is taken of the disease in the bovine race, the wonder is that we have not been sooner sceptical on this point.

An outbreak of the disease occurring in a country district, and therefore more striking than if it had occurred in a large centre of population, lately attracted my attention, the more so that, were it not for a contemporaneous outbreak of measles, there was at the time a marked absence of other disease.

In the month of January of this year many cases of pneumonia occurred in the neighbouring districts, and of these cases several died. The following case was one of them, which came under my notice:—

CASE.—I was called on the 12th February to see a farmer, aged fifty-three years, not of a strong constitution, given to occasional outbreaks of intemperance, but at other times hard-working and industrious. On the 8th he worked very hard in loading carts with manure, the locality being a bleak hill-side, exposed to easterly and north-easterly winds. He got very warm, and taking no precaution, continued for some considerable time exposed to the inclement weather in the light clothing in which he worked, and before returning home he felt, as he expressed it, as if "chilled to his marrow." On reaching home he took some warm stimulant, but, very soon, dull pain set in under the right scapula; he began to cough, and on the third day he expectorated red-coloured sputa. I saw him on the 12th, when his aspect was anxious and shrunken, his colour pale; he complained of dull pain in the base of right lung, with dulness on percussion, with crepitation about half way up the lung, of a sharp dry character, and there was abundant expectoration of a very dark-coloured thin fluid. Pulse, 92; temp., 101·2°. I ordered good nutriment, a little brandy, hot poultices of linseed with a small quantity of mustard, a mixture of aqua. acet. ammon., spt. æther. nit. in camph. mixture. I saw him next day, and found that dulness and crepitation had set in in the left lung, and that its extent had increased in the right. Temp., 100·4°; pulse, 84, very soft and compressible; large quantities of a dark fluid coughed up. I ordered brandy, quinine in large doses, and mixture of carb. ammo. and polygala, but he did not get a dose of the medicine and died early next morning, in accordance with my prognosis.

About three weeks afterwards I attended his brother, in the same neighbourhood, under very similar circumstances; but being a man of more vigorous constitution and eight years younger, he recovered. In this case, however, the thermometer rose to 104° and the pulse to 130. He, too, had been exposed to severe cold while heated, and in both cases the houses, though clean, were approached by yards filled with manure, and were situated on cold bleak elevations.

I now come to the outbreak of the disease in Portlaw, from which place the residence of the foregoing patients is five miles distant. The village of Portlaw stands within a square of half a mile, containing 370 occupied houses and 94 at present unoccupied. The houses in which the larger number of the cases I now write of occurred are far beyond the ordinary workman's house in construction, containing three bedrooms, 13 feet square, 14 feet in height, and a large kitchen or living room. Sixty of them are two storeys in height, the others but one; each has a walled-in yard, containing a privy and cesspool, which opens behind the back-wall of the yard, and 6 feet further out is a sewer into which smaller ones run from the yards to carry off slops. The cesspools are opened and cleaned periodically, being in the meantime covered with flags and clay, but they are too often overful and the sewer is often blocked. It is evident from this that there is always a certain amount of material of a nature to give rise to zymotic disease present, I may say, equally in each of these houses. The habitations in which the other cases occurred are decidedly bad, both in structure and conservancy, so that as this element of disease has been constant, the question merely arises whether it has been so placed as to come into action. It has not for a long time given rise to any epidemic, such as typhoid fever, which could be positively charged upon it. The epidemic of measles was present in all parts of the country, and the rapidity with which it spread points rather to extension from case to case, or as being the result of climatic influence than to the action of local nuisance.

Case.—On the 17th February I was called to see a carpenter, living in one of the two-storied houses, who, having been out of work for some time, and having got a contract job, set to work at it with energy. The work was out of doors. After a day or two he got a chill, the weather being raw and damp, with a sharp north-west wind; with this chill he got violent acute pain under the left clavicle, cough and rusty sputa. He went to bed, and when I saw him on the third day he was in acute suffering. Temperature normal; pulse 130; tongue white and furred: dulness and coarse crepitation all over the left side; the sputa very thin and dark-coloured. I gave him subcutaneous injection of morphia, hot turpentine and poultices externally, with carb. ammon., polygala and brandy, but he died the day after I saw him.

At the same time a man living at the distance of thirty yards in the same row of houses—a night watchman—became ill in the same manner, only that the right side was affected. He died on the third day, but in this case I was not surprised at his speedy decease as he was broken down in health by long service in India, where he had severe hepatic disease, and before this illness set in I was watching him for suspected aortic aneurysm. He attributed this attack to having worn wet clothes during great part of a night on duty.

I will not detain you with any detailed cases as it is unnecessary for the object I have in view, but will state generally that between this time and 1st May I had altogether, under my care in Portlaw, 28 cases of this disease, of whom 26 recovered; of those, 18 lived in houses of the better class, such as I have described, and were subject to the same influences. Only in one instance had I two cases in one house, one of whom, however, was attacked by the disease while residing a distance of six miles from home, where he had not been for three weeks previously, and returned to be nursed. I must here remark that I had, during this period, over 150 cases of measles under my care, only two of which died. Though I do not include these last amongst the cases of pneumonia, they died of pulmonary congestion, being the only two amongst all the cases of measles who presented severe pulmonary symptoms, though the eruption and symptomatic fever were in many of the adults intense. I set down no case as being one of pneumonia unless there was distinct dulness on percussion, crepitation, and coloured expectoration. Many of them were very severe, fourteen were distinctly of the croupous character, and in most of these more or less permanent mischief has remained, though in not one was there evidence of pleuritic effusion. In one case the lung has become so completely hepatized that, at present, the difference of measurement between it and the left side is very marked, and the body is quite distorted by lateral spinal curvature. Having treated so many cases with such a marked success, I may be asked what course of treatment did I pursue. It was old-fashioned and common-place. I must premise that I generally saw the cases before the fifth day, often on the third or fourth. I prescribed the pediluvium at first, gave bland warm fluids, such as milk and water, tea, whey, and gruel freely; the old-fashioned diaphoretic saline—aqua, acet. ammon. and nit. potass., combined with the vinum antimonialis, vinum ipecacuanhæ, and small doses of morphia, the latter often subcutaneously, and in a few cases freely for the relief of acute pain. In the early stage, when the pulse ran high and the heat of skin was great, I used aconite freely. I applied externally hot turpentine, hot linseed poultices, dry cupping and, in a few cases, leeches. Six cases were of the broncho-pneumonic type, most of these were very severe, some almost touched the verge of the next life. In this form I found no more reliable medicine than the vinum antimonialis, which even in old people can be given to a considerable extent with advantage by administering it in small doses frequently repeated. One case, a female aged 32, mother of three children, caught cold pulling ferns on the hillside in wet weather. The bronchial congestion was intense, and to all appearance I never saw any one nearer death than she was on the eighth day—loud bronchial râles and fine crepitation over both lungs, profuse brickdust sputa, dulness on percussion over base of both lungs, blue lips, and glazed eyes. Temperature, 104·6°; pulse, 136. I gave her ʒ$\frac{1}{2}$ spt. turpentine in emulsion every three hours, and on the next day, after eight doses, she was comparatively convalescent and made a rapid recovery. Having been called to one case, a boy aged 13 years, who had been exposed for some hours to a sharp north-east wind twenty-four hours before, I found him with severe pain below right scapula, dulness and crepitation and coloured sputa just commencing, but in this stage bright red; his pulse 120, temperature 103·6°. I ordered him internally $\frac{1}{2}$ dr. liq. ergotæ every three hours, with a poultice externally, and next morning I found—temperature 98·6°, pulse 74, crepitation gone, and he made a rapid recovery. I had not a suitable case to try it again. In young people I often found that the poultices induced too rapid resolution, the secretion from the mucous membrane becoming more abundant than the patient is able to get rid of, and this, in young and feeble subjects, becomes a serious source of danger. In such cases I have found a small blister, and covering the part when it is removed with cotton-wool under gutta-percha paper, prove most useful.

In one case, that alluded to above, where such hepatisation remains, I think life was saved by the rapid administration of calomel, opium, and hippo. In some few cases where the temperature remained high on the 7th or 8th day, I gave quinine. Although I thought it had good effect in reducing the temperature, I am not in love with it as a remedy. I found small doses of brandy act more quickly and with equally permanent good effect. Of course all through the illness I maintained the strength with beef-tea and other nutrients, but, though I gave wine and whiskey or brandy in small quantities when I thought them required, I had not in any case to have recourse to the lavish use of stimulants which I sometimes see practised by my junior *confrères.*

Now, to sum up :—Within a small area, 28 cases of well-defined pneumonia came under my notice, 18 of these were placed, as to residence and exposure to noxious influence, in almost identical circumstances, the majority of the remainder in circumstances highly analogous. In the large majority of all the cases each patient described his illness as originating with some special exposure to cold air or wet clothes, the weather during the entire period being severely cold, with a prevalence of sharp winds. Each patient was nursed in his poor, and too often impure, home by wives, mothers, sisters, and brothers, all in the closest contact, yet no two cases occurred in the same family or the same house save in one instance as stated above. I am inclined to think that we are to look to climatic influence, acting on frames predisposed by exposure to noxious matter, for the extension of the disease and not to infection or contagion.

HYPERPYREXIA IN RHEUMATIC FEVER.

By W. LANGFORD SYMES, L.K.Q.C.P.I., &c., &c.;
Late Surgeon "Pacific" and "Orient" Royal Mail Services.

[Read in the Medical Section, February 26, 1886.]

Some interesting discussions having taken place on this subject from time to time, and on the best means of treating this most dangerous complication, I deem it my duty to record my own limited experience, as every particle of reliable information is of the utmost importance, especially as the propriety of the application of iced water to the body in states of high fever is still a "questio vexata" amongst the profession, and universally deprecated by the general public.

The following case of rheumatic fever of a peculiar type came under my notice while on a voyage to South America, and it illustrates very well the liability to a sudden rise of temperature at any period of the disease, the height which it may attain, and the marked success of the much-dreaded treatment by the iced pack:—

Case.—*Acute articular rheumatism; hyperpyrexia; temperature* 108° *Fahr.; delirium; acute endocarditis; use of iced pack; recovery.*—Mrs. B., aged twenty-seven, passenger from Liverpool to Valparaiso, was exposed for some hours to cold wind and driving rain on the 5th November, 1884, immediately before embarking. She had previously been in good health, and had no family history of any complaint. When off the Spanish coast, about five days later, she complained of severe pains in the ankles, wrists, knees, and elbows, which joints were swollen and tender. The temp. was 100°, pulse 140; tongue coated with brown fur; bowels confined; skin "clammy" with offensive perspiration, which had a distinctly acid reaction; headache and feeling of general malaise—in fact, all the ordinary symptoms of subacute articular rheumatism. The heart was perfectly sound; kidneys acting freely, the urine being full of lithates, but containing no albumen.

She was placed in hospital between blankets. The joints were wrapped up in cotton wadding and covered with oiled silk. She was put on "fever diet," and took the following mixture every three hours:—

℞. Tinct. aconiti - ♏ 2.
Nepenthe - ♏ 15.
Sodii salicylatis - gr. 15.
Spt. chloroformi - ♏ 15.
Aquæ ad - ℥ss. Tertiis horis.

The bowels were opened by enemata when necessary, and in nine or ten days she was up and walking about the decks perfectly free from any pain or fever.

On the night of November 28th, having been convalescent for six or seven days previously, and able to get about, she got wet through while lying asleep in bed, by a sea coming in through the porthole; and next morning (29th) complained of the pains returning in the joints, and the old symptoms coming on again, the temperature being 99·4°, and the pulse 120. The joints were again wrapped in cotton wadding as before, and she was put on the same mixture. That evening the temperature rose to 102°, and the pulse to 140. The joints were very painful, so much so that she could not move in bed, the shoulders and hips now being involved. The heart was sound, acting regularly, but seemed at times a little excited, having a somewhat "thumping" action, for which I applied emplast. belladonnæ. Her mind was clear, she had no headache, and was perspiring freely. On the following morning (Nov. 30th) her husband came to me at 8 a.m., saying his wife was "so much better," and wanted to know if she might get up, as she had "no pains whatever," and was "sitting up in bed." I found her, as he described, sitting up, looking very much excited and nervous, bathed in profuse perspiration, her eyes glistening, breathing rapidly, speaking very incoherently, and deliriously wanting to get out of bed and go on shore immediately. On taking her temperature the thermometer registered 106° in the axilla. Pulse 140, markedly intermittent, irregular in force and rhythm, and very much excited. She was alternately yawning, "going off" into short syncopal attacks, sighing deeply, and throwing her arms and legs about most recklessly. It was now quite evident what had occurred. On examining the heart a loud blowing, systolic sound was audible at the apex, extending round to the inferior angle of the scapula, and not conducted into the cervical vessels, indicating mitral regurgitation. The heart was labouring violently for two or three pulsations at a time, and then became almost inaudible, dropping a beat at most uncertain intervals, and having no regularity whatever either in force or rhythm, no two beats seeming alike. I immediately gave her 20 grains of quinine, with 5 minims of tincture of digitalis and a little brandy, as an antipyretic, with a view to subdue the high fever; and although this was partially retained on the stomach, the temperature, instead of, as I expected, having fallen, had risen in an hour to 108°. The delirium had increased. Her tongue was perfectly dry, and no improvement ensued either in the heart or pulse.

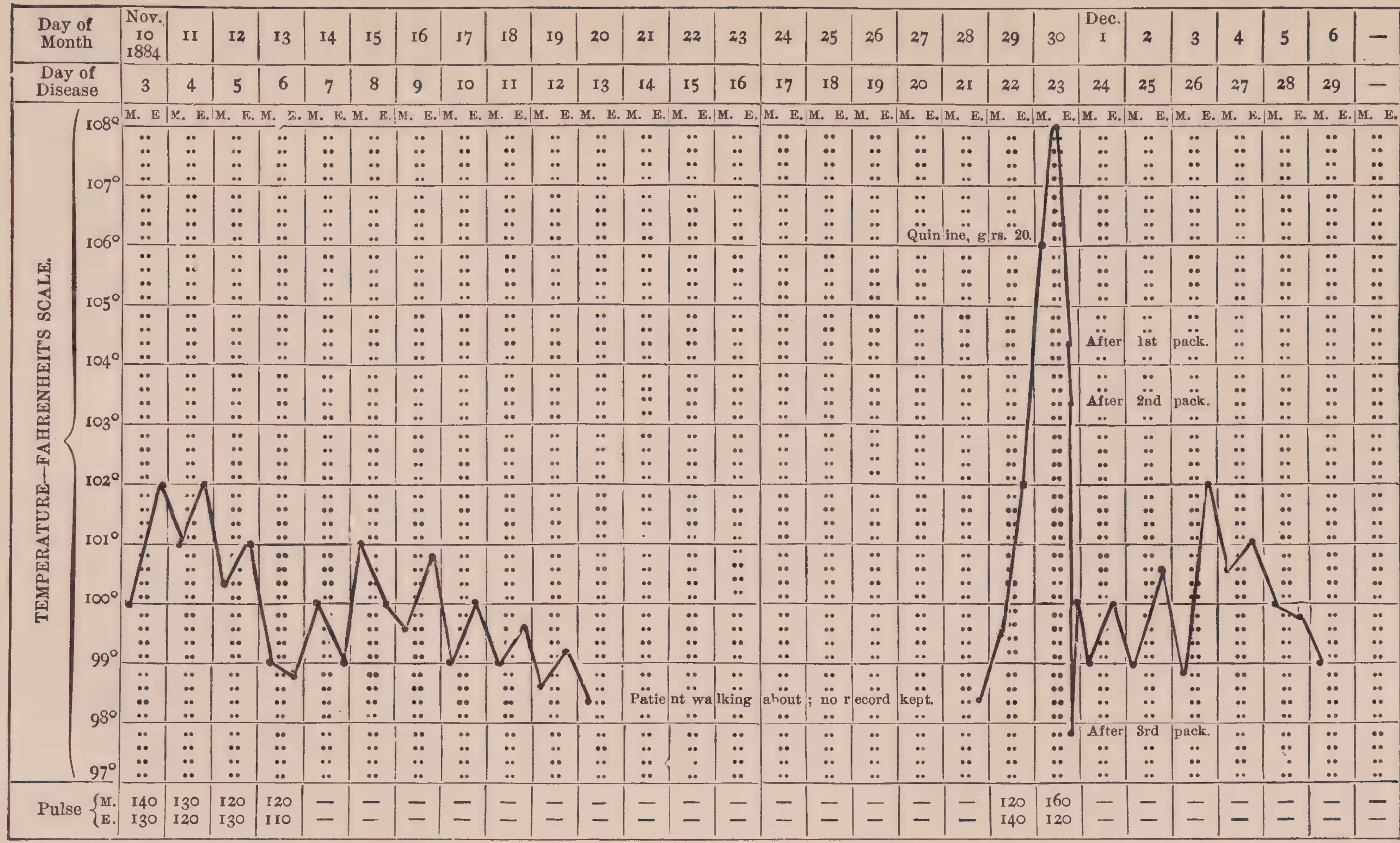
Seeing this effort to reduce the temperature had failed, I was at a loss to know what to do, fearing lest the shock of an iced bath might end fatally, being too severe and overpowering for the heart in such a precarious condition, and that she might die of syncope while in the bath. However, having no second opinion to consult on the matter, and knowing that the case must terminate in death if left alone, and that very soon—in spite of the remonstrances of her husband and numerous friends, I proceeded to apply the "iced pack" in the manner taught by Professor Finny (who has written a small pamphlet on the subject). Having divested her of all clothes, and placed a mackintosh underneath, towels wrung out of iced water were wrapped round her head, chest, arms, legs, and back. She was covered with blankets, and left thus for ten minutes. The first effect of this was most marked. She completely lost all her delirium, spoke rationally, and expressed her delight at feeling the icy cloths around her. She took some beef-tea and about a teaspoonful of brandy, and seemed much better in every way, and this during the first ten minutes. The towels when removed were burning hot, and the thermometer stood at 104·4° in the axilla, registering a fall of 3·6°. The towels were again applied as before, and remained for ten minutes, when the temperature had fallen to 103·4°. After the third and last application the temperature had fallen to 97·8°, thus making in all a sudden fall of 10·2° degrees in the space of 35 or 40 minutes.

As will be seen from the chart, the temperature rose again that evening to 100°, and three days later to 102°. However, from this point she gradually began to improve; the pains diminished, and she was put on—

℞. Tinct. ferri perchloridi	-	♏ 15.
Glycerini	-	ʒss.
Spt. chloroformi	-	♏ 15.
Infus. calumbæ ad.	-	℥ss. Ter in die.

CHART OF TEMPERATURE, &c.

Name, Mrs. B.; *Age*, 27; *Disease*, Acute Rheumatism; *Result*, Recovery.



She finally landed in Chili on December 15th free from any acute rheumatic symptoms other than a damaged mitral valve, which still remained to remind her unfortunately at some future date of the dangerous illness through which she passed.

Remarks.—The foregoing case presents some interesting features, not that it is in any way unique, but an instance of remarkable recovery under very adverse circumstances. Commencing as an ordinary case of sub-acute rheumatism, and convalescence supervening on the usual treatment, its subsequent course was most unexpected, being marked by a sudden and severe relapse, happily not often met with.

This teaches us that the very mildest cases of rheumatic fever are fraught with extreme danger, and that it is very difficult, if not altogether impossible, with our present knowledge, to foresee and predict the onset of hyperpyrexia, or the occurrence of complications during its course. The importance of frequent and careful examination of the heart cannot be too stringently laid down, inasmuch as this organ is that most frequently attacked by secondary rheumatic inflammation; and serious mischief may be going on in the endo- or peri-cardium without the patient evincing any very decided symptoms. True it is that delirium usually presents itself as the first cardinal sign of cardiac inflammation in the course of rheumatic fever. It is not to be relied on, however, as a pathognomonic symptom, as it occurs in this disease quite independently of heart complication; and there has been extensive inflammation of the cardiac serous membranes without the least delirium occurring. In this case, however, endocarditis was present, and the first symptoms of a change for the worse were—talking during sleep, a delirious change in the patient's expression and manner, and complete cessation of all pain, she being able to move any joint with perfect freedom, which had hitherto been quite useless and painfully swollen. The temperature continuing to rise, these symptoms indicated very plainly that metastasis had taken place, and the force of the fever had been directed to some new organ hitherto intact. As is most common in cases of this kind, the heart was now the seat of secondary inflammation, and that in a very severe form. The endocarditis was so sudden and acute that I feared pulsation could not continue very long; and this reflection questioned in my mind the propriety of the cold bath, which objection, however, I very fortunately set aside.

The exact changes in the patient's condition before the onset of hyperpyrexia chiefly took place during the night, so that I cannot record them accurately in their order of occurrence. However, I found that she talked continuously in her sleep, was very restless, and when I saw her next morning, delirium, mitral endocarditis, and hyperpyrexia were all present. With regard to the hyperpyrexia, I think nothing could be more satisfactory than was its treatment in this case by the iced pack. Not only was the temperature brought down from 108° to 97·8° in less than an hour, but its soothing effect on the excited and delirious condition of the patient was most marked, the mind becoming clear the moment that the ice was applied to the head. This sedative effect of an iced pack I have also repeatedly observed in cases of *delirium tremens* and sunstroke, where the patient gets excited and feverish, and on more than one occasion have I seen sleep follow its application.

It must not be expected that the temperature, when brought down to normal by the iced pack, will remain at or below that standard for any length of time; for, without any fresh cause or aggravation of the symptoms in any way, it rose in the foregoing case to 102° three days afterwards, as if it were resuming its proper position in the chart, and thus corresponding proportionally with the other symptoms. Rarely, however, if ever, does it again reach its original height without death resulting.

It has been contended, on physiological grounds, that when cold is applied to the surface of the body it reduces merely the external temperature of the skin, while that of the viscera and internal organs is not affected—nay, that it is actually raised, as determination of blood to the viscera takes place, bringing the thermogenic functions of these organs into play, and thus causing a corresponding increase in the production of animal heat. Be this theory of physiology as it may with reference to the influence of external cold on a healthy individual, the therapeutic fact remains that the application of cold water to the surface is the best means we possess at present of reducing high fever; that it does this effectually, and that it ought to be resorted to in every case where the temperature is over 105°. The simplicity of its application renders the iced pack more suitable for private practice than the actual cold bath, and hence for this reason it will probably become more generally useful.

That it is attended with some slight risk I think no one will contradict, and this chiefly from one of two sources—either sudden shock to the nervous system, which is quite feasible, considering the enormous difference (76°) between the temperature of the skin at 108° and the water at 32°, evidenced in each case that I know of by the occurrence of severe rigors; or else sudden determination of blood, ending in apoplexy of some organ. These objections, however, are merely theoretical, and, unless occurring frequently, ought to be disregarded in such a crisis, where other means have failed, and where time wasted may signify life lost.

It seems a pity that a treatment so generally beneficial in extreme cases of hyperpyrexia should need defence; and yet the aversion which the public entertain towards it is so strong, and so bigoted is their prejudice against it, that they regard it in many cases simply as an unwarranted experiment. So much is this the case, that the experience of this treatment is almost exclusively confined to hospitals, the numerous over-zealous friends and relations of patients rendering it almost impossible in private practice by their decided disapproval.

Touching the interesting and important subject of the prediction of complications in rheumatic fever, very little is known at present. It has been stated that cessation of perspiration is indicative of hyperpyrexia setting in ; but in this case, and in several others that I know of, the perspiration was very profuse during the whole attack, and yet hyperpyrexia and endocarditis were both present in very severe forms.

In a case of very severe rheumatic fever which I attended in private with another practitioner, where the temperature rose to

106° (no wet packing being allowed), complicated with endocarditis and ending fatally by meningitis, the following symptoms were observed in this order before the heart became affected :—

1. The urine, which before had been loaded with lithates, suddenly became perfectly clear and transparent, the specific gravity being also diminished. This was observed as the first sign of a change in the course of the fever, about eight hours before the heart was attacked, no other change being noticed in this excretion.

2. The temperature rose from 104° to 105° while taking quinine and aconite in full doses.

3. Patient commenced talking and "wandering" in his sleep.

4. The first sound of the heart was noticed to be gradually getting weaker, sometimes being hardly audible, but the pulse remaining very good and strong, as if some effusion existed in the pericardium. However, no other sign of pericardial mischief was present.

5. Active delirium.

6. Joints rapidly improving, being able to move them freely without pain. Throwing his legs and arms about recklessly.

7. Sighing, and slight syncopal attacks coming on, alternating with excited and irregular action of the heart; but no murmur accompanied or substituted either sound, so far.

Within an hour after this a faint systolic bellows murmur was heard at the apex, extending round towards the axilla; not audible in the vessels of the neck; gradually increasing in intensity until it was heard distinctly all over the cardiac area. This was noticed about eight hours after the initial alteration in the urine.

Very intense headache, contracted pupils (insensible to light), and muscular twitching now supervened, followed by cerebral vomiting and coma, which ended rapidly in death, with a temperature of 106°.

It is difficult to say how long inflammation of the serous covering of the cardiac valves takes to develop the physical sign of murmur; but it would seem that it must be present for at least some hours before the valve becomes altered in shape, either by erosion of its edges, or the growth of minute vegetations on its surface. However, if we find the foregoing train of symptoms present, although no adventitious sounds can be detected by the stethoscope, I think we may safely conclude that inflammation has already set in in the heart, to be followed in a short time by the pathognomonic sign of pericardial friction sound, or endocardial murmur. Whether the altered condition of the urine is a common antecedent to complications in rheumatic fever I cannot state; but in this case it was decidedly marked, and, I think, had some bearing on the subsequent rise of temperature, the onset of delirium, and the sudden change in the course of the fever.

Be this as it may, the fact remains that at present we have no sure and infallible sign by which, in any given case of rheumatic fever, we can predict that it will or will not be accompanied by complications, or that the temperature will rise to an unusual height. This, as well as many other difficulties in connection with this affection, will have to remain for its solution until the combined experiences of practical physicians and the scientific researches of learned physiologists unite to enlighten us in the pathology, prognosis, and treatment of rheumatic fever, and reduce to a minimum the mortality from this most treacherous disease.

PERIOSTITIS FOLLOWING ENTERIC FEVER.

By H. C. TWEEDY, M.D., Dubl., F.R.C.S.;
Physician to Steevens' Hospital, and Examiner in the Royal College of Surgeons, Ireland.

[Read in the Medical Section, Friday, March 26, 1886.]

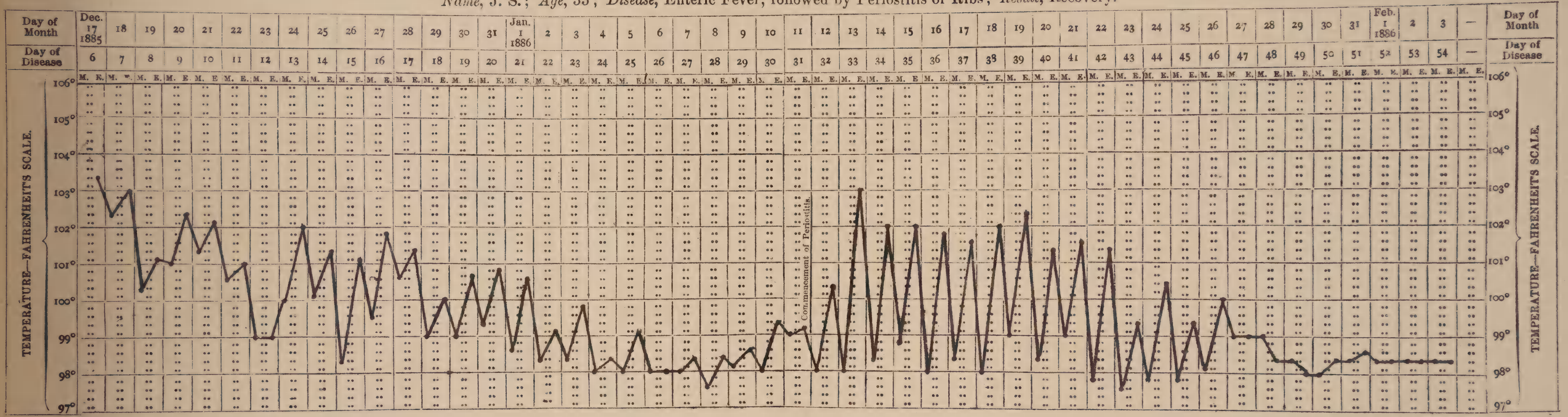
Amid the long catalogue of medical and surgical affections which occur as sequels to enteric fever, a place has in recent years been claimed for a form of periosteal inflammation possessing certain definite characteristics; and although little or nothing is to be found in the ordinary text-books on the subject beyond a bald acknowledgment of the fact, yet too much attention has been drawn by writers of note to the occurrence of this disease in connection with typhoid fever to permit of its being so completely ignored. In consideration, therefore, of the comparative rarity of this affection, and the fact that it has not been noticed, as far as I am aware, at any of the meetings of the Academy of Medicine in Ireland, I have ventured to lay before this Section some brief notes of a case of the kind which I had recently an opportunity of observing in Steevens' Hospital.

J. S., aged thirty-three, a groom by occupation, was admitted to hospital on the 15th December, 1885, with well-marked symptoms of enteric fever—smart diarrhœa, tenderness and gurgling, characteristic tongue, and a temperature of 103·5 deg. He was stated to have been ill six days before admission, and the truth of this statement was confirmed by the appearance of rose-spots a couple of days after. The case proved simple and uncomplicated, progressed favourably, and on January 2nd, the twenty-second day of his illness, the evening temperature was 99 deg., and continued so with very slight variations for some days, during which time he expressed a great wish to get up, saying that he felt quite well, and was continually clamouring for animal food. This was, of course, denied him, and it was not till the 9th January (his evening temperature having been practically normal for eight days) that he was for the first time allowed a mutton chop. The following day there was a rise in the evening temperature, but unattended by any further constitutional disturbance. Fearing that there might be a recrudescence of the fever, the patient was carefully watched; but though the temperature continued to rise there was no other symptom of relapse. The lungs were also carefully examined, but no mischief could be detected there. He now himself drew attention to a tender spot, about the size of a crown piece, just below the right sterno-clavicular articulation. There was not, however, any redness or tumefaction to be found at the time. On the evening of the 13th January the temperature had risen to 103 deg., and continued high for ten days. There was by this time a well-marked oval swelling plainly discernible underneath the sternal attachment of the great pectoral muscle, and extending from the second to the fourth costal cartilage. It was extremely painful, and the pain was much augmented when the fibres of the muscle were put upon the stretch. The tumour was elastic to the touch, and some fluctuation could be felt, especially in the transverse direction; but there was no redness or discoloration of the skin, and I may here state that there was no history of syphilitic taint. My colleague, Mr. Hamilton, was good enough to examine the case for me several times, and by his advice the only treatment was constant poulticing, as well as the continuance of the ordinary iron and quinine mixture of the hospital, which the patient had previously been taking for some days. The temperature began gradually to fall on the 20th, and was perfectly normal on the 28th, continuing so from that out. The tumour also gradually subsided, became less painful, and finally disappeared altogether before the man left hospital.

I have already remarked that some writers have stated that periostitis is a true sequel of enteric fever. Sir James Paget, in an interesting paper in the "St. Bartholomew's Hospital Reports" (Vol. XII., 1876, p. 1), gives, as far as I can learn, the first detailed account of this affection. He states that "it has its most frequent seat in the tibiæ, but occurs also on the femur, the ulna, and the parietal bone; that it rarely occurs on more than one bone in the same person; that it makes its appearance during convalescence, and is sometimes followed by necrosis, although never by the

CHART OF TEMPERATURE.

Name, J. S.; *Age*, 33; *Disease*, Enteric Fever, followed by Periostitis of Ribs; *Result*, Recovery.



delirium fever or other severe symptoms attendant on acute necrosis." Referring to periostitis of the ribs, he adds that he has never seen it associated with necrosis; that it invariably commences, as in the case now detailed, in a well-defined round or oval swelling on one or more ribs, and always on the front of the chest. It usually suppurates, forming sinuses through which thin and pale pus continues to flow for many months, although sometimes it subsides without any suppuration or more change than a slight thickening of the periosteum.

While, however, giving to Sir James Paget the credit which is due to him of being the first to describe this affection with any minuteness, it is but fair to mention that ten years previously, in the proceedings of the Surgical Society of Ireland (April 6, 1866, *vide Medical Press and Circular*, p. 426, Vol. I., 1866), we find that "Dr. Grimshaw exhibited a portion of the upper jawbone of the right side, which had exfoliated immediately after an attack of fever." It is not stated whether the fever in question was enteric or not, but Dr. Grimshaw informed me that the case had been described as one of gastric fever.

Dr. Afflek (*British Medical Journal*, May 9, 1885), cites three instances of this affection which came under his observation in the typhoid wards of the Edinburgh Royal Infirmary. These three cases of periostitis occurred in a total of 117 cases of typhoid which were under treatment in 1884, and differed from the cases recorded by Sir James Paget in that two of them occurred at the height of the fever, and the third when convalescence had barely begun.

The two first cases were in young men aged twenty-one, one of whom was admitted in the third week of the fever, with periostitis commencing in the right tibia. The other, who was admitted at the commencement of the fever, showed symptoms of marked periostitis in the right humerus in the third week, and this was followed by a similar condition of the right tibia. The third case was that of a girl aged nine, who was admitted with a very severe attack of typhoid fever, and was attacked in the fifth week with periostitis of the right humerus. Additional cases corroborative of the above have been published by M. Mercier, *Revue Mens.*, Jan. 3, 1879; Dr. Hayward, of Liverpool, *British Medical Journal*, January, 1885; Dr. Jackson, of Manchester, *British Medical Journal*, Feb. 28, 1885; Dr. King, of Chester, *British Medical Journal*, May 9, 1885; and others, including Professor Strümpel, of Leipsig—and prove beyond question that periostitis does occur associated with enteric fever, whether as a mere coincidence or, as the weight of evidence seems fairly enough to demonstrate, as a true sequela of the disease.

Assuming the latter belief to be correct, how may the occurrence of periostitis under such circumstances be accounted for? Many and various have been the theories propounded.

In speaking of periostitis of the ribs, Sir James Paget says:—"It so nearly resembles ordinary scrofulous periostitis of the ribs that I have sometimes thought it should be regarded, too, as only an evidence of scrofula educed by the feebleness of nutrition consequent on the fever. Yet I have never seen it associated with the signs of scrofula, and it has occurred after typhoid in some of so robust and apparently unblemished constitution that it would seem absurd to impute scrofula to them." He also throws out the suggestion that each fever may have "its own proper sequels," and is in this sense, though perhaps in less degree, as specific as in its fever period; and he adds further that he has not seen periostitis occurring after any other than typhoid fever, or a fever which he supposes to be closely related to it. In an article published some time ago in *Le Progrès Médical*, Routier divides such cases into those where only the external layer of periosteum is affected, those that are only subperiosteal, and those where the bone is also affected. He believes the local inflammation is always the result of injury, a blow or a violent muscular contraction, these causes being sufficient to produce the condition when the body is in the enfeebled state after an attack of enteric fever. Dr. Haywood suggests a septicæmic origin in some cases, just as some of the instances of parotitis, marasmus, and phthisis, after enteric fever, are supposed to arise; and he mentions in connection with this that in one of his cases periostitis appeared about several of the long bones; suppuration occurred with external discharge, but, after new attacks of periostitis had ceased to occur and the old sinuses had healed (which they did very rapidly), the patient sank under acute phthisis, of which, previously to the enteric attack, there had been no evidence.

Dr. Affleck offers the rational explanation that it is probable that the affection depends upon the lowered nutrition of the osseous tissues as the result of a severe or prolonged attack of enteric fever, most of the cases in which it has been noticed appearing to be of this character. "In this fever pre-eminently the nutritive changes are manifold and profound, and depend partly upon the pyrexia, but especially upon the great weakening of the assimilative function connected with the morbid alterations in the mucous membrane of the intestines, which may make itself felt even after all febrile action has passed away." To sum up briefly what is known of this comparatively rare affection, we learn—1st. That it occurs most commonly during the stage of convalescence, although cases are recorded in which it appeared as an actual complication of enteric fever. 2nd. That it attacks most frequently the tibia and the ribs, but that it has been observed on the femur, the humerus, the ulna, and the parietal bone, and that it rarely occurs on more than one bone at a time. 3rd. That the course of the disease is usually chronic, suppuration generally taking place after a time; that sometimes necrosis follows periostitis of the long bone, although this termination has not been noticed in cases of periostitis of the ribs, while occasionally, as in the instance cited, the case terminates in a spontaneous cure.

THE CARDIAC MURMURS OF THE MITRAL AREA.

By C. J. NIXON, M.B., LL.D., Univ. Dubl.; M.D., R.U.I.;
Fellow and Examiner, Royal University, Ireland.

[Read in the Medical Section, Friday, March 26, 1886.]

In discussing the varieties of murmur which are developed in connection with the mitral valve, and audible over the mitral area, I purpose to deal chiefly with the special points of interest in connection with the murmurs themselves, not with concomitant symptoms of the lesions which they indicate. The murmurs to which I shall direct special attention are :—

I. PRESYSTOLIC MURMUR.
II. POSTDIASTOLIC MURMUR.
III. ORGANIC SYSTOLIC MURMUR.
IV. FUNCTIONAL SYSTOLIC, OR POSTSYSTOLIC, MURMUR.
V. MITRAL BRUIT DE SCIE.

I. PRESYSTOLIC MURMUR.

The first two murmurs, it is needless to say, are the common physical signs of mitral stenosis, and it is necessary to study certain peculiarities of each. Taking, first, presystolic murmur, the points to which I would specially direct attention are—its site of maximal intensity, its acoustic characters, the conditions of the mitral valve with which it is connected, and the signs which may simulate it. Since Fauvel and Gendrin first connected this murmur with a constricted mitral orifice, no physical sign of cardiac disease has been more thoroughly investigated, and none rests on a more secure basis as a sign pathognomonic of a certain physical condition. There may be some difference of opinion amongst physicians as to the precise acoustic character of the murmur—a difference which to some extent depends upon the fertility of the imagination in determining resemblances to the nature and quality of sounds generally. Thus, one writer remarks, that since the time of Laennec we have had cardiac sounds described as filing, grating, rasping, croaking, crowing, whining, caterwauling and blubbering. With such an *embarras de richesses* it is not surprising that some observers regard presystolic murmur as blubbering in quality—others, as rasping, as resembling the symbols, Rrrb, voot, or rup. I think, however, the term which best conveys the idea of its acoustic character is that it is vibratile. These differences of opinion, trivial and unimportant as they are, in no way influence the general acceptance of the murmur as a positive sign of mitral stenosis. Its site of maximal intensity is usually at a point slightly internal to the apex beat, though in many cases it is heard with great distinctness well to the left of the apex, on a line with the nipple. In very exceptional cases the murmur is carried downwards to the tricuspid area, as in two cases recorded by Dr. Sansom, where it was conveyed by convection from a calcareous auriculo-ventricular ring along the inter-ventricular septum to the base of the ensiform cartilage.

It may, no doubt, appear heterodox to deny that this murmur is invariably associated with stenotic disease of the mitral orifice, and yet I confess that, in some cases of extreme palpitation of the heart of a temporary character, I have heard an intersonal apex murmur which, from its rhythm, was apparently presystolic. This murmur was, however, developed under such conditions of cardiac excitement that I scarcely feel warranted in urging the view of the occurrence of a functional presystolic murmur. I would merely ask—Can a murmur be developed in such cases from a slight amount of regurgitation taking place just at the moment of commencement of ventricular systole, before the valves are forcibly brought into perfect apposition and, consequently, before their condition of tension is reached?

A mode of production of this murmur, independent of any organic disease of the mitral valve, has been urged by the late Dr. Austin Flint, which deserves some notice. I cannot, indeed, refer to this view without an expression of regret that the distinguished physician who enunciated it has, within the last few weeks, suddenly passed away, leaving a record in work done of which his countrymen may be proud. Dr. Flint holds that in some cases of aortic patency the mitral curtains are floated together by the regurgitating stream of blood, so that the mitral direct current passing between the curtains throws them into vibration, and gives rise to the characteristic blubbering murmur. In connection with this view I may observe that it has occurred to me more than once in examining the heart to find, over the mitral area, what was apparently a presystolic murmur of mitral stenosis; on listening over the aortic area a well-marked *bruit de scie* was audible. In those cases I have been able to satisfy myself that the diastolic basic murmur conducted to the mitral area gave a deceptive idea of the existence of a mitral direct murmur. In reference to this point I may quote an observation of Walsh:—"A murmur may fail to present the precisely same synchronism at different spots of the chest-wall, even within the limits of the deep-seated cardiac region. Thus, the rhythm of a reflux aortic murmur, precisely diastolic at the base, may cease to be so at the ensiform cartilage, as also, if audible there, at the left apex. At these points it synchronises more with the long pause, and approaches the first sound, or inclines to trench on systolic time."[a] I shall merely add that if a presystolic murmur could be produced in the way suggested by Dr. Flint, it would be difficult to explain its absence in all cases of aortic regurgitation where there was much dilatation of the left ventricle.

In noting the different conditions of the cardiac sounds with which presystolic murmur may co-exist, I believe the following order will represent the frequency of the changes produced:—

(*a.*) Presystolic murmur, with a sharp, clicking, first sound.

(*b.*) Presystolic murmur, with a systolic murmur of mitral reflux.

(*c.*) Presystolic murmur, with partial or complete extinction of aortic second sound at apex.

(*d.*) Presystolic murmur, preceded by postdiastolic murmur.

(*e.*) Presystolic murmur, with rough functional pulmonary murmur.

[a] Diseases of the Heart. Dr. Walsh. 4th edition. Page 79.

Fig. 1.

Fig. 3.

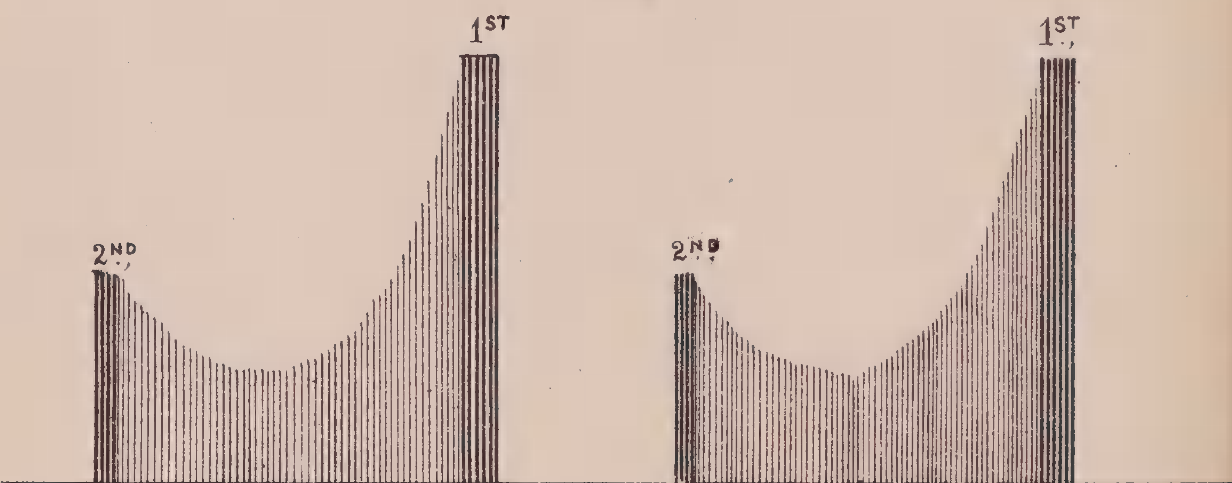


Fig. 5.

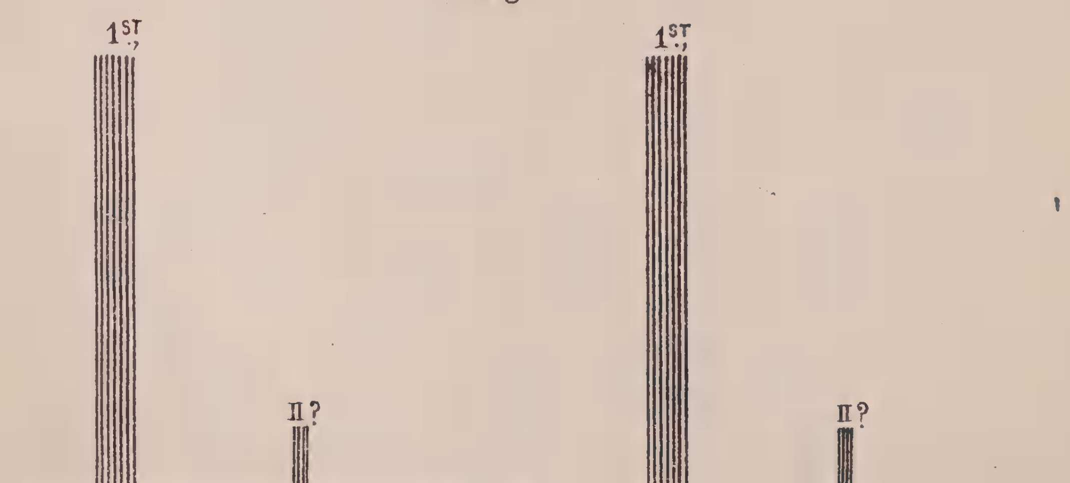


Fig. 2.

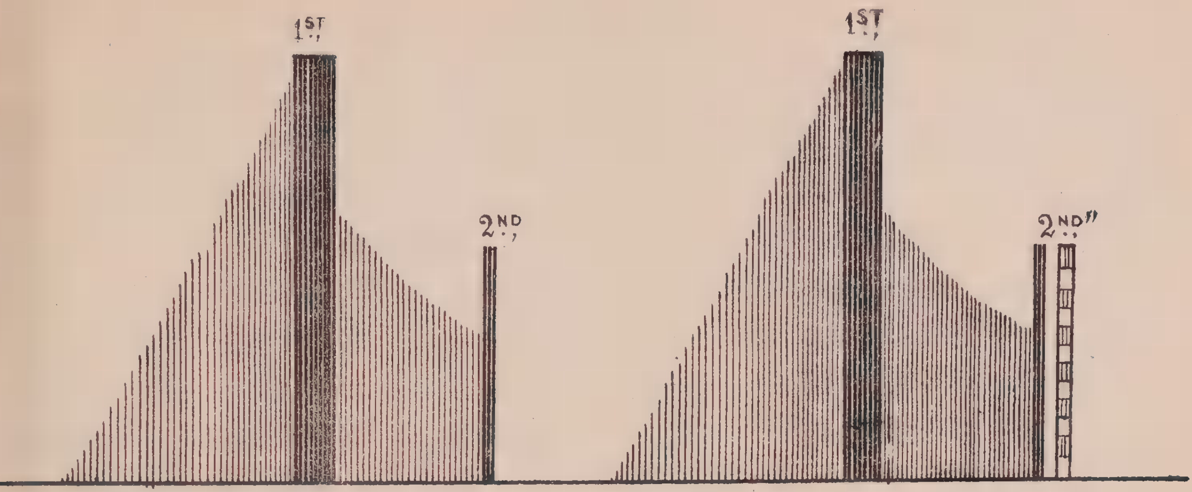


Fig. 4.

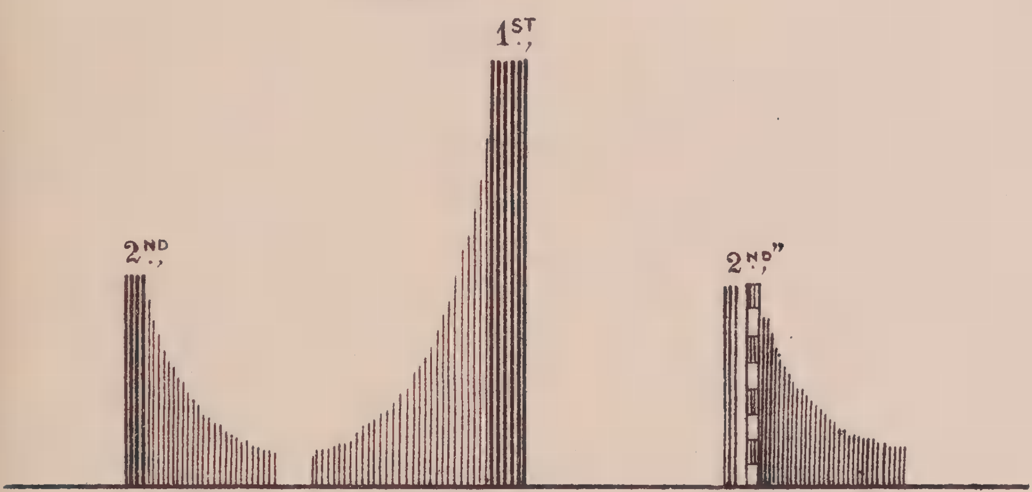
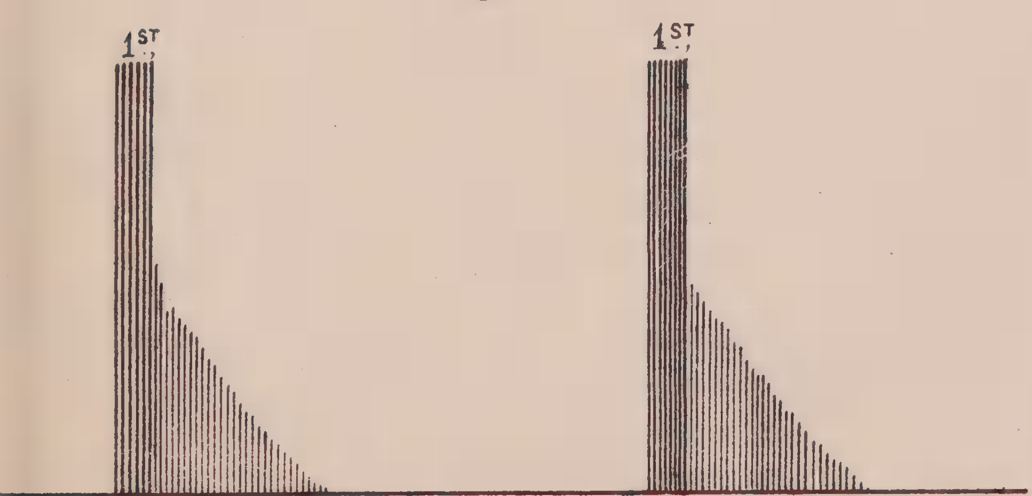


Fig. 6.



AB 86.

In all these conditions there is generally a marked accentuation of the pulmonary second sound, and in the majority of cases this sound is doubled.

There can be no doubt that mitral stenosis of a marked degree may exist without the development of any murmur, with the presence merely of a mitral systolic murmur, or simply with a clicking first sound. Usually the absence of the direct murmur is due to weakness of the heart generally, and of the left auricle specially. Its absence has been noted when tricuspid regurgitation takes place, so that the tension of the left auricle is thus relieved. Frequently the direct murmur can be reproduced by getting the patient to make some exertion, as sitting up suddenly in bed, swinging the arms about, &c. In like manner, a presystolic thrill can be developed or intensified by getting the patient to sit up and lean to the left side; the thrill is then readily appreciable to the hand placed over the mitral area. In some cases the absence of murmur may be accounted for by taking into consideration the change in the position of the left ventricle owing to extreme hypertrophy and dilatation of the right side of the heart. The left side is completely dwarfed in size, does not reach the surface of the chest, hence the vibrations of sound developed at the diseased valve are not audible. The disappearance of presystolic murmur is, however, in most cases a temporary phenomenon; after an absence of several days it often re-appears with marked intensity without any assignable cause. Figs. 1 and 2 afford graphic illustrations of presystolic murmur followed by clicking first sound, or by a systolic murmur.[a]

II. POSTDIASTOLIC MURMUR.

Closely connected with presystolic murmur comes the study of postdiastolic murmur. I use the term "postdiastolic" advisedly, as it is the accurate chronometric description of the murmur. If the principle of intersonal murmurs be admitted, there can be no

[a] For the preparation of the diagrams I am indebted to Mr. Ambrose E. I. Birmingham, Demonstrator of Anatomy in the Medical School of the Catholic University.

objection to the use of the term "postdiastolic." Both presystolic and postdiastolic murmurs are, no doubt, diastolic murmurs, if we regard them solely in relation to the condition of the left ventricle during the period of their development. Theoretically, there is some confusion in describing mitral murmur as diastolic, because it is the habit to speak of the sounds of the heart as systolic and diastolic, and to connect murmurs strictly with morbid conditions affecting the parts of the heart at which those sounds are developed. For this reason I ventured to suggest, in a communication on mitral stenosis which I brought before the Medical Society of the College of Physicians, that presystolic murmur might be considered as occurring in two forms—a short presystolic murmur, corresponding to the auricular systolic murmur of Gairdner, and a long presystolic murmur, where the morbid sound occupied the entire of the long pause; but I fully recognise that this nomenclature does not satisfy the conditions met with in the disease. Overlooking a merely theoretical objection, we find that the murmur under consideration is so closely related in time to the second sound, that the correctness of associating it with that sound must be admitted. As the murmur does not influence directly the mechanism of the production of the second sound, and as it distinctly follows it, instead of being exactly timed with it, or replacing it, the term "postdiastolic" is preferable, in my opinion, to "diastolic" murmur. If the existence of the murmur be determinable at a certain precise period of the cardiac rhythm by accurate clinical observation, it is irrational to ignore its presence on the ground of over-refinement, or hyper-division of the heart sounds. Postdiastolic murmur, in contrast to presystolic murmur, is smooth and suction-like in character. Its significance is that it indicates an extreme amount of mitral narrowing, and its recognition consequently becomes a matter of importance. In extreme mitral narrowing, the tension in the left auricle, during the momentary arrest of the flow of blood from it into the ventricle, during the contraction of the latter, must be considerably increased. The left ventricle actively dilates under a negative pressure, according to Goltz and Gaule, of 23·5 millimetres of mercury (the "vacuite postsystolique" of Marey); so that fluid veins are formed, and produce a sound of sufficient intensity to render a murmur audible.[a] Usually this murmur runs through the entire of the long pause, becoming faintly pronounced in the middle of it, and finally terminates in the rough presystolic murmur. The graphic representation of the murmur may be given as shown in Fig. 3.

Frequently this long murmur is split into two, there being a distinct break in the continuity of the sound in the middle of the long pause, when the murmur may be represented as in Fig. 4. Or the presystolic murmur may be entirely absent, when the signs of stenosis are represented by a clicking first sound, with partial or complete extinction of the aortic second sound, or with a clicking first sound succeeded by a systolic murmur, as in Figs. 5 and 6. Any of the conditions just mentioned may be associated with a systolic murmur of mitral inadequacy.

Dr. Broadbent, in a recent valuable and interesting communication on mitral stenosis, regards the development of the signs of this affection as divisible into three stages following each other in a fixed order:—

1st stage—Rough, vibratile, presystolic murmur, accentuated pulmonary second sound, aortic second sound audible at or beyond apex.

2nd stage—Presystolic murmur, with short, sharp, first sound, and disappearance of second sound at apex.

3rd stage—Disappearance of the presystolic murmur, a loud, sharp mitral first sound with or without a systolic tricuspid murmur.

I am not quite prepared to admit the chronological development of these signs, as given by Dr. Broadbent, though I freely concede the existence of the various signs he mentions at different periods of the disease. By a curious coincidence, when writing these lines on Sunday last, I was asked by Dr. Delahoyde to see in consultation with him a clergyman who presented what I regard as the typical

[a] Taking into consideration the mechanism of the production of postdiastolic murmur, it might be appropriately designated a ventricular diastolic murmur, whilst the presystolic murmur would be correctly termed an auricular systolic murmur.

signs of mitral stenosis of long duration. Twenty years ago the patient had been told by Sir Dominic Corrigan that he had heart disease, and he was specially warned to avoid violent exercise, &c.

The signs noted at the time of my visit were:—Apex beat displaced about one inch to left side, and slightly upwards; short presystolic thrill terminated by a sharp knock over site of impulse beat. At apex, on auscultation, a short, rough, presystolic murmur audible, followed by clicking first sound, and a faintly pronounced second sound. At a point about an inch to the left of the impulse beat, and somewhat above it, the murmur was heard with great distinctness, followed by the sharp first sound, but in this situation the second sound was no longer audible. At the base of the heart the second sound was doubled, the pulmonic element being distinctly accentuated. The usual signs of hypertrophied and dilated right ventricle existed.

Two other points in connection with the signs of mitral stenosis remain for a brief consideration—the alteration of the first sound which is usually noted, and the partial or complete suppression of the aortic second sound at the apex.

With reference to the first point, I may mention that I do not think there are good grounds for accepting Dr. Silver's view, that the presence of a sharply accentuated first sound preceded by a presystolic murmur is an indication of a funnel-shaped as distinguished from a diaphragmatic mitral valve. I cannot conceive the segments of the mitral valve producing by apposition of their surfaces, if such a condition could take place, a sound like that under consideration. One of the best-marked cases of a diaphragmatic mitral valve, with the usual button-hole slit, which ever came under my observation, was evidenced during life by a presystolic murmur and clicking first sound—the specimen I had the honour of exhibiting before the Medical Society of the College of Physicians.

It appears to me that the alteration in the first sound in mitral stenosis is due entirely to the tension and vibrations of the smooth, greatly thickened but flexible, mitral curtains. We have here to take into consideration the difference between the initial and final tension of the mitral valve, upon which the intensity of the valvular sound depends. In cases of aortic patency Traube has called attention to the suppression of the first sound at the apex of the heart. Owing to the regurgitation of blood into the ventricle, there is such an approximation of the initial to the final tension of the valve segments, that sound may not be sufficiently produced so as to be audible. In extreme mitral narrowing, on the other hand, the amount of blood in the ventricle is considerably diminished, there is a very slight amount of initial or presystolic tension of the mitral valve produced during the auricular contraction, so that if the ventricle contract with vigour, the difference between this and the final tension may be so great as to produce the intensified sound. I am not sure that this explanation is satisfactory in all respects, but I believe it is, at least, a condition, if not a cause of the phenomenon in question.

That the second aortic sound is greatly lessened in intensity, especially at the apex, in mitral stenosis, no one will deny. The difficulty in determining its existence at the mitral area, taken in connection with the altered character of the first sound, has been a fruitful source of error with regard to the timing of presystolic murmur. The diminished intensity of the second aortic sound is obviously due to decrease of tension in the left ventricle and aorta. I must, however, add, that in my experience, contrary to that of Dr. Broadbent, the complete disappearance of the second sound at the apex is rare in mitral stenosis. I believe there are but few cases where some evidence, however faint, of the existence of a second sound at the apex cannot be appreciated. In such cases the sound completely vanishes at a point removed, but slightly, to the left of the impulse beat.

The partial or complete extinction of the aortic second sound is a sign of great importance in the diagnosis of cases of mitral constriction unattended with murmur, or existing with a murmur of mitral reflux.

The conditions which are most apt to simulate presystolic murmur are pericarditis, with a single friction sound limited to the area of the cardiac apex, and occurring before the impulse; and reduplication of the first sound. In cases of renal disease with cardiac hypertrophy the sign first described by Potain under the name of *bruit de galop* is frequently met with. Three sounds are heard at the apex—the two first of which represent a doubled first sound, one element of which may be taken for a presystolic murmur. In the absence of a doubled second sound, Johnson holds that the presystolic sound represents the contraction of the hypertrophied left auricle. The acoustic character of the doubled sound, the accentuation of the aortic second sound, and the signs of general hypertrophy of the heart, associated with albuminuria, should be sufficient to indicate the nature of the lesion.

III. ORGANIC SYSTOLIC MURMUR.

Since Elliotson first described the murmur of mitral regurgitation in 1830, a very important change of opinion regarding its significance has taken place. It was for a long time regarded as a positive sign of organic disease of the mitral valve, and the patient in whom the sign was recognised, although he might eat, drink, and sleep well, was always regarded as a sort of latent medical volcano from which an eruption of symptoms dangerous to life might at any time take place. Of late years, however, this view has been shown to be erroneous, and the occurrence of a mitral systolic murmur from functional causes is freely conceded.

Taking, first, for consideration the murmur of organic mitral reflux, we find that it is heard at its maximal point of intensity over the mitral area, and for a short distance horizontally to the left of this point. When well pronounced it is usually transmitted towards the left anterior axillary line, and it is frequently audible in the interscapular region and at the inferior angle of the left scapula. It is usually followed, when compensating hypertrophy of the right ventricle has taken place, by an accentuated pulmonary second sound; occasionally by the *bruit de rappel.* The existence of thrill over the cardiac apex accompanying the murmur is frequently detectable, though the phenomenon of apex thrill is much more commonly met with in mitral stenosis. In a certain proportion of cases there can be no doubt that the murmur of organic mitral regurgitation, instead of being developed at its point of maximal intensity over the mitral area, is heard best in the direction of the pulmonary area. This was specially noted in 1844 by Skoda,[a] who thought the murmur was due to some changes in the intima of the pulmonary artery. Meyer subsequently described the peculiarities of the murmur, but it attracted the special attention of Naunyn in 1868, and it is now generally known by the appellation of Naunyn's murmur. The point of maximal intensity of this murmur is in the second left intercostal space, about one and a half inches from the edge of the sternum; consequently it is slightly removed from the pulmonary area, and corresponds to the situation of the left auricular appendix. It seems to me that too much significance is attached to the murmur, as it is, in comparison with the cases where an organic systolic mitral murmur is heard best at the apex, very rare; and I believe the explanation of its transmission towards the direction of the auricular appendix is that it is due merely to such alterations in the mitral valve as determine, by convection, the development of the murmur in the situation indicated, just as in the cases reported by Dr. Sansom, already alluded to, where, from calcareous deposition in the left auriculo-ventricular orifice, a presystolic mitral murmur was conveyed to the tricuspid instead of to the mitral area. Theoretically, following the law that murmurs are developed in the direction of the circulating blood, the murmur of mitral regurgitation should be best heard, not at the apex of the heart, but higher up over the body of the left ventricle, or in the situation of the left auricle. The attachments of the chordæ tendineæ to the diseased valve serve, however, to conduct downwards the vibrations of sound to the musculi papillares, these to the muscular walls of the left ventricle, and so the murmur is usually most audible in the mitral area, except in the cases referred to by Naunyn. Dr. Balfour holds the peculiar view that Naunyn's murmur is frequently met with in the dilatation of the heart consecutive to spanæmia, such as is developed in chlorosis, progressive pernicious anæmia, or such like conditions. Dr. Balfour believes that the murmur of mitral reflux is carried towards the pulmonary area through a

[a] Abhandlung über Percussion und Auscultation. Wien, 1844.

dilated left auricle, the appendix of which lies in contact with the chest wall in the second left interspace. He further holds that the murmur of mitral reflux may be heard in this situation, whilst it is inaudible over the mitral area. At another time, in discussing the basic cardiac murmurs, I hope to show that the murmur described by Dr. Balfour is really an instance of the murmur which is, *par excellence*, most associated with functional conditions—viz., a systolic murmur in the pulmonary artery. I may here merely say in reference to Dr. Balfour's view that, so far as I know, no other observer has met with the occurrence of a systolic mitral murmur audible over the left auricle in the second left intercostal space, without an indication of its presence at the apex of the heart, and that it is by no means proved that the left auricular appendix comes to lie in contact with the chest wall.

The cases of Naunyn's murmur reported by Dr. Balfour are in reality, I believe, instances of murmur occurring in a displaced pulmonary artery—displaced to the left owing to changes resulting from dilatation of the right side of the heart. Both Drs. Russell and Byrom Bramwell have noted, in cases of dilatation of the right side of the heart occurring in pernicious anæmia, that a needle passed into the second left intercostal space close to the sternum transfixed the conus arteriosus and did not transfix the pulmonary artery as it does under normal conditions.

I may now briefly refer to some of the conditions under which a mitral systolic murmur is met with independently of any organic disease of the valves.

Most members of the Academy have had under observation, I am sure, cases of cirrhosis of the kidneys where, with the signs of an hypertrophied heart, a systolic murmur of marked intensity could be heard intermittingly over the mitral area. This murmur is specially audible under conditions which increase intraventricular pressure, is usually accompanied by signs of widely-spread arterial degeneration, and probably is the index of failing ventricular power. When cases such as I refer to came first under my observation, I believed that organic disease of the mitral valve existed, but *post-mortem* examination of a number of such cases showed that this view was erroneous. In most of these cases there were no appreciable signs of dilatation of the left ventricle, but there was very considerable hypertrophy of its walls and of the myocardium throughout the heart. In some cases the muscular structure had undergone fatty degeneration; in others a marked fibroid change was noted, in parts beneath the endocardium, in the papillary muscles, and in the walls of the left ventricle. The segments of the mitral valve were smooth, unaltered in texture, or merely presented at their edges slight nodular swellings at the points of attachment of the chordæ tendineæ, obviously not sufficient to impair the competency of the valve. In most cases there was neither absolute nor relative enlargement of the auriculo-ventricular orifice.

There can be no doubt that great dilatation of the left ventricle is frequently attended by the development of mitral regurgitation. In the case which we ordinarily speak of as a weak and dilated heart, where there are—a flapping character of the heart sounds, increased area of cardiac dulness, and an extremely weak and irregular pulse, mitral reflux intermittingly occurs until, under favourable conditions, the myocardium acquires tone. In aortic patency, the occurrence of what I have termed secondary mitral incompetence arises from extreme dilatation of the ventricle, and the condition so induced may be regarded as a safety-valve function which obviates the tendency to fatal syncope from paralytic distension of the ventricle. The conditions just referred to as causing mitral inadequacy may, I believe, be explained in two ways. There is either such an amount of dilatation of the auriculo-ventricular zona tendinosa that the mitral valve becomes incompetent, or reflux is due to a relative shortening of the papillary muscles in consequence of the extreme dilatation of the cavity of the ventricle.

In connection with the occurrence of mitral murmur in dilated heart, I may mention the views entertained as to the influence of certain occupations in producing a mitral regurgitant murmur without organic changes in the mitral valve. Seitz, Forget, Fräntzel, and others, state that, in the over-exertions following a campaign or similar laborious occupation, the heart undergoes such a dilatation of its cavities as to render the valves incompetent; and

Peacock mentions that miners in Cornwall, from repeated ascent of ladders, suffer from cardiac dilatation and mitral insufficiency.

Lastly, mitral systolic murmur has been developed in certain conditions which favour the development of *ante-mortem* clots in the heart, but murmur from this condition has been more frequently found in connection with the right side of the heart.

IV. FUNCTIONAL SYSTOLIC OR POSTSYSTOLIC MURMUR.

I now come to the consideration of purely functional systolic mitral murmur—that is, a murmur which depends on temporary and curable conditions, and in which there is no alteration in structure of the mitral valve. I shall not dwell on the opinions held by Bamberger, Flint, and Andrew, that systolic mitral murmur can be produced by irregular vibrations of the valve, by intra-ventricular vibration, or by alterations in the quality of the blood, without the occurrence of mitral regurgitation. Accepting the views of Chauveau and Savart in explanation of the mode of production of *bruit de soufflet*, I believe that in cases where a mitral systolic murmur exists it is an indication of regurgitation; and I agree fully with Dr. Bristowe, who regards it as an axiom "that the existence of a systolic murmur at the apex of the heart is a sure indication of incompetence of one or other of the auriculo-ventricular valves." It is scarcely conceivable that murmurs having the same acoustic characters, heard over the mitral area, could in one case be due to reflux of blood, in another to irregular tension of membranous structures, such as the valve segments or intra-ventricular tendinous cords.

It has often occurred to me that undue importance may be attached to the existence, *per se*, of a faint bellows murmur, audible over the mitral area, when developed in conditions of great excitement of the circulation. I do not, in any way, mean to imply that the existence of systolic mitral murmur should not lead to the closest investigation into the physical condition of the subject of it. On the contrary, I believe it to be of the utmost importance to determine whether the murmur is due to organic disease, or depends on functional conditions, trivial in character and removable. I have had, within the past few years, in connection with some appointments which I hold, an opportunity of investigating the condition of the heart in young and vigorous adults of both sexes: in one instance in connection with a training college, where admission can be gained only upon a medical certificate of good health; in another, in reporting on the physical state of applicants for admission into a branch of the public service. In most of these cases there is considerable excitement of the circulation of a nervous character; and I have been surprised at the frequency with which a systolic *bruit*, best heard over the mitral area, was developed. In some instances a diffused murmur was heard over the greater part of the cardiac area, and was clearly pulmonic in origin; but in a certain number the murmur was, as far as I could determine, one of mitral reflux. It was only after repeated examinations, when the great frequency of the pulse had subsided, and the murmur ceased to be audible, that I was satisfied that no organic disease existed. How many persons have been excluded from the public service or rejected for insurance because under conditions of excitement an apex murmur was developed from purely functional causes—causes which I believe are more efficient in producing this murmur than is generally supposed? I know of two members of my own profession, practising in this city, who were prevented entering the army medical service because, under nervous excitement, an apex murmur was developed from purely functional conditions. Both of these gentlemen are, I am happy to say, in good health and perfectly free from any sign of heart disease. I shall add, that it would be of the utmost importance if an extensive series of observations were made and recorded of the sounds of the heart in persons believed to be in a condition of health under various conditions of excitement of the circulation. I would venture to hope that this may be done by some of our brethren in the public services where favourable opportunities for conducting such examinations exist.

In a short paper which is published in the *Dublin Journal of Medical Science*, June, 1873, I ventured to classify the conditions under which a functional mitral murmur is met as follows:—

First,—It may occur, and be constant in duration, in dilatation of the left ventricle associated with hypertrophy, or in simple hypertrophy of the left chamber. In both of these instances advanced degenerative changes of the myocardium will, I believe, be generally found.

Secondly,—Where the auriculo-ventricular orifice becomes so dilated as to render the valves incompetent.

Thirdly,—In adynamic conditions of the system, leading to defective innervation of the heart, as in cases of cerebral disease, and in those cases of low typhus fever recorded by Dr. Stokes, where the first sound at the apex was replaced by a soft blowing murmur.

Fourthly,—In certain neuroses of the heart which produce an irregular action. In this class I would include some cases of heart complication occurring in chorea, and those due to excessive use of tea and tobacco.

And fifthly,—In those cases which have been described by Drs. Hayden and Da Costa, where the murmur was due to an irritable condition of the heart, with palpitation, or traceable to various conditions of the system—as anæmia, purpura, nervous debility.

Instances where murmur arises from the presence of a clot in the cavity of the ventricle, and where it is due to obstructed pulmonary circulation, are not included in these classes.

Murmurs occurring in course of, or subsequent to, attacks of acute rheumatism and typhoid fever should be added to the above list.

The points of differential diagnosis between functional mitral murmur and that arising from organic disease were enumerated as follows :—

(*a.*) The functional murmur is inconstant and variable in intensity.

(*b.*) Usually present in the recumbent posture, it generally ceases when the patient sits up.

(*c.*) It is loudest not at the apex, but over the body of the left ventricle.

(*d.*) There is absence of signs of pulmonary distress, and of accentuation and doubling of the pulmonary second sound.

Fig. 7.

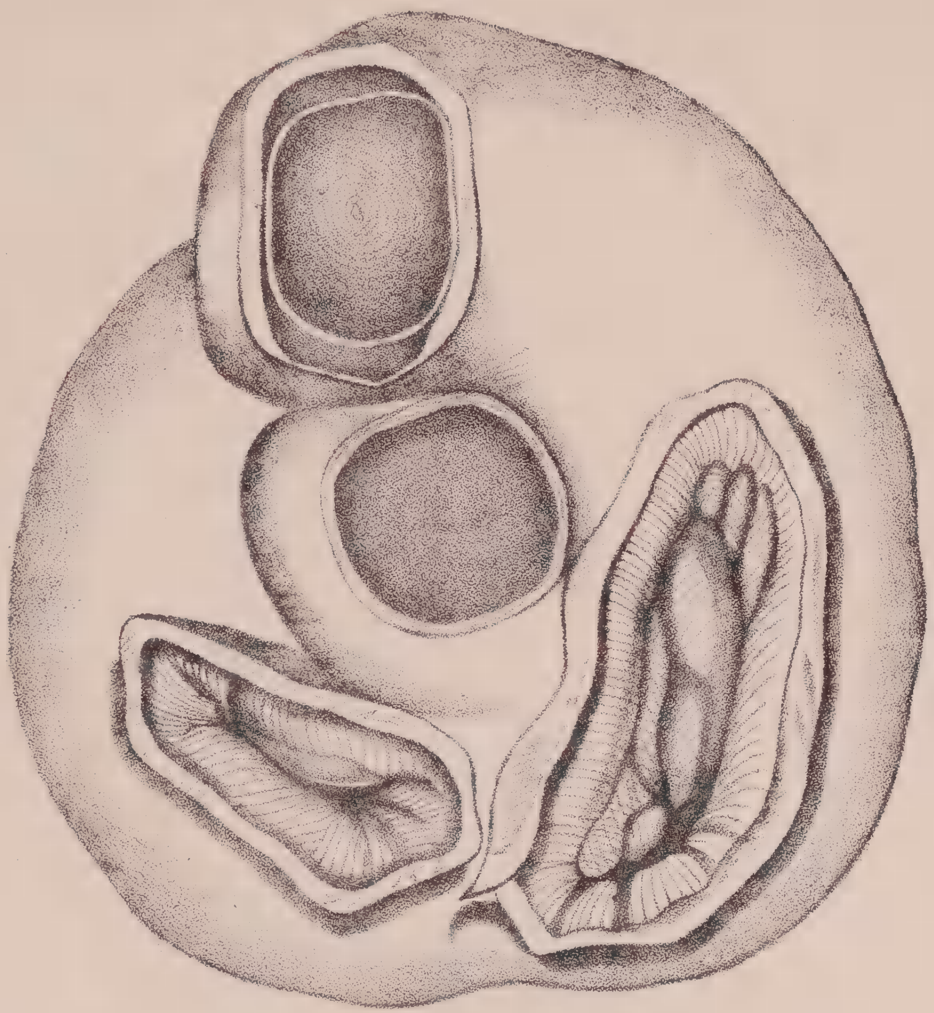
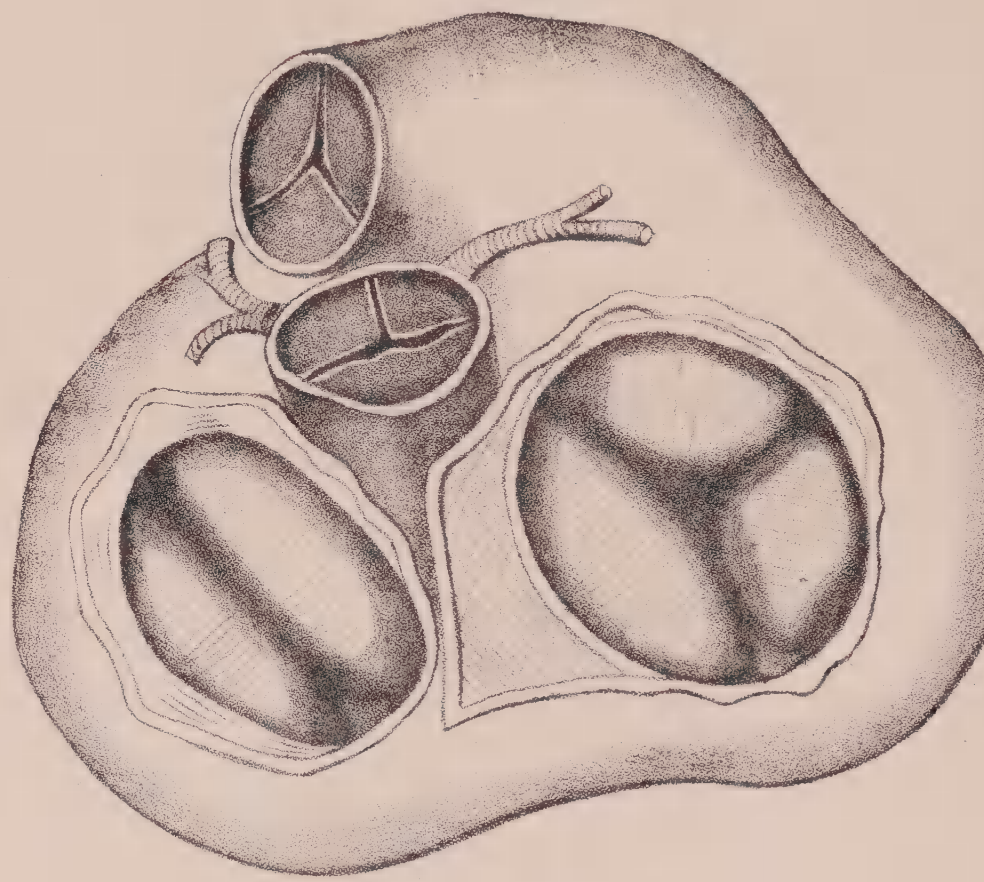


Fig. 8.



FALCONER.

Dublin.

(*e.*) There is no alteration in the size or position of the heart.

(*f.*) The radial pulse is variable as to rate and volume.

There is one other condition not mentioned which I consider of importance to note here. The murmur of functional incompetency is not, as a rule, a substitute murmur; a left ventricular first sound is generally audible, and the murmur can be recognised as following the systolic sound, hence its proper designation is "postsystolic murmur."

Most of the conditions referred to in the differential diagnosis of the murmur can be explained by assuming the existence of an atonic condition of the muscular fibres of the left ventricle. The site of maximal intensity of the murmur being slightly above the apex is somewhat difficult to explain. Possibly here the murmur obeys the law of convection, whilst in organic disease of the valves the vibrations are carried towards the apex of the ventricle by the structures in contact with the diseased valve.

Time will not permit of my again discussing the various theories regarding the mode of production of functional regurgitation, if, indeed, any advantage could arise from my doing so. In the paper referred to I ventured to suggest that it might result from an irregularity, or want of correspondence, in the action of the sets of fibres of the ventricle which obliterate its cavity and those which close its valve. I shall here only dwell for a moment upon a theory as to the mode of production of the murmur put forward by Dr. Byrom Bramwell. I do so with some interest, as the view entertained has a certain relation to that urged by myself, but it aims at an explanation of the murmur at least remarkable for its simplicity. Dr. Bramwell states that the mitral orifice is surrounded in its posterior two-thirds by the muscular fibres of the ventricle, whilst the anterior third, which is fibrous—I lay special stress on this—is formed by the fibrous continuation of the two posterior aortic sinuses to which the great anterior flap of the mitral valve is attached. The occurrence of mitral incompetency is explained very simply by a want of tone in the muscular fibres of the left ventricle and of the muscular fibres which surround the mitral orifice—the muscular sphincter, as it is termed by Dr.

Bramwell. I am afraid the theory of mitral regurgitation, based upon this view of its mechanism, will scarcely bear rigorous examination. No one values more than I do Dr. Bramwell's contributions to the literature of our profession, but it seems to me that in this matter he is clearly at fault. His explanation would appear to be based upon the experiments of Ludwig and Hasse at Leipzig, made in 1880, showing that the size of the auriculo-ventricular orifices are fully one-half smaller in systole than in diastole, as is shown in Figs. 7 and 8. It appears, further, to have been suggested by an interesting *résumé* of Hasse's and Ludwig's observations in a lecture delivered at Cambridge by Dr. Donald MacAlister upon "The Form and Mechanism of the Heart;" but a careful perusal of this lecture does not, in my opinion, afford grounds for the description of the mitral orifice given by Dr. Bramwell. As described by Henle, the left auriculo-ventricular zone is a half ring composed of strong fibrous tissue interposed between the auricle and the ventricle, the ring being completed by the continuity of the anterior flap of the mitral valve with the two posterior aortic valves. This ring forms a perfect line of demarcation between the auricle above and the ventricle below. It has attached to it above the fibres of the auricle, whilst below the fibres of the ventricle are connected with it in the following manner:—The external longitudinal fibres arise from it; pass then downwards until they reach the whorl or vortex at the apex, where some of the fibres pass inwards to form the papillary muscles; the remaining fibres spread out into an inner layer of muscular fasciculi, which are continued up to the fibrous rings at the base of the heart. An important point to note in connection with these fibres is that they arise from the fibrous rings, pass over the external surface of the heart, and return again to the fibrous rings, internally, either directly or through the intervention of the chordæ tendineæ and valve flaps. Between this external and internal layer of muscular fibres intervenes a set of fibres, the middle layer, which are also attached to the rings at the base, and which pass downwards with increasing degrees of obliquity, until, at a certain distance from the apex, the fibres can be said to pass

Fig. 9.

transversely, and here, no doubt, they assume the form of a sphincter muscle (see Fig. 9). But this arrangement of fibres does not take place in the supra-papillary region of the ventricle. It would, in my mind, be a disadvantage to have a sphincter muscle above the termination of the papillary muscles and forming part of the auriculo-ventricular zone. It would be likely, during its contraction, to interfere with the integrity and smoothness of the aortic ring; and further, there would be no occasion for its existence here, as the small volume of blood which is left in the ventricle after its contraction, and which fills the supra-papillary space (the aortic vestibule of Sharpey), has an important use assigned to it. The change in shape of the auriculo-ventricular rings during systole is obviously due to the combined action of the longitudinal and oblique fibres which are attached to it, and it remains to be proved whether an atonic condition of those fibres, by not sufficiently narrowing the ring, can lead to incompetency of the valve.

There is one fallacy in connection with the detection of an apical murmur that must be carefully guarded against. It occasionally happens that a sound like in acoustic character to *bruit de soufflet* is audible during inspiration corresponding to the systole of the ventricles, and consequently systolic in rhythm. The sound is sometimes heard during expiration, but this is unusual—in fact, the important element in the recognition of this sign lies in the murmur being almost invariably absent during expiration, and it can usually be caused to disappear at the time the patient forcibly expires and ceases to breathe. This murmur is clearly of respiratory origin, and it is produced by displacement of air during the systole of the ventricles in the tongue-like process of the lung which commonly lies over the apex of the heart, which is sometimes developed in a marked degree, and usually emphysematous.[a] A drawing, taken from a specimen in the dissecting-room of Trinity College, was kindly lent to me by the University anatomist,

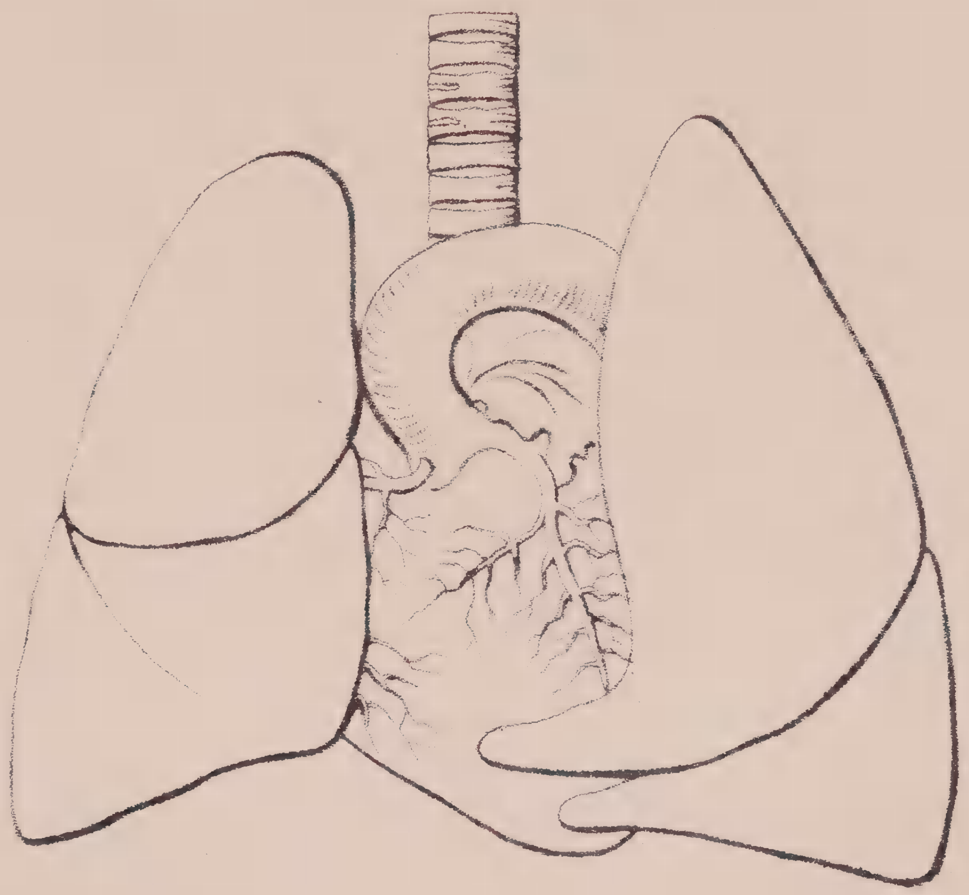
[a] My colleague, Dr. M. Boyd, kindly called my attention to a case at present under his care in hospital in which the physical signs corresponded precisely with those mentioned.

Dr. Little, and it accurately represents the condition of a lung with an emphysematous-tailed process overlapping the heart, which I exhibited before the Pathological Society in the Session of 1874–75, in which an extra pericardial sound was developed from the movements of the heart against it. Fig. 10 represents the condition referred to.

V. MITRAL BRUIT DE SCIE.

There is but one other murmur developed at the mitral area that I shall refer to, dealing with it with all reserve, as it is one of which my own experience is limited to one case, the pathological nature of which was not confirmed by *post mortem* examination. Some years ago I had under observation in hospital a patient in whom a well-marked *bruit de scie*, identical in character with the murmur developed in most cases of aortic patency, was heard over the mitral area. The murmur was altogether different in acoustic character from the double murmur heard occasionally in mitral stenosis—that is, the presystolic and postdiastolic murmurs. The first murmur was systolic, and was transmitted towards the axilla; the second murmur was strictly diastolic, being exactly synchronous with the second sound. No murmurs were audible over the aortic area; and the cardiac and pulse tracings which were obtained showed no evidence of aortic incompetence. Corresponding to the apex there was a well-marked diastolic impulse of limited extent and expansile character, accompanied by a buzzing thrill and signs of elongation of the heart. Dr. Hayden, who reports the case, in his work on "Diseases of the Heart and Aorta," says:—"The diastolic murmur was of strictly apex origin; an apex diastolic (not postdiastolic) murmur is one of the rarest phenomena in cardiac acoustics, and can, in my judgment, only be due either to abnormal communication between the aorta or pulmonary artery and one of the ventricles, or to an aneurysm springing from the wall of either ventricle." Upon this opinion I offer no comment. Personally, I have not met with a case of aneurysm of the heart where the diagnosis was verified by *post mortem* examination; but I consider,

Fig. 10.



in dealing with the subject of murmurs developed at the mitral area, the views of an observer like Hayden deserve more than a passing notice. I may add, that in a specimen of aneurysm of one of the aortic sinuses opening into the left ventricle, exhibited before the Pathological Society by Dr. Stokes, an accurate diagnosis of the case had been made by Dr. Hayden from the existence of the murmur now referred to.

I have to thank the members of the Academy for the patient way in which they have listened to a tedious disquisition upon a subject so trite as that which I have just discussed. It would be difficult, indeed, to add much that is fresh and novel to the literature of cardiac disease, though, in some points, I do not think what I have ventured to bring before you is open to the criticism—"that what's true is not new, and what's new is not true."

In reviewing the literature of cardiac disease we have good reason to regard with pride the work that has been done by the Dublin School; and though it is not given to many to rival the genius of Corrigan, the philosophic teaching of Stokes, or the erudition of Hayden, it is open to each one, and incumbent on him, to pay the debt which he owes to his profession, by adding to the general storehouse of facts which help to build up the science of medicine.

A CASE OF ENTERIC FEVER, FATAL BY PERFORATION.

By Dr. WALLACE BEATTY;
Senior Assistant-Physician, Adelaide Hospital.

[Read in the Medical Section, March 26, 1886.]

The following case of enteric fever, fatal by perforation, presents some points of interest which I think justify me in bringing it before the Medical Section :—

Case.—The patient, a woman, aged thirty-four years, was admitted to the Adelaide Hospital under my care on February 21st last. She took ill on February 9th, twelve days before admission, with malaise and loss of appetite ; her bowels were confined. She did not take to bed till February 13th, the fifth day of her illness. On that day she suffered for the first time from frontal headache, which was severe. A week after, February 20th, the twelfth day of her illness, diarrhœa set in, preceded by a severe rigor, which lasted half an hour. Tenderness in umbilical and epigastric regions, and severe abdominal pains set in with the diarrhœa. The pains continued till her admission to hospital on the following day. On the morning of the day of her admission, before she left her home, she had rather a copious hæmorrhage from her bowels ; this was the only occasion during her illness on which hæmorrhage occurred. On admission, Feb. 21st, thirteenth day of illness, she was weak and prostrate; answering questions seemed to weary her, but her mind was clear. She complained of cramp-like abdominal pain. There was marked tenderness over umbilical and right hypochondriac regions ; abdomen was much swollen and tympanitic, but not at all tense ; spleen was enlarged ; there were no spots ; there was no subsultus ; pulse was regular, but weak. Evening temperature, 101·2°. Evening pulse, 110.

February 22nd, fourteenth day of illness.—She slept well after a sedative draught ; abdominal pain still present, especially in right hypochondriac region ; sudamina were abundant on chest and abdomen. She had great thirst. Bowels were moved six times in twenty-four hours; stools liquid and light coloured. There were no spots. Morning temperature was 97·4°. Evening temperature, 103°.

February 23rd, fifteenth day.—She slept fairly well during the night. Condition was much the same as on the preceding day; pulse was weak. Bowels moved seven times in twenty-four hours; diarrhœa was checked by an enema of starch and opium. The mind continued clear. Morning temperature was 99·2°, and pulse 120. Evening temperature was 100·4°, and pulse 126.

February 24th, sixteenth day.—She slept well during the night. Pulse was weak; bowels moved three times in twenty-four hours. Morning temperature was 97·6°, and pulse 108. Evening temperature was 102·2°, and pulse 128.

February 25th, seventeenth day.—Pulse weak, more frequent, 140. She complained of cramp in the abdomen, with general tenderness. Morning temperature was 99°, and pulse 140. Evening temperature was 102·4°.

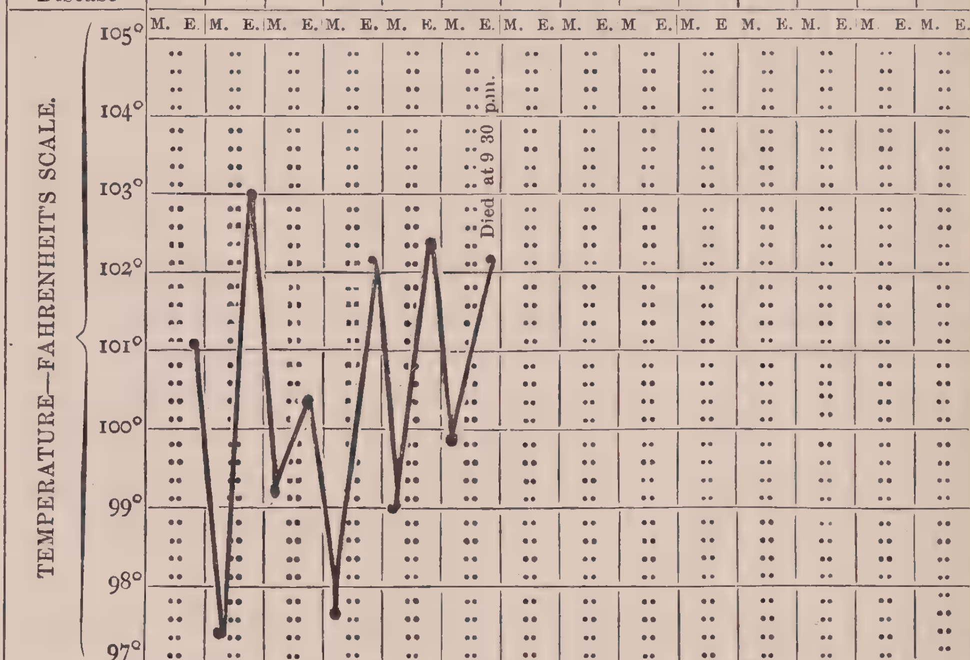
February 26th, eighteenth day.—Marked abdominal tenderness; knees drawn up. Bowels moved six times in twenty-four hours. Pulse very weak, 138; mind clear. Morning temperature was 99·8°, and pulse 138. Evening temperature was 102·2°. At about 7 o'clock in the evening she seemed rather better, and spoke cheerfully to the nurse. At 9 15 p.m. she was seized suddenly with severe abdominal pain, immediately after being turned in bed, which was done with care and gentleness by the nurse. Her breathing became very shallow and hurried; pulse became imperceptible, and heart sounds inaudible. She died at 9 30 p.m.

A *post mortem* examination was kindly made for me by Dr. Bewley thirty-six hours after death. Abdomen was greatly distended from gaseous accumulation in the intestines. There was intense diffused peritonitis; intestines were glued together by soft lymph; sero-fibrinous effusion was present in some quantity. Spleen was enlarged. At the lower end of the ileum there were five ulcers. In the floor of the ulcer highest up, situated at a distance of ten and a half inches above the ileo-cœcal valve, there was found a circular perforation about half an inch in diameter. The ulcer was large; edges undermined; its long axis transverse to direction of intestine. The other ulcers were situated closer to the ileo-cœcal valve. The floor of each was formed by the muscular coat, and presented the characteristic smooth surfaces, while the edges were undermined; in diameter the largest was about shilling size—the smallest, situated just outside the margin of this ulcer, was very minute. The intestinal lesion was limited to these five places. No escape of intestinal contents was observed. The liver and kidneys were healthy.

CHART OF TEMPERATURE, &c.

Name, M.; *Age*, 34; *Disease*, Enteric Fever; *Result*, Death.

Day of Month	Feb. 21 1886	22	23	24	25	26	—	—	—	—	—	—	—	—
Day of Disease	13	14	15	16	17	18	—	—	—	—	—	—	—	—



Pulse M.	—	—	120	106	140	138	—	—	—	—	—	—	—	—
Pulse E.	110	—	126	128	—	—	—	—	—	—	—	—	—	—
Motions	—	6	7	3	3	6	—	—	—	—	—	—	—	—

This case appears to me to be of interest, in that while the ulceration was of very limited extent—there being only five ulcers, two of which were very small—yet the case was attended with perforation of one of the ulcers and peritonitis. The time at which perforation occurred is doubtful. Perhaps it may have happened on the day before her admission to hospital (twelfth day of her illness), when she was seized with a rigor, diarrhœa, and severe abdominal pain. There was no decided collapse. Possibly peritonitis may have preceded the perforation. It may be noted that there was constipation at first, diarrhœa not having come on till the twelfth day. She did not get a purgative. Hæmorrhage occurred on one occasion only—the thirteenth day. As to the temperature there was a very considerable fall each morning, amounting to nearly six degrees on the fourteenth day of illness, and to nearly five degrees on the sixteenth day. Headache was not present till the fifth day.

ON THE QUANTITATIVE ESTIMATION OF ALBUMIN, UREA, AND SUGAR IN URINE.

By F. R. CRUISE, M.D., Univ. Dubl.;
President of the King and Queen's College of Physicians in Ireland.

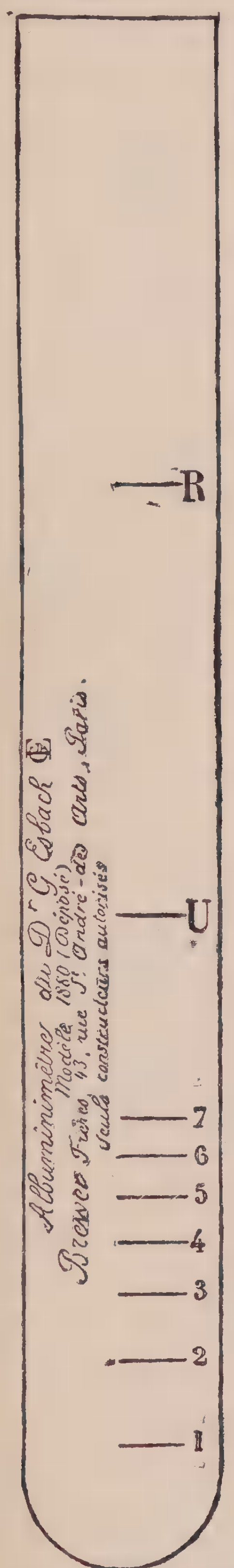
[Read in the Medical Section, Friday, December 18, 1885, and Friday, April 30, 1886.]

Last year I brought under the notice of the Academy of Medicine in Ireland some brief observations on the value of the waters of Contrexéville in the treatment of various morbid conditions of the urinary secretion. On the present occasion I intend to supplement those experiences by pointing out simple methods for the quantitative determination of albumin, urea, and sugar in urine.

It is hardly needful for me to premise that the present communication is not intended to be an exhaustive one, but merely a contribution, *from a clinical point of view*, towards a subject the interest of which daily increases.

Last autumn I spent a fortnight at Contrexéville, and had the advantage of working daily in the laboratory of Dr. Debout D'Estrées, the accomplished Medical Inspector of that station. There I had the opportunity of extending and confirming my knowledge of the beneficial effects of these waters, especially in reducing albumin and sugar in the urine, and of observing the rapidity and precision with which Dr. Debout D'Estrées verifies the progress of the cases under his observation. Amidst much that I saw to admire in his examination of urine, chemical and microscopical, I was particularly struck with the methods he used for ascertaining the quantity of albumin and sugar in any given specimen, and I propose to describe them now. In addition, I shall detail a very simple and rapid mode of determining the quantity of urea.

Fig. 1.



It is quite unnecessary for me to allude here to the importance, in any given case of albuminuria, of being able to ascertain with facility the quantities of albumin and of urea present from time to time. The amount of albumin often indicates the extent of renal mischief, especially of the inflammatory type—an element hardly ever quite absent even in chronic organic kidney disease, and frequently playing a very leading part. The quantity of urea is a still more critical item, inasmuch as its presence in sufficient quantity is a fair guarantee of the absence of present danger to life in chronic cases, while its marked deficiency points to imminent risk.

The simple method I am about to describe for albuminimetry is that devised in 1880 by Dr. Esbach, Chief of the Chemical Laboratory at the Hôpital Necker, in Paris. Very few physicians engaged in active practice are able to spare sufficient time to determine the quantity of albumin by the conventional method—namely, boiling a given quantity of the urine so as to precipitate the albumin, then filtering, and finally drying and weighing the deposit. This process occupies several hours of attention, requires suitable apparatus, and not a little manipulative skill. Most of us are habitually contented with the rough and ready plan of boiling the urine in a test tube, then allowing the deposit to settle, and finally guessing the amount from the depth of that deposit. Now, Dr. Esbach's method is nothing more or less than this latter rude device, carried out in a most ingenious and scientific fashion. He takes a glass tube, Fig. 1, of fixed capacity, and graduated according to the results of a carefully-conducted series of experiments. Into this tube the urine in question is introduced up to the letter U, then the reagent (a solution of picric and citric acids, of fixed strength) is added up to the letter R. The urine and reagent having been mixed thoroughly, the tube is set aside standing for twenty-four hours. At the end of that time the deposit of picrate of albumin has fallen to the bottom in a dense mass or coagulum, and is read off by means of the graduations on the tube, *which give the proportion of albumin in grammes in each litre of the urine under examination.* The time occupied in these manipulations amounts to *about one minute.* It is essential that the urine used should be *acid;* if it is neutral or alkaline acetic acid must be added until it reddens litmus paper.

I may observe that Dr. Esbach's test solution of picric acid serves not only for quantitative analysis of albumin, but also for its detection if present. Thus, if we put a couple of drachms of it into the tube, and add the urine drop by drop (filtered if turbid), if albumin is present a cloud *instantly* appears. Experience has taught me that lithates in the urine are thrown down by the reagent, and we must be on our guard to avoid deception. In such cases the lithates will be observed in minute dots all down the tube, and the deposit, in place of being a coagulum, will be a thick slimy fluid. Esbach's tubes for albuminimetry, with descriptive pamphlet, formula for reagent, &c., can be had from Messrs. Brewer, frères, 43 Rue St. André des Arts, Paris. Dr. Veale, in the *British Medical Journal* for May 10th, 1884, gives an excellent paper on this method; and a brief notice of it appears in the *Lancet* of January 23rd, 1886, by Mr. Blomfield.

I have already alluded to the vital importance of being able to ascertain the quantity of urea excreted, especially in the cases of albuminuria which so constantly fall under our observation. An easy process for this determination is that of Dr. Esbach, whose method of albuminimetry I have just described. It is one—and I believe the most facile and perfect—of the many processes by which the urea in a given quantity of urine is decomposed by a solution of hypobromite of sodium, and its amount ascertained by measuring the volume of nitrogen evolved. It is a pleasing duty to remark that this method is really the invention of a distinguished Irish chemist, Dr. Edmund W. Davy. Dr. Davy used the hypochlorite of sodium as the decomposing agent, and at the present day chemists are divided in opinion as to whether the hypochlorites or hypobromites are to be preferred. With both the principle is identical. Dr. Davy published his method in 1854, during the time that I was a pupil in the old Carmichael School of Medicine in North Brunswick-street, where he then lectured. Reference to his original paper (which will be found in the number of the *Philosophical Magazine* for June, 1854) will show how very closely Dr. Esbach has followed his footsteps, even in the apparatus he uses.

Dr. Esbach's method is as follows:—He takes a glass tube, 15 inches long, closed at one extremity, and having the orifice at the other end neatly ground so that it can be effectually stopped by the operator's thumb, previously covered with an India-rubber finger-stall. The tube is graduated in cubic centimètres and millimètres, and, corresponding to 140 millimètres, a mark is engraved all round the tube, so as to be visible however it may be held. To use the ureomètre the operator proceeds as follows:—

Fig. 2.

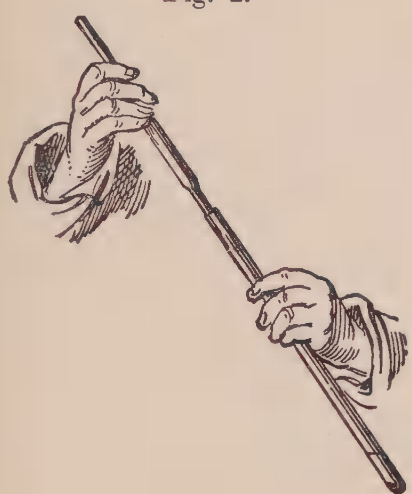
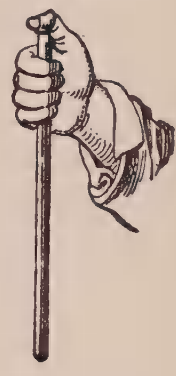


Fig. 3.

First he introduces, by means of a pipette, about eight centimètres of a rather strong solution of hypobromite of sodium (Fig. 2), adding distilled water from a dropping bottle until the mixture reaches 140 millimètres. Care must be taken to see that the total reads *exactly* at 140 (Fig. 3). Next he takes one cubic centimètre of the urine in question, and having discharged it rapidly into the tube (Fig. 4) closes the latter promptly (Fig. 5),

Fig. 4.

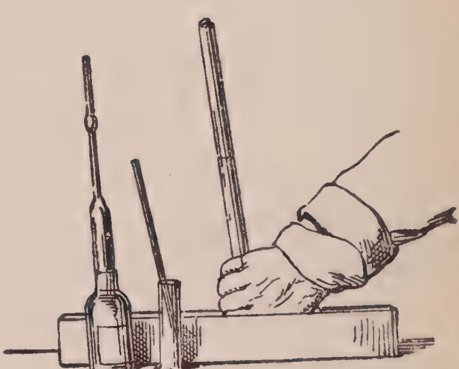
Fig. 5.



and by a couple of movements of inversion and a good shake, thoroughly mixes the contents. Decomposition sets in at once, and a considerable froth is developed. This settles quickly into a few large bubbles by holding the tube in the position indicated in Fig. 6, swaying it gently a few times; he then plunges the end of the tube, closed by the thumb, into a water bath, and removes the thumb (Fig. 7). The level of the fluid is rapidly

Fig. 6.

Fig. 7.



lowered by the pressure of the nitrogen evolved. As soon as all is steady he lowers the tube nearly horizontally, as in Fig. 8, again closes it with his thumb, under water, removes it from the water bath, as in Fig. 9, and holding it upright, reads off the amount of fluid remaining. This will be less than the 140 millimètres with which he commenced operations, *and the difference between the two amounts gives the volume of nitrogen evolved.*

Fig. 8.

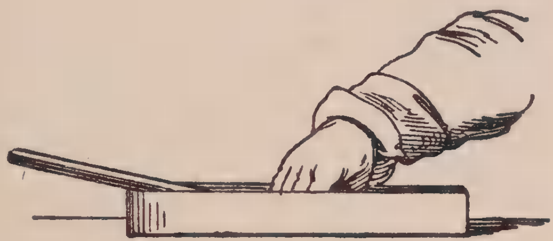
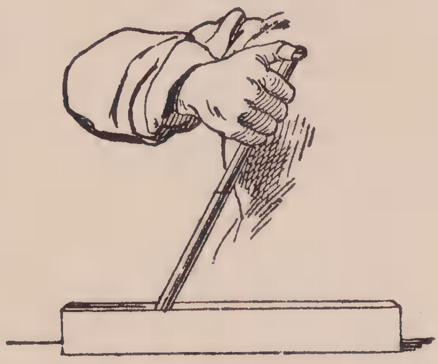


Fig. 9.



The next point is to estimate the urea from the volume of nitrogen. If we were left to calculate this, making allowance for the gaseous tension at the moment, we should have a rather troublesome problem to solve; but Dr. Esbach saves us all this, as follows:—*First*, he provides us with a corrective instrument which he terms a Baroscope (Fig. 10). It consists of a bent glass tube, in the elbow of which is a column of mercury, above which on one side is the tube simply closed, on the other side is a small quantity of water, and a thin expanded glass bulb. The working of this instrument is easily seen:—According to the temperature and gaseous tension the mercury is depressed or the reverse, and an index marks the amount. *Secondly*, Dr. Esbach provides us with a set of tables, carefully worked out upon the basis of the foregoing data, which show us the amount of urea, as indicated by the two figures we have—namely, the volume of nitrogen in millimètres, and the indication of the baroscope. For example, if the volume of nitrogen amounts to 50 cubic millimètres, and the baroscope points to 72, we find at a glance that the amount of urea is 13·9 grammes per litre. Taking, for a working estimate,

Fig. 10.

each gramme as 15 grains, and the litre as 35 oz., we find that such a urine contains 205 grains per litre—nearly 6 grains per oz. Supposing, then, that the patient passes an average of 60 oz. of urine per day, he is eliminating just 360 grains of urea daily—quite a fair amount under ordinary circumstances. If, on the other hand, the urea falls to half or quarter of the above quantity, we have good reason to fear toxic symptoms. When the urine is albuminous, it is necessary to boil and filter it to get rid of the albumin, which would lead to an error, and a difficulty in reading off the quantities after decomposition, owing to the persistent froth it causes.

All these steps take some time to describe, but once learned are very quickly gone through. The average time needed for the quantitative determination of urea by this method is *five minutes.* The needful apparatus, with descriptive pamphlet, formulæ, tables, &c., may be had from Messrs. Brewer, frères, of 43 Rue St. André des Arts, Paris.

We shall now discuss the quantitative determination of sugar in urine, and, I trust, find that the process can be accomplished by a method at once easy and rapid in use, and satisfactory in result. It is needless for me to dwell upon the importance of quantitative analysis of saccharine urine, because we all know that without such information the progress of a case of diabetes cannot be properly watched, or the effect of treatment—medicinal or dietetic—estimated at its correct value.

I need not delay upon the subject of *qualitative* analysis—many excellent tests are familiar to every student of medicine; therefore I shall pass directly to the subject of the *quantitative* analysis of diabetic urine.

The principal methods by which we are able to determine the amount of sugar in urine are:—

I. The fermentation test.
II. Volumetric analysis.
III. Polariscopy.

I.—FERMENTATION TEST.

The fermentation test, at the best, is but a rude device, and as it occupies many hours, is unsuited to the exigencies of every-day practice.

II.—VOLUMETRIC ANALYSIS.

The method by volumetric analysis, although far more rapid and precise, is open to grave objections. It occupies a considerable time—rarely less than an hour. It requires a well-appointed laboratory, and special skill on the part of the operator. Moreover, it is liable to many considerable errors, amongst which I may mention the principal—

(*a*) The copper test-solutions are liable to deterioration by keeping, to such an extent that they must be tested carefully before each analysis. If long kept they may throw down the copper on simple boiling, even without the presence of sugar, leading to an unfounded belief that it is present when it actually does not exist. Hence, if previous testing of the solution be omitted, it is quite possible to reach a false conclusion as to the existence of diabetes. I need not point out how serious an error this might be, supposing, for example, that we were examining the urine of a patient presenting himself for life insurance, &c.

(*b*) The point of decolorisation, or reduction, is not easily determined, except by a very practised eye, and is not satisfactorily reached unless in daylight.

(*c*) When we have to deal with strongly saccharine urine it is scarcely possible to avoid errors, which, being multiplied in proportion to the relation the quantity of urine used bears to the total excretion, become very considerable indeed. To make this point clear let me give an example. Supposing that the urine in question contains 20 grains of sugar per oz., or $2\frac{1}{2}$ grains per drachm, and that we use the ordinary Fehling's solution, of which 26 cc. are decolorised by 2 grains of sugar, an error of half a drachm in excess added to reach the point of decolorisation, leads to an error of $1\frac{1}{4}$ grains in the operation. Now, if the patient excretes 100 oz. of urine in 24 hours—a very usual quantity in diabetes—the error of the half drachm causes a huge one in the total. The half drachm is the $\frac{1}{1600}$th part of the total urine, and hence the initial error of $1\frac{1}{4}$ grains would lead to a final error of 2,000 grains. If the excess of quantity amounted only to 15 drops, or quarter of a drachm, still the final error would be 1,000 grains, and so on in proportion. We can, to a certain extent, avoid such an error by conducting the latter stages of the analysis *very slowly*, adding the urine guttatim and watching the effect, but this process, I need not observe, involves much delay. Thus, it is evident that volumetric analysis is tedious, requires much skill, and is liable to serious error. It may be urged, in extenuation, that any strongly saccharine urine may be previously diluted to a fixed extent, and allowance made at the final computation. This, however, involves extra calculation and delay.

(*d*) Chemists are not agreed as to whether other substances besides sugar, occurring in urine, do not exercise a reducing power on the copper solution, and thus further error comes into existence. For all these reasons it seems obvious that volumetric analysis is unsuited to the busy practitioner, who, to obtain reliable information, must confide the specimen to an expert.

III.—POLARISCOPY.

Is there, then, no method of estimating sugar in urine—simple in application and satisfactory in result, which may be resorted to for daily use?

I learned one from Dr. Debout D'Estrées while I was at Contrexéville last autumn, and I shall describe it now. It consists in the use of a polariscope, specially constructed for the purpose, by the aid of which the estimation of sugar can be accomplished with approximate accuracy in less than ten minutes and without the need of any special skill. After an extended trial during the last six months I am quite satisfied with this method.

A vast number of cases of diabetes come to Contrexéville, on account of the remarkable effect of its waters in this disease, and Dr. Debout D'Estrées makes his analysis as follows :—

First—He ascertains the presence of sugar by a qualitative test, say by boiling the urine with caustic potash ; and

Secondly—Having discovered its presence, he then takes about an ounce of the urine and, after decolorising it by the addition of solution of acetate of lead and filtering, he places a portion of it in the polariscope and reads off the quantity of sugar at once.

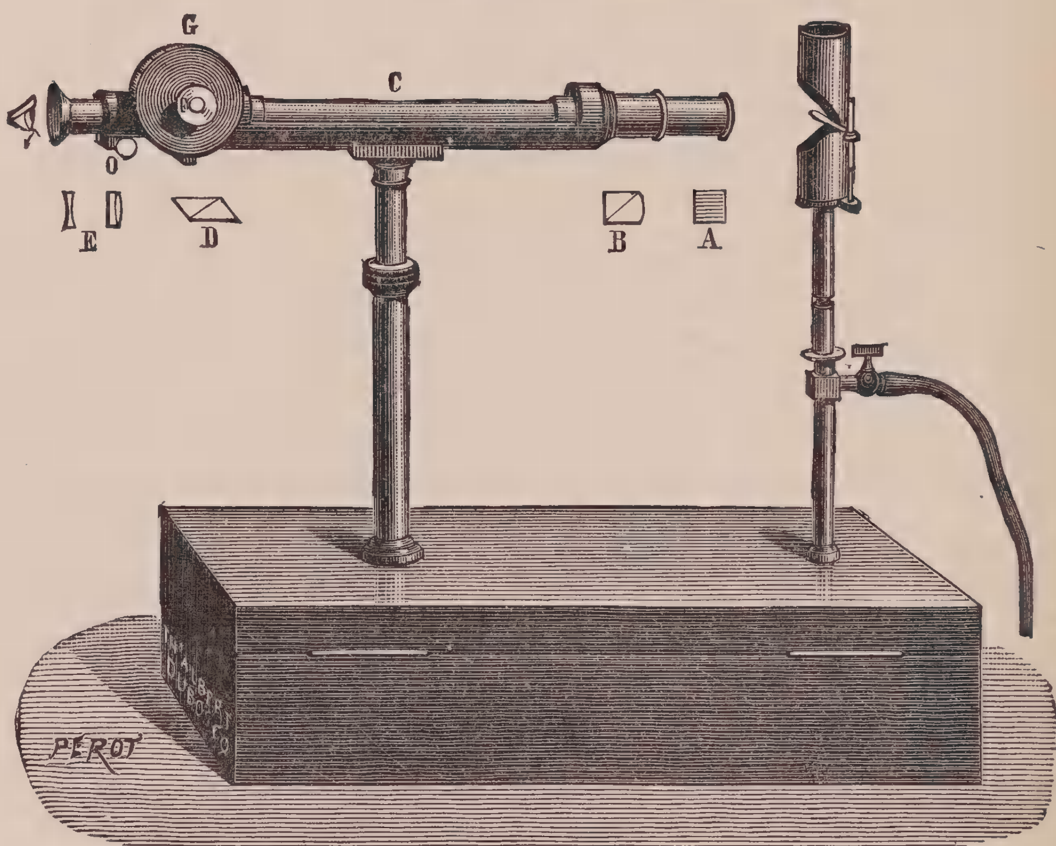
Dr. Debout D'Estrées uses Laurent's instrument. I prefer the Yvon-Duboscq diabétomètre, on account of its simplicity and moderate price. I can scarcely bear higher testimony in its favour than by mentioning the fact that my friend Dr. Tichborne, Chemist to the Apothecaries' Hall of Ireland, is satisfied with its performance. Mr. Stephen Yeates, our well-known eminent scientist, is of the same opinion.

Let me say a few words about the Yvon-Duboscq diabétomètre. The fundamental principle upon which the polariscope depends, as a means for determining the existence and amount of sugar in a given solution, is the well-known fact that a saccharine fluid always *rotates* polarised light, and that the amount of rotation is in *exact proportion* to the density of the solution. With regard to the present instrument it would be out of place to enter here into minute details of the principles on which it is constructed, and its complete exposition would occupy far too long a time. Suffice it to say that it belongs to the class of "half-shade" polariscopes (polarimètre à pénombre), the first of which was, I believe, constructed about 1860 under the direction of the Rev. Professor Jellett, now Provost of Trinity College, Dublin.[a]

[a] Those interested in the subject may consult with advantage Landolt's Handbook of the Polariscope. English translation published by Macmillan & Co., London. 1882.

In these instruments the mechanism for sensitiveness is arranged to produce a circular field of vision, divided into halves, which in certain positions of the analysing Nicol prism are unequally illuminated, but in one particular position exhibit a uniformly faint shade. This latter position, which admits of being fixed with great accuracy, is taken as the point of reference, and to prevent confusion of colours a monochromatic light is used.

Fig. 11.



The accompanying woodcut, Fig. 11, with a brief explanation, will render clear all that is needful for the successful manipulation of this diabétomtère.

The light emanating from a monochromatic yellow flame, obtained by burning a piece of salt in a Bunsen jet, at first passes through a chamber (A) filled with a weak solution of bichromate of potassium, and then traverses the half-shade polariser (B). It continues its course through the tube (C), which holds the liquid under examination, and passing thence is received by the analysing

Nicol prism (D), and finally reaches the observer's eye after passing a Galilean eye-piece (E) placed so as to render the vision distinct.

The Nicol prism is secured in a movable collar, the angular movement of which is measurable. For this purpose the above-mentioned collar has a cogged circumference, which is operated on by a tangential endless screw. The stem of this screw (F) has attached to it a circular disc (G) marked with divisions, each of which is experimentally adjusted to represent one gramme of sugar for each litre of the urine under examination in the tube (C). Independent of the screwhead (F) and disc (G), the Nicol prism can be moved as required by the regulator (O), so as to attain the point of reference while the disc (G) is fixed at zero.

To use the diabétomètre the operator first adjusts the point of reference by allowing the polarised light to pass through the tube (C), previously filled with distilled water, and then rotates the Nicol prism by means of the regulator (O) until the field of illumination is perfectly evenly lighted up, as in Fig. 13.

Fig. 12.

Fig. 13.

Fig. 14.

Now he substitutes for the tube of water one containing the saccharine urine in question. Immediately on doing so he finds one-half of the field of vision darkened, as in Fig. 14, and to rectify this he must turn the disc (G) until the two halves of the illuminated disc are *perfectly evenly lighted.* This can be done with the greatest accuracy, as, if the disc is turned the least degree too far, the other half becomes darkened, as in Fig. 12. A little practice enables the operator to do this within a quarter of one of divisions of the disc. The new position of the disc (which indicates the rotation of the Nicol prism) points out the amount of that rotation—or, in other words, records the amount of the sugar in the urine, each division representing one gramme per litre.

I do not mean to assert that the quantitative examination of diabetic urine by the polariscope is absolutely free from the possibility of error. If the quantity of sugar is very minute error may occur to a small extent, and if the tube containing it is too short the accuracy is further jeopardised, but certainly with a well-constructed instrument these errors are very trivial compared with those which beset volumetric analysis.

Certain precautions are needful with the Yvon-Duboscq diabétomètre. The urine must be perfectly clear, and to render it so it is needful to decolorise it by agitation with animal charcoal, or by adding some solution of acetate of lead and then filtering. I prefer the acetate of lead for several reasons. The animal charcoal must be perfectly fresh, and even then it destroys a fraction of the sugar. The acetate of lead solution has only one drawback—namely, that it dilutes the urine, and allowance must be made for this at the final computation. To lessen this difficulty I use a burette marked with a graduation at 50 centimètres and again at 55 centimètres. I fill up to 50 cc. with urine, and then add 5 cc. of liquor plumbi. This gives a definite proportion of one-tenth, which must be added to the total sugar indicated—for example, if the amount indicated is 50 grammes, it is necessary to read it at 55, and so on in proportion.

The result of my experience is, that for the adjusting of the diabétomêtre, clearing of the urine, and its examination, a period of *ten minutes* amply suffices.

The Yvon-Duboscq diabétomètre, with full instructions and all needful appliances, can be had from the makers, Messrs. T. & A. Duboscq, 11 Rue des Fossés, St. Jacques, Paris.

In conclusion, I venture to recommend to all busy practitioners who wish to make fairly accurate, but easy, quantitative analyses of albumin, urea, and sugar in urine, the methods which I have now described, and which I brought under the notice of the Academy of Medicine in Ireland during the present session of 1885–86.

CLINICAL NOTES ON A CASE OF ACUTE LICHEN PLANUS.

By J. MAGEE FINNY, M.D., Dubl.;
Fellow and Vice-President, King and Queen's College of Physicians in Ireland.

[Read in the Medical Section, Friday, April 30, 1886.]

Cases of lichen planus are of sufficiently rare occurrence in this country as to make me think that the recital of one such case will prove not lacking in interest. The very acute course the disease ran, its very extensive manifestations over almost the entire body of the patient, the head and face alone being exempted, and its rapid and permanent cure, are points in its clinical history which may be expected to enhance that interest.

I have paid considerable attention to the study and treatment of diseases of the skin, and for the last twelve years have one day in each week had the advantage of seeing, at the extern department of the hospitals to which I have been physician, a great variety and number of skin diseases, but I never met with so exaggerated or so marked a case as that which I now bring under your notice; and I think my experience and observation agree, probably, with those of other physicians in the United Kingdom:—

Mrs. H., aged fifty-three, a mother of six children, and of previously healthy medical history, consulted me, on August 29, 1885, for an extremely itchy skin disease, from which she had been suffering for the preceding six weeks. Menstruation had ceased eighteen months before, without trouble, and, except for an irritable rash on the neck twelve months ago, which was seemingly of an eczematous nature, she had never had any skin affection. She was a stout, pasty-looking woman, had a "bad stomach," and occasionally had suffered from rheumatic pains in her shoulders. Her occupation was keeping a lodging-house, and she lived comfortably;

but latterly she seems to have been easily irritated and worried by little things, and to have unduly felt the burden of every-day life. She has been easily tired for some months, and, what is a notable fact, on two occasions, while out in the street, her bowels suddenly acted, although they had moved naturally in the morning.

The " skin disease" for which she consulted me showed itself at first in isolated areas on the legs, below the knees. Later on, it came out on the flexor aspect of the arms and forearms, under the knees, and particularly over the vastus internus, and then on the dorsum of the feet, down towards the toes. Subsequently, while under observation, the rash extended over the whole back (being very thick over the loins), lower part of abdomen, nates and thighs, and on the upper extremities, the front of the wrists, and the dorsum of the hands and fingers. The palms and soles were not exempted, and, although the eruption was scanty in these localities, it was very evident along the fibular side of the sole and over the inner eminence of the palm. At the acme of the disease, which may be said to have been reached about the third week in September, no part of the body was free of the eruption, except the face, scalp, and neck. The photograph [exhibited], taken October 10th, faithfully depicts the millions of papules which constituted the eruption.

The colour of the eruption was at once striking, being, when fully developed, of a dull-red or purplish-red, not unlike a syphilide. It consisted of numerous papules, with a flat surface abruptly raised above the adjoining skin. The papules varied in size from a split pea to a threepenny piece (2–6 lines), and were smallest on the fingers and toes, and largest on the thighs, nates, and back. They varied also in consistence, being just perceptible to the finger on the backs of the hands, while they were large, thick, and buttony over the back and loins, and conveyed to the hand the horny hard feel of the variolous pustule when dried up in the third week of smallpox, and produced so much discomfort to the patient that lying on the back was avoided.

The papule wherever situated had the same characteristics. It was solid, abruptly raised, round or polygonal in outline, with sharply-defined edges; the surface was flat, smooth, with a waxy or glazy appearance. Many of the papules were slightly depressed or quasi-umbilicated, and to some a slight filmy desquamation was attached.

The central pitting was not universal, nor did it correspond to the passage of a hair. As Crocker[a] has shown, it is probable that the sweat-ducts, rather than the hair follicles, determine the situation of the papules. The papules were isolated, and even in the large patches on the back, knees, &c., they seemed to maintain their individuality, though by crowding they touched each other. In these *plaques* the skin had a remarkable mapped-out or tesselated appearance, and the increased pigmentation lay rather between than throughout the teeming papules.

During the acute period of the attack, and up to Oct. 10th, there was considerable induration of the skin, due to infiltration, so that the movements of the joints were much interfered with.

The itching was of a most aggravated character, with much irritation and tingling, and equalled the pruritus of scabies and gouty eczema combined. Associated with it, and, possibly, consequent thereon, there was much nervous depression. The patient was in very low spirits, with constant sighing and almost complete loss of appetite and sleep—in fact, for many nights hypnotics and narcotics were required to secure sleep.

Emaciation was shown before long, and was out of proportion to the length of illness and confinement to bed. The diagnosis was not very difficult, when the whole features of the case were considered, although they did not conform to the descriptions of either L. Planus or L. Ruber as given by Hebra.

The treatment consisted in alkaline and size baths, sedative lotions, and the internal administration of liq. arsenicalis. In the beginning I employed pilocarpine hypodermically at intervals. To this I was led by the idea that free diaphoresis would relieve the infiltration and thickening of the skin. The patient invariably expressed herself relieved by the sweating which $\frac{1}{6}$–$\frac{1}{4}$ gr. readily produced, and to it she attributed most benefit. Within the last few days I find in von Ziemssen's "Handbook of Skin Disease" that good resulted from its use by Dr. Köbner. What seemed, however, to afford more relief to the pruritus than anything else, including sedative lotions, were warm baths and the use of a lotion containing carbolic acid and liq. carbonis detergens.

I made a tentative though full treatment of chrysophanic acid to the right leg and foot, but I am bound to say that I could not see that it was of the slightest use.

The administration of Fowler's solution of arsenic was commenced in 5♏. doses on Sept. 12th, and the dose was gradually

[a] Lancet. Vol. I. 1881.

increased up to 13 minims three times a day, when it produced its physiological effects on the mucous membranes of the eyes, stomach, and bowels. It was then intermitted, and after a few days resumed in smaller doses. It was, however, persevered in till the end of October, a dose of 6 minims being well tolerated. Improvement was very doubtful for the first fortnight, but it was then observed that no new spots made their appearance, while a diminution in the severity of the itching and an absorption of the infiltration set in.

On 7th Nov. not a papule remained, though the pigmentation and stains were seemingly unchanged, so that the skin had, as the patient aptly expressed it, an appearance like a "speckled turkey egg." These stains existed for some months subsequently.

When the patient went from under my care she expressed herself as feeling better in general health and spirits than she had felt for several months before the lichen appeared.

The foregoing case represents a very severe, though typical aspect of diffuse lichen planus. Many cases are of a very limited area, such as on the glans penis,[a] and its course may be very chronic or acute.

Erasmus Wilson first described the disease as a distinct form of lichen in 1869, and looked on it as one of great rarity. He met with but fifty instances among ten thousand miscellaneous cases of skin disease. It is closely allied to, if not identical with, the lichen ruber of Hebra, as many observers have noted these two conditions to have followed one upon the other.

My experience can add no facts on this question. It is, however, a matter for regret that more accuracy of description is not observed in the rather indiscriminate use, by even recent writers, of the terms lichen ruber and lichen planus.

The ætiology of lichen planus is a subject of some difficulty, and this, doubtless, is intimately connected with and due to the imperfect knowledge we have of its pathology. Is it a disease of a localised inflammatory nature, or is it a manifestation of a general disorder?

[a] *Vide* two cases recorded by Dr. Bulkeley. Arch. Dermatology. Vol. VII., pp. 135, 392.

The following may be said to represent the more recent anatomico-pathological facts of this disease:—

I. Crocker[a] considers the anatomical process at the onset to be a superficial inflammation independent of the hair follicles, and he distinguishes two varieties—

(*a*) Connected with the blood-vessels.

(*b*) Due to proliferation of cells of the rete.

The sweat-ducts often appear to determine the localisation of the papules.

II. Neumann found the chief changes to be in the upper layers of the cutis, and to consist in thickening of the layer of horny cells and increase of the rete.

III. Dr. A. Weyl (the writer in von Ziemssen's "Handbook of Skin Disease," 1885, just printed) considers that the process begins as a perivascular proliferation of the vessels of the upper layers of the cutis, and is followed by an infiltration of cells into the papillæ following the course of the nerves.

The general consensus of opinion seems, however, to be in favour of the view that the nervous system is primarily at fault.

Dr. Tilbury Fox,[b] and more recently Dr. T. Colcott Fox,[c] considers that the local disease is not of an inflammatory nature, but is a simple chronic neuroparalytic hyperæmia in localised areas with its consequent effects.

There is in the majority of cases[d] a history of debility arising from overwork, nervous depression, and improper nourishment, and the symptoms which accompany it are chiefly referable to nervous exhaustion and prostration. Moreover, the symmetry of the eruption and the benefit which accrues from a nerve tonic like arsenic point to its neurotic origin.

As to its treatment, arsenic may be looked upon in the light of a specific for lichen planus, having the authority and weight of the sanction of E. Wilson and J. Hutchinson. Duhring also strongly

[a] Lancet. 1881.
[b] Skin Diseases. 3rd Edition. 1873.
[c] Brit. Med. Journal. 1879. Vol. II., p. 292.
[d] Duhring. 3rd edition.

recommends it, and lays stress upon its use being persevered in; and a recent writer, Dr. Lavergne,[a] urges the necessity of administering it in considerable doses. Köbner[b] records a case of rapid cure of lichen ruber by the hypodermic injection of liq. arsenicalis (diluted) after it had failed by the mouth, the itching having yielded to the first few injections.

On the other hand, while there is so great an array of cases in which arsenic may fairly be credited with good, it is right to note that Dr. Tilbury Fox[c] (in 1873) states, "With regard to arsenic, I can only say that it has always made my cases worse." R. W. Taylor[d] recorded in 1874 four cases of severe acute lichen planus, a disease of very great rarity in America, in which alkaline baths and tinct. saponis c̄ pice of Hebra produced a cure in four to six weeks.

Recently Dr. George Thin detailed[e] six cases of lichen ruber which had been cured by Prof. Unna[f] by external means alone, discarding the use of arsenic altogether. To this series Dr. G. Thin adds another instance in his own practice, where by soft soap and white precipitate ointment he cured a case of lichen planus of the glans penis.

Unna's prescription is—

Ung. zinci benzoat.	-	-	500
Acidi carbolici	-	-	20
Hydrarg. bichlorid.	-	-	0·5–1

and needs care in its use over a large surface, as carbolic acid shows itself in the urine in three to four days. The cases this treatment is said to be specially suited for are those in which great depression exists and much itching, and Unna claims for it both the speediest means of procuring alleviation to the pruritus and of producing a cure.

[a] Journ. Cut. and Ven. Dis. Vol. II., p. 303. 1884.
[b] Deut. med. Wochenschrift. 1881. VII., p. 3.
[c] Loc. cit.
[d] Archives of Dermatology. 1874. P. 36.
[e] Brit. Med. Jour. P. 425. 1885.
[f] Monatschrift. für prakt. Dermatolog. 1882. No. 1.

Dr. Thin, commenting on this treatment, remarks that this subject is one of more than therapeutic value as showing that lichen ruber is more of a local disease than it is usually considered.

In my case both carbolic lotion and the internal administration of arsenic were employed; but I think the arsenic deserved the full credit for its cure.

A CASE OF ENTERIC FEVER WITH UNUSUAL SYMPTOMS AND MORBID APPEARANCES.

By JAMES LITTLE, M.D.;
Physician to the Adelaide Hospital.

[Read in the Medical Section, Friday, March 26, 1886.]

At this late hour I will merely state a few points which to my mind justify me in presenting these morbid specimens to the Academy. In the first place, they illustrate extremely extensive intestinal lesions occurring in the course of enteric fever; and, in the next, while the intestinal lesions were extremely extensive, the symptoms during life were almost entirely cerebral and thoracic. My colleague, Dr. Beatty, and I thought it would be worth while showing the two cases as forming a contrast; for, in Dr. Beatty's case the lesion which caused death was abdominal, and yet the amount of intestinal mischief was very slight; in other respects my case was rather a peculiar one, and shows a difficulty in the diagnosis of enteric fever:—

A woman, thirty-three years of age, was admitted into the Hardwicke Hospital, under Dr. Nugent, on the 30th December last. She was brought by her husband in an extremely weak condition, being unable to give any information with regard to herself or the duration of her illness. The husband said that she had been ailing for about a fortnight. She had had rigors, and was now complaining of pains in her back and limbs and of headache. She was in a very exhausted and helpless state, being unable to stand or support herself in any way. She was a very full-blooded and plethoric individual, her face was very flushed, and the pupils were contracted and the conjunctivæ injected. The skin was burning and dry, and the temperature 103° F. There was slight delirium, the mind being somewhat confused, and her temper irritable. Her husband said that she had been drinking a good deal previously. There was intense thirst, which could only be appeased by drinking numerous bottles of soda water. Dr. Nugent (to whose kindness I am indebted for these notes of the first portion of the patient's illness) examined the chest, but found no evidence of lung affection. The abdomen presented nothing in particular—there was no tenderness, nor pain on pressure. The bowels were constipated. Though no rash was seen, it appeared quite evident, taking all the symptoms together, that she was suffering from typhus fever. Upon the foregoing diagnosis she was admitted into the Hardwicke Hospital, as already stated, on the 30th December. At times she was wildly delirious, requiring to be held in bed. She was discharged on the 23rd January, twenty days after a critical fall of temperature, and was then in apparent health. She only remained in apparent health four days; then she took ill again, and was admitted into the Adelaide Hospital on the 6th February, and she died on the 1st March.

During the first week, as I was absent, she was under the care of Dr. Bewley; afterwards she passed under my care. When I saw her, Dr. Bewley had come to the provisional diagnosis of enteric fever from the existence of a few spots on the abdomen; but they differed from the ordinary spots of enteric fever in this—that they were out when she came into hospital, and they remained out five or six days, instead of appearing in successive crops, as is usually the case. I always felt a doubt about the diagnosis of enteric fever from these circumstances. The abdomen did not present the usual appearance of enteric fever at all. It was very slightly distended. There were no spots to be seen after the woman passed under my care. She had some diarrhœa certainly, but the stools were not very characteristic, and always contained solid matter. The chief symptoms under which she suffered were cerebral and thoracic—*i.e.*, she had constant, muttering delirium; and, on auscultation over her chest, sibilant and muco-crepitant râles were everywhere audible. I cannot attach much importance to the temperature—which was sometimes as high as 105° F., and then, in the course of twenty-four hours, went down to normal—as she several times had antipyretics. The high temperature was just as frequent in the morning as in the evening. Taking all these circumstances into consideration—the delirium, the râles through the chest, the absence of abdominal tenderness in enteric fever, and the entire absence of enteric pulse (there being a small pulse more like what one gets in typhus)—I felt a doubt about the diagnosis. The spleen was somewhat enlarged, and so was the liver. I was myself of opinion that the probability lay in favour of its being a case of acute tuberculosis. I am aware that sibilant and muco-crepitant râles are almost invariably present in the third and fourth week of enteric fever, but they were more persistent in the present case than they usually are. When she died we found there were present the results of enteric fever in an exceptionally well-marked form. I have here [exhibiting the morbid specimen] the upper portion of the small intestine, the ileum and a portion of the jejunum, where there are an immense number of ulcers to be seen. Here is an immense ulcer in the lower portion of the ileum, and, when we come down to the favourite seat of ulceration in enteric fever—namely, the ileo-cœcal valve—we find a large ulcer there. But the peculiarity of it was the extreme state of ulceration in the large intestine. It was like the appearance of the skin in an exaggerated case of smallpox. There were seventy or eighty ulcers in the large intestine, extending down to within three inches of the rectum. The specimen is worth showing on that ground alone. It is difficult to suggest an explanation of the case and to say whether the patient had another fever altogether when in the Hardwicke Hospital, of which she recovered, and was subsequently admitted into the Adelaide Hospital suffering from enteric fever. That may possibly be the case. But, among the reasons for submitting the specimen, here are these—first, to show that the amount of abdominal pain and distension may be no measure of the actual amount of morbid change in the intestinal walls; and, secondly, that the diagnosis of enteric fever must sometimes be made without attaching too much importance to the peculiar pulse, or to the peculiar range of temperature, or the presence of successive crops of rose spots.

CASE OF CEREBRO-SPINAL MENINGITIS.

By RICHARD A. HAYES, M.D.;
Physician to Steevens' Hospital.

[Read in the Medical Section, Friday, May 28th, 1886].

P. O'S., aged twenty, R.I.C., was admitted to Dr. Steevens' Hospital, on the evening of March 10th, 1886. He was then surly and stupid, and did not appear to understand what was said to him. It was stated that at 8 o'clock that morning he had complained of a chill, and later in the day, when being brought down to the hospital, was very cross and troublesome. His temperature on admission was 104° F.; pulse, 120. At 3 a.m. the next morning he was seized with vomiting, and shortly after became delirious and very violent, frequently crying out. He was given ℞ Chloral Hyd. gr. 15, Pot. Brom. gr. 30, without effect. The draught was repeated, but as it did not quiet the patient, and his struggles were difficult to restrain, Dr. Trouten—who was at the time acting as resident-surgeon—placed him under the influence of chloroform, which had to be continued for four hours; he then became quiet, remaining with his head slightly thrown back and covered with his arms.

11th March.—At 9 a.m. his temperature was 98° F., and pulse 90. I saw him at 10 o'clock, and found him lying on his left side, his head thrown back. He kept his arms over his eyes, and strongly resisted any attempt to remove them, or examine him in any way—groaning loudly when touched; his lower limbs were semiflexed, his pupils were normal, his pulse was full but irregular, his breathing was rapid and shallow. I found a slight but well-marked purpuric rash on his trunk and limbs. His bowels had not been moved since his admission, but he had passed water. His tongue was white and furred. Nothing abnormal in heart, lungs, or abdominal organs could be detected. I ordered his head to be shaved, an ice-cap to be applied, and a blister to the back of his neck; also Ol. Crotonis ♏. i. in honey, and Ammon. Brom. gr. 15 every fourth hour. He remained quite unconscious during the day, maintaining the same attitude, crying out when touched or moved. His bowels acted freely after the bolus. Evening temperature, 102°; pulse, 110. Towards night, as he was decidedly weaker, he was ordered half an ounce of whiskey every three hours.

12th.—Next morning his temperature had fallen to 100° F.; pulse, 120, and irregular. When I saw him he was conscious, and answered questions, but with difficulty; complained of feeling weak, and of pain in head and also in neck when pressed upon; there were constant twitchings of the face, especially about the mouth; the purpuric spots were unaltered, but in addition I found several crops of herpes on legs and arms, these being specially abundant on both knees. He was now passing large quantities of water, which contained no albumin. He remained much the same all day, complaining of thirst, and taking much milk and beef-tea; in the evening he became restless, and was given a draught of Ammon. Brom. and Chloral. He had a good night, evening temperature being 102°.

13th.—He was clearer this morning, and said his head was better; temperature, 101° F.; pulse quiet, and more regular; still complains of thirst; tongue cleaning; the rash was fading. Evening temperature, 100° F.

14th.—This morning he was much worse. Temperature, 101°; pulse, 144, and weak; respiration, 38. Was again unconscious, twitchings of face very marked, further crops of herpes on legs; ordered free stimulation; evening temperature, 102°; pulse still weaker, respiration being rapid, sometimes up to 60 per minute.

15th.—When I saw him he was evidently sinking fast; morning temperature, 104°. He died at 11 a.m. No *post mortem* could be obtained.

CANCER OF BRONCHIAL GLANDS AND LUNG.

By M. A. BOYD, M.K.Q.C.P.;
Physician to the Mater Misericordiæ Hospital.

[Read in the Medical Section, May 28, 1886.]

In bringing before the notice of this Academy the subject of cancer of the bronchial glands and lung, I am not calling attention to anything new, but I think I may venture to assert it is a subject the importance of which cannot be exaggerated, and the difficulties in the diagnosis of which all physicians must admit.

From the enormous amount of adenoid tissue within the thorax, and the well-known affinity cancer has for that structure, we should reason *a priori* that intra-thoracic cancer should be of not infrequent occurrence; and from the marked similarity between it and phthisis, especially the so-called senile variety of phthisis, in many of its symptoms, the existence of the disease is, I am convinced, frequently overlooked.

The characteristic symptoms that are said to indicate intra-thoracic cancer of either the mediastinum or glands, such as fixed pain in the chest, red currant jelly expectoration, and enlargement of cervical glands, are not present in a fourth of the cases, while symptoms that indicate phthisis are present in the great majority of them, and in some the presence of cancer is not even suspected until wide-spread infiltration of either the lung, pleura, or pericardium reveals its existence. Enlargement of one or more cervical glands when present with symptoms of a tumour within the thorax, not aneurysmal, would be to us conclusive of the presence of cancer, but this evidence is more often wanting than not. The same remarks apply to the existence of pain which is seldom present to any extent until symptoms of pressure on the large nervous trunks occur, and this is generally so late a symptom that all doubt as to the nature of the case is set at rest before pain becomes a prominent symptom. Red currant jelly expectoration is, in my experience, a very rare symptom.

I am aware that in these assertions I am not in keeping with the generally-received opinions on the subject, but, unfortunately, every-day experience proves to us that generally-received opinions are often erroneous, and we find, often when it is too late, that all the statements in standard works are not invariably to be relied on. Especially so is the generally-accepted one that cancer of the lung or mediastinum is generally secondary to cancer in other organs, so that our conclusions are unsatisfactory, even when symptoms arouse our suspicion and we look for its presence elsewhere, generally not to find it.

Cancer of the bronchial glands and lung, in my experience, nearly always begins as primary cancer in these situations, and very rarely as secondary infiltration from other parts, and when it is secondary there is generally well-marked engagement of the lymphatic system in other situations before it becomes intra-thoracic.

The intra-thoracic situations, where malignant disease is met, are two—namely, the bronchial glands and lung, and the anterior mediastinum. The former situation is, from a clinical point of view, the more important, as malignant disease in these structures simulates phthisis in a sub-acute form so closely that its differential diagnosis in some cases is almost impossible.

From observation of a good number of cases I am convinced this is the case; and a case of malignant deposit in the bronchial glands infiltrating the lung, ending in ulceration and in the formation of cavities, is frequently set down as one of hopeless phthisis, a *post mortem* on which would be of no interest, and all record of the frequency of the disease is in consequence entirely lost.

With mediastinal cancer such an error is not so likely to occur, as, like other intra-thoracic growths, such as aneurysm, the symptoms are more the result of the pressure it exercises on the important vascular and nervous structures around it, and which attract attention long before infiltration of the lung or general cachexia show themselves. Added to this the growth is more superficial and more easy to be recognised. With the former variety lung symptoms predominate, with the latter cardiac and vascular ones.

Hæmoptysis appearing early, and being constant in its occurrence, would seem to be the most reliable sign, and was present in more than half of thirty-nine cases recorded by Dr. Risdon Bennett in his Lumlian lectures on intra-thoracic cancer; and Dr. Walshe speaks of it as having occurred at some period in nearly all of his cases.

When persistently present it affords evidence of the situation of the growth in the bronchial glands, or in the lung itself, with pressure on the bronchial veins, as distinguished from mediastinal cancer, which latter, as Dr. Cockle remarks in his work, may afford no evidence of its existence until pressure symptoms or infiltration of the surrounding parts, especially the pericardium or large vessels, indicate the presence of a tumour within the thorax.

I will now confine my remarks principally to the symptoms that indicate cancerous engagement of the bronchial glands and lung, and their similarity to phthisis, and afterwards to those that indicate engagement of the mediastinum.

I have mentioned hæmoptysis as a constant and oft-recurring symptom. Next in importance, as pointed out by Dr. Rossbach, would seem to be early congestion of the larynx, with aphonia, and without paralysis of the vocal cords, which latter symptom I will allude to afterwards as conclusive of mediastinal cancer.

Spasmodic cough, with very little expectoration in proportion, as indicating early irritation of the bronchial nerves, frequently accompanied by dyspnœa and vomiting, is another early symptom. Next, dulness on percussion over some portion of the chest anteriorly; but this is not always present in the early stages of cancer of the bronchial glands, though it is the earliest symptom in mediastinal cancer. Feeble respiratory sounds on one side, with bronchial râles and tubular breathing, with prolongation of the expiratory sounds, and symptoms indicating pressure on, and narrowing of, the bronchial tubes, are frequently present. Difficulty in swallowing from pressure on the œsophagus, with cough and vomiting excited by the effort, and a venous hum over the upper portion of the sternum, when the head is thrown back, as pointed out by Dr. Eustace Smith when describing the symptoms indicating enlarged bronchial glands, might be placed next in order.

Pleural effusion into either one or the other side, containing blood when withdrawn by aspiration. This latter phenomenon is said to be characteristic of intra-thoracic cancer, though microscopic examination of the pleural fluid, as well as of the sputa, in these cases, generally give negative results as regards the presence of the characteristic cells.

If we add to many of the above symptoms in advanced cases and in old people, muco-purulent expectoration, emaciation, and night sweats, and a moderately high temperature, we have a chain of phenomena that resemble phthisis.

When cancer begins in the anterior mediastinum there may be no cough whatever in the early stage, but there is marked retro-sternal dulness, with sense of resistance, and generally retro-sternal pain. Here pressure symptoms begin to show themselves early, as indicated by displacement of the heart, fulness of the veins in the neck, with cyanosis, and œdema of the face and upper extremities, and later on dyspnœa, with stridulous breathing, aphonia, and paralysis of one or other of the vocal cords, as the vagus or recurrent laryngeal respectively get pressed on. Paralysis of one or other of the vocal cords, with symptoms of a tumour within the thorax, in the absence of aneurysm, is regarded by Dr. Burney Yeo as absolutely diagnostic of malignant disease.

Though the symptoms of cancer of the bronchial glands resemble phthisis very closely, we shall find in the signs occasionally features that raise doubts in our minds as to their being phthisical ones. The dulness on percussion is generally not apexial but lower down over the lung, and is elicited posteriorly, opposite to the same situation in front, and is generally more extensive than in phthisis when the disease is of some duration. There is also generally an absence of all crepitus, though bronchial râles and feeble inspiratory sounds, with prolongation of expiration, are present.

When the bronchial glands are much enlarged and compress the bronchi, vocal fremitus and vocal resonance are both absent, and absence of all breath sounds, with collapse or consolidation of the lung generally, present a condition you will not find in phthisis, no matter how extensive the consolidation.

Though many of the above symptoms are present in a good number of the cases of primary cancer of the bronchial glands, in some one of the symptoms will be most marked; and the first of the following cases illustrates the prominence of pleural effusion over all other symptoms:—

Case I.—John G., aged thirty-eight, a bottle blower by trade, was admitted to the Mater Misericordiæ Hospital on 27th of last July, suffering from cough, dyspnœa, lividity of the face, and slightly bloody expectoration. He never had syphilis, rheumatism, or any other illness, though he drank hard at times, and was able to attend to his work, in which he was constantly exposed to draughts for a period of fourteen hours at a time.

There was no history of malignant disease in his family. Three weeks before admission, while engaged at his occupation, he caught cold, which showed itself by prolonged rigor, sickness of stomach, and headache, with feverishness. He also felt pain of a catching character in the right mammary region. He was confined to bed for a few days with these symptoms, but after this time was able to attend his work again, experiencing only slight pain in right side, cough, and shortness of breath. For a week before admission these symptoms became worse, and on admission he presented the following ones:—Lividity of lips and face, with puffiness of eyelids, and prominence of the veins on front of thorax and also in neck; no enlarged gland to be seen. His voice was hoarse and aphonic, and there was urgent dyspnœa, especially on exertion, with cough and bronchial fremitus over chest. Patient seemed in no way emaciated, and did not suffer from night sweats. There was absolute dulness on percussion over entire aspect of right chest, except for an inch or two under clavicle, where it was tympanitic, and, behind, dulness reached as high as centre of scapula. In front the dulness encroached across the upper portion of the sternum, to the extent of an inch or two left of sternum, and here the dulness gave great sense of resistance.

There was feeble inspiration through the right lung, with bronchial râles in both; and vocal fremitus and vocal resonance were both absent on right side. The heart was feeble in its action, but not displaced, and no murmur present either in it or great vessels.

From enlarged measurements of the right side, and the above symptoms, I considered there was fluid in the pleura, though I could not explain the symptoms of venous pressure from this alone, and could only conjecture there was a tumour within the thorax also. I aspirated the right pleura the day after his admission, removing nine pints of fluid, the latter portion of which was bloody.

After removal of fluid the dulness was more distinct over upper portion of sternum, and could be defined. His dyspnœa was considerably relieved, and more air seemed to enter right lung; cyanosis of head and face, however, continuing, and veins on thorax showing more prominently. A week later his right arm and face became very swollen, and recurring dulness over right side, indicating fresh effusion. I again tapped the pleura, and removed six pints of bloody fluid. Some days after second tapping his temperature rose, his dyspnœa increased, and dulness at base of left lung showed itself. Suppression of urine, with delirium, set in, and he died on August 18th. The *post-mortem* showed a large mass of cancer, both hard and medullary, infiltrating the bronchial glands and lung on the right side, beginning at the bifurcation of the trachea, and involving in its course the pleura, which was thickened, lymphy, and contained a quantity of bloody serum, and infiltrating the pericardium over auricle, the superior cava of which it compressed. The left lung was congested throughout, and solidified around its main bronchus from recent inflammation.

The following case, in which symptoms of phthisis with difficulty in swallowing were the most prominent features, shows a different train of symptoms:—

Case II.—James M'C., a waiter, aged thirty-seven years, was admitted to the Mater Misericordiæ Hospital on the 22nd of last September, complaining of cough, with muco-purulent expectoration, mixed with blood, night sweats, and emaciation and symptoms of stricture of the œsophagus.

Patient gave a good family history; no phthisis on either side of family. He drank freely, taking as many as eight or ten glasses of whiskey daily, undiluted. Never suffered from any illness until six months before admission, when he noticed a difficulty in swallowing his food, accompanied by cough and vomiting; food stopping at a spot which he referred to centre of sternum. There was no pain at the place of constriction, and no sense of pressure or dyspnœa. He coughed up frequently considerable quantities of blood, bringing up nearly half a pint six weeks before admission, occasionally jam-tinted or prune-juiced in colour. On examination the heart was found healthy; no murmur. No enlargement of cervical glands. Throat was congested, and voice aphonic. Dulness was elicited over upper portion of sternum, extending to sub-clavicular space on right side, with tubular breathing and bronchial râles, but no crepitus. No dulness posteriorly, breath sounds being normal here. An œsophageal tube of the smallest size could be passed, and the stricture only admitted the next size up to the time of his death. All attempts to pass a larger one failed. After catheterisation, liquid nourishment could be taken, but it always induced cough at first, ending in vomiting. The temperature throughout varied, rising occasionally from normal to 101·2°. His emaciation and night-sweats increased, the tube being passed daily to allow of nourishment being taken till 17th October, when the dulness over the upper portion of sternum and right apex showed a marked increase, and on October 21st no tube could be passed, the attempts to do so causing considerable pain. Pulmonary congestion set in, and the patient died on October 28th.

I regret that in this case no *post mortem* was allowed, as we should, no doubt, have found enlarged cancerous bronchial glands pressing on and constricting the œsophagus, springing from the fork of the trachea, which would seem to be the most usual site for cancer in this situation to begin.

Case III.—A washerwoman, aged fifty, was admitted to my ward in the Mater Misericordiæ Hopital, with feverish symptoms, pain in left side, dyspnœa, wasting cough, and muco-purulent expectoration, mixed with blood, which was constantly present.

The patient was seen in my absence by the assistant-physician, who diagnosed pleural effusion of left side, with dulness on percussion at base, absence of vocal fremitus and resonance; but, though no dulness existed in the upper portion of the lung, there was a complete absence of all breath sounds, which seemed rather difficult to explain. The woman showed marked hectic with malar flush, and dyspnœa out of all proportion to the pleural effusion. Her illness began about three months previously with pain in left side, before which a cough had existed for some months, with blood-spitting, but symptoms were not urgent until a few weeks prior to admission. There was dulness in front, behind upper portion of sternum, extending to and becoming continuous with cardiac dulness; but the percussion over both apices was normally clear. Respiration was peurile at right apex and over right lung, but there was complete absence of all respiratory sounds over left apex. Dyspnœa and orthopnœa were most troublesome, and the temperature remained high up to the time of her death, which occurred three weeks after her admission. The *post mortem* showed dark-coloured serum in the left pleural cavity, to the extent of two or three pints; the pleura and lung adherent at its upper and anterior aspect, with a thickened mass of hard bronchial glands surrounding the root of the left lung, compressing the bronchus and bronchial veins. The lung itself was contracted to half its size, and puckered here and there from emphysematous lobules. The upper lobes were studded in places with cancerous nodules, both medullary and scirrhous, which spread in a radiating manner from the root; between these the lung was solidified and non-crepitant.

I may mention that in both these cases microscopic sections of the deposit showed the typical cells; and in the two first cases, though there were aphonia and congestion of the larynx present, there was no paralysis of the vocal cords evident.

I need not here allude to secondary cancer occurring in these situations, my object being to show that primary cancer is, of the two, much more frequent, and that in advanced cases likely to be confounded with phthisis.

The general conclusions I have come to with regard to the disease are as follow:—

That primary cancer of the thorax is of not infrequent occurrence generally in hard drinkers, and that the symptoms of it are developed apparently after catching cold.

That it is to be found in two situations within the thorax—namely, the bronchial glands and the anterior mediastinum, and of the mixed variety, scirrhous and encephaloid.

That persistent blood-spitting is a very usual symptom in its growth, and indicates infiltration of lung and pressure on bronchial veins; and that the so-called characteristic red-currant-jelly expectoration is a rare symptom.

That fixed pain in the thorax is not always present; and, when it is so, is more usual in the mediastinal form of the disease.

That the disease is generally accompanied by aphonia and congestion of the larynx in its early stages, and by paralysis of one or other of vocal cords in a later stage.

That when in the bronchial glands it is invariably accompanied by symptoms of pressure on either one or other bronchus, or on the œsophagus, causing symptoms of stricture.

That pleural effusion containing blood is very frequent on the side affected; and in its growth forward from the root of the lung it involves the pericardium, and causes pressure on the superior cava or pulmonary veins.

That when in the bronchial glands its latter stages may simulate advanced phthisis very closely.

That it may occasionally be distinguished from phthisis by its pressure symptoms on the bronchiæ; its dulness being more central and more extensive, with absence of crepitus, and more urgent dyspnœa, while no bacilli are found in the expectoration.

That malignant intra-thoracic disease is generally fatal in from two months to six, though Dr. Walshe gives the average duration as from three months to thirteen. In the cases I have just mentioned the disease was fatal in each case within seven months.

I need not here allude to the difficulty in distinguishing between intra-thoracic malignant disease and aneurysm, where no murmur is present, and pressure symptoms are the most prominent feature in the case. The difficulties of forming a differential diagnosis in such cases are indeed great—nay, sometimes almost impossible; and we have mainly to rely on the evidence afforded by arterial disease elsewhere to assist us.

SURGICAL SECTION.

ON THE ADVANTAGES OF THE PRINCIPLE OF DRY DRESSINGS IN ANTISEPTIC SURGERY.

By KENDAL FRANKS, F.R.C.S.;
Surgeon to the Adelaide Hospital.

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The discussion on the Treatment of Wounds at the recent International Medical Congress at Copenhagen differed in a remarkable manner from a similar discussion at the London Medical Congress in 1881. At the London Congress the great point at issue was whether the Listerian principle—that is, the principle on which all antiseptic surgery was based—however varied in detail, had been fully established; and the opponents of the principle itself were many and strong. At the late Congress, however, all this was changed: the central point, round which the discussion turned, was the best method of putting the principle in practice. The principle itself found but weak if any opposition. Now, that discussion showed one thing conclusively—namely, that however much the principle was valued, however great had been the improvements imported into surgery by the rigorous adoption of Lister's method, still we had not reached finality, that still much remained to be done, and that there still were ends to be gained in our methods of dealing with operation wounds which Lister's method did not reach.

"The ideal wound," says Mr. Watson Cheyne, "is a subcutaneous one, kept at perfect rest." That definition we may unhesitatingly adopt. By the rigid application of Lister's principle we are able to keep, not the air, but the harmful matters contained in the air and adhering to the surroundings, out of our wounds, and therefore we practically put them on the same footing as a subcutaneous wound. But even a subcutaneous wound will not heal kindly unless it be kept at perfect rest, and in operation wounds we must keep the same principle before us as of the greatest importance. Now, to quote Mr. Watson Cheyne again, and I do so because he is the great exponent of the Listerian method, he tells us that the causes of unrest may be mechanical or chemical. The chemical causes are chiefly two—first and most important, the unrest due to the presence of organisms in the wound, a cause which aseptic surgery has abolished; secondly, the irritation caused by the antiseptic itself. It is not perhaps possible at present to remove this cause of unrest entirely, but it may be reduced to a minimum, at least to such a point that the irritation is not sufficient to interfere with the healthy course of the wound. This Sir Joseph Lister attains by the employment of a protective in the form of specially prepared oil silk which keeps the constant action of the carbolic acid out of the wound. Among the mechanical causes of unrest are especially to be noted movements of the part, the presence of foreign bodies in the wound, tension, whether from tight stitches or retained secretions, &c. Now, it is in respect to some of these causes of unrest that Lister's method is open to improvement. Mr. Watson Cheyne allows so much when, in speaking of the ideal wound, he says:—"We have not yet attained this ideal, for even with the aseptic method there is a certain amount of unrest caused by the antiseptic employed, by the stitches, by *the apparatus for drainage*, and *by the dressing itself*." Now, in this sentence we have lines laid down upon which we must travel if we wish to attain to the ideal.

No one will nowadays deny the importance, when possible, of infrequent dressing. The longer we can leave a wound undisturbed, without interfering with its aseptic condition, so much the better. The dressing of an operation wound causes disturbance both constitutionally and locally. We have frequently seen with what anxiety a patient, and especially a young patient, looks forward to the time of dressing, and in the most skilful and tender hands it is not often possible to remove a dressing, to cleanse the part, to take out or shorten the drainage tube and to put on a fresh supply of gauze, without causing to some extent pain; or even if pain be not inflicted the nervous dread of it is enough, in some patients, to make us wish that we could do without the change altogether. But even when we can neglect the constitutional disturbance excited, can we be always sure that the local movements of the part may not do great injury? Take, for example, such a case as an excision of the knee—even with the best devised splint it is a matter of the greatest difficulty to prevent some degree of motion between the fragments when we are obliged to change the dressing, and often the result is materially influenced by it. Yet, again, frequent changing of the dressing exposes the wound to the danger of infection each time the wound is exposed, and though we may have good grounds for believing that the precautions we adopt to insure asepticity are efficient, still, if we can avoid running the risk, we should do so, provided that we do not lose sight of the great principle involved. What I maintain is that as long as we are obliged, by the nature of the dressings we use, to change those dressings frequently, so long we shall fail to obtain that rest for our wounds which is the groundwork of all systems of treatment.

Now, with carbolic gauze dressings we cannot have absolute rest during the healing process. In large wounds, or wounds from which there is much discharge, the dressings must frequently be removed within twelve hours, and in almost all wounds this must be done the day after the operation. Now, as the greatest amount of discharge comes away during the first twenty-four hours, if a fresh dressing is applied at the end of this time we may be able to leave it on for several days, but it is never safe beyond a week. In many cases a change of dressings is required every second or third day; and the great guide for this is the appearance of moisture at the edges of the gauze. The reasons which oblige us to follow this course are mainly due to two causes—the volatile nature of the antiseptic, and the small power of absorption of the material forming the dressing. We all know the ease and rapidity with which carbolic acid volatilises, and hence the great difficulty we experience in keeping our carbolic dressings in a fit state for use. Oil of eucalyptus has the same fault, and, even in the hands of Sir Joseph Lister himself, disastrous results have followed from the deterioration of the dressings due to this cause. But in corrosive sublimate we have an antiseptic of far greater potency than either carbolic acid or eucalyptol, and one which has the great advantage of not being volatile at ordinary temperatures. Since the investigations of Koch its power is established. A solution of 1 in 300,000 is sufficient to inhibit the development of organisms, while 1 in 20,000 will destroy even the spores of the bacillus anthracis—the most resistant of all germs. Here, then, we have an antiseptic which will retain its power indefinitely in our dressings. When we use it we need not fear that the antiseptic power of the material will deteriorate in a few days or even weeks.

Secondly, we find that one of the faults inherent in carbolic gauze is its small power of absorption. I have made several experiments to test the relative absorbing power of several materials which have been used for surgical dressings. Ordinary unprepared gauze, as we buy it in the shops, contains a certain amount of fatty substances which interfere with its absorbing power. Such gauze will absorb about three times its own weight of fluid; but if we first wash it in hot water with carbonate of soda we remove its fatty constituents, and its absorbing power becomes thereby increased. It will then hold five times or five and a half times its own weight of fluid. A pound of such gauze, for instance, will, when fully saturated, weigh from six to six and a half pounds. But an equally important consideration is that it will absorb this rapidly, say in a few minutes. If a piece of this gauze be thrown on the surface of some water it will in a few seconds sink to the bottom of the vessel. But it is quite otherwise with Lister's carbolised gauze. Being prepared with resin and paraffin, its absorbent power is very sensibly diminished. If thrown on the surface of water it will continue to float for hours and days. If completely submerged in water for twenty-four hours it will absorb one and one-fourth times its own weight of fluid, while in three days the quantity of fluid it will take up will be only two and three-fourths times its own weight. This I have proved to be the case, not only with the carbolic gauze manufactured at the Adelaide Hospital, but also with that which we buy in the shops. Thus we see that carbolic gauze holds a very low place in the scale of absorbing materials. In some places on the Continent, notably in the hospital at Freiburg, in Baden, gauze is used prepared with corrosive sublimate. It is first freed from fat in the way I have described, and it is then charged with corrosive sublimate, previously dissolved in water, to which a small proportion of glycerine has been added. When ready for use the gauze contains about 1 in 500 of corrosive sublimate, and will absorb about five times its weight of fluid. This makes a very nice dressing, but has the disadvantage of sometimes causing much irritation of the skin.

But we have other materials suitable for wound dressings which are far more absorbent even than the gauze freed from its fatty particles. The three most powerful in this respect are absorbent cotton, which will hold fifteen times its own weight; turf moss, which will hold from eight to nine times; and wood-wool, which will absorb about eight and one-fourth times its own weight of fluid. Thus, absorbent cotton stands first on the list. But it has one great disadvantage, it does not possess that eagerness in absorption, as the Germans call it, which is possessed by turf-moss or wood-wool. What I mean by this is that if absorbent cotton be placed directly over a wound, if the quantity of discharge be very great, it will thoroughly saturate that part of the cotton which lies immediately over the point of exit in the wound, and will show itself externally long before the circumferential part of the dressing has even become damp. It is otherwise with the other two materials. Both wood-wool and turf-moss will gradually diffuse the discharge through the dressing until the greater portion of it has become nearly saturated before the external part of the dressing will show evidence of the discharge coming through. I have been in the habit of charging the turf-moss with corrosive sublimate in the strength of 1 in 400; the wood-wool is prepared with the same antiseptic in the strength of $\frac{1}{2}$ per 100. Both of these materials require to be put up into gauze bags previously to use, owing to the difficulty otherwise of applying them over a wound. Lately I have been using a combination of wood-wool and absorbing cotton, devised by Von Bruns, of Tübingen, and I have little hesitation in saying that it is the most convenient and suitable dressing I have hitherto found. It is composed of eight parts of wood-wool and two parts of absorbent cotton, so intimately mixed that it can be cut or torn into the size required and applied directly over the deep dressing without having recourse to gauze bags. Out of numerous trials I made of it I found on two or three occasions that it failed to keep the wound aseptic, and this failure I could attribute to no cause other than that which I now have reason to believe the true one—that the wood-wool wadding was not always reliable. It is prepared like wood-wool itself, with corrosive sublimate, but I have ascertained lately they contain the antiseptic in very different proportions. Wood-wool, as I have stated, contains 1 in 200 of corrosive sublimate, while wood-wool wadding contains only 1 in 3,000. This, I believe, is altogether too weak. I represented this to one of the firm of manufacturers, Messrs. Essinger & Co., and he has undertaken to prepare it for use of the strength of 1 in 1,000, which, I think, would be sufficient for our purposes. This material will absorb from nine to ten times its own weight of fluid, so that in this respect it stands even higher than wood-wool or turf-moss.

Now let me say a few words as to the advantage of using a highly absorbent material as a dressing for wounds. It dries the wound by sucking up from it the discharges as fast as they form, provided that there be a free vent for the discharges to reach the surface. This is well illustrated by the drying effect of blotting paper when applied to the wet surface of a freshly written page. This suction power, if I may so express it, is also used by microscopists to remove an excess of fluid from beneath the cover-glass. In a similar manner an absorbent material sucks up the fluid from a wound, and the more absorbent the material is the more rapidly it takes up the discharge. The importance of this is of the first order. "Moistening and putrefaction, drying and preservation, go together," says Mr. Gamgee, in a recent address[a] (and in this statement we must agree with him). "It is along damp courses and alluvial plains that contagia spread, not on sandy hills; so with wounds—the dry ones heal, the wet ones rot."

[a] Lancet, Oct. 17, 1885, p. 706.

But a highly absorbent material has another advantage. The more fluid it will absorb the longer it will take before it is saturated, and, therefore, the longer it can remain applied to the wound. That is self-evident. But the length of time such a dressing can remain undisturbed depends on other considerations also. In a typical Lister's dressing we know that not only is the wound covered with an impermeable protective, but that the carbolic gauze also is covered by an impervious mackintosh, which effectually excludes the air, and thus prevents the absorbed fluid in the gauze from evaporating. No doubt with carbolic gauze dressings this is absolutely necessary, as otherwise the volatile antiseptic would evaporate and the dressings become inert. But with an antiseptic so stable as corrosive sublimate this is quite unnecessary. We can allow free access of the air to the dressings, and that without fear that the antiseptic properties of the dressings will be materially affected. The dressings give up some of the fluid to the air, and are thus enabled to suck up more from the wound. It would thus appear that if we apply in the first instance sufficient material to prevent the discharge percolating through it within the first twenty-four to forty-eight hours, during which period the discharge is always most abundant, there would be no apparent limit to the length of time a dressing might remain undisturbed. The dressing would thus be a permanent dressing, and one of the most serious causes of unrest in the wound would be abolished. With such a dressing applied in a case where no precaution has been neglected to obtain absolute asepticity in the wound, there is no reason why the dressing should be disturbed at all during the healing process, except it be to remove a drainage-tube or other foreign body from the wound, such as unabsorbable ligatures or sutures. Now, in order to make this method of dry or permanent dressing perfect, we must either use exclusively absorbable ligatures, sutures, and drainage tubes, or we must abolish them altogether. At present we cannot do without ligatures and sutures, but we can use exclusively those made of an absorbent material, and so far we possess nothing more suitable in ordinary cases than properly prepared catgut. And here let me say a word about the catgut. We use almost exclusively at the Adelaide Hospital Sir Joseph Lister's chromised carbolic catgut, prepared by ourselves, according to the directions he himself laid down, and I cannot recall a case in which it proved untrustworthy, either by its remaining unabsorbed in the wound or by its absorbing too rapidly. It is only unreliable when it is improperly prepared. The same may be said of the catgut prepared with corrosive sublimate according to Neuber's method. This has the advantage of being very easily prepared. The raw catgut, which of course must be of good quality, is first washed with soap. It is then placed in sublimate solution (1 in 1,000) for twenty-four hours, after which it is transferred to a 1 in 1,000 solution of corrosive sublimate in alcohol. In this it is kept till required for use, or it may be preserved in oil of juniper. I have several times tried the catgut prepared with sulphurous acid, sometimes called the green catgut, but I have nearly always found that it remains in the wound unabsorbed for an indefinite length of time, and is very liable to keep a sinus open until it is discharged. So much for the ligatures and sutures. By using those made of proper materials we need have no fear as to their behaviour under a permanent dressing. It is otherwise, however, with the drainage tube. Attempts have been made, both by Neuber, of Kiel, and Macewen, of Glasgow, to substitute absorbable drainage tubes for the rubber tubes usually employed. Without entering into particulars, it will, for my present purpose, be sufficient to say that they have not been sufficiently successful to warrant their universal adoption. In many cases they have not been absorbed at all. Hence it was that Neuber devised a system of what may be called "natural drainage," by means of which the drainage tube may be almost entirely abolished. I say almost, for drainage tubes are still of the greatest service in those cases where a large cavity has to be drained, as in empyema, psoas abscess, and the like. But in all ordinary operative wounds, such as removal of tumours, amputations, excisions of joints, even such joints as the knee and the hip, drainage tubes can be dispensed with, not only with safety, but with material advantage. As the details of this method are not generally understood, let me shortly call your attention to the chief points to be observed.

In the first place, there should be absolute hæmostasis. Not only must the larger vessels be secured, but the capillary oozing should be checked. The best plan for this is, I believe, to fill the wound with sponges soaked in hot water containing about 1 in 2,000 of corrosive sublimate, and then drawing the skin together over the sponges to press with the hand till the solution is squeezed out of them. This has the double advantage of stopping oozing and of thoroughly antisepticising the wound. The next point of importance is to insure that there shall be no cavities left in the wound after the skin flaps have been brought together. If a tumour has been removed the space it occupied is abolished by carefully sewing together with catgut the opposing sides of the cavity in which the tumour lay. This is done by beginning at the bottom of the wound and inserting the sutures in successive tiers, until nothing is left but the skin, which is sewed together last. In an amputation, say of the thigh, the periosteum is divided from half an inch to an inch anteriorly to the place where the bone is to be sectioned. This periosteal flap serves as a cover to the divided end of the bone, and its edges are, in the first instance, carefully sutured together. Then the divided muscles are stitched together over this, as much as is possible. Fascia is joined to fascia, and finally skin to skin. In joining the tissues together in this way the deepest stitches are fastened most tightly. We make them a little looser as we progress towards the surface, and finally the skin sutures are the loosest. By this means the discharges are compelled to take a skinward course; in some cases canal-like spaces are left extending from the depth of the wound to a gap left between the skin flaps, or to an artificial hole punched in the skin, through which the discharge can escape. And let it be borne in mind that by this method of deep sutures the discharge from the wound is reduced to a minimum. Where spaces or cavities are left in a wound, there secretion is encouraged to form, and there it will not cease to collect, until by a process of granulation, or by the organisation of a blood clot, the cavity ceases to exist; but when the divided parts are all carefully approximated, so that no such cavities shall exist, then the parts become rapidly agglutinated and immediate union ensues. With this object in view we should always endeavour to suture periosteum to periosteum, muscle to muscle, fascia to fascia, and skin to skin.

In carrying out these details it will appear that in order to carry off any discharge which may form, and to prevent the possibility of any bagging taking place, we must arrange for the exit of the discharge in that situation which each case will show to be most suitable. In some cases this is easy enough, but in others special circumstances must guide us. As I do not intend in this paper to enter into the statistical side of the question, let me give you here a few illustrative instances. In applying the method of dry dressings and buried or deep sutures to the radical cure of hernia, as soon as the wire sutures closing the rings have been secured, and all hæmorrhage has been arrested, I have in all my later cases sown together first the divided layers of fascia. The edges of the skin are then brought together with a continuous suture, but the stitches have been put in obliquely, so as to draw more on one side than on the other, and in this way to leave a pucker at the upper angle of the wound, that is the end furthest removed from the pubis, and I have invariably found this pucker to be ample for the purposes of drainage. The dressings are applied as follows :—A piece of sero-sublimate gauze is wrung out in carbolic solution and applied wet over the wound and over the contiguous skin. Then over this is placed either one or two pads of wood-wool or prepared turf-moss, and the whole is firmly bandaged on with an ordinary calico roller. With the exception of one case in which I used wood-wool wadding, which I believe now to be inefficient from an antiseptic point of view, every case has been left for ten days without the dressings being disturbed. When taken off on the tenth day I have in each case found the wound dry and soundly healed. In most cases the external portions of the catgut sutures have come away on the dressing, the internal portion having been absorbed. The pads were dry and caked. The same result exactly followed in the case of a woman aged thirty-three, the subject of an irreducible femoral epiplocele of the right side. In her case the sac and omentum were ligatured *en masse* close to the crural ring, and were then excised.

I sewed together the external and internal walls of the crural canal, stitching them also to the pedicle. No special space or pucker was left for drainage, and when the first dressing was removed the wound was soundly healed. In none of these cases did any sinus remain to cause after-trouble, as so often results when a drainage-tube has been used. On the 2nd of April, last, I excised the right lobe of the thyroid gland from a lady aged thirty-four, who had been admitted into a pay ward in the Adelaide Hospital. A great portion of the lobe had been transformed into a large cyst. All the deep parts were carefully sutured together, so as to leave no spaces in the neck. A small pucker was left in the lower end of the skin wound. The wound was dressed with sero-sublimate gauze and turf-moss pads. The wound was perfectly healed on the tenth day, when the dressings were removed for the first and last time. The patient left hospital on the thirteenth day. In operating for cancer in the breast I have been in the habit for the past few years of completely cleaning out the axilla in all cases, as recommended by Kocher, of Berne. This proceeding, of course, renders necessary a much larger wound than when the breast alone is removed. In applying the principle of deep sutures I begin at the axilla, so as to obliterate the cavity necessarily left by the removal of all the lymphatic glands from the part. Then the fascia and any divided muscles are stitched together, and finally the skin, a gap being left at about the junction of the upper third with the lower two-thirds for drainage; that is, that in this situation about an inch or an inch and a half of the skin wound is not sutured. I have almost uniformly found the healing process completed under a simple dressing, when this was removed on the tenth or fourteenth day; in a few cases a second dressing was required. On the 27th of September last I removed the entire left breast for scirrhus from a lady in Waterford, aged seventy-one, assisted by my late colleague, Mr. Warren, and by Dr. Martin, of Portlaw, and Dr. Findlater. The axilla was cleaned out at the same time. Deep sutures were used; no drainage-tube was inserted, and the wound was amply surrounded with the dry dressings. I did not see her again for a fortnight, for as a record of the temperature was sent to me daily, and this remained normal throughout, I was satisfied that the wound would run an aseptic course. On the fifteenth day the dressings were removed for the first time, and the wound was found soundly healed in its whole extent.

In certain cases, however, it is not sufficient merely to leave gaps between the edges of the skin to carry off the discharges, nor even do holes punched in the skin at the most depending parts effect all we can desire. In such cases we must have recourse to other methods for securing ample drainage. Take, for example, a case of excision of the knee-joint. Here we may expect a large amount of discharge, not only during the first twenty-four hours, but for several days. In such cases I have adopted a method of securing free drainage without the use of drainage tubes, for which we are indebted to Neuber of Kiel. The method consists in this:—The incision to expose the joint is the ordinary transverse incision below the patella. The operation having been completed, before bringing the parts together the extremity of the incision at either side is made bifid, so as to enclose between them a narrow tongue of skin. The base of this tongue is below, the apex above. Each of these tongues is then turned inwards, so as almost to touch each other in the middle of the limb, behind the bones. The apices are secured in this position by means of a catgut suture. When the parts have been brought into position and all the suturing finished, we have on each side a channel formed, which runs from the deepest part of the wound to the outside. The dry dressings are then applied, and I prefer to have the splint embraced in them. Such a dressing can be left on for weeks. On the 4th of May last I excised the knee-joint of a boy, aged nine, at the Surgical Home in Rathmines, in the presence of Mr. J. K. Barton, Mr. Warren, Mr. Swan and Dr. Swaine. The method for drainage described above was employed, the bones were sutured with strong silver wire, deep sutures were inserted and the splint and limb enveloped in dry dressings. These dressings remained undisturbed till the twenty-first day, and when removed the wound was found firmly healed in its whole extent, except for two small granulating spots about the size each of a sixpence, which showed where the skin had been turned in. They were quite superficial and soon healed. I saw the boy last on the 30th of October; he could then walk well on the limb without support of any kind. There was no tenderness on pressure anywhere, even over the course of the two silver sutures in the bones, which have never been removed.

On the 2nd of June last I excised the hip of a little girl, aged six, in the Adelaide Hospital. I found the head of the bone necrosed and lying loose in the cavity of the joint, which was filled with pus. About two-thirds of the neck of the femur was excised, and the cancellous tissue in the great trochanter and adjacent part of the shaft thoroughly scraped out. The acetabulum was also scraped clean. The lesser trochanter was also necrosed and lay in a small pond of pus. Deep sutures were also employed in this case, but owing to the large cavity which it was impossible to obliterate, I inserted two drainage tubes and left besides a wedge-shaped opening between them. Besides applying the dry dressings the wound was filled with iodoform. The case was dressed only on the sixth day, when the drainage tubes were taken out, and once again on the eighteenth day. When this dressing was removed, the healing process was found to have been completed—that is, that in this case of excision of the hip, perfect healing was obtained under three dressings. In September last my colleague, Mr. J. H. Scott, obtained a similar result. I have cited these cases, not because they are exceptional in our experience of what can be effected by the method of dry dressing when grafted on to the great principle of antiseptic surgery, but because they illustrate the different methods of carrying out this practice.

ON THE SURGERY OF THE LATE EXPEDITION TO SUAKIN.

By R. F. TOBIN, F.R.C.S.
Surgeon, St. Vincent's Hospital, Dublin ; late Assistant Professor of Surgery, Army Medical School, Netley.

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During the late military operations at Suakin, it was my good fortune to occupy the position of Field-Surgeon ; and, although I have nothing very new or important to communicate, it may be that it will interest you to hear from an eye-witness the surgical history of that brief campaign. The appointment styled Field-Surgeon is one new in our army ; it is one that has not yet been recognised by the supreme authorities, and the duties of which are not defined in the army medical regulations. [The subject is treated of in Mr. Longmore's work on "Gun-shot Injuries," pp. 438, 439.] Let me, therefore, say a few words about it. Perhaps I shall best convey to you the nature of the appointment by pointing out the conditions that call for its existence. If I do so by uttering a truism in connection with an acknowledged fact, I hope I shall offend no one. That skill in the practice of operative surgery, especially of antiseptic surgery, can only be obtained by practice, is a truism. That in times of peace the ordinary duties of an army medical officer give few opportunities for such practice, is an acknowledged fact. When, therefore, on the breaking out of war, medical officers sent out from various stations are called upon to take charge of wounded, it seems right to put at their disposal, for the purpose of consultation, &c., some one whose hand, to use a familiar phrase, is "in" at surgery. Men skilled in surgery will be glad of such an assistant ; men whose training has been limited to operating on the dead body will be steadied and encouraged by the presence of such a one in their first operations on the living. Men, on the other hand, who from any cause are reluctant to operate, will ask him to undertake for them whatever operations fall to their lot.

The operating tent at Suakin was a large double pole tent, of a pattern in use in India. To the poles were hung irrigators, and to the walls all round were stitched calico bags, filled with ready-made Lister's dressings of various sizes. salicylic, boracic, and iodoform wool, lint, bandages and other surgical appliances. The floor of the tent was kept sprinkled with carbolic acid solution.

There were carried into the base hospital at Suakin, 129 wounded non-commissioned officers and men. The officers were, as a rule, taken direct to the hospital ships, and, as I did not see much of them, I do not include them in my statistics or remarks. The official returns classify the 129 wounded as follows:—

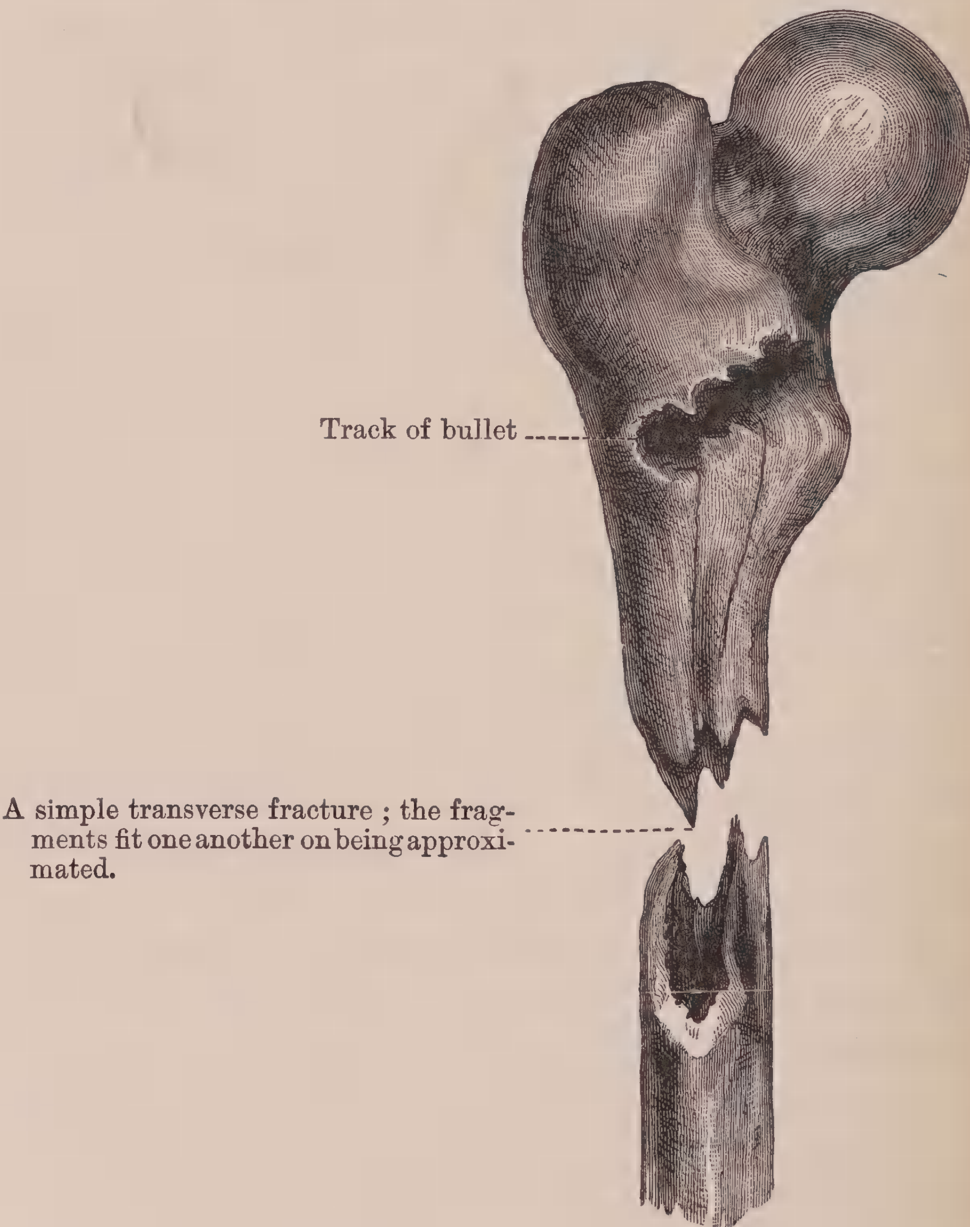
Regions of the Body Wounded	Admissions with Wounds		
	Total Wounded	Projectile or Weapon by which Wounds were Inflicted	
		Rifle-shot	Sword or Spear
Wounds of the Head - - -	6	2	4
" " Face - - -	6	4	2
" " Neck - - -	5	3	2
" " Chest - - -	11	8	3
" " Abdomen - -	3	2	1
" " Back and Spine -	13	9	4
" " Urinary Organs -	2	2	—
" " Upper Extremity -	32	20	12
" " Lower Extremity -	51	43	8
Total - - -	129	93	36

N.B.—The Royal Marines are included in this return.

Of the 129 who were wounded, 124 recovered. The five deaths occurred as follows:—One from a perforating wound of lung and liver, one from a similar wound of abdomen, with perforation of the intestines; three from wounds of the lower extremities. Two of these latter wounds were fractures of the femur in the neighbourhood of the hip-joint—a class of wound which has never, under the most favourable conditions, furnished a less mortality than 70 per cent., and one of the two was complicated with other injuries that rendered it necessarily fatal. I shall have occasion to refer to both these cases later on, as they presented points of special interest. The third patient, who died from a wound of the lower extremities, had received a long superficial spear wound on the inner aspect of the thigh. He was brought into hospital about six hours after the receipt of the injury. After the wound had been well irrigated with a five per cent. solution of carbolic acid, it was closed, except at its most dependent point. Although the wound seemed to do well, the patient died on the third day, with symptoms of acute septicæmia. No doubt the spear which wounded him had been smeared with some putrid matter, absorption of which had taken place before he reached hospital.

Let me ask you to criticise these statistics narrowly. I ask you to do so in the interests of our profession, and in the interest of the British soldier. To turn out cured in England a soldier wounded on the plains beyond Suakin is, I need scarcely remind you, a very complex undertaking, of which the immediate treatment of the wound is but a part. It is an undertaking requiring much forethought on the part of the administrative authorities, and skilful carrying out of their arrangements by every branch of the executive. It is, moreover, a highly expensive undertaking. The British tax-payer, however, appears not to mind expense, as long as he gets value for his money. It is to satisfy him on this point, and to encourage him to further liberal outlay on the army medical department and its equipment, that I have brought these statistics before you. Taking into account the nature of the climate, the difficulties of conveyance by land and sea, the septic conditions implied by flies, that made black by their presence every spot where they could obtain food, I think you will agree with me that to save 97 per cent. of wounded was a satisfactory performance. It is right, also, to point out that, excepting the case of septic spear-wound just referred to, no patient died who had not received a wound of an almost mortal character, while some apparently hopeless cases made excellent recoveries.

Let me first tell you of the two cases of fractured femur that did not recover. One was a private of the 5th Lancers. There was an alarm in their camp on the night of their arrival, and the bullet from the carbine of a comrade struck the man in question as he was in the act of getting up. The bullet entered the right buttock, at



a point six inches behind the anterior superior spine of the ilium, passed through the rectum one inch and a half above the sphincter, and then, fracturing the left femur, passed out on the outer aspect of the thigh. There was much hæmorrhage into the rectum, and collapse, from which the patient never rallied. On examining the wound after death, the following most strange condition was found. I believe it is unique in the history of gunshot fractures. The bone was found to have been fractured two inches below the point where it had been struck. The specimen which I hand round will explain the occurrence better than any words of mine.—(*See* figure.) I need only add that there was no communication between the wound and the fracture, which was, therefore, a simple fracture of the femur from a rifle-bullet.

The other case of fracture that terminated fatally was in the same neighbourhood. You know that the treatment of such fractures is one of the most vexed and unsatisfactory questions in surgery. Amputation has never yielded better results than is represented by a mortality of 84 per cent.; while a mortality of 71 per cent. has attended non-interference. The shock of the operation appears to have been the fatal factor under the first plan of treatment, while under the second it has been the prolonged suppuration attending the separation of necrosed fragments. In the case in question there was a comminuted fracture implicating about two inches of the bone below the lesser trochanter. The wound of entrance was on the outer aspect of the thigh, that of exit on the inner. I enlarged the former wound, removed the central fragments, made smooth in a transverse direction, with a saw, the ends of the upper and lower fragments, and secured them together by means of a strong silver wire passed through holes drilled in the usual way. The shock, however, of even this much interference was too much for the patient. He died twelve hours after the operation.

Notwithstanding its failure in the case just mentioned, this plan of treatment is one that commends itself to me in cases where there is a comminuted gunshot fracture of the femur limited to a few inches of the shaft. Much to urge in its favour occurs to me; but there is this against it, that it is sometimes impossible to ascertain that the fracture is one suitable for such an operation without what I shall presently show to be a dangerous amount of examination; and I would, therefore, limit it to wounds occasioned by bullets known to inflict fractures of the kind indicated. It would also, I think, yield better results as a secondary than as a primary operation.

The other gunshot wounds of the extremities, of which there were in all sixty-three, were interesting, chiefly in illustrating the difference between wounds inflicted by the Martini-Henry and the Remington rifles. The shape, hardness, weight, and velocity of a bullet materially influence the kind of wound it produces, and the projectiles in question differ in each of these particulars. The effects of velocity were further accentuated by the fact that, whereas the bullets of the Martini-Henry that inflicted wounds were nearly all fired close at hand, those of the Remington came from long distances. While at Netley I was often struck, on the return of soldiers from the many small wars we have been carrying on during the last six years, by the marvellously little amount of damage occasionally inflicted on joints that had evidently been traversed by bullets. The shape and velocity of the bullet, had it been ascertainable, would, no doubt, have explained the occurrence. In papers read by Professor Longmore and Surgeon Kirker, R.N., before the International Medical Congress in 1881, detailing experiments made by them, will be found most interesting information on this subject. The Martini-Henry bullet caused much fissuring of the bones it struck, and a large lacerated wound of exit, through which muscle and fragments of bone protruded. The appearance and condition of these wounds at Suakin fully verified the experiments of Professor Longmore just referred to. In treatment they presented this difficulty, that if you attempted to interfere with any fragments at all, you loosened the connection of fragments you would have preferred to have left undisturbed. In fact, a surgeon who had not a great respect for fragments would not save a single bone struck by a Martini-Henry bullet at high velocity. He would lay hold of a partially protruding fragment to see if it were detached, and, examining it, he would so loosen it that he would consider its removal necessary. In removing it he would disturb some other fragments to which the first had attachments, and which in its turn would come in for examination and removal. So proceeding, a hopeless gap is easily established, and amputation rendered necessary. I speak from experience, gained by mistakes which I have made, and which I am not afraid to acknowledge, if so doing will prevent their repetition. Whoever would apply conservation to bones broken by such weapons as the one to which I am referring must be content to do so without ascertaining the condition of the bone he would conserve. He must take it for granted that he is dealing with a number of long fissured fragments held together by periosteum; he must gently push back into their places such of them as protrude, and arrange as best he can for the drainage and "asepticity" of the wound, and the securing of the injured parts in a position of immobility. Excuse me if I appear to you to speak dogmatically. It is the first time, as far as I know, that anyone who has verified, by observations made on active service, the important experiments of Professor Longmore already referred to, has spoken on this subject. My observations convince me that experiments on the dead body afford a perfectly reliable means of ascertaining the effect of projectiles on living tissues. And as there is no doubt that each weapon inflicts its own peculiar wound, a wound having often not only special characters but also requiring a special treatment and a special prognosis, it seems highly desirable that further experiments should be carried out, till the characters of the wounds produced by every weapon of warfare have been accurately and fully ascertained.

The wounds produced by the Remington rifle were altogether different from those of the Martini-Henry. As in no case did death or necessity for removal of a limb follow such wounds, I can only describe their external appearances. Their leading feature was the slight disturbance, both of soft and of hard parts, caused by the bullet. Its track was, as a rule, so closed up by the coming together of parts which it had separated without destroying, that union was easy and rapid. Let me, in illustration, refer to two cases of gunshot wound of the elbow. Private J., Shropshire

Regiment, was wounded on March 13th. A Martini-Henry bullet completely shattered the lower two inches and a half of the humerus, and drove many of the fragments through a large lacerated wound of exit situated over the internal condyle. In its course it divided the ulnar nerve. The joint was resected, and the ends of the divided nerve stitched together. The wound healed; but so extensive had been the injury both to the bone and the soft parts that a useless limb, which was subsequently amputated, resulted.

Private C. B., of the Berkshire Regiment, was wounded on April 2nd, in the zereba at Tamaai. The bullet, a Remington, entered between the olecranon and internal condyle, and, passing through the olecranon, lodged underneath the skin on the external aspect of the arm. There was almost no loss or displacement of tissue in the wound, which appeared to have closed round the bullet as it passed. The arm was secured in a straight position, to keep the olecranon in its place, and a Lister's dressing applied. The wound healed kindly, with full movement of the joint, and without evil results of any kind.

These two cases give a very good idea of how different are the wounds produced by the two weapons of which I have been speaking.

Wounds of the head furnished some of our most serious cases, and all made good recoveries. I will refer to two of them.

Private F., Berkshire Regiment, was wounded on March 11th, during a night attack on the camp of the Ordnance Department. He received three sabre-wounds of the head, and five spear-wounds on other parts of the body. One of the former was situated on the right side of the head where the hair meets the forehead, and was caused by two sword-cuts, which were so placed as to include between them a piece of the frontal bone, three by two inches, which lay loose on the wound, and a corresponding portion of the underlying brain, which was deeply incised, and bulged upwards in the wound. The detached piece of bone was removed; the prominent brain-tumour was interfered with as little as possible, and all his wounds were dressed antiseptically. He made a good recovery, without impairment of any of the senses.

Corporal J. B., 1st Coldstreams, was struck on March 24th by a bullet, which entered at the internal canthus of the left eye and came out $1\frac{1}{2}$ inches above the right ear, in a line with its anterior boundary. The wound of exit was a lacerated one, through which brain-substance and fragments of bone protruded. The right eyelid was closed and much distended, not, however, with the injured eyeball, as was first supposed, but with brain-substance; the eyeball must have dropped out at the time of the injury. To ensure drainage, the orbicularis muscle was cut across at the external canthus, and the wound of exit was enlarged with the same object. This patient also made a good recovery, and when I saw him at Netley on my return he was up and about, and in full possession of his faculties.

To refer to wounds of other organs would, I fear, carry me beyond the limits of time fixed for the reading of a paper. I will, therefore, conclude with a few general remarks.

The anæsthetic most commonly used was ether. Although given most freely, it never produced any dangerous symptoms, whereas chloroform, though used in but a few cases, had very nearly to be credited with a fatal result. Ether, although it would take up more room, should, I think, be the recognised anæsthetic of the services.

When amputation was considered necessary, it was usually done by prolonging the incision made for the examination of the wound till uninjured bone was reached, and, using this as a lateral incision, a skin-flap, a long anterior one when possible, was cut by incision; the second flap was also generally cut by incision, and was made to fit to whatever shape circumstances had caused the first one to assume.

To ensure drainage, the drainage-tubes were, as a rule, stitched to the edges of the wound. This is a very necessary precaution where wounded men pass from one medical officer to another, and where, therefore, tubes that have been inserted may escape notice.

Another useful detail to attend to is the giving to the patient a duplicate of whatever dressing may have been originally applied, wrapped, of course, in a close envelope to prevent its being soiled.

On active service it is impossible to say when, where, or under what circumstances the second dressing of a wound may take place, and a precaution such as I have suggested ensures a continuance of one treatment. I cannot refer to-night to the antiseptics used, but it may be well to state that ten yards of gauze and one pound of antiseptic wool was the average expenditure per wounded man.

This concludes what I have to say to-night of the surgery of the Suakin expedition. The etiquette of the service has prevented me from referring, in the course of my remarks, to the work done by particular medical officers. There was, however, associated with us one of whom, as he was not in the service, I may speak, and without the mention of whose name my paper would not be complete. He is one whom the world recognises as one of the pioneers of surgery—one who left a high position and came out to us to make himself useful, and who succeeded in doing so most perfectly, whether he was advancing with the bearer-companies to pick up wounded, assisting us with his advice in consultation, or giving at an operation the help that means doing the operation without holding the knife. He did so, moreover, with a suppression of self that asserted itself in spite of him, and won the admiration of all. A hundred grateful and pleasing reminiscences crowd on me as I conclude this paper with the name of Professor Ogston, of Aberdeen.

ON A METHOD OF MAKING AN ARTIFICIAL OPENING IN THE MEMBRANA TYMPANI, AND OF KEEPING IT OPEN FOR A CONSIDERABLE PERIOD, AND ON ITS THERAPEUTIC INFLUENCE.

By WILLIAM M'KEOWN, M.D., M.Ch.;
Surgeon, Ulster Eye, Ear, and Throat Hospital, Belfast.

[Read in the Surgical Section, January 22, 1886.]

Various questions in aural as in other departments of surgery urgently require reconsideration. Surgeons, like other persons, are too prone to run in grooves, and often for a very long period. If anything in surgery or medicine is unsatisfactory, we should examine the data on which conclusions are based, and put aside dicta, no matter by what authorities supported, and act on our own judgment, after due deliberation.

Whilst we have in recent years, undoubtedly, made advances of an important character in the knowledge and treatment of some of the diseases of the ear, yet the surgeon has only to look to his own experience, and to take up any of the standard authorities, to satisfy himself that there is room for the exercise of the greatest diligence, and scope for the greatest talent, in this department of surgery.

I need only instance a few of the conditions of every-day occurrence to bear me out in what I say. Take, for example, the so-called dry catarrh of the middle ear; rigidity of the ossicles; relaxation and tightening of the membrana tympani; obstruction of the Eustachian tube; long-continued tension of membrana tympani—what is really the verdict regarding many methods of treatment? Incisions of the membrane; artificial perforations of the membrane, whether by knife, cautery, or caustic; section of the tendon of the tensor tympani; section of the posterior fold of the membrane; medicated injections within the tympanum; passing of bougies in the Eustachian tube—are all regarded as being only occasionally serviceable. Several of the methods are credited with not only not being beneficial, but as being positively prejudical in the long run in some cases.

I do not purpose going into questions so extensive, but shall confine myself to incisions and perforations of the membrane—what has been the practice, and what it should be.

We all know of the temporary results of puncture of the membrane by Sir Astley Cooper, and of his abandonment of that operation. Now, what really could have been expected from such an operation in any case when vascular, and nervous, muscular and connective tissue changes had been going on for months or years? What could relief of tension for a few days effect in such cases? Analogies will occur to you all in various departments of medicine and surgery. The punctures could have been of no service except in recent cases. What good could such a puncture have effected in a thickened membrane? The thickened membrane healed at once, and the original condition for which the operation was performed returned.

Plain incisions are little better than punctures when not used for injection through the tympanic cavity. How an incision healing in two or three days could relax a tightened membrane it is impossible to see. Plain incisions do not relax parts, as far as we have ever seen, unless the parts are widely stretched during healing, and certainly a plain linear incision in the membrane could not do this; but in relaxation of the membrane, I think—and this is the only thing we can say of the curative influence of such plain incisions—they do tighten up the membrane more or less.

The profession has been for a long time exercised with the question of how to make and keep a perforation permanently open, and nature has for so far persistently refused to allow success to any procedure hitherto employed. The cutting out of a piece of the membrane, the destruction by electric cautery or caustics, and the introduction of bougies and eyelets, have been equally unavailing; and the consensus of the profession seems to be that those efforts, whilst fruitless as regards the immediate object in view, have often been injurious to the hearing. Now, whether we should, in the face of these warnings, still pursue the object by these or similar methods, is a matter each one can determine for himself. No doubt, these methods have led to more or less congestion of the parts, and thus frustrated the aim of the surgeon.

But suppose we lay aside this effort to make and keep an artificial opening permanent, and keep before us the curative influence of an opening—large, and made with the least injury to the parts—may we not hope, in many cases, for a permanent improvement, though the wound should heal? This leads too to another question. Why should we reserve the operation for cases that have become desperate? In throwing out this suggestion I cannot ignore the statements made about attempts at making an artificial opening permanent, leading to diminution of the hearing in the long run; and I wish now to state, in the most emphatic manner, that the making of an incision, large or small, does not diminish the hearing, but that the adjuncts to the incision, when leading to irritation, may do so. I cannot say how often I have incised the membrane in various ways, but so far as I have been able to follow patients—and this is a difficult matter with extern patients of an hospital—I do not know that incision has ever diminished the hearing.

I have endeavoured, by several surgical expedients, to make permanent openings. I may indicate them: I have blistered a part of the drum of the ear, removed the elevated epithelial covering, made a triangular section of the blistered membrane, turning it back, hoping that the surfaces would adhere under pressure, but my hopes were disappointed; gradually the reflected flap was drawn back to its old position and closed the wound. I then tried another method. I made a triangular flap, and near the base of the triangle, and parallel to the base, made a small incision, then by small probe pushed the apex of the reverted flap through the incision. I thought that at the points where the cut surfaces of the flap came in contact with the second incision I would have union, but although I did this in several instances the flap, under the influence of the healing process, was gradually drawn back, after a time more or less long, to its original position. I kept the parts in position by a pellet of wool, with some antiseptic ointment, but still the force of nature was too great. Then finding my efforts—which, before trying, I thought promised so well—to make a permanent opening fail, where I wished to give prolonged relief of tension, or to keep the membrane and ossicles free from violent and sudden fluctuation, and to expose the cavity and see the state it was in, I have simply made a triangular incision in front of or behind the handle of the malleus—or, indeed, in other positions if there were any special indications—and turned the flap down. The blood exuded is the simplest and best tenacious agent. The aperture so made remains open, according to the size of the flap, from two weeks to two months and upwards.

The making of the flap is not a very difficult matter if certain plain indications be followed. The surgeon should have good light, and illuminate the membrane by a frontal mirror. I like good diffuse daylight, but if I have not that I use artificial light. I use as large a speculum as the ear will admit. I do not use any of the special knives constructed for incision. Here are the knives I use. They are made in a very simple manner. One is a Graefe's knife, ground down in blade (except for about a line and a half at the point) and handle, so that I can see well what I am doing, neither blade nor handle obstructing my view. The other is a blunt-pointed knife—actually a tear-passage knife—likewise ground down in the handle, and this I use if I have not at first trial united the two sides of the triangle. I always cut from below upwards, the slope of the membrane inwards favouring that method. If section have been completely finished, the flap usually falls down outside the membrane, and insufflation aids this; but if by chance it should fall inside, that is not a matter of any importance. I then protect the ear and let it alone.

I have had some very interesting experiences in cases of section of the membrane not at all in harmony with prevalent ideas. I will indicate some. That not hearing a watch or speech for a long period, even with total obstruction of Eustachian tube, is against recovery, or that it indicates a condition of the nerve past amendment, is not to be entertained. The patient is not to be dismissed as incurable. Make a section or sections of the membrane before you give an opinion. I had a patient, a girl, at hospital with inherited syphilis; the membranes were collapsed; by catheter or insufflation no air passed. She could not hear the watch anywhere. She could not, for six months, understand a word said by her friends. I made incisions in the membrane, and the same night she could hear and answer questions. A lad who had been going deaf for some time, and who could not hear a word for a week, the day after incision conversed with me quite well, and recovered perfect hearing. I might mention other cases, but it would be too tedious. The lesson is—not to dismiss cases apparently hopeless according to ordinary tests without giving a trial, particularly if there is any change in the membrane or Eustachian tube.

It has been proposed to make a section with the view of examining the condition of the ossicles. It is a sensible, feasible, and with precautions, a safe expedient. One of the most successful cases I ever had was a girl, aged eighteen, who could hear a loud voice close to the ear, and could hear the watch in contact with the bone. The drum was white and thickened. I made a flap with great difficulty. Besides the thickening of the membrane, I found a very thickened and granular condition of the lining membrane of the inner wall of the tympani cavity. I used absolute alcohol. She came to hear well a moderate voice at 25 feet. I saw her a year afterwards and the improvement continued.

As to the hearing of the watch being—as laid down by Sir Astley Cooper, and insisted by later writers of eminence—a condition precedent to improvement, from what I have said it will be understood that I do not assent. The hearing of the watch is a question of degree simply and of quality of sound. The non-hearing is not an indication of unimprovable deafness at all. A person who does not hear the watch may come to do so. A patient I treated by triangular incision, who could not hear previously the watch with either ear, came to hear it quite well; and as to the voice, from hearing at 2 feet came to hear at 18 feet when perforations were open; and when they closed, though the hearing diminished somewhat, the improvement that remained was very great.

The perforation of the membrane for purposes of diagnosis is often referred to and practised, but I think sometimes it would be serviceable in cases not thought of. Recently I had under my care a gentleman who had been under treatment on two previous occasions for catarrh of the Eustachian tubes and old-standing affection of one ear. One ear having a very relaxed membrane at the lower part could only hear a distinct voice at a foot, the other at three or four feet. I made a flap of membrane in the very bad ear, but without notable immediate improvement. Afterwards there was a change for the better, but the noises in the latter ear were very changeable and annoying, and the hearing of the good ear fluctuated greatly. Mastication annoyed him particularly. He begged of me to perforate the good ear. I did so. Whilst in my consulting room on several occasions violent fluctuations took place. The membrane being largely open it could not be from any sudden closure of Eustachian tube. It must have been from sudden action of the tensor tympani, and I thought it probably occasioned by an irritative condition of the Eustachian tube. I injected into the naso-pharyngeal space several times a 30 gr. solution of chloride of zinc. All the fluctuations ceased as regards the good ear. The wound healed eventually. As to the bad ear, the condition at various times indicated clearly the care necessary in giving a prognosis and the little things to be heeded. The perforation through the relaxed part kept open for several weeks. On one occasion when he heard badly I put a probe through the perforation whilst the speculum was in the ear, and from hearing at 4 feet he came to hear at 25 feet. Thinking that the contact of the probe with the speculum, the membrane, and the inner bony wall of drum caused the improvement, I removed the probe—still he heard well; then the speculum—still he heard well. What was the cause of the change? Did he hear because of the bringing together of loosened ossicles? I had tried a pellet of wool against the membrane, but it effected nothing. Taking everything into account, I concluded that the change was through the tensor tympani, and that an irritative condition of the Eustachian tube was the most important factor on both ears, and so acted. I may add that the triangular flap of the bad ear, after remaining open for several weeks, healed, and that after healing the tension was greatly increased, and that the last time I saw him he heard a moderate voice about 20 feet.

I do not wish you to think that the triangular incision is altogether without risk. The patients must take care of themselves. I have a soldier under my care with whom I have had some trouble. In one ear I had made a section; all went well. In the other ear I made a section. Afterwards, within a couple of days, he exposed himself late at night, and he has been attacked with a most severe purulent inflammation of the middle ear; but he got quite well of this.

In an old man, whose deafness had been getting steadily worse for 15 years, and in whom perforations remained open for six weeks, great improvement took place, both in the hearing and the noises, and steadily progressed after the perforations closed. Mere age and long duration of an affection are no bar to improvement.

I saw yesterday a lad of nineteen, in whom a perforation remained open for two months, with improvement of hearing from 1 foot to 4 feet; and in the other ear for one month, with improvement from 3 feet up to 16 feet. Other previous treatment had been unavailing.

I cannot at all give statistics, from the fact that many of the patients cannot be followed. I do not know of any case in which the operation has diminished the hearing. I know of some in which no improvement has taken place, of several in which quite unexpected amelioration took place, and of many in which substantial and permanent improvement has been effected.

I would note here an important fact, and one which puts strongly before us the great therapeutic influence of the operation I have described. In cases where, on insufflation, no air seems to enter at all, or only in very slight current, after the incision, in a variable time air can usually be blown freely through the ear either by the Valsalvian method or by Politzer's. It demonstrates clearly the great influence on local congestions about the Eustachian tube. And we may infer, when this takes place in the Eustachian tube, the same will take place in the cavity of the drum and other parts. A burden is, as it were, lifted off the ear, the constant teasing and irritation are removed, and nervous and vascular equilibrium is restored, not only in the middle but internal ear.

My object then, you will observe, is to use incision, not for merely mechanical purposes, but to use it as a true therapeutic agent.

PHARYNGOCELE AND DILATATION OF PHARYNX, WITH EXISTING DIVERTICULUM AT LOWER PORTION OF PHARYNX LYING POSTERIOR TO THE ŒSOPHAGUS, CURED BY PHARYNGOTOMY, BEING THE FIRST CASE OF THE KIND RECORDED.

By W. I. WHEELER, M.D. Univ. Dubl.; F.R.C.S.;
Past President, R.C.S.; Surgeon to the City of Dublin Hospital.

[Read in the Surgical Section, February 19, 1886.]

Having already had the pleasure of bringing before the Surgical Society of Ireland the operation of pharnygotomy for the removal of a foreign body, and a second case before the Academy of Medicine for the removal of a tumour of the pharnyx, both successfully treated by me, I have now the gratification of detailing another most interesting and instructive case, the first of the kind recorded:—

Case.—Capt. E. E., aged fifty-seven; antecedent history averagely good. During recent years he suffered from frequently recurring attacks of tonsillitis and bronchitis (laryngitis?), whereupon the submaxillary glands became temporarily enlarged and painful. In 1880 he had had a severe attack of erysipelas, and in 1884 he suffered from hepatic dropsy. His voice was of a stentorian character, and during his military career he prided himself on being able, by his own word of command, to drill an entire brigade. Subsequently to his retirement from the service he still frequently exerted his voice to its extreme limit. During the autumn of 1884 he first complained of the present affection, which caused him the following train of symptoms:—He was unable to remain lying down for any length of time in the same position without feeling a sensation of choking, as though his throat were impeded by some large mass. So great did the distress become that he was obliged to leave his bed and sit in a chair. In rising from the horizontal to the perpendicular position he was subject to intense fits of coughing, in which (using his own phraseology) "his eyes were starting out of his head and his brain bursting." During a night he would expectorate about a saucerful of "clear, sticky fluid, with occasional froth." Talking was always followed by coughing, and was very indistinct, the sound resembling that of the voice of one whose vocal chords had been eroded, so much so that at times he became unintelligible, and, if the effort to speak was to much extent persisted in, the result became a hoarse whisper. To swallow food, except in small particles, was extremely difficult, and even then they stuck in his throat only to be subsequently coughed up, and helped into his mouth by manual pressure, tasting exactly as when swallowed. Fluids also were partaken of with occasionally the same result. Capt. E. observed a small tumour on the right side in the submaxillary space in front of the sterno-mastoid, pushing forward, which, though occasionally diminishing in size, always to some extent remained. It was at about this period that he sought advice—the tumour was pronounced to be a cyst and was treated accordingly. However, it gradually increased, growing forwards and upwards over the ramus of the jaw and extending to below the thyroid cartilage. At times, audibly to himself, air would escape with a gurgling sound into the pharynx, and the tumour, when percussed, had a tympanitic sound, such as that produced by percussing a coil of the large intestine; its anterior edge was very thin and appeared to be next the skin; the entire tumour was freely movable on the deep tissues, as was the skin covering it. Flexing the neck and drawing the tumour backwards produced a marked improvement in the voice, which relapsed into its normal hoarseness upon the tumour being allowed to resume its natural position. The first time I saw Capt. E. was in consultation with Dr. Mayne on the 9th of January, 1885; the tumour then appeared as already described. On June 8th of the same year I again saw him; during the interval the tumour had increased somewhat in size. I passed a sound into the dilated upper portion and examined him by electric light with the laryngoscope, which showed the larynx to be in an extreme state of congestion, and the right vocal chord appeared paralysed. On the 13th June he came under treatment in preparation for an operation advised by me, and which, on the 30th June, was undertaken in the following manner:—The patient was placed upon a suitable table and anæsthetised by Dr. Duffey; I was further assisted by Drs. Mayne, Harley, and Middleton. I may here pay a trifling tribute to the revered memory of my much-esteemed and greatly-lamented friend, the late Mr. Tufnell, by testifying to the excellent assistance so capably rendered to me by him upon this occasion as well as upon many others. I made an incision from approximately the centre of the ramus of the lower jaw, extending downwards and outwards in the line of the platysma to well below the thyroid cartilage. The platysma, along with the other muscular tissue, was carefully dissected off the tumour, which was then seen to be very thin; upon opening its most prominent part a gush of air escaped, and the tumour immediately fell back and collapsed. The opening was cautiously enlarged and then retracted, by which means the pharyngeal cavity was exposed. The most prominent portion of the tumour was precisely upon the site of the sinus pyriformis, hence at this juncture of the operation the arytenoid cartilages and vocal chords became visible, also the epiglottis and epiglottic folds. The action of the larynx was beautiful to look at, as one had the opportunity of seeing now by the unaided eye the contraction of the chords each time the patient uttered any sound; the approximation of the cartilages, and the opening and closing of the rima glottidis, revealed a mechanism and routine as perfect as it was interesting, beautiful, and instructive. The foramen through which the most prominent part of the tumour bulged appeared to have the following boundaries:—Above, superior constrictor; inferior, middle constrictor; posterior, stylo-pharyngeus; and anterior, palato-pharyngeus or salpingo-pharyngeus. The mucous membrane of the pharynx was continuous with that of the tumour, which at this point might be styled aneurysmal herniosa of the pharynx.

Simultaneously with opening the tumour the patient's voice returned almost to its normal condition. I cut a large piece out of the pharynx extending from the ramus of the jaw to the lowest part of the dilation, at which point I suddenly discovered the existence of a pouch filled with particles of food situated at the inferior portion of the pharynx and lying posteriorly to the œsophagus, measuring approximately the dimensions of a walnut, for the purpose of eradicating which it was necessary to draw up the pharynx at its lowest point where it joins the œsophagus. This having been accomplished, the pharynx was now brought together by sutures; subsequently the muscular tissues and platysma; finally the skin, but leaving a few openings for drainage.

The patient's recovery was uninterrupted; his temperature remained almost normal, rising at night to 99·8°. He was fed by a spoon with gruel, beef-tea, &c., and remarked that the relief obtained by him had continued without intermission from the time of the operation. Nineteen of the sutures were gradually discharged, both through the mouth and externally; some of the deeper ones, however, remained for a length of time, the last being discharged in January, 1886. The patient's health has since been excellent, his voice clear, and his power of deglutition normal.

My impression with regard to the primary cause of the tumour is that Capt. E.'s habit of so violently exerting his voice originally led to the dilatation of the entire pharynx; this would result in certain muscular fibres, originally mere anatomical items, becoming in some places pathological sphincters. The tonsillitic and bronchitic attacks (chronic) had relaxed the mucous membrane, which remained flabby and weak. Some more than ordinary violent effort had probably then produced a slight protrusion of the mucous membrane, at what subsequently proved the upper portion of the tumour, and which the same causes daily augmented, such as the entrance of food and drink, &c., which by acting as irritants would cause excessive secretion from the mucous membrane; this being retained, gave further cause for dilatation, and also produced the symptoms above described. The secretion "thick, sticky," may have been a pseudo-saliva of parotid nature, which, being an unremitting secretion, may have accumulated in dilatation continuously.

The distress experienced in assuming the horizontal position on the right side was produced by the pressure of the large mass of the tumour upon the structures of the neck, and that experienced upon the left side resulted from the constant flow of a little secretion across the rima glottidis. In rising to the perpendicular, the natural tension of all cervical muscles caused a sudden gush of fluid and consequent coughing; then by each violent cough the tumour would become further distended—hence, not alone the ordinary retardation produced by coughing, but also the increased pressure of the now enlarged tumour upon the right side, gave additional rise to cerebral stasis (veins in vicinity of tumour very numerous).

The constant inability to speak articulately resulted from the mucous membrane being stretched by the tumour, by which the right arytenoid cartilage and cord would be drawn towards the right, as though the right cord were paralysed. Proof of this was obtained in the improved phonation observable when the tumour was drawn backwards and the neck relaxed.

The incessant cough was caused by irritation of the superior laryngeal, resulting from its being stretched on the right over the tumour in an outward and downward direction. A special tendency on the part of those fibres of the pneumogastric supplying the pharyngeal plexus to be acted upon by this irritation, would induce spasm of the muscles forming the sphincter of the opening already described. Increased difficulty of speaking during phonation was caused by greater dilatation, the result of repeated attempts to speak louder.

The arytenoideus proprius, arytenoideus posticus, and lateralis, were not paralysed from the fact of the inferior laryngeal nerve not being affected, and their natural contraction may, by drawing the tissues forwards and inwards, have tended further to close the opening by a valve-like movement.

I have been induced by two reasons to bring my notes of this case before the Academy—viz., the paucity of literature upon the subject already in our possession, and also the statements made upon high authority to the effect that these pressure diverticula of the pharynx are incurable. Indeed, hitherto I have been unable to discover one single case for which operation has been even attempted. Ziemssen has recorded twenty-seven autopsies in cases of pressure diverticula, and remarks that "the radical cure of diverticula from without is, at the present time, one of our vain wishes." He adds, "Yet we should hope that this operation, conducted on Lister's plan, might, at some future day, be performed without danger." I did not adopt Lister's plan. These diverticula and dilations have been divided into two classes—pressure diverticula and traction diverticula; the existence of the latter in the pharynx at any time is denied, and is pronounced as exclusively belonging to the region of the œsophagus. Pressure diverticula are of such rare occurrence as to be seldom if ever seen; yet from them ensues the worst form of dysphagia, and finally death by inanition. They are, almost without exception, situated at the lower part of the pharynx, and at the superior portion of the œsophagus, sometimes being lateral and sometimes in the median line. Very rarely more than one actual diverticulum exists; the smaller excrescences being in reality but protrusions of the pharynx; the larger ones are more correctly described as pouches hanging from the pharynx, between the spine and œsophagus. The walls of these pouches are thick and firm, and their covering was thought to be identical with that of the pharynx, but since muscular tissue in them, if, as usual, not totally absent, exists only at the neck of the pouch, it is therefore evident that such sacs are formed by the mucous membrane bulging between the muscular fibres—hence the application of the term pharyngocele.

Concerning the ætiology of such pouches, the majority of statements must be hypothetical, because patients, as a rule, fail to assign any cause, yet some cases are carefully described, and reliable accounts given for the probable reason of their developments.

A limited region or spot on the wall of the pharynx loses its power of resistance against the pressure exercised upon it in each act of swallowing. The morsel of food or fluid swallowed, compressed by the rest of the pharyngeal muscles, will press most forcibly against that spot which most readily yields. The mucous membrane begins to bulge slightly, and with this constant and frequently repeated act gradually protrudes more and more until finally a sac is formed in which food may remain at other times than during deglutition; then from the weight the sac will be distended and dragged downwards, and so it is developed into one of those sacs which hang down behind the œsophagus, between it and the vertebral column, and which must, when distended, press the œsophagus forward, so that the cavity of the diverticulum is in direct continuation with the pharynx, and thus all ingesta fall into the diverticulum, while the compressed œsophagus, whose opening into the pharynx is thrown out of the axis, receives no more food unless with great difficulty by means of artificial manipulation, manual compression of the diverticulum, or the introduction of a sound (Ziemssen). Various causes may influence a spot in the pharynx to give way and become dilated. Some small foreign body may be retained in the pharynx and separate the muscular fibres, through which the mucous membrane will protrude.

In one case the diverticulum appears to have been originated by a cherry-stone, in another by a crust of bread, in a third by a chicken bone, and again by the rupture of the pharyngeal muscles from violence. Should there be a stenosis at the upper portion of the œsophagus it would facilitate the formation of a diverticulum. The reason for these pressure diverticula being situated at the base of the pharynx on its posterior surface is easily understood, for here the muscular structures are very thin, running in parallel lines, while above they run obliquely and are freely interlaced. This affection is much more frequent in males than in females, and is undoubtedly a disease of middle and old age. The symptoms in the later stage of this disease will be a tumour of the neck in the œsophageal region, either at one or at both sides, out of which food can be forced by pressure, when it will return into the mouth, and, consequently, the enlargement will temporarily subside. Introduce a sound; it will enter the pouch without being able to pass beyond it, and the greatest dexterity will be required in the transmission of the instrument beyond the diverticulum into the stomach.

Traction diverticula belong more to the œsophagus, and are, therefore, outside the range of my present subject. Before concluding, however, I may remark, that they are usually originated by some inflammatory swelling of the parts adjacent to the œsophagus; in consequence of contraction following such inflammation, a portion of the œsophageal wall will be drawn outwards, most frequently at a point corresponding to the division of the trachea. That the best treatment for diverticula of the pharynx is that which I adopted in the present case, and which I have just described, there can be but little doubt. The most suitable treatment for traction diverticula of the œsophagus is, however, quite a different matter, and is a subject upon which time forbids my dwelling to any extent. Beside the exclusive use of fluids, and the introduction of food through a tube, electric irritation of the œsophageal muscles is recommended. In my opinion, the proper treatment would be gastrostomy or jejunostomy, provided further experience of that operation proved its superiority.

EXTROVERSION OF THE URINARY BLADDER, TREATED BY WOOD'S OPERATION.

By EDWARD H. BENNETT, M.D., F.R.C.S.;
Professor of Surgery in the University of Dublin, &c., &c.

[Read in the Surgical Section, February 19, 1886.]

In this communication I beg to submit to the Section an account of the treatment of a case of extroversion of bladder occurring in the female, which, although apparently successful in its results as a surgical proceeding, has been, I am certain, the remote cause of the patient's death. I exhibit, in illustration of my remarks, three casts taken from the patient at each step of the operation and the dissection of the parts involved, removed *post mortem.*

The original condition represented in the first cast was that commonly present in this deformity. The symphysis pubis was unclosed, and the bodies of the pubic bones projected as prominent tumours on either side of the middle line. The umbilicus was absent from its normal position, and was represented by a pale glossy surface in the pubic region, which was directly continuous with a red mucous prominence above the site of the vulva. On the lower part of this could be seen the openings of the ureters constantly excreting urine. Passing towards the perinæum rudiments of the clitoris and of the labia minora could be seen on the margins of mucous surface, and a minute opening limited by the hymen just at the margin of the perineal integument. The anus was placed more forward than normal, and was defective in its sphincter, so that prolapse of the bowel took place on the slightest compression of the abdomen, when the child cried, or when defecation took place.

I saw the child not long after its birth, some four and a half years ago, and advised that operation should be done when it should be about a year old. The only other matter I was asked to decide at my first examination was the sex of the child; for, although a very competent surgeon was present at the birth, and had declared the sex, still there was some doubt amongst the wise women which I had to set at rest.

There was no difficulty in the determination, as I could readily recognise beneath the extroverted bladder, or, to use a more correct term, beneath the patch of mucous membrane on which the ureters opened, and on which they poured out the urine, distinct rudiments of the cleft clitoris, the nymphæ, and the vaginal opening.

After the appointed interval the child was brought to me, well nourished, plump, and strong, and I did not hesitate to undertake the first steps of Wood's operation.

The child being placed under the influence of ether, I turned down from the skin over the centre of the abdomen a large flap of skin, dissecting it from above down until I reached the thin structure forming the umbilical scar. This flap was large enough to completely cover, when turned down, the entire red bladder mucous membrane, and reached as low as the clitoris, its epidermal surface being applied against the bladder membrane, and its subcutaneous adipose tissue directed forwards. Two symmetrical flaps were next raised from the lower lateral regions of the abdomen, having large broad bases in the position which the labia majora should occupy, the upper angles of their bases meeting the angles of the centre flap. These were turned inwards to the middle line to cover either half of the central flap, against the exposed subcutaneous surface of which their bleeding surfaces were placed, while their juncture was effected by suture in the middle line. So the exposed surface of mucous membrane was covered by a skin-flap whose epidermal surface was directly applied over it, and its sides covered in by skin-flaps superposed on the first and united over it in the mesial line. These are the exact details of Professor Wood's operation, which is designed to form, first of all, a covering for the exposed vesical surface, and, secondarily, with it to form a cavity capable of containing the urine, and even it may be storing it like a true bladder.

The operation performed, the child was placed in bed, attention was paid to its position, &c., but things in a few hours seemed to be going astray—the child was cross and irritable, with high fever. Indeed I could not make out the reason of the distress until the day after the operation, when the explanation appeared. True scarlatina—which had been prevalent in the children's ward, and for which it had been closed for a time and thoroughly, as we thought, disinfected—broke out and ran its course with moderate severity. It did not interfere with the progress of the union, and by the time the fever passed off the union was perfect in all parts, except a very little of the lower extremity of the mesial union, where urine constantly poured between its edges. I do not think the scarlatina did the least harm, although it did not make the child either an agreeable or manageable patient.

I now allowed a long interval to elapse to let the flaps settle down and alter if they meant to do so. After nearly a year I attempted to increase the union of the parts downwards in the mesian line by paring the surfaces of the skin on each side, for this was clearly the indication, as the red vesical membrane protruded with the ureter-orifices beneath the site of my first operation. In this attempt I failed, in great measure because, for one reason or another, the child was so cross and restless that I found my sutures cut away.

Again, after a long interval, I operated and obtained success by the use of sedatives and very careful binding of the limbs together so as to prevent as far as possible any strain on the union. This attempt was completely successful, and I now, as the casts show, had the union effected down to the clitoris, the halves of which are seen at the lowest point of the mesial scar. The bladder membrane barely was visible.

Again I let some months go by before trying to fix any apparatus to compress the opening, so as to retain a small amount of urine in the new-formed bladder, or to guide it into a suitable external receptacle. I looked on the surgical part of the proceeding as complete—only the mechanical remained. I could put my hand on the details of appliances for the male affected by this deformity, but not on such for the female, and I need not delay to point out how much more difficult the adjustment and staunching of such for the female must necessarily be.

I could not find any help in this most essential part of the treatment by the study of the efforts and failures of others dealing with a female patient—but, although the operative treatment does not appear more difficult in the female, it seems to me that no matter what its perfection may be, the mechanical problem is vastly more complex in the female than the male. All authorities are agreed that the deformity is met with much more frequently in the male than in the female, and in this I presume lies the explanation of the scantiness of the information I have sought for in my difficulty. In the accompanying casts I have recorded the appearances of the original deformity of my patient, and the results of each operation as completed, step by step. The covering of the bladder was after the method of Wood.

I must now, in view of the ultimate issue of the case, direct attention to two clinical facts which I observed in its long progress which bear on the issue:—First, the character of the urinary flow observed before any operation and its subsequent change. Second, the fact that since the last and finishing operation, since last spring, the child had gradually failed, and of late very rapidly. All this time no symptom could be detected sufficiently marked to indicate the real cause; only the *post mortem* satisfied my suspicions.

In the earlier times of the case, before operation, I used to demonstrate to the class the guttatim flow of the urine from the ureters, and test the time which was required for chemical substances, such as iodide of potassium, to pass from the mouth into the urinary excretion. In the later times, while the ureters still could be seen after the first operation, we noticed that often an interval occurred in the excretion which was followed by the squirting out of a considerable quantity of urine from one or both openings. It was evident that some power of retention in the ureters existed which was absent at first. The subsequent covering of the bladder-surface prevented further observations of these phenomena, and the cause of the child's failing health was, I may say, concealed from us, while we did not at the time attribute its full value to the change in the mode of excretion of the urine already noticed. The child became weaker and thinner, and afflicted with almost constant vomiting, and died. I was fortunate enough to be able, with some difficulty, to obtain a *post mortem* examination, which makes it evident that death resulted from purulent pyelitis, brought about by chronic stricture of the orifices of the ureters.

This contraction one is inclined at first to attribute to some constriction brought to bear on the openings by the contraction of the cicatrices of the operation wounds, but this is not sustained by an examination of the parts. All these cicatrices are remote from the ureters, and the constrictions exist in the tissues immediately surrounding them as they open on the mucous membrane. Some little uncertainty exists, for I could not examine these minute details until I had removed all the genito-urinary organs from the body, and so the attachments to the bones have been severed; but I feel confident that constriction is due to inflammation of the ureters both of their lining membrane and of the tissues immediately surrounding them at their orifices.

I should notice that while the opening of the vagina is so small that during life I thought it might be defective, the dissection discloses that it is normal, as are also the uterus and its appendages.

ON BONE-DRAINAGE IN HIP-JOINT DISEASE.

By W. THORNLEY STOKER, M.D., F.R.C.S.;
Surgeon to the Richmond Hospital.

[Read in the Surgical Section, March 19, 1886.]

The nomenclature of chronic hip-joint disease has been so various and so inexact, and some of the attempts to divide the affection into stages so far removed from correspondence with clinical experience, that I find it necessary to preface any remarks I have to make on the subject of treatment, by a notice of the errors which have been made in these respects, and the corrections of them which recent surgery exhibits. It is not possible to lay down a rule of practice without defining the exact condition to which that rule applies. How inexact the old division of coxalgia into stages, as evidenced by alterations in the length of the limb, is, must have been a matter of observation to every surgeon who has watched the disease, and found that the apparent lengthening, which was supposed to constitute the leading character of the so-called first stage, nearly as often follows the apparent shortening which marks what is sometimes called the second stage, as it precedes it. In point of fact, there is no real line—either clinical, anatomical, or pathological, to be drawn between those stages; while the third, or that in which real shortening occurs, although in every sense a separate condition, owes the deformity from which it received its name to one of a number of various causes which the nomenclature jumbles together without any attempt at classification, although their pathology is as different as their treatment should be. A division like this, based on symptoms, and not upon the changes of which symptoms are but the expression, could have no value even if correct, and has no correctness independent of its want of value.

So many writers have divided the periods of hip disease in so many ways that it is impossible to reconcile them on paper, but every practical surgeon knows that whatever divisions he finds in books, at the bedside he arranges his cases into two classes, which correspond with sufficient accuracy to those described by Ford in his book, which was the first expression of clear views about this disease:—

First, those in which the joint structures are in a state of inflammatory or other diseased change not yet amounting to destruction, and in which apparent alteration of length either in the direction of lengthening or shortening occurs;

And second, those in which disintegration, more or less complete, has taken place, spoiled the articulation, and caused real shortening.

With the second class, embracing necrosis and abscess, I have no concern at present, as my remarks apply to the treatment of those less-developed instances of disease where the integrity of the articulation is not yet hopelessly impaired, where the inflammatory changes in the bones or soft tissues are susceptible of curative treatment, and where more or less complete recovery may take place.

Putting aside, as a matter for separate consideration, the important question as to whether particular cases are simple, strumous, or tubercular in their kind, the classification of most help to the practical surgeon is that which, based on the situation in which the disease has originated, divides it into *Femoral*, *Acetabular*, and *Arthritic.*

With regard to the arthritic form, recent pathological investigation seems to prove that joint diseases never occur primarily in cartilage or ligaments, that the engagements of these tissues is secondary to that of the bone or synovial membrane, that even the ligamentum teres—which from the vascular supply it carries has been so often stated to be the starting-point of hip disease—is never primarily engaged, and that the frequency with which it is found disorganised is due to osteitis of the epiphysis, with which it has so vascular a connection. Accepting as a fact that morbus coxæ, like other joint diseases, has its commencement in either bone or synovial membrane, we have to consider in which it most commonly begins, and why it does so. Here I follow the authority of Mr. Barwell, who, giving it that his observation proves the infrequency with which the synovial membrane is primarily engaged as compared with the bones, points to the protected position of the synovial membrane of the hip as guarding it from the effects of cold and injury, and shows that from the anatomical conformation and mode of development of the bones entering into this articulation, they are rendered unusually susceptible of disease. The epiphyses both of the acetabulum and of the femur, together with the neck of the latter bone, lie within the synovial sac; and, unlike the bones entering into the formation of other large joints, are the seat in early life of great intra-articular vascular activity, and of a consequent liability to inflammatory changes involving the joint—in obedience to the pathological law that morbid changes tend to become extended in proportion to the vascularity of the soil in which they take place. If it be asked why an opposite order of affairs obtains in the knee, where we know that chronic disease usually has its development in the synovial membrane, and spreads to the bones, it is answered by the exposed position of the membrane in the knee as contrasted with the hip, and by the exclusion from the former joint of all epiphyses, except the anterior portion of that of the femur. Of the two bones—in either of which osteal coxalgia may have its commencement—clinical observation shows that the femur is by far the most commonly affected, partly because of its more active vascularity, and partly from its greater exposure. My own observation of hip disease—which has been sufficiently extensive in a city where it is extremely common—jumps entirely with that of Mr. Barwell, who expresses himself that "there is no doubt whatever that nearly all cases of chronic infantile hip-joint disease originate in the bone, and more especially about the pelvic or epiphysal junctions."

An interesting paper in this connection, which argues not only for the femoral origin of the disease, but for its strumous character, is that published in the *Lancet* of December 24th, 1881, by Mr. J. Greig Smith, in which he classifies the so-called white swellings of joints into those commencing in the synovial membrane, which he names "synovio-arthritis," and those taking origin in the pink marrow in the ends of the bones, which he christens "medullo-arthritis." In relation to the latter he points to the connection between the pink marrow of the cancellous tissue and the lymph-glandular class of organs, and to its kindred liability in connection with these organs to strumous disease. He reminds us of the liability of abscesses formed as a result of this disease, when occurring in the ends of the large bones, to burst into the neighbouring articulation; and he details two cases in which abscesses having so involved the knee, he evacuated them by boring into the femur where originally diseased, and had excellent results. He speaks of the frequency of medullo-arthritis of the upper end of the femur as a common starting-point of hip-joint disease, and details his method of boring into the trochanter and neck of the bone for its relief—an operation to which I will presently refer. His paper is a plea for the tunnelling of diseased bone in the vicinity of joints, and the free scooping away of diseased cancellous tissue, so as not merely to make a drain for the subsequent relief of the disordered structure, but to actually remove it as far as possible at the time of operation.

I have said so much of the pathology of morbus coxæ, in order to lead to the conclusion I hold in common with the authors I have named, and many others, that this complaint is usually osteal in its origin, and seated at first in the femur—as upon that postulate is founded the treatment proposed by Dr. Frederick Kirkpatrick, which is essentially the same as that which I advocate. I deal with only one point of treatment, and applied to only one class of cases. The mode of treatment is that by boring into the neck of the femur through the great trochanter, so as to obtain bone-drainage, and the case to which it is applicable, that of osteitis, or medullo-arthritis—as you please to regard it—of the femur, in the vicinity of the hip-joint. I by no means wish to underrate other methods of treatment, or to set them aside, but their consideration is outside the scope of my present communication; and while rest, counter-irritation, and so forth, may have their use as special means in special cases; and while I esteem rest, for instance, as an almost necessary aid to the very procedure I now speak of, I desire to assert my strong belief in the value of a treatment which, by affording a drain to an inflamed tissue, and giving vent to diseased products, does for early hip disease what surgeons have long done for kindred conditions in other situations and in other tissues.

If it be conceded that in certain given cases hip disease has its starting point in the femoral portion of the joint, and that an inflammatory condition constitutes this commencement, it can hardly be denied that an opening made into the diseased structures without involving the cavity of the joint is a rational and logical treatment. It only requires that it shall be possible to diagnose a case of commencing femoral coxalgia, and that the surgeon shall find it possible to select cases where the disease is mainly confined to the femur, and is in such a modified condition as to afford hope of a good result from the treatment. The cases, of course, affording the best chances of good result are those which are neither strumous nor tubercular. But all we have learned in late years of the latter cachectic conditions points to the propriety of removal of the diseased tissue and free drainage in them, no less than in the simple condition. That osteal cases can be diagnosed from arthritic, and femoral from acetabular, is sufficiently evidenced by the examination of any good text-book, and, as for the condition of disease in the particular part, the selection of cases fit for operation is a matter of clinical experience more than of written rule. I am strongly of opinion that even where the disease has engaged the soft structures to a limited extent, the operation of tunnelling the bone may effect a cure, as the relief of the bone itself by the exit given to inflammatory matters removes a cause of irritation from the synovial membrane, and must have a beneficial effect. I think, therefore, that while the typical cases for this treatment are those in which the disease has only shown itself in the femur, it is also applicable to instances in which synovitis exists. Further, the plan of tunnelling may be used in the late stage of the disease, as advised by Mr. Greig Smith, as a means of draining the joint of pus or particles of necrosed bone.

The logical excellence of this treatment, and the results claimed by Dr. Kirkpatrick for his method, which is essentially the same, induced me to give it a trial, with the good effect I will presently detail.

When the theory of the treatment and the success claimed for it are considered, it is a matter of surprise that it has not been more employed, and yet we may search the text-books dealing with joint disease and not even find it mentioned. To Dr. Kirkpatrick belongs the merit of having, at the meeting of the British Medical Association in 1867, directed attention to this plan of treatment in diseases of joints—the hip among others. He described the mode of cutting down on the trochanter major, and tunnelling into it with a trephine or drill. But he adds the use of caustic potash, which he applies freely in the wound, and from the use of which I dissent. I cannot see much harm in the use of the caustic, but I certainly see no good, and as excessive surgery is not the best surgery, I am opposed to its employment. I prefer the more precise practice of the use of instruments, and object to the caustic as not only unnecessary, but uncertain and disobedient. Putting this aside, any credit which attaches to the introduction of the treatment unquestionably belongs to Dr. Kirkpatrick. Mr. J. Greig Smith, writing on the subject fourteen years later, describes a procedure essentially the same, omitting the caustic and employing a Volkmann's spoon or scoop to remove diseased cancellous tissue from the interior of the affected bone.

I have applied the treatment of bone-drainage by tunnelling the trochanter and neck to two cases of hip-joint disease. One of them, in a child aged five, had commenced as a femoral case, and was operated on when the soft tissues of the joint had become considerably engaged, but before any pus had formed. It has improved to a marked degree, in no point more evidently than in the relief from pain which the child has experienced. I do not make more detailed mention of the case, as it is still under treatment.

The second case is that of a girl named A. S., aged eleven years, who was admitted into the Richmond Hospital in July, 1885, at the request of my friend, Mr. S. T. Reede, of Carrickmacross, suffering from disease of the right hip-joint of four months' standing. She limped and walked with difficulty, had starting pain in the hip, and pain at the inner condyle. The limb was apparently shortened, and the foot everted. No swelling existed behind the trochanter, or in the groin. The trochanter was slightly enlarged, and tender when pressed on. She was in the late part of the first of those two stages into which, as I have said, the best clinical experience divides the complaint. As her general health was suffering, her lameness and deformity increasing, as rest had failed to give relief, and as it was a purely femoral case of disease, I trephined the trochanter on July 8th. No additional constitutional disturbance showed itself. She was kept in bed until July 23, when her pain had so far abated that she could walk without inconvenience, and she left hospital to return to her country home. At the time of her discharge the sinus continued to discharge, but in very small quantity. The discharge lasted for about three weeks, during which time she was kept chiefly in the recumbent position on a sofa. At the end of this period she was gradually allowed to resume her former active habits, and I saw her on December 7th in a perfectly sound condition, able to run and jump with any girl of her age—the only evidence that she had ever been in the hands of a surgeon being a dimple over the lower part of her trochanter.

When we think of the sad progress of most of the cases of this kind, and of the great length of time occupied by other treatments, even when successful, we ought at least to give the gravest consideration to the propriety of a measure which, like this, is capable of effecting a cure in a little over a month.

The method of operation I follow is to make a vertical incision down to the trochanter, with its centre over the point at which the bone is to be penetrated; to remove the bone to the full depth of a trephine of half inch diameter, and if further tunnelling towards the head be deemed necessary to perform it with a drill or strong director. I then fill the wound with strips of gauze to check immediate bleeding. The gauze is removed next day, and the wound henceforward kept covered with a moist carbolic dressing. As soon as it has effected its purpose of drainage, the wound closes without further treatment.

Mr. Greig Smith has laid down a very good rule as to the point at which to enter the trochanter, so as to get safely into the neck. Three prominent points can be felt around the great trochanter. These are the anterior superior, anterior inferior, and posterior inferior angles, and they nearly form the angles of an equilateral triangle. The junction of the anterior and middle thirds of the base line is the proper place to perforate the bone. Comparison of the femur at different ages will show that in entering the bone from this point it is necessary, in order to preserve the integrity of the compact tissue covering the neck, to give the tunnel a more oblique direction upwards in young objects, and approaching the horizontal line in older ones, because of the different angle assumed by the neck as age advances.

Inspection from above shows that any penetration at a point *posterior* to that laid down, would involve a risk of opening through the back of the neck of the bone.

Founded on what I have said, I put forward these propositions:—

1st. That tunnelling the trochanter and neck is a reasonable and good practice in cases of femoral coxalgia, and is calculated to afford drainage and remove the products of disease.

2nd. That it is in better accord with surgical science, while adopting the essential principle of Dr. Kirkpatrick's plan, to avoid the employment of caustic and rely upon the more precise use of instruments; and

3rd. That the extended application of the plan, so as to remove the diseased bone or even to drain a joint containing pus, as proposed by Mr. G. Smith, is a surgical proceeding worthy of every examination.

BONE DRAINAGE IN THE TREATMENT OF THE EARLY STAGES OF HIP-DISEASE.

By WILLIAM STOKES, K.B.,
President of the Royal College of Surgeons; Surgeon to the Richmond Hospital.

[Read in the Surgical Section, March 19, 1886.]

NOTWITHSTANDING the signal improvement that has been observed in the results obtained after resection of the hip-joint since the introduction of antiseptic practice, the operation is undoubtedly not maintaining the position in surgical estimation that other joint excisions—notably those of the elbow, shoulder, and knee—at present occupy. I believe this to be mainly due to a twofold cause. 1st. The rarity of the cases in which the disease is sufficiently limited to admit of complete and thorough removal by the knife, and secondly, when this is possible, the difficulty, indeed impossibility, of maintaining that absolute rest and fixation of the limb which is so essential to the satisfactory and speedy reparation of such a wound. The disheartening statistics, analysed so well by Dr. Yale, in his recently published excellent paper in the "Annals of Surgery," furnished to us from Germany by Bilroth, Volkmann, Huëter and others, would be sufficient to make even an operator most confident in the powers of his art, apparently in the most favourable cases, undertake the procedure with no small apprehension as to the result. For example, from Dr. Yale, in a statistical record of 731 cases operated on and collected by Sayre, Culbertson, Caumont, Volkmann, Korff and Grosch, we learn there were 285 deaths, giving an average mortality of 41·4 per cent., a mortality which, doubtless, would be much diminished if an earlier stage of the joint trouble were selected than is usually the case, though I would not go so far as Huëter, who held that the operation of resection was indicated " as soon as an extensive suppuration of the joint manifests itself, or as soon as the course shows that the termination in suppuration can be no longer prevented."

There can be little doubt that this is a somewhat exaggerated statement, and contrary to the experience of most surgical observers; for certainly I can recall cases, doubtless exceptional, in which, after joint suppuration, recovery took place with ankylosis, the patient's condition in the end being as favourable a one as if a successful resection had been performed.

Take now the results obtained as a rule in hip-joint disease, in its early as well as in its late stages, by what Ollier has termed "methodical expectation." Are these much more encouraging than the results of resection? In the suppurating cases they are especially disappointing, and this, which is my experience, I believe coincides with that of most surgeons. I would not, perhaps, go as far as Huëter, who held that suppuration in the hip-joint is a "nearly absolutely fatal process;" but I do not think I am far astray in holding the opinion—one based on the observation of a large number of cases in an hospital experience of over twenty years—that at least 50 per cent of the cases of suppurative coxitis are never healed, and a very large proportion of them are lost; and such disappointing results, also, are only too often observed in cases where the best hygienic resources are available. What surgeon is there, of even a moderate hospital experience, that cannot recall a dreary record of the results of suppurative coxitis—amongst others, faulty ankylosis, persistent and incurable fistulæ, atrophy of the limb, pelvic abcesses, and septic complications? Such conditions all concerned must regard with grave apprehension, and suggest the consideration as to whether, in many cases, a more active treatment in the early stages of the disease than that usually employed, may not with advantage be adopted to avert suppuration in the joint, and other equally serious sequelæ—conditions, which may, and often do, necessitate resection, or the still more serious operation of amputation at the hip-joint.

The routine treatment that in the majority of cases is mainly relied on in the early stages of hip disease is, as a rule, a threefold one—methodical inactivity of the limb, the establishment of superficial eschars over, or in the immediate neighbourhood of the disease, and the administration of certain medicines which are believed to have the power of neutralising the baneful influences at work in those who are unfortunately possessed, or supposed to be possessed, of a tuberculous diathesis or habit. The history of this line of treatment is only too often one of conspicuous failure. Let me not be misunderstood. No one is more sensible than I am of the value of rest in all forms of inflammatory disease of joints or bones; but in those cases there is too great a tendency to rely mainly, if not exclusively, upon it; but I confess to being distinctly opposed, in hip disease, and my remarks apply equally to spinal disease, to the establishment of superficial eschars, believing them to be not only therapeutically valueless, but in many cases distinctly harmful, and also sceptical as to the alleged beneficial effects derived from the exhibition of the so-called anti-strumous remedies.

Since the appearance of Sir B. Brodie's work on diseases of the joints,[a] the views therein stated as to the early changes in the majority of cases of coxitis are those which are still accepted by most surgeons, and I believe rightly so—namely, that, in the first instance a preternaturally vascular condition of the cancellated structure of the bones, "with a less than usual proportion of the phosphate of lime in its composition, there being at the same time a deposit of fluid, apparently serum, in the cancelli." I believe, too, that this condition is, as a rule, associated with, and often preceded by a limited and localised periostitis, and that this twofold lesion is in most cases due to a traumatism which often is of so trivial a character as to have escaped the notice of either the patient or the parents.

Assuming, as we may do, that in these cases we have to deal with an inflammation in the cancellous tissue of the bone, and one usually associated with more or less periostitis, which effectually aids the compact tissue of the bone in blocking up and imprisoning the pent-up inflammatory exudations in its interior, is the routine practice of applying blisters, establishing issues, stretching the leg, or fixing it by some

[a] Pathological and Surgical Observations on the Diseases of the Joints. By Sir Benjamin Brodie, Bart. London. 1850.

apparatus such as Thomas' Splint, the best means of preventing or arresting the onward course and development of a drama that but too often ends tragically in suppuration in the joint, resection, amputation, and often death? My belief is that such practice in the early stages of the greater number of these cases is one which is insufficient; that, for a treatment to be effective, provision must be made for liberating the pent-up serous fluids, the product of the inflammatory action in the deeper portions of the bone.

It appears to me that in dealing with morbus coxæ, surgeons often fall into the twofold error—first, of not estimating correctly the exact seat of the primary lesion; and, secondly, of relying too exclusively on the routine methods of treatment. The late Mr. Cooper Forster has observed that, "practically, the question what structures are primarily or secondarily affected is not important." Mr. Edmund Owen also observes that "it is not of practical importance to attempt a diagnosis as to whether the disease exists chiefly in the femur, acetabulum, membrane, cartilage or ligament." However true this may be in the later stages of the disease, as regards the early ones such a doctrine is, in my opinion, questionable, and for this reason—that, if the lesion originates in the bone or its periosteum, early active operative interference on the part of the surgeon will, in many instances, promptly check the further progress of the disease—a result which cannot be anticipated with so much confidence when it is situated in the synovial membrane, the articular cartilages, or round ligament. The second error, one already indicated, so often fallen into, is the indiscriminate adoption of the system of long-continued inactivity of the limb, which is attended with a mischief to the system generally, which more than counterbalances whatever local good it may be attended with. The frequent occurrence, as an early symptom, of pain at the inner side of the knee, both Mr. Hilton and Mr. Owen maintain, is evidence that the ligamentum teres is the seat of the primary lesion, the obturator nerve sending a branch to it, as well as one to the inner side of the knee. But this, although, doubtless, a frequent symptom, is by no means a constant one, and besides, a lesion of the ligament referred to could not be said to be the sole factor causing the pain. Mr. Barwell, I think, comes nearer the truth in holding that an epiphysitis is the most usual starting point of the disease; but my belief is that Sir B. Brodie has estimated more accurately than all other observers what the most frequent primary lesion is—namely, a central osteitis, which, I think, is most frequently of traumatic origin, and involves, for the most part, the trochanter, neck and head of the bone. The condition can in its early stages, in most instances, be recognised as of this origin, by the pain being more acute than when the lesion is situated in the soft structures, abduction and flexion of the limb not being so well marked as in the cases where there is much synovial inflammation and effusion, the position always assumed when there is much intra-articular tension, by the pain being, as a rule, referred chiefly to the situation of the trochanter, by increased local temperature, all these co-existing with a more or less febrile condition of the patient.

Assuming that this view is correct, the practical question at once comes to the front—namely, as to the best means of dealing with it. Is mechanical rest for a protracted period, one to be estimated not by days and weeks, but months, and often years, coupled, perhaps, with blistering, issues, setons, local depletion, &c., the best means of arresting the progress towards the calamitous goal of intra-articular suppuration, destruction of cartilage and death of bone? Are we right to trust to the so-called *vis medicatrix naturæ*, or should we not look on nature in this instance as being rather more bent on mischief than on good—an influence to be thwarted rather than assisted? We have heard a great deal of late years of "preventive medicine," but the principle of prevention in surgery is just as important as it is in medicine, and there seems no reason why, in the condition under consideration, it should not be applied. There seems no reason why Sir B. Brodie's brilliant achievement—one that stands in the front rank among modern surgical advances—namely, the operation of trephining the head of the tibia to evacuate the contents of an abscess—should not be applied in cases of osteitis before the stage of suppuration had been reached. This is, in fact, the aim and object of the system of bone drainage to which I wish to draw attention—a treatment which appears to me to be mainly indicated in the early stages of hip disease, especially in those forms of it in which there is evidence that the lesion is a central osteitis.

The plan of bone drainage I have adopted is practically identical with that first advocated by Mr. Kirkpatrick, of this city—a mode of treatment which, I think, has not received the consideration at the hands of surgeons that it merits. I was pleased to learn recently that in Germany—in Bonn, Heidelberg and other places—it has been employed, and is steadily acquiring repute as a surgical resource in suitable cases of bone and joint disease. I have tested it in various stages of these diseases, and from what I have seen my belief is, that it is in the early periods of coxitis, notably the "femoral" variety, that the best results from its use may be anticipated.

The results obtained in some cases of this disease which were treated in the Richmond Hospital have been mainly instrumental in making me arrive at the opinion I have indicated above. In illustration of this view, I would specially refer very briefly to three cases of hip disease which I treated by incising the soft structures over the situation of the inflamed bone, perforating the latter, and freely applying potassa c. calce along the tract of the wound both of the soft and osseous structures. The first of these cases was that of a female child, aged eight, that was under my care in the Richmond Hospital in the spring of 1879. There was the history of a fall received about eight weeks previously to her admission into hospital. She was a delicate, badly nourished child, and already there were the usual signs well marked of the early stages of coxitis. Foremost among these was the extreme pain which was referred chiefly to the great trochanter and parts in its immediate neighbourhood. There was also evidence of articular effusion into the joint and muscular atrophy, and obliteration of the gluteal fold had already set in. Assisted by my colleagues and Dr. Kirkpatrick, who came to witness the operation, I made an opening into the bone in the manner already indicated, together with a free application of the caustic. This was followed soon after by relief to the extreme pain the patient suffered from. Two applications of the caustic were subsequently made to prevent the wound closing, after which it was allowed to fill up by granulation. The result was in every way most satisfactory, and after two months the patient left the hospital, able to walk without assistance of any kind. The second case was under treatment last autumn. The patient, a healthy, well-nourished female, aged eighteen, was admitted into the Richmond Hospital under my care, suffering most acutely from most of the usual symptoms of hip disease. The pain was so acute that any manipulative examination of the joint was hardly possible without an anæsthetic; even the weight of the bedclothes was borne with difficulty. The violent starting pains the patient had, at night especially, made me very apprehensive that ulceration of the cartilages had set in. This case was treated precisely in the same manner as the first—namely, incision, perforation, cauterisation, followed by keeping up a moderate extension of the limb by weights and pulleys. This case I recently exhibited here at the meeting of this section, held on the 11th December, when the members had an opportunity of forming an estimate of the value of the result obtained. The patient returned home immediately after being exhibited here, and has remained perfectly well ever since. The third case was that of a female child, aged ten. The disease in her case was far more advanced than in either of the other cases I have spoken of. So much so, that at first I was not at all sure that it would not be better to proceed to resection at once. However, I elected to try the bone drainage treatment at first, having regard chiefly to the fact that in the event of its proving unsuccessful, it would not materially interfere with any subsequent operative measure of greater magnitude, should such be deemed justifiable. The case being still under treatment it is, of course, premature to speak with any confidence as to the ultimate result, but when I compare the present satisfactory condition of the child with what it was on her admission into hospital, I cannot but be most hopeful. I may say this, however, that unless something very unforeseen takes place, my belief is that the outcome of the case will be just as satisfactory as it was in the others in which I adopted the treatment of bone drainage—a result which, surgically considered, will be of greater interest and importance than either of the other cases, having regard to the fact that in it the disease was so much more fully developed.

ON THE NATURE OF SCROFULOUS GLANDS IN THE NECK, AND THEIR SURGICAL TREATMENT.

By KENDAL FRANKS, F.R.C.S.,
Surgeon to the Adelaide Hospital, Dublin.

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I SHOULD scarcely be justified in discussing the methods of dealing with that condition of the glands of the neck which centuries have agreed in calling *scrofulous*, were I not in the first place to attempt to explain what is meant in the present day by scrofula. According to the views which we hold on this subject will our treatment depend. If we look upon scrofula in all its stages as a *diathetic* disease, then our treatment will be directed to correct this tendency; in other words, we shall have recourse to constitutional measures as the most rational—as those which promise the greatest success. If we, on the other hand, hold that scrofula is *infective*, that it owes its origin to a virus making its way into the system from without, then our treatment should in the first place be directed against the virus, to keep it out of the system or to eliminate it. Between these two modes of estimating the nature of the disease we have a third, which combines them both—namely, that scrofula is an infective disease due to a definite poison, which can only find its development, however, when planted in a suitable soil; that is to say, when it gains an entrance into the tissues of a person presenting a peculiar constitutional habit, "the greater vulnerability of parts and the greater pertinacity of disturbances," as Virchow has defined it.[a] If we accept this latter view—and recent investigations, I believe, have fully established its truth—scrofula must cease to exist as a distinctive disease, and the neutral zone between

[a] Krankhaften Geschwülste, Vorles. xxi.

scrofula and tubercle, which has been the battle-ground of pathologists in years past, must disappear. We shall never, I suppose, eliminate the term scrofulosis, but we shall use it in a different sense from that to which we have been accustomed. In the light of the pathology of to-day we cannot accept as a definition of scrofula that it is "a tendency in the individual to inflammations of a peculiar type, the distinctive features of such inflammations being as follows—they are usually chronic, apt to be induced by very slight irritation, and to persist after the irritation that induced them has disappeared."[a] In spite of the attempts of pathologists to give a definite meaning to the term, no one will deny that the word scrofula has been used to designate most of those diseases whose pathology was a chaos of confusion, and that Henle's description of it was well merited, that "scrofula is the receptacle into which one vaguely casts all the ailments which afflict children under fourteen years, and of which we do not know the cause."[b] In discussing, then, the true nature of scrofula, it will be interesting to review shortly its history, and to trace the lines which have led to our present state of knowledge concerning it.

The term was originally given to the disease by the Romans in order "to give prominence to the similarity presented by the thickened neck, the diminished prominence of the chin, and the swelled upper lip—all consequences of the swellings of the glands—with the physiognomy of the hog."[c] In the sixteenth and seventeenth centuries the resemblance between the caseous masses found in the lungs in phthisis and in scrofulous lymphatic glands in the neck was observed, and Cullen, amongst others, began to range phthisis in the list of scrofulous diseases. "The views generally held at that period on this question," says Birch-Hirschfeld, "may be summed up in the sentence that scrofulosis reigned supreme over the domain of tuberculosis, tubercular phthisis itself being looked upon as a scrofulous disease." Laennec heralded a new epoch. Tubercle now became the chief factor, and all processes in

[a] Treves. Scrofula and its Gland Diseases, p. 37. 1882.
[b] Handbuch der Rationellen Pathologie. 1846-53.
[c] Birch-Hirschfeld. Ziemssen's Cycloped. Vol. XVI., p. 744.

which cheesy substance was found were classified with tuberculosis, scrofulous glands being considered only as a small subdivision of tubercular disease. But at this time little was known as to the nature of tubercle itself. The term was applied to nodules, but it had little other significance. Laennec pointed out that these nodules, originally grey and clear, had a tendency to become opaque and yellow, or, in other words, to caseate. Thus, in the lung two distinct conditions were found—the grey tubercle and the yellow, the latter being, however, the result of certain changes in the former—that is to say, the grey tubercle had a tendency to caseate in its centre; and by the fusion of several of these, larger caseous masses were formed, which went by the name of yellow tubercles. The identity of scrofulosis in the glands in the neck and of tuberculosis in the lung was then again called in question, when Virchow came forward and demonstrated that this cheesy substance was not necessarily due to decay of the grey nodules, but might occur in the most heterogeneous tissues. Tubercle was now restricted to the grey semi-transparent nodules, the size of a millet-seed, hard and firm, which have since been known as miliary tubercles; although these, from their want of blood-vessels and their low organisation, frequently tended to cheesy metamorphosis. Virchow accordingly laid down that no cheesy product was to be accepted as tuberculous unless sufficient evidence had been given of its origin from miliary tubercle. Such evidence was not at that time forthcoming in respect to the cheesy masses found in scrofulous glands—hence scrofulosis was not to be considered as a form of tuberculosis. Virchow went even further, and the cheesy infiltration of the lung, called "tuberculous" by Laennec, was not tubercular at all, but should be termed "scrofulous broncho-pneumonia." But even this position of the question was not destined to remain long unassailed. Microscopists turned their attention to the study of the so-called grey or miliary tubercle. That it was the ultimate specific cause of tuberculosis began to be doubted, as certain tissue changes, which were acknowledged to be due to tuberculosis, were found to occur sometimes in which these distinct grey tubercles could not be discovered. In such instances the microscope revealed certain minute bodies, to which the name "submiliary tubercles" was given, and it was then shown that the millet-seed bodies, termed miliary tubercles, were made up of a collection of these microscopic nodules. This submiliary tubercle, called by Köster "primitive or elementary tubercle," is composed mainly of cells. In the centre of a typical specimen we find one or more giant cells. Forming a zone around this are many so-called epithelioid cells, and around these again is a zone of leucocytes. "All these cell elements are supported by a fine reticulum, which is generally concentrically arranged at the periphery, and towards the centre is observed to be continuous with the processes that commonly come off from the giant cells. The affected district is non-vascular."[a] This is the description of "primitive tubercle" as we understand it to-day; but it must not be supposed that this accurate histological account of it was received without much opposition. Indeed, it would seem that in its earlier days pathologists were so engaged in their conflicts concerning it that the fight over the identity or non-identity of scrofulosis and tuberculosis remained for many years in the position in which it was left by Virchow. During this period of dormancy caseous or cheesy nodules were looked upon as the products of the degenerative changes which followed on scrofulous inflammation. There was nothing specific in them. But this controversy on scrofulosis was only dormant, and it broke out with renewed vigour in the years 1865–69. A new method of attacking the question was instituted by M. Villemin. Anatomical and microscopical methods gave way to the method of inoculation, and Virchow's ground was cut from beneath his feet. These inoculation experiments showed two things—firstly, that if the cheesy matter from scrofulous glands were injected into an animal, it produced general tuberculosis as readily as tuberculous matter; and secondly, as shown by Wilson Fox, that if tuberculous matter were introduced beneath the skin, it gave rise to enlarged caseous and suppurating glands—in fact, to all the symptoms of a localised scrofula. The conclusion to be drawn from these experiments seems obvious—namely, that these local manifestations of what was termed scrofula were in reality tuber-

[a] Treves, op. cit., p. 9.

cular, and that if any difference could be made out between these terms, it could only be one of degree. Such a wholesale subversion of Virchow's views, which had taken possession of pathologists for several years, could not naturally be accepted without dispute. The experiments themselves were largely discounted, and it was maintained that, as the so-called acute general tuberculosis, which was described as following on inoculation, occurred in "animals," it was not the same as general tuberculosis in man. It now became apparent that it was more necessary than ever to understand the histological characters of the true tubercle, to distinguish it from simple inflammatory neoplasms, and to determine whether the giant cell, with its external zones of epithelioid cells and leucocytes, was pathognomonic of tubercular diseases. In 1870 Wagner prepared the ground for a more accurate knowledge of the microscopical characters of these primitive tubercles, and the more complete elucidation of their differences from simple inflammatory neoplasms, made almost immediately afterwards by Schüppel, established their distinctive character. With this definite idea as to the true characteristics of tubercle itself, he proceeded to combat Virchow's views, taking for his text Virchow's own dictum—that no cheesy product was to be accepted as tuberculous unless sufficient evidence had been given of its origin from miliary tubercle. This missing link was now supplied. "This demonstration," says Birch-Hirschfeld, "has been furnished by Schüppel's numerous and searching investigations to such an extent that no further doubt is left as to the frequent occurrence of well-characterised tubercles in the lymphatic glands—partly in the form of secondary development with pre-existing tubercles of other organs—partly, however, as primary tuberculosis, to which latter category belong those very conditions which have been regarded heretofore as the most characteristic products of scrofulous disease.[a] Friedländer goes a step further when he states that "all the more important scrofulous affections are intimately associated with the formation of tubercles."[b] From this period the number of diseases in which

[a] Op. cit., p. 751.

[b] Volkmann's Samml. Klin. Vorträge, No. 64.

tubercle was discovered went on increasing year by year. Köster, in 1869, found them in the so-called granulations in "white swelling." In 1879 Volkmann demonstrated that "white swellings" were nothing but tubercular osteoarthritis, and that they owed their origin most frequently to a focus of tubercle in the epiphysis. In 1881 Lannelongue showed, in his work on "Cold Abscesses," that the walls of the sac were studded with tubercles; and in 1883 Kiener demonstrated the identity between caries and tubercular osteitis, and showed that this specific osteitis may assume either the encysted or diffused form. The advocates of the identity between scrofula and tuberculosis seemed thus to be triumphing along the whole line, when a new difficulty arose. The giant cell, with its peculiar arrangement of cells around it, was declared not to be pathognomonic of tuberculosis. In 1875 Ziegler showed that it was found in inflammation artificially produced by inserting two little glass discs beneath the skin or in some of the cavities of the body in dogs and rabbits, and M. Martin obtained them around other irritating particles, or by introducing micro-organisms into the circulation. The question was thus again an open one, and stood thus: inoculation experiments had crowned the proof, afforded by anatomical and clinical observations, that scrofulosis and tuberculosis could not be separated, but hitherto there was no peculiarity of the structures which was pathognomonic of tubercle as a specific element. The infective nature of tuberculous and scrofulous matter alike was established, but the *materies morbi*, the virus itself, had not been recognised. Such, briefly, may be said to have been the point reached up to the spring of 1882. We all remember the sensation which was caused, not only among scientists, but in the world at large, by the announcement of Koch in March of that year, that he had discovered the bacillus of tubercle; that he had succeeded, by the method of double staining with aniline dyes, in demonstrating the existence of the microbe; that by cultivations on solid media he had isolated it; and that by inoculations of the pure cultivations in animals he had proved its virulence. Whatever doubts may have been at first thrown upon the discovery, whatever scepticism may have been at work to detract from it or to prove it false, all have disappeared, and there are few who to-day will deny that Koch's bacillus is the active agent in the production of tubercular diseases, wherever they are found or in whatever guise they appear. Virchow's statement—that no cheesy product was to be accepted as tuberculous unless sufficient evidence had been given of its origin from miliary tubercle, must now be modified and enlarged. In place of it we may assert that all inflammatory products, whether in the stage of cheesy degeneration or not, in which the bacillus of tubercle can be demonstrated, are to be accepted as tuberculous. At the same time we must remember that their absence is not conclusive evidence against the tuberculous nature of a diseased part, as in certain stages of a tuberculous mass the bacilli may disappear. What assistance this discovery has afforded to the physician, or what influence it has had upon his art, is not my object to discuss. For the surgeon it has opened up new fields of labour, and has had a most powerful influence on his work. "The sphere of tubercular diseases," writes Volkmann,[a] recently, "with which the surgeon has to do, has increased so enormously during the last fifteen years, insomuch as a great number of diseases with which he has to deal day by day are recognised to belong to tuberculosis, that, I say, the surgeon has now almost more to do with this disease than the physician."

Addressing, as I am, the Surgical Section of this Academy, let me call your attention to the enormous advantage which the surgeon possesses over the physician in the treatment of tubercular diseases. The diseased tissues are, or can be, exposed to view; frequently it is in the surgeon's power to remove the whole tuberculous patch, and with it the tissues in the immediate neighbourhood, while the patient still lives; and his opportunities for doing so, and the organs that are accessible to him, are increasing year by year. Now, in order to establish a disease in any organ as being undoubtedly tubercular, Volkmann[b] enumerates three conditions as co-existing:—Firstly, the well-known structural condition, which

[a] Langenbeck's Archiv. für Klin. Chirurg., 1886, p. 109.
[b] Op. cit., p. 125.

has been styled "true tubercle;" secondly, the presence of the tubercle bacillus; and, thirdly, the positive results yielded by inoculation experiments. When these three conditions are verified in any diseased part, it cannot be questioned that the affection is one of true tuberculosis. This criterion has been applied to a large number of diseases, and has given positive results. I cannot now do more than enumerate a few of those diseases with which we are familiar under other names, but which henceforth must be classed as tubercular. On the skin we find lupus and certain ulcers, which have commonly been classed as scrofulous; similar conditions of the mucous membranes, and frequently fistula in ano; in the deeper structures, most forms of chronic abscesses, especially those associated with scrofulous glands, diseased bone, &c.; and in diagnosing these abscesses the so-called pyogenic membrane is of great use. "The characteristic abscess membrane," says Volkmann, "occurs only in connection with tubercular abscesses, and it must be looked upon, therefore, as an absolutely safe criterion for diagnosis." All those forms of disease of the bones and joints which have hitherto been described as caries, Pott's caries, white swelling, strumous or scrofulous disease of the joints, fungous degeneration and soforth, belong also undoubtedly to tubercular infection, and the same is true whether the disease originate in the epiphyses of the bones or in the synovial membrane. Lastly, of all parts of the body in which positive proof of the existence of tubercle has been given, the lymphatic glands are most frequently engaged, because their very function exposes them to infection. Under the head of tubercle we must range all those affections of the glands which have hitherto been termed scrofulous enlargement, strumous disease, and caseous degeneration—in fact, by far the larger number of cases in which we find the glands of the neck enlarged.

My excuse for having gone at some length, and yet as briefly as I could, into the pathological aspect of the subject, must be that upon our view of the question in this light the further question of surgical treatment is based; and also that in most of the works in our own language the tubercular nature of the disease is not recognised sufficiently. In support of this assertion I will quote a few passages from some of the more recent surgical works which deal specially with the subject of scrofula and tubercle. In the first volume of Ashhurst's "International Encyclopædia of Surgery," p. 242, in the article on Scrofula and Tubercle, written by Mr. Butlin, and published in 1882, he thus speaks of the morbid anatomy of scrofula:—"There is not in scrofula, as in tubercle, a pathological body, either microscopical or of larger size, peculiar to the disease. All the changes are those of inflammation; but the products of scrofulous inflammations may be analysed, chemically and histologically, without the discovery of any substance or structure which may not equally occur in any, or indeed in every, inflammation." This was, no doubt, written immediately before Koch's Address before the Physiological Society of Berlin, but it does not even represent the position of the question as it stood for several years prior to the discovery of the bacillus. The identity of the two diseases had been fully recognised by leading pathologists, as I have endeavoured to show; the only question waiting for solution being, "What is tubercle?" Again, in Mr. Treves' work on "Scrofula and its Gland Diseases," published also in 1882, p. 20, after giving the evidence for and against the identity of these diseases, he proceeds to discuss the value of the inoculation experiments, especially with respect to the conclusions drawn from them by M. Villemin. He then makes almost a pathetic appeal in favour of scrofula and its future distinctive existence. He says:—"But even now some pathologists still advance the opinion (which I give in the words of M. Villemin) that 'tubercle alone gives tubercle by inoculation.' Those who retain this view are compelled to exclude from scrofula all its classical features—the caseous gland, the cold abscess, osteitis, periostitis, and 'white swellings;' and all that they can leave for the disease are a few superficial lesions, the products of which will in time be probably found to be inoculable—and then, for M. Villemin and his followers, scrofula will be an extinct disease." Well, terrible as the alternative is, we must face it; and as we must now hold that "tubercle alone gives tubercle," and that, as the bacilli from scrofulous lymphatic glands, when inoculated, give rise to tubercle, we must also hold that scrofulous lymphatic glands are tubercular, Mr. Treves will have to accept his alternative and acknowledge that scrofula as a distinct disease has ceased to exist. Let me cite another extract, one from Dr. Clifford Allbutt's lecture on "Scrofulous Neck," as it illustrates the dawn of the tubercular aspect of the disease in these countries. He says in his lecture published last year, p. 12: "Our knowledge of the tubercle bacillus is as yet too young to tell us whether scrofulous neck always depends upon the introduction of the tubercle bacillus by way of the mucous membrane of the throat or otherwise—too young, indeed, even to tell us whether the tubercle bacillus is an essential part of scrofulous neck at all. We know that it may be found there, and that scrofulous neck has of late years been regarded as morphologically tubercle, so that we are safe at least on the broad postulate that inoculation from sources of corrupt or caseating pus does very commonly set up a more or less generalised tuberculosis." I do not wish to assert that every enlargement of the cervical lymphatic glands is tubercular. We eliminate in this discussion those forms of adenitis which are associated with various infectious and simple inflammatory processes; we omit the so-called rheumatic bubo which appears in otherwise perfectly healthy individuals, without discoverable cause, most frequently in the axillary and inguinal glands, and which often terminates in suppuration—in fact, only those affections of the gland are here discussed to which the term scrofulous is usually applied, and these, we assert, are due to the specific action in them of the tubercle bacillus. The diagnosis in the early stage is extremely difficult, indeed sometimes impossible, between the tubercularly infected gland and the simple form of adenitis which results from some local irritation. Thus the glands in the neck may enlarge from the irritation caused by a carious tooth, or an attack of tonsillitis, or a simple cold. But as the irritating cause disappears the swollen gland will subside, though sometimes it takes a long time to do so. But in the tubercular gland it is quite otherwise. The irritating cause may disappear or have been so slight as to escape notice, but the infected gland remains stationary for a time or continues to enlarge, and presently we find other glands following in its wake, and enlarging too. In the early stage our only means of diagnosis is to wait, and what was at first obscure will soon be cleared up by the future progress of the case.

Now, to understand the process of infection in the cases referred to, we must for a moment look at some of the characteristics of the bacillus itself. It cannot develop in a temperature below 86° or above 106°; therefore, outside an animal body it cannot grow in this climate. Inside the body it grows and multiplies. A bacillus takes a month to reach its fullest development. It multiplies by fission and by the formation of spores. These spores are much more resistant than the bacillus itself. Drying the bacilli or the spores does not destroy them. It is not therefore difficult to understand how easily the infection may spread. The sputa of phthisical patients, for example, swarm with them. When these sputa dry, the bacilli and their spores, in a dry but active state, are scattered far and wide in the air, and may come in contact with a suitable soil whereon to develop. An ordinary healthy individual does not present this soil. The bacilli may be inhaled into his lungs, may be swallowed with his food, or mixed up in his saliva—they may even lodge on some abraded mucous surface, and yet may do no harm. The antiseptic power of healthy living tissue is fully recognised. But there is undoubtedly some defect of constitution, some "vulnerability" of the tissues, which we do not understand, which furnishes a fruitful soil. Call scrofula, if you will, the peculiarity of the soil on which the bacillus grows; but the seed itself and its growth, and the manifestations of its development, let us call tubercle. We shall thus have clear ideas, at least, as to what we mean; we shall not continue to confound together the soil, and the seed which grows on it. Now, look at the behaviour of the bacillus when once it finds a local habitation congenial to it. As I have said, a bacillus requires nearly a month for its full development, and shows hardly any signs of growth under a week or ten days. During this period they may be cast off in the bronchi by the action of the cilia; in other regions they may be expelled with the secretion of the part. But suppose they gain an entrance through some abraded part, they are taken up by the lymphatic vessels and carried to a neighbouring gland, or they may develop in the tissue where first they found a resting-place. In the case of the glands in the neck, we can generally trace the history back to some such local infection. Thus a child suffers from eczema of the head, and the child is of a delicate constitution; we are told it is scrofulous. Or the child has a purulent otorrhœa or ulcers in the mouth, or it suffers from a discharge from the nose. Later on a gland in the neck, usually only on one side, enlarges. Or a young adult, and sometimes even one more advanced in life, suffers from a cold, from an attack of tonsillitis, or from enlarged, sometimes caseating, follicles in the pharynx (and I have not infrequently traced it to this cause); and even after this ailment has subsided, and is perhaps forgotten, a gland or more begins to swell. Long after the exciting cause has ceased to act, the gland either remains *in statu quo*, hard, nodulated, movable, or it may develop rapidly all the series of phenomena which are known as scrofulous. Other glands enlarge, and frequently we find all the glands on one or on both sides of the neck sharing in the process. This whole series of events is explained by what we know of the behaviour of the tubercle bacillus. By some such channel as I have mentioned the bacillus enters; it is taken up in the lymph streams, and is arrested by the first lymphatic gland it meets. In such cases too much importance cannot be attributed to the functions of the lymphatic glands. They act as defensive outposts, as filters to free the lymph of infective and deleterious matters which it may contain, but in doing so they themselves suffer. In process of time the bacillus grows within them, multiplies, and gives rise to the formation of true tubercle. These tubercles become yellow in the centre, they coalesce, and in process of time form a caseous mass. At this period one of three things may happen—in other words, the gland may follow one of the proverbial "three courses." First, it may get well, though this is not a frequent result. Secondly, it may suppurate,[a] make its way

[a] This suppuration is not a necessary result of caseation, but is probably due to the entrance of some form of micrococcus by the same or some other channel as that by which the bacillus entered.

to the surface, and discharge its contents, and this may end in cure. Or, thirdly, it may extend, infecting gland after gland. Even here, too, nature may accomplish a cure by a slow process of successive suppurations, which may eventually eliminate the disease. But should nature fail to do so, the last gland between the bacilli and the general circulation may become infected, the last outwork of defence may be carried, and then the whole system is exposed to general infection, and a more or less acute general tuberculosis results. But what is to determine which of these three courses shall be followed? It seems to depend on one or other of two factors—firstly, on the number of the bacilli which have gained access to the gland; secondly on the constitution of the patient. Koch has shown that it is of the greatest significance whether the infection has been produced by a few bacilli or a large number. In his inoculation experiments, he found that a large number of bacilli, after first producing a diffuse caseous infiltration, both at the site of injection and in the neighbouring glands, gave rise rapidly to general tuberculosis. If the number of bacilli introduced were smaller, the length of time which preceded general infection became longer; and when the bacilli were few, this period might be very long, or in some cases generalisation might not occur at all. In spontaneous tubercular infection in man the dose is usually small, though sometimes it is often repeated; and hence it is that in tuberculosis of the glands in the neck general infection of the system most frequently does not occur. The constitutional condition of the patient plays a part only second to the strength of the dose received. I have already alluded to the fact, recognised by all, that a certain state of the system—a certain constitutional susceptibility—is necessary in order that an individual may contract tubercular disease; that is, that every person does not offer a suitable soil for the growth of the bacillus. What is equally true is that persons may possess this vulnerability in different degrees, and, moreover, that the same person does not afford at all times the same suitability of soil. Clinical experience has taught us this over and over again, and upon these axioms are based all our constitutional methods of treatment, the importance of which in every case cannot be over-estimated. In every case, whether we adopt surgical measures or not, we should aim at so improving the health of the patient by those means, which it is not within the scope of this paper to discuss, that to the best of our ability we shall put him into a condition to contest successfully with his unseen foes.

Taking, now, these two factors, we see that even when a gland has become infected we may hope for spontaneous resolution, provided that the infecting dose has been small, and provided that the patient's constitution has so improved that the gland tissue is healthy enough to cope with the bacillus. Now, what do we observe in such a case? The bacilli gradually disappear from the caseous mass, and may not be finally found at all, and the broken-down tissue may be absorbed, or it may calcify, and recovery ensue. This is, unfortunately, a rare occurrence. But supposing these factors are not sufficiently strong to bring about such a desirable end, the patient may eliminate the disease by a process of suppuration. The caseous masses in the gland—and they are usually multiple—begin to show evidence in the centre of suppuration. This may go on to a considerable extent before any external symptoms of suppuration appear. So that because a gland is hard and movable, and is not tender to touch, we cannot assume that that gland does not contain many central abscesses of small size. Gradually these abscesses coalesce; the gland becomes tender; it enlarges more and more; inflammation extends to its capsule and to the neighbouring parts, and soon adhesions form. By slow degrees the skin over it becomes involved; it becomes firmly adherent; it thins; the abscess opens and discharges, and a sinus is left, which may, and generally does, keep open for weeks or months, discharging a thin pus with cheesy matter contained in it; and when it heals it leaves behind it an ugly and disfiguring and puckered scar. But, even so, the patient is well quit of his disease. More frequently, however, the bacillus has made its way into neighbouring glands. One by one these enlarge, and the same slow process of suppuration begins; another sinus forms through unhealthy purplish skin, and another scar is left; and the cycle is repeated till the whole neck is seamed and unsightly. Is not this a true picture of the disease, and one with which we are too commonly familiar? If more were needed to paint it in its true colours, let me give it in the eloquent words of Dr. Clifford Allbutt:—"That the ugly finger of scrofula should be laid chiefly upon children, young men, and maidens, has this pathos in it, that it disfigures them at the spring-time of life—at that time when hope and promise make all life precious, and all death seems the loss of untold treasure; when beauty and gaiety have their fleeting day, and for the loss of them the world is poorer."

But even while this local process of decay is going on the general system is not safe from its ravages. As long as these caseous masses of tubercle exist in the body, so long must we bear in mind that the danger of generalisation is possible. But even if we were sure that by repeated and prolonged suppurations the danger of extension were removed and the bacilli expelled, the scarring of the neck is not the least consequence which may follow. Remember that this disease would never have originated but for a peculiar constitutional susceptibility. Will that susceptibility be diminished by months or years of suffering? Will it not rather be enormously increased? During this period "septic matters are absorbed into the blood, recurrent and variable hectics dissipate the appetite, flesh, and strength, so that the patient finds himself at the end of it all, if not unsound in his internal organs, at any rate a far worse man than he would have been had this trial been spared him."[a] Let him become again exposed to tubercular infection, let him breathe impure air impregnated with tubercular poison, and who shall say that he will escape with healthy lungs? It is believed by some that in subjects predisposed to scrofula the involvement of the glands in youth affords them some sort of protection against phthisis subsequently. But this view cannot be upheld. Only the other day a girl came into hospital with extensive disease of both lungs, and her neck was seamed with old scrofulous scars. The worst case of tuberculous glands in the neck I ever saw was in a girl who died of phthisis.

Such, then, being the nature of these so-called scrofulous glands,

[a] Clifford Allbutt. Op. cit. P. 12.

and such the consequences which we have to face, I think I am justified in asserting that the time has come when the treatment of this disease, except in its earliest stage perhaps, should pass from the domain of the physician, and should be entrusted to the surgeon. In the earlier stage, while still the diagnosis remains uncertain, the treatment is one of expectancy, tempered by constitutional measures. But when once the diagnosis is assured, and there is no evidence that resolution is likely to take place, bearing in mind the nature of the disease we have to deal with, and the consequences which may follow from waiting too long, I am strongly convinced that the surgeon's art should intervene to assist Nature, and to do that quickly which unassisted she would do if she could, but often fails in accomplishing, or only succeeds when her victory has cost her dear. When as yet the poison has infected but one or two glands, we have good grounds to hope that by their removal we may eliminate from the system every focus of infection. Every case is not suitable for the same method of treatment, and in some cases we have little choice left to us as to what method we shall adopt. Of all the various surgical methods at different times suggested, there are three which I shall specially refer to as being the best. Each one is applicable to a different class of cases, and, although they have been applied indiscriminately by their special advocates, I think there are well-marked lines which should divide those cases in which a special method is applicable from those in which it is not. These three methods are—(1) scooping, (2) cautery puncture, and (3) excision.

Of the first method—scooping—I need say but little, as the subject has been very fully and ably discussed in a lecture by Mr. Teale, which was published last year.[a] I shall merely quote a few of his conclusions, with which I entirely agree. 1. "That surgery can secure the healing in a few weeks of gland cavities and sinuses, even though they have existed for years, and of wounds resulting from the removal of caseous and suppurating glands." 2. "That in dealing with sinuses, gland abscesses, and

[a] On Scrofulous Neck, by T. Clifford Allbutt; and on Surgery of Scrofulous Glands, by T. Pridgin Teale. 1885.

decayed or semi-decayed lymphatic glands, the action of the surgeon must be vigorous and thorough." 7. "That in dealing with a sinus the channel should be enlarged by the knife or by Bigelow's dilator, and the whole of its granulating surface should be scraped off. Where a sinus is shallow and covered by thin blue skin this imperfect covering should be rasped away by the scraper, and any cutaneous overhanging edges should be trimmed off by the scissors." 8. "That, in dealing with a sinus or an abscess, the surgeon should not rest content until he has discovered and eradicated the gland, always remembering that, if it be not obvious, there is sure to be a small track leading through the deep fascia to the missing gland. This opening should be enlarged so as to admit the spoon of Lister's scraper." These conclusions which I have quoted explain the classes of cases to which, I think, this method is suitable, and to which I believe it should be restricted. These classes embrace cases in which sinuses exist, or in which a superficial abscess communicates through an opening in the cervical fascia with a suppurating and caseous gland beneath. It is also sometimes a good treatment to adopt when the changes going on inside a gland have given rise to inflammation of its capsule and to the formation of firm adhesions between it and its surrounding tissues. For in these cases enucleation of the entire gland is extremely difficult. I have employed this method in many cases, and my experience of it has strongly impressed me with the belief that the classes of cases I have mentioned are those to which the method should be restricted. In all of these cases the method of excision is fraught with too much difficulty to authorise us to adopt it in preference to this apparently simpler plan; but, on the other hand, it should not be extended to cases in which enucleation can be readily performed, and for this reason:—In most of these tuberculous glands the gland capsule shares in the disease, and in the scooping operation this is necessarily left behind; moreover, there may be, and there frequently are, enlarged and tuberculous glands in the neighbourhood which have not yet softened. These escape the spoon, and later on become the foci from which fresh infection starts. Of much less importance is the fact that the incision after enucleation seldom takes more days to heal than that after scooping takes weeks. On these grounds I hold that the scooping operation should *only* be done in those cases in which excision is inapplicable.

The second method of treatment is that by cautery puncture. It has found its chief advocate in these countries in Mr. Treves,[a] and it has also found much favour in France. Mr. Treves expresses the opinion that it is "one of the very best operative measures at the disposal of the surgeon for the cure of scrofulous glands." He thus describes it:—"In this operation I make use of a thermo-cautery point about as thick round as a No. 7 catheter. This point, having been heated to a bright-red heat, is thrust through the skin into the substance of the gland, and passed in three or four directions in the body of the tumour before it is removed. . . . If no pus or cheesy matter follows the removal of the iron, a simple zinc dressing may be applied, but if any such matters escape, then a poultice should be ordered. . . . It is more adapted for adherent than for movable glands." This method I have lately employed, and in certain cases I think it should be productive of good. Its *modus operandi* would seem to be that the intense heat destroys the tubercle bacilli and their products, and it leaves a channel for their escape externally which does not allow of extravasation into the neighbouring cellular tissue. It seems to me that in softened adherent glands it would probably be more effective than the method of scooping.

The third method of dealing in a radical manner with these tuberculous glands is by excision. It is specially suitable in those cases where the glands are not adherent. The operation is easier the harder the glands are—that is, the less advanced they are in the process of suppuration. The great advantages I claim for this method are that we can in most cases entirely remove all the affected parts, including the capsule of the gland. The wound is a clean incision, mostly through healthy skin, and therefore, with strict antisepsis, heals by first intention, and leaves a scar which in process of time becomes white and often unnoticeable. When the glands are non-adherent, they can be enucleated with great

[a] Scrofula and its Gland Diseases. P. 193. 1882.

ease. On the 8th of this month, through an incision an inch and three-quarters long, I removed eight glands, three as large as walnuts, and all of them caseous. Seven days after, the wound was healed, and the scar even then scarcely noticeable. But when the tubercular process has been allowed to run its course for a long time, and adhesions have formed, then the real difficulty is experienced, and often a tedious dissection is required before the diseased glands can be removed. And here let me say that this difficulty is often of our own making. We meet a case in which two or three glands in the neck are swollen and hard. A careful inquiry into the history of the case will satisfy us that the glands are scrofulous. But instead of dealing with them at once, we allow the disease to spread, more glands to become infected, and adhesions to form, whilst we wait. Wait for what? To try the effects of this or that external application, painting with iodine, or a host of other remedies, which experience should have taught us in that stage are no remedies at all. Sometimes the delay is due to our unwillingness to make a cut which may leave a scar, and then nature takes the matter in hand—the gland softens, an abscess forms, and the end of it is that a far worse scar is left than would have resulted from timely intervention. Besides, all this time other glands are preparing to go through the same process. Such things ought not to be. What I maintain is, that the earlier we attack these glands the less we shall have to do, and the smaller and more insignificant the scar will be; and, what is more important still, the focus of the disease may be got rid of through quite a small opening. Often, however, we have at least this satisfaction, that we cannot blame ourselves if the disease has become extensive. It may be so when first we see the patient, but even in the most extensive cases of scrofulous glands we may hope for a cure by surgical art. I have notes of fifteen patients from whom I excised tuberculous glands. Two were operated on twice and two three times, making in all twenty-one operations. Some of these were comparatively simple cases, two or three glands only requiring removal. Some of the cases were most extensive, and of these three present points of special interest.

The first case was that of a boy aged seventeen, whose neck was enormously swollen on the right side, owing to the enlargement of the glandulæ concatinatæ and submaxillary glands. I operated on him twice. The first time was on June 12th, 1883. A curved incision, about four inches long, was made behind the sterno-mastoid muscle, and all the enlarged glands removed. They were all caseous, several presenting centres of suppuration in the middle of the cheesy masses. I mention his case because he was attacked with erysipelas three days after operation. The erysipelas began low down on his back, spread gradually over his whole body, with the exception of the parts underneath the antiseptic dressings. This part remained healthy throughout, and the wound healed well. Some months after I removed the submaxillary glands. A similar mishap occurred in a second case, but both made perfect recoveries.

The second case I wish to mention was that of a married woman, aged twenty-three, who presented a long chain of glands extending from behind the left ear to the clavicle. They were movable and hard in the upper two-thirds, but below they were soft and suppurating. I excised them all at one operation on April 3rd, 1884. The abscess cavity was scraped out, and a drainage-tube inserted through a counter-opening close to the clavicle, the wound being itself completely closed. The dressings were not disturbed till the seventh day, when the wound was quite healed. The drainage-tube was removed, and the sinus was completely healed on the fifteenth day. She left hospital three days later. I saw her again a few months ago, a year and three-quarters after operation. There were no more enlarged glands, and the incision was reduced to a fine white pliant scar.

The last case to which I shall refer is that of a girl aged eighteen, who was studying to become a schoolmistress, but was rejected on the ground of health, as the left side of her neck was bulged out with a large number of swollen glands. She was greatly disfigured. I operated on the 29th of April, 1884. The incision reached from the mastoid process to within an inch of the clavicle. All the superficial and deep glands were involved, those lying underneath the sterno-mastoid muscle being very difficult to enucleate. I removed in all forty-eight glands at this operation. She made a slow recovery, but a good one. I saw her the other day, nearly two years after operation. There is a long scar, but it is soft, white, and flexible; and she can so arrange her hair as to make it almost imperceptible. She is in excellent health, and has had no return of the disease; in fact, as we might expect, it is not possible to feel any glands in that side of the neck. I mention this case especially as showing that excision can be safely done, even in such an extreme case.

Before concluding this paper, there is an objection which I should like to answer—an objection which I have heard raised, especially in regard to these extensive operations; and it is this—Is it not a very serious matter to interfere to any great extent with the lymphatic system in the neck? That objection is entirely theoretical; I have not seen in any of the cases on which I have operated any evil consequence referable to this cause. Besides, we must bear in mind that caseous glands are, to all intents and purposes, functionally destroyed. If there is any reason in the objection at all, the blame must be left at the door, in the first instance, of the tubercular affection; and, secondly, at the door of those, be they doctors or patients, who allow the disease to progress to such an extent—a disease, which, I trust, I have shown can be arrested in its early stages by the timely and judicious intervention of surgical art.

A CASE OF UNUNITED FRACTURE OF THE HUMERUS, AND A METHOD OF TREATMENT THEREOF BY MEANS OF METAL SCREW-TAPS.

By HENRY FITZGIBBON, M.D., F.R.C.S.
Surgeon to the City of Dublin Hospital.

[Read in the Surgical Section, May 21, 1886.]

The occurrence of pseudarthrosis or non-union of the shafts of long bones is fortunately not very common, but when this condition is met with, especially in the humerus or femur, the limb is so useless, and a source of such distress to the patient, that he seeks operation in the hope of having it rectified. Hitherto the proportion of success which has attended these operations upon the humerus has been far from satisfactory, and the number of failures so large as to make it a question of anxiety and doubt whether the prospect of success is, in the majority of cases, sufficient to justify the undertaking.

The objects of all the proceedings which have been suggested or adopted have been the excitement in and about the fractured bone of reproductive inflammation, together with the co-aptation and subsequent fixation in close proximity of the fragments for a sufficiently long time for adhesive inflammation to be set up in the bone itself, in its periosteum, and in the surrounding soft parts, and for the formation of lymph and plastic matter towards the production of callus and union. With these objects friction, violent rupture of the fibrous union, Malgaigne's needles, the seton, and Dieffenbach's operation by perforation, were designed.

Resection, which was performed in 1760 by White, of Manchester, was condemned by Sir Benjamin Brodie; it has, however, been deprived of much of its terrors by the advancement of antiseptic surgery within the last ten years, and may now be undertaken with comparative safety. I need not refer to the numerous plans that have been devised for fastening together the resected ends of bones by wires, sutures, and pegs, to all of which there is the objection of the difficulty of introduction and often the impossibility of removal. These difficulties are greatly diminished by the instrument and method which I employed in the case I am about to report to the Academy :—

Case.—E. T., a woman, aged about fifty years, sustained a fracture of the humerus, on the 12th of March, 1885, by direct violence from a fall against a rail. It was situated in the upper third of the bone below the insertion of the teres major, a comminution being displaced inwards and backwards by the action of this muscle. To the pressure caused by the displacement of this fragment against the brachial vessels I attribute the failure of the bone to unite in a normal manner. The circulation below the seat of fracture being seriously interfered with for a considerable time, the lower portion of the bone had not sufficient vascularity to develop reparative inflammation, although the limb was skilfully and carefully put up by Mr. Wheeler upon the day after the injury, when the woman was admitted into the City of Dublin Hospital. She came under my care upon the 22nd of April, five weeks after the occurrence of the fracture; there was then no attempt at union of the bone. Subsequently, as it refused union in the Hospital, I sent her to the sea-side with a fixed apparatus upon the arm.

On the 14th of November last, nine months after the injury, she returned to the City of Dublin Hospital, the bone still ununited and the limb useless. As she determined to have an operation performed, I selected to resect the bone and to attempt to notch the fragments, so as to overlap, as Volkman has done, and then fix them by means of long silver or steel screw-taps, made sufficiently long to allow their heads to remain exposed at the external incision, so as to facilitate their subsequent removal. I had drills and screw-taps made to correspond accurately in size, fitted to a brace by square heads, so as to be changed with ease for each other, as required. I performed the operation on the 26th of November, making a free incision over the seat of the fracture, along the external and posterior aspect of the arm. There was some difficulty in clearing the end of the lower fragment without injury to the musculo-spinal nerve, which came into view. Having dissected off a mass of fibrous attachment, I succeeded in turning this fragment freely out of the wound; but I found it impossible to do so with the upper portion of the bone, owing to its shortness and to the fact that a comminution which was united to it had also become so matted to the brachial vessels that I could not, without great risk clear the bone from them. I was, therefore, obliged to abandon Volkman's operation, and, after removing their fibrous covering and attachments, I simply resected both ends so as to present a plane surface to each other. I then drilled each fragment at a right angle to the direction of the shaft and replaced the drills by long silver screw-taps, the heads of which remained in view at the wound. I warped a strong double silver wire round these taps close to the bone and strained the cut surfaces into apposition by means of a wire twister; I then dressed the wound antiseptically, and placed the limb upon a Middledorf's triangle, with an important modification which was suggested to me by Mr. Dallas Pratt, which was the addition of a flange for the arm to rest upon when the patient was lying down. On the twenty-first day after the operation I put the patient under ether, and removed the screw-taps by adjusting the brace upon them and reversing its action. I had no difficulty in fixing it upon the heads of the taps, and they came out with rapidity and ease, leaving the wire behind, which I afterwards withdrew through the wound, the bone remaining perfectly stationary until firm union had taken place. The screws retained their bite perfectly, up to the time of their removal, not being in the least loosened by suppuration or inflammation, as it has been stated is invariably the case when ivory pegs or bone screws have been used for fixing fractures.

She has been staying for some time in the country since she left the hospital, but she came to town at my request and attended here the evening of the last meeting of this Section of the Academy, when she was seen, and the arm examined by several Fellows and Members of the Academy, there being firm union of the bone, and all the motions of the limb restored.

Stemson mentions that MacCormac, in order to obviate the difficulty of removing wire sutures, had warped them in a figure of 8 round pegs put through the bone. Sir William MacCormac has kindly sent me the reports of this operation, in which it appears the pegs were ivory, and no mention is made of their being screws or being adapted to any mechanical appliance by which they could be easily withdrawn.

This instrument, which was made for me by Messrs. Booth Bros., is fitted with a number of drills and screw-taps, the latter being made to fit accurately into the holes when the drills are withdrawn. It was my intention, had I been able to notch the ends of the bones as suggested by Volkman, to have drilled the fragments in three or four points, leaving the drills *in situ,* so as to keep the holes in the fragments in apposition to each other, and then, by removing the drills one by one and replacing each of them by a screw-tap, secure the notched portions of the bone firmly to each other. I am satisfied that this may be done with ease, by means of the instrument before the Academy, in any case in which one portion of the bone to be united can be brought to overlap the other to any extent.

The use of Middledorf's triangular splint, modified in the manner I have shown, appears to me to have many advantages over almost any other appliance that has been described for supporting the limb and keeping it at rest after the operation.

The points upon which I venture to say that the introduction of metal screw-taps through the fragments of ununited fractures is to be recommended, are as follows:—They fulfil the objects of Deiffenbach's operation by exciting reparative inflammation in the bones which had become quiescent. They can be used as a means of warping wire suture so as to draw the fragments together, as attempted by MacCormac, over ivory pegs, while the thread of the screw keeps the wire from slipping up along them, as it is apt to do upon a smooth peg; and, above all, they can be removed by means of their adjustment to a reversible brace as soon as they have ceased to be of service in fixing the bone. I feel confident that in many cases of compound fracture in which the obliquity of the line of fracture renders it difficult—nay, often impossible—to keep the fragments from sliding upon each other, the introduction of a single silver or steel screw-tap would prevent the recurrence of displacement, and it could be taken out after very few days when muscular spasm had ceased and lymph commenced to be thrown out. Such cases are every day met with in our city hospitals, and I fully intend, when next I encounter one such, to make use of a screw-tap in the manner I have described.

ELECTROLYSIS FOR THE TREATMENT OF URETHRAL STRICTURE.

By P. J. HAYES, F.R.C.S. Edin.;
Surgeon to the Mater Misericordiæ Hospital, Dublin.

[Read in the Surgical Section, May 21, 1886.]

Stricture of the urethra is an affection so frequently encountered in practice, and, at times, so tardy to yield, when treated by even the most skilful hands, that I do not hesitate to bring before this section of the Academy my very brief experience of electrolysis—the comparatively novel method for overcoming urethral coarctation, practised by Drs. Robert Newman, of New York, and S. T. Anderson, of Bloomington.

My attention was first seriously attracted to electrolysis as an agent for effecting decomposition of what may be termed stricture-tissue, by Dr. Anderson's communication, published in the *Lancet*, December 5th, 1885. Since then I have tested, by practical application, the value of this procedure, and the effects produced so far I have recorded in reports of three cases treated privately and at hospital.

The results I have observed appear to me so encouraging that I strongly advocate trial of electrolysis, not alone in simple cases, but for even the most complicated forms of stricture. I base this general recommendation on the following grounds—firstly, the procedure will, at least sometimes (I feel disposed to say frequently), prove capable of effecting unhoped-for benefit; secondly, its application will not at all interfere with the subsequent employment of other measures—be they minor or serious—which the surgeon may deem advisable for individual cases.

Before entering upon particulars respecting the application of electrolysis, it may be well to give notes of the cases to which I have alluded.

Case I.—J. D., a young man, aged twenty-four, sent from the country by my friend, Dr. Darby, of Monasterevan, was admitted to the Mater Misericordiæ Hospital, December 28th, 1885.

History.—The patient enjoyed excellent health until about eleven years ago, when he suffered from "gastric fever." Within two years after this illness he began to experience slight pain during the act of micturition; also a few drops of urine used occasionally escape from the urethra without his being conscious of the fact. Later he was attacked on some occasions by what he termed intermittent fever, and during the periods he suffered from notable difficulty in passing water. The stream gradually became smaller, but, save for disturbances of febrile character, no distressing symptoms occurred.

In the early part of last August, when the patient was engaged working a mowing machine, the driving seat gave way, and he fell heavily to the ground, his perinæum striking the edge of the iron seat which accompanied him in his fall. He felt much hurt by the impact, but there was not any discharge of blood from the urethra.

Almost immediately after this accident micturition became frequent, painful, and attended with increasing difficulty. Some weeks later a medical practitioner saw the patient, but he was not able to effect the passage of any instrument through the urethra.

Dr. Darby was consulted about the case early in December, when symptoms had become rather urgent. At his instance the patient was placed under my care in hospital. On admission the young man was carefully examined by Dr. Conway Dwyer, House-Surgeon, and an extremely tight stricture, which permitted the escape of urine in drops only, was found at a distance of five inches and a half from the meatus. After many attempts Dr. Dwyer succeeded in passing a No. 0·5 bougie through the stricture. This instrument was retained *in situ* during twenty-four hours; then it was found possible to introduce a No. 2 catheter. The catheter was tied in during part of the day, but towards evening symptoms of urethral fever appeared, necessitating removal of the instrument.

Three days later evidences of febrile disturbance disappeared; therefore, on January 6th, I endeavoured to slip a filiform bougie through the stricture. My efforts proved fruitless, and Dr. Dwyer was equally unsuccessful, although several trials were made by him at different periods of the day. Business having called Dr. Darby to town, he saw J. D. with me. The opinion then formed was that external urethrotomy would be required to insure sufficient relief. Whilst reflecting on the circumstances of this case the idea occurred to me that I should give electrolysis a trial before subjecting my patient to the risks inseparable from a cutting operation. Consequently I directed Messrs. Fannin & Co. to construct for me a urethral electrode, and to provide a small Leclanchè battery containing twenty-one cells. I again—January 10th—vainly tried to make some impression on the stricture, using, in turn, various sounds and bougies. Then I brought the urethral electrode, which had been connected to the negative pole of the battery, into close contact with the stricture, whilst a sponge electrode, belonging to the positive pole, was applied to the region of the groin. A current, at first from ten cells, was employed, and as the patient did not experience painful sensations the number of cells engaged was gradually increased to eighteen. After the lapse of about fifteen minutes I found that slight pressure exerted by the electrode against the seat of stricture caused a sense of yielding; at the same time, I noticed two or three spots of blood-stained froth appearing at the meatus.

The current was discontinued, and gentle manipulation enabled the electrode to pass easily into the bladder. Elated with the anticipation of being able to effect rapid dilatation, as the sequel of electrolysis, I committed the error of tying in a No. 5 catheter; but twenty-four hours later the development of febrile symptoms, as on a former occasion, required its withdrawal. Under appropriate treatment symptoms again subsided, and on the eleventh day after operation I passed for the patient in quick succession bougies 6, 7, 8 of the English scale. A week later I introduced with little difficulty Nos. 8, 10, 11, and the patient left hospital on January 28th.

In order that progressive decomposition of all cicatrical tissue should be effected by the occasional use of larger electrodes, J. D., at my request, visited me on the following dates—March 16th, April 20th, April 30th, and May 11th. At the latest visit J. D. told me that for some time back he believed the stream passed per urethram was better than it had been twelve years ago. I found a No. 9 electrode could easily traverse the stricture; therefore its point was kept at the narrow part of the canal during eight minutes, whilst a current from twelve cells was allowed to act.

I may here mention that at previous *séances* six, nine, twelve, and fifteen cells were brought into action—the rule, however, being a nine-cell current, having an average duration of twelve minutes.

I here wish to call attention to errors in my practice. At the first *seance* I used a current stronger than was needful. I ought not to have passed a catheter after the operation; and again I did wrong in using bougies. Once electrolysis has been found suitable for overcoming the resistance of a stricture, other treatment becomes certainly superfluous, and probably injurious.

Case II.—M. G., aged thirty-nine, came to me, March 2nd, by direction of my friend Dr. Whitty, of Waterford, for treatment of a stricture situated five inches from the urinary meatus.

The history given was briefly as follows:—Seven years back, after repeated attacks of gonorrhœa, progressive difficulty of micturition was observed. Ultimately this trouble became so pronounced that, according to the patient's statement, eight minutes would elapse between the commencement and termination of the act.

M. G. remained for a considerable time under the care of the late Dr. Cavet, who treated the stricture by gradual dilatation. This process was subsequently conducted by Dr. Whitty with so much success that fourteen months ago the urethra would admit a No. 12 bougie.

During the past year the patient neglected his stricture, chiefly because the introduction of instruments usually caused him pain, followed by some bleeding from the urethra. As he at length desired to be relieved—from symptoms which had recurred—if possible by means other than gradual dilatation, Dr. Whitty sent him to me.

On examination I found the stricture capable of receiving the bulb of a No. 5 electrode; consequently I employed a current from at first six, then nine, and ultimately twelve cells. The duration of the *séance* was ten minutes. Soreness of the urethra was experienced during the act of micturition for twenty-four hours after operation, when it quite disappeared; and as the stream passed per urethram was of good volume, the patient was not subjected to further treatment until April 30th, when a No. 8 electrode was used with a twelve-cell current. M. G. came to town again on yesterday, and this morning I employed at first a No. 7 electrode, and subsequently a No. 9, with a current from eighteen cells. Pain was not experienced save when, towards the end of the *séance*, I made some pressure against the posterior extremity of the stricture. I shall keep this patient in town for a repetition of the procedure, to be made eight days hence.

Case III.—O. N., aged forty-two, acquired stricture eighteen years ago. He has been occasionally treated by me since 1879. Two strictures exist. The first is situated six inches from the meatus, the second nearly half an inch farther back. Both strictures are of resilient character, and heretofore required to be dilated from time to time by the gradual method, followed by occasional passage of a bougie entrusted to the patient.

Two months back I substituted a No. 5 electrode for the instrument ordinarily employed, and having established a twelve-cell current, I allowed it to act during ten minutes. The patient felt so comfortable since the operation that he did not think it necessary to visit me until he received my request that he should come at intervals, until, with the aid of larger electrodes, the stricture substance should be reduced to a membrane-like cylinder. On yesterday I devoted twenty minutes to acting on both strictures with a No. 7 electrode, the number of cells engaged ranging from nine to fifteen. The patient experienced no discomfort during the performance—"Indeed," he said, "the sensation was not at all so bad as that of having a solid instrument passed."

I was invited to try electrolysis with a fourth patient, the subject of an extremely tight stricture, impervious to instruments, and complicated by false passages.

This stricture, which was, in a measure, of a traumatic nature—internal urethrotomy having been practised some years previously—did not yield before the electrode, and I have had no opportunity of learning from the gentleman who has charge of the case whether the smallest benefit resulted from our efforts. I only allude to this circumstance because it is as necessary to report failure as to record success when criticising a more or less untried mode of treatment.

With regard to history, so far as I have been able to ascertain, electrolysis was first used for treatment of urethral stricture by Willebrand and Wertheimer. Then Mallez and Tripier treated several patients with encouraging results. Keyes, Beard, and

Rockwell, also experimented with electrolysis, but all the earlier operators seem to have employed strong currents capable of producing escharotic effects, as evidenced by frequent discharge of blood and charred tissue from the urethra.

Dr. Robert Newman, in 1874, appeared as the advocate of an improved method for employing electrolysis in urethral surgery—the main features of his practice being the use of very mild currents, just perceptible by the patient, allowed to act during periods of about five minutes, the *séances* to be repeated at intervals of two or more weeks, so long as treatment might seem needful.

Dr. Newman devised bougies provided with terminal metal bulbs of various sizes, and having measured the precise distance of the stricture from the meatus, he marked the interval by slipping an India-rubber ring over the bougie or electrode, so that when the instrument was passed sufficiently far to secure contact of the bulb with the stricture, the rubber ring should touch the meatus, thus insuring electrolytic action for the stricture alone.

Recently I had the advantage of holding direct communication with Dr. Anderson; the outcome of his experience I have incorporated with other observations in the short summary which forms the conclusion of this paper.

Electrolysis is best adapted for the treatment of annular strictures rather limited in length, and it should be employed as follows:—The situation of the stricture having been ascertained, the tip or exposed metal bulb of an electrode ought, if possible, be lodged within it. Should this measure prove impracticable, the tip must, at all events, be maintained in close contact with the anterior face of the stricture. Then a small galvanic battery is to be connected by its negative pole with the urethral electrode, whilst the positive may be attached either to a moist sponge-electrode, or, better still, to a thin metal plate covered with moistened chamois leather. The positive electrode can with advantage be applied either to the patient's perinæum or against the inner side of one thigh.

I believe the best indication as to the strength of current to be employed will be the patient's own sensation. The current should be perceived, but it ought not be pushed to cause pain. I have found no difficulty about employing a current from twelve, fifteen, or even eighteen cells of the small battery now exhibited. My rule, however, is to rely chiefly on either a nine or a twelve-cell current. The guiding principle should be not to attempt more than it is needful to effect at each *séance*, to use mild currents, to manipulate the electrode with gentleness, and never try to push it through the stricture. The electrode will effect far more by being allowed to remain in the stricture than by being caused to quickly traverse it. The *séances* may be repeated every ten days if desired. Now, as to the action of the electrode. No doubt, cauterising will be produced when strong currents are employed, because caustic alkali must be set free from the decomposed tissue around the negative electrode; but when mild currents only are allowed to act, it would seem probable that a gradual breaking-up of fibroid tissue can be effected by a combination of chemical decomposition and vital absorption until but a thin lamella of cicatricial tissue remains to mark the seat of stricture. Much and varied observation will be required before this supposition can be substantiated or negatived.

The argument I have here attempted to support is not that electrolysis ought to be set up in antagonistic rivalry to other well-established modes for dealing with strictures, but that it fully merits careful trial; also that with patience and attention to the details I have outlined, many strictures can be expeditiously and safely rendered permeable where, in the absence of this agent, urethrotomy would be required and performed with perhaps no small degree of risk to the patient's life.

OBSTETRICAL SECTION.

AXIS-TRACTION IN INSTRUMENTAL DELIVERY, WITH DESCRIPTION OF A NEW AND SIMPLE AXIS-TRACTION FORCEPS.

By WILLIAM C. NEVILLE, M.A., M.D., Ch.M.;
Master in Obstetric Surgery, Univ. Dubl., &c., &c.

[Read in the Obstetrical Section, November 27, 1885.]

That the principle of axis-traction in delivery by the forceps has hitherto been so little appreciated, except by specialists, is, I think, chiefly due to the comparative cost and complexity of the special instruments required rather than to any fault in the principle itself. Some weight must also be attached to the hesitancy felt by most men to abandon old grooves of thought or action. And this feeling is all the more natural in the present case, where it is sought to replace a simple and time-honoured instrument by one which *primâ facie* is much less easy to manipulate, and which (in the beginning at least) claimed acceptance upon the grounds of theory rather than of long and well-tried utility. Whatever its defects, the old double-curved forceps had served as well, preserving through all its modifications the same general construction that it had when first adopted with a curious simultaneousness by Pugh, Levret, and Smellie. But the more useful and the more essential an instrument is, so much the more ought we strive to eliminate its defects and to add to its powers. Nor need we be discouraged by the number of workers or the multitude of their failures.

Certain defects of the double-curved forceps have long been recognised, and quite a number of more or less successful attempts had been made to neutralise them before M. Tarnier commenced his work upon the improvement of the instrument. Those, however, who have studied his extremely able essays upon this subject cannot withhold from him the credit not only of having exposed with freshness and clearness the defects of the forceps as commonly used, but also of having been the first to construct one which answered to the true demands of theory. But Tarnier's forceps, even in its latest and most convenient form, is somewhat complex and expensive. Its general plan has been adhered to, and its construction simplified by Professor A. R. Simpson, whose modification has been endorsed by Drs. Barnes and Playfair. This is the axis-traction forceps chiefly used in this country, and the only one with which I have had personal experience. Modifications of Tarnier's forceps have been devised by Professors Hegar (Freiburg), and Lusk (New York), and others. Before dealing with the various questions concerning axis-traction forceps I shall state, as clearly as I can, the mechanical defects of double-curved instruments, such as that of the late Sir J. Simpson or of Dr. Barnes. Two principal defects may be specially considered:—

1. *It is impossible, in using them, to exert the whole of the traction force in the curve of the pelvic axis—i.e., along the curve of least resistance to delivery.* A certain excess of traction force must be exerted, some of the force being wasted in a vicious pressure upon resisting maternal parts. This fault is the mechanical result of the conditions under which traction is made with any ordinary double-curved forceps, whatever the position of the head at the time of its application. Let us take, for example, such a forceps applied to a head engaged in the pelvic brim. The mechanical conditions will be understood by referring to the accompanying woodcut. The forceps being correctly applied in the sides of the pelvis, the axis of its pelvic curve may be assumed as corresponding—quite sufficiently so for practical purposes—with the axis of the upper part of the pelvic canal. Traction being now made on the head, the traction force must, on mechanical principles, take effect in some such direction as is indicated by the line TF—*i.e.*, the axis of actual traction must fall more or less in front of the axis of desirable traction, TA. But descent of the head must take

place along the curve of the pelvic axis, CCC, or from its present position in the direction of the axis of the pelvic inlet, TA′A. Applying now the principle of the "parallelogram of forces," we may take TF′ = to the actual traction force used, and resolve it into the two component forces, TA′ and TP′—the former acting in the axis of the inlet, and the latter at right angles to it along the plane of the inlet. The component force TP′ represents a vicious pressure against the pubes, which could be avoided if we were able to exert the primary traction force altogether profitably in the direction TA. If we know the angle A′TF′, and the traction force

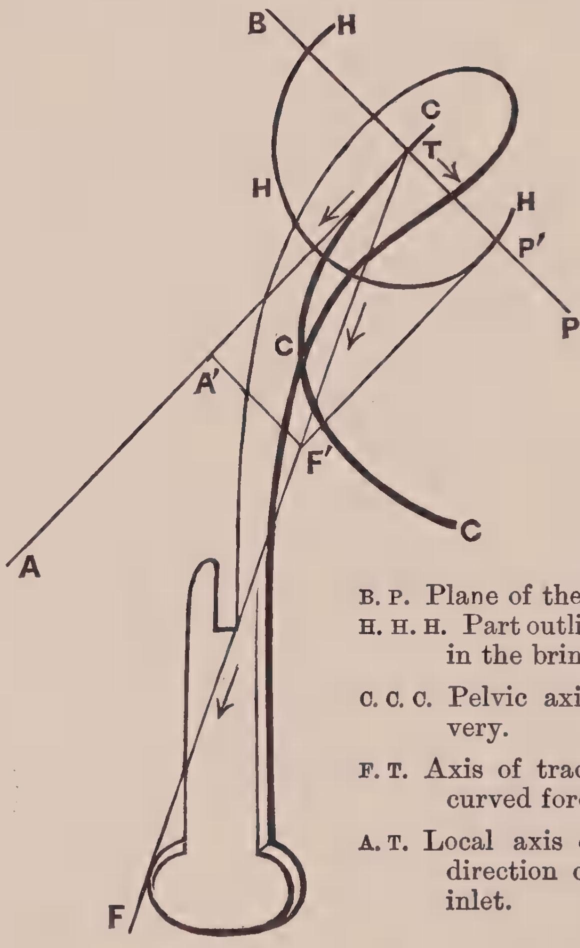


Fig. 1.

B. P. Plane of the brim.
H. H. H. Part outline of head engaged in the brim.
C. C. C. Pelvic axis, or axis of delivery.
F. T. Axis of traction with double-curved forceps upon point T.
A. T. Local axis of traction in the direction of the axis of the inlet.

TF′, we can evidently determine the actual amounts of the two forces TF′ and TP′. Tarnier, to whom this demonstration is due, calculated that a traction force of 40 kilogrammes applied to the head at the brim would be equivalent to a force of 30 kilogrammes acting in the axis of the inlet + one of 26 kilogrammes acting in the direction TP. In practice so great a waste of force can, it is true, seldom occur, at least when the forceps is in skilled hands. No expert would employ traction simply in a line continuous with that of the handles. But whatever corrective device he adopts can only give a haphazard correctness to the traction.

We see, therefore, that the force being misapplied, some of it is wasted in what must needs be a more or less injurious pressure upon maternal structures. Nor is the injury to the mother only. It follows also as a corollary from the preceding argument, that *the ordinary forceps is much more apt to slip off and bruise the fœtal head than such a one as would pull constantly in the curve of the pelvic axis.* For with the former the traction is applied slantwise in a direction downwards and somewhat forwards against an anterior (pubic) resistance. Hence, when this resistance is great, there is a likelihood that the forceps may slip forwards off the head; or, what amounts to the same thing, that the head may be forced backwards from between the blades of the forceps. This danger is obviated by an instrument which would draw upon the contained head directly in the proper axis of delivery.

2. The second defect of the ordinary forceps consists in the *restraint which it necessarily puts upon the head while traction is being made.* It may enforce movement in a certain direction, but in doing so it must also greatly hamper the movements by which the head tends to accommodate itself to the varying resistances which oppose its passage through the pelvis. Delivery is thus effected at the cost of extra force employed and extra pressure brought to bear upon maternal structures. It has been argued that even during traction the sensitive hand takes note of and even helps such movements of accommodation. I very much doubt whether, during difficult forceps deliveries, involving the expenditure of much muscular effort, such a degree of sensitiveness can be claimed even for an unusually well-educated hand; nor can we forget that the forceps is not always in the hands of an expert. But rotation most commonly occurs, nevertheless, despite the form of the traction which opposes it, and the accompanying rotation of the forceps handles informs the operator—not before but after—the commencement of the movement. It is quite impossible to contend

seriously that the operator can know from his sensations when and to what extent exactly rotation should occur. The most that can fairly be expected of him is that, when the head is rotating, care should be had not to hamper the spontaneous movement by too rigid a grip of the forceps handles. But the head is generally best left to pick its own way through the pelvis—hampered as little as possible by the forceps, which should provide motive power only.

What is wanted is a forceps so constructed as to admit of the whole force employed being exerted in the true axis of delivery, or approximately so, and which would, at the same time, leave the spontaneous movements of the head uninterfered with.

Many of the older obstetricians recognised the desirability of making the axis of traction coincide, if possible, with the axis of delivery, and felt the failure of the classical forceps in this respect. Attempts were then naturally made to remove or minimise this defect. One of the most commonly practised of these consisted in a manœuvre which still usefully survives. To the tractile force as usually employed is added another force, pulling almost directly backwards in the vicinity of and just above the lock. While the chief operator pulls on the handles in the usual way, an assistant pulls almost straightly backwards by means of a napkin looped round or through the lock of the forceps; or, in the absence of an assistant, the operator pulls downward, through the handles, with his right hand, and backwards, from the lock, with his left. The effect of these two forces simultaneously applied is to neutralise the vicious pressure towards the pubes which would result from the first alone. Their resultant is a single force acting more or less nearly in the proper axis. Such manœuvres, no doubt, are of much help in difficult cases, where the head is at the brim, and a proper axis traction forceps is not at hand. In such cases it enables the operator to try traction in some new axis at least, though not—save as a matter of chance—in the true pelvic axis. It is obviously too haphazard a method to rely on for finding the latter with anything like certainty; while, without an assistant, the operator's two hands being employed in making traction, he is unable to keep note of the head's progress, or to learn at once if the blades are slipping.

Towards the beginning of this century, Stein recommended the employment of traction by means of a fillet passed through the fenestræ as a corrective and supplement to traction by the forceps handles. Instead of the fillet, steel tractors, loosely fitted into the fenestræ, have of late been recommended for a like purpose. In 1844 Hermann, of Berne, devised a forceps, the special feature of which, in addition to a peculiarly accentuated pelvic curve, consisted in a T-shaped tractor which could be fitted to the shanks close above the lock. This tractor could be fixed at will, either on the anterior or posterior surfaces of the shanks, with the object of either pulling or pushing them backwards while traction was being made on the handles. Hermann's tractor was, therefore, really only a special appliance for carrying out the manœuvre already described. Hubert, of Louvain (1860), hit upon the plan of making a forceps with handles bent backwards at right angles about their middle. Subsequently Hubert did away with the bent handles, substituting in their stead a steel bar which jutted backwards from between the handles immediately below the lock. The principle of these forceps—that traction acts in a direct line from the point at which it is applied to that on which it acts—was exactly the same as that subsequently utilised (1868) in Aveling's sigmoid forceps. Indeed, Hubert expressly alluded to the advantages of a forceps having the general form of an Italian S, so that it is not a little surprising to find it stated in a recent "System of Obstetric Medicine and Surgery,"[a] that "the greatest share in the invention of the modern type of axis-traction forceps must be awarded to Aveling."

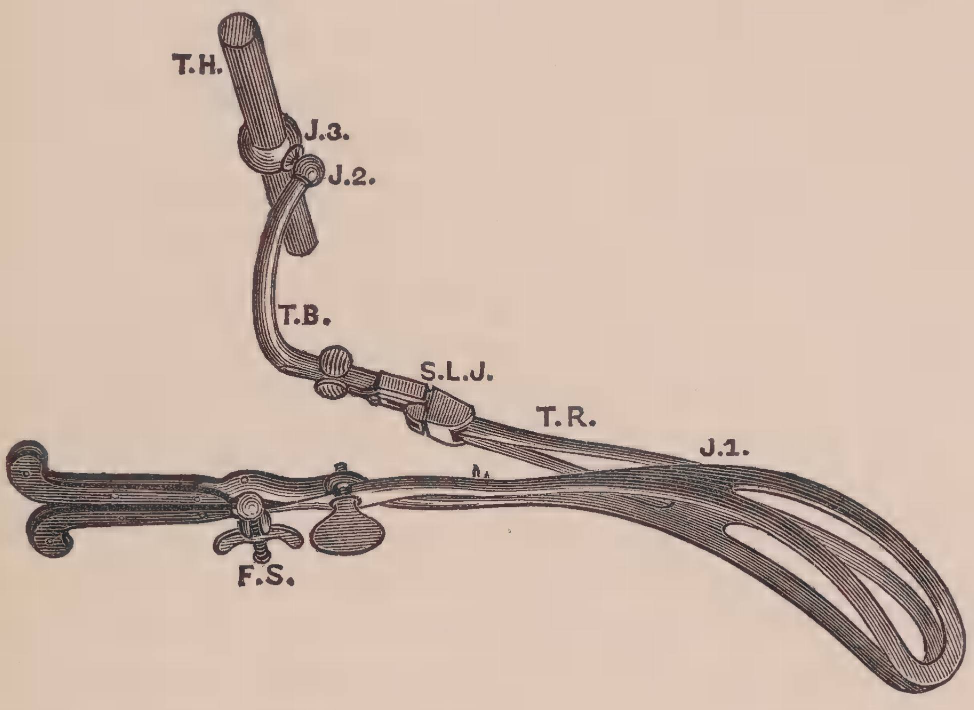
These few typical examples suffice to show how, before Tarnier's time, one defect of the classical forceps was commonly recognised. The common aim of these and other attempts at forceps reconstruction was to obtain an instrument by means of which the axis of traction could be brought, approximately at least, into coincidence with that of the pelvis. In this aim they

[a] By R. and F. Barnes. Vol. II., p. 510.

only partly succeeded. Such an example as Aveling's forceps will serve to demonstrate the cause of failure. If with this forceps traction be exerted from the very extremity of the handles, the line of the pull would, doubtless, be nearly correct. But, in practice, such traction is impossible, or almost so, with this instrument; the handles must be grasped over so large a portion of their extent as almost to obliterate their compensatory curve, and make the line of traction differ but little from what it is with common forceps. The recurved traction-bar or sharply reflexed handles of Hubert's forceps certainly gave a less delusive advantage. But none of these forceps gave any certainty, by their construction, that the axis of traction would really coincide with the axis of the curved blades—*i.e.*, practically with the pelvic axis itself; none of them eliminated the varying effects of side strains ("twists"), without which, in such cases, it is practically impossible to exert powerful efforts by traction applied over a large surface; and none of them even touched the important question of leaving the head unhampered, so far as its accommodation movements are concerned, during its enforced descent through the pelvis.

It is needless here to consider the various stages through which Tarnier's forceps went before reaching that in which it is now used by him and his adherents—suffice it to describe the latest and best model constructed in 1879. This consists essentially of an ordinary double-curved forceps (French type). Along the convexity of the lower part of the blades, just below the level of the fenestræ, two steel rods (T R) are attached by pivot-joints (J 1). These are the "traction-rods" which lie parallel with the shanks, and terminate a short way above the lock of the forceps. When not in use they can be kept fastened to the shanks. When the forceps has been applied and locked these rods are loosened and fitted into a peculiar rigid socket, or lock-joint (L), which is continued into a sharply recurved steel bar (T B) attached below, by what amounts to a universal joint (J 2 and J 3), to a transverse "traction handle." The whole instrument consists then of two parts with distinct uses—one, the ordinary forceps, which serves the purposes of prehension and application; the other, a specially constructed apparatus for traction. And, inasmuch as traction is made solely by the latter, a "fixation-screw" (F S) is necessary to keep the blades in firm opposition to the head while traction is being made.

Fig. 2. Tarnier's Forceps (1879).



T. R. Traction-rods.
T. B. Traction-bar.
T. H. Traction-handle.
J. 1. 1st joint.
J. 2. 2nd ,,
J. 3. 3rd ,,
F. S. Fixation screw.
S. L. J. Sliding lock-joint, uniting traction-rods to traction-bar.

Let us now examine the powers of this mechanical contrivance. Tarnier claims—

1st. That by it traction is made in the direction of the pelvic axis.

2nd. That the force acts as nearly as possible in the centre of the head.

3rd. That during delivery the head preserves its mobility unimpaired.

4th. That with it the operator has a constant guide as to the movements of the head, and the direction in which he is to apply his traction.

The construction of the forceps is such that so long as the traction-rods are kept parallel to the shanks of the forceps, the axis of traction must correspond with the axis of the blades—*i.e.*, approximately with the axis of that part of the pelvis in which the head is situated. Now, as the head descends into the pelvis, the application-handles of the forceps turn spontaneously forward under the pubic arch, being allowed to do so by pivot-joint J 1. All the operator has to do, therefore, is to follow the handles forwards, keeping the traction-rods parallel to the shanks. The joints J 2 and J 3 allow the head to rotate freely, even during traction, and the movement of rotation is at once indicated by the partial rotation of the application-handles. Hence, as shown by Tarnier, these handles not only indicate the direction in which traction is to be exerted, but also the movements undergone by the head during delivery. Supposing the forceps to have been properly applied—the axis of its curved blades coinciding with that of the pelvis—then it results that traction must be applied directly, and as nearly as possible to the centre of the head. In other words, traction is made directly upon the head in the line in which it is expected to descend, and not at an angle to that line, as has been proved to be the case with the ordinary forceps. Hence the less danger of the blades slipping or injuring the head during delivery.

Another advantage of Tarnier's model has not received the attention which it merits. This is, that the force is applied from a fixed point which cannot be altered or in any way affected by the operator, traction being made upon the cross-handles, and the force transmitted from this point directly to the blades, all side-strains being avoided by the universal jointing of the traction apparatus. Thus we know exactly the kind and direction of the force we employ—the latter, of course, being variable according to the angle which the traction-rods make with the shanks of the forceps. The nearness of the perinæum limits, however, our powers of varying the direction of traction. The mechanism of this forceps must be considered, therefore, as justifying its inventor's claims for it. We find in it a great and scientific advance upon all the older instruments. Its defects are in the matter of constructive details and not of principle. Keenly criticised as it has been, the number of its modifications already designed by leading obstetricians in all countries shows that its principles at least can no longer be successfully disputed.

Criticism, however, has not been wanting. In this case it has been of two kinds—dealing, firstly, with the principles; and, secondly, with the particular construction of the forceps.

Among critics hostile upon the question of principle, the eminence of M. Pajot entitles his observations to chief consideration. He objects to this forceps because—

1st. It is an instrument "deprived of all action as a lever, an action of greater efficacy than that of simple traction." The latter dogma cannot be argued here—suffice it to say that its correctness is much more than doubtful.[a] The former statement is misleading, since there is nothing to prevent a trial—should such appear necessary or desirable—of lever action (through the application handles), at the same time that traction is exerted through the special traction apparatus. I have employed both of these forces, with apparent advantage, while using Simpson's modification. In difficult cases it is well, from time to time, to test the hold the blades have of the head, and this can be done by using the left hand for traction, while the right one employs a leverage action through the application handles.

2nd. The proper direction for traction cannot be ascertained "when the head remains mobile above the brim, and does not yield to energetic tractions." Admitting this to be in a manner true, how is the ordinary forceps any better in such a case? Is not the correct axis of traction, under these circumstances, much more likely to be attained with Tarnier's than with the ordinary forceps?

3rd. The same answer may be made to the criticism that the application-handles cannot indicate the direction for traction when the head becomes impacted.

4th. M. Pajot admits that with this forceps traction may be made

[a] *Vide* Galabin, Obstetrical Journal, Nov., 1876.

almost in the axis of the pelvis, but regards this advantage as superfluous, " the pelvis itself being the true redressor of faults in traction." Few, I think, will agree with an argument which, if it means anything, means that it is superfluous to economise force and to save maternal and fœtal structures from a vicious pressure, because, despite all the waste and bruising which results from a misapplication of force, the pelvis may cheerfully be entrusted with the rectification of our mistakes.

The fixation-screw, a necessary adjunct to Tarnier's forceps, has been objected to as likely to cause a dangerous because continuous pressure upon the fœtal head. Now, this is a fixation and not a compression screw—attached close to the lock, where it has least power of compression. Nor should it be forgotten that the screw can be readily relaxed during the intervals between tractions, while, with an ordinary forceps, properly applied and locked, extreme compression is made all but impossible by the breadth and spring of the blades. But more important than any presumptions on such a subject is the fact that an immense clinical experience with Tarnier's forceps and its modifications has failed to sustain the objection made to the screw.

A more plausible objection than any of the above is that during delivery by Tarnier's forceps we lose to some extent that sentient knowledge of the grip and progress of the head which we get from a broad grasp upon the rigid handles of the classical forceps. It has been stated that with the axis-traction forceps the blades may slip slowly off the head without the operator being aware that they are doing so, because of their jointed traction-apparatus. But, as a matter of experience, borne witness to by many who have used both forms of forceps largely, this slipping occurs quite rarely with the axis-traction forceps when once properly applied. And even if slipping does occur, it is likely to be more gradual and in a safer direction during the use of this than of the ordinary kind of forceps. Under similar conditions the traction-force is less, and the jointing of the traction-apparatus affords an additional safeguard against sudden violence. The indicating handles show the progress of the head, at least as well as can be felt, even by the most educated hand, during difficult delivery, involving exhausting tractions with the common forceps. And, if the operator be in doubt, he can judge of the grip by feeling the head at intervals through the application-handles, even supplementing tractile with lever movement, as before explained. Moreover, it should be a rule in forceps delivery to take frequent note of the progress of the head and grip of the forceps during traction by means of vaginal examination. The force required to deliver, especially with axis-traction instruments, is seldom such as requires the continuous use of both hands for its employment.

The foregoing are the chief objections which have been made to Tarnier's forceps, and none of them seems to bear close examination. Besides, in such controversies, to score a point is not to win a victory, though critics commonly assume that it is so. The faults of this forceps—its unwieldiness, complexity, and the undeniable clumsiness of some of its parts—notably of the junction between the traction-rods and the traction-bar, are faults of detail and not of principle, or even of general mechanism. The idea is here found—in the rough, it may be—but such as it is, a fertile idea, which in practical application only can be improved on.

To Professor A. R. Simpson we are chiefly indebted in this country for having been the first to advocate the principles of the new forceps. He devised, besides, a simple and manageable modification of Tarnier's forceps, which has been largely used. The main points that are noteworthy in this modification are—the traction apparatus is here grafted on a somewhat modified and lengthened Simpson's forceps, which has the advantage of being less cumbrous than the French model used by Tarnier. The entire traction-apparatus is permanently fixed to the forceps, so that there are no separate parts of the instrument. The traction-rods, as in Tarnier's forceps, are attached beneath the fenestræ by pivot-joints; but that which belongs to the left (*i.e.*, the lower) blade is screwed on to a flattened "locking-plate," which is united by a joint that permits of lateral movement to the "traction-bar." This latter finally passes at a right angle through a rounded opening in the transverse "traction-handle," this mode of attachment allowing of rotatory movement of the whole forceps. The blades having been applied to the head, the free traction-rod is attached by a button on its extremity into an open slot in the locking-plate. The accompanying illustration (Fig. 3), after one by A. R. Simpson, shows diagrammatically the nature of these arrangements. The compensatory curve in this forceps is placed in the traction-rods, which lie alongside the shanks until close to the lock, from which they curve sharply backwards. In Tarnier's forceps we have seen that the traction-rods terminate above the lock, the compensatory curve being introduced into the comparatively lengthy traction-bar. In Simpson's, as in Tarnier's forceps, traction in the axis of the curved blade is insured by maintaining parallelism between the traction-rods and forceps shanks.

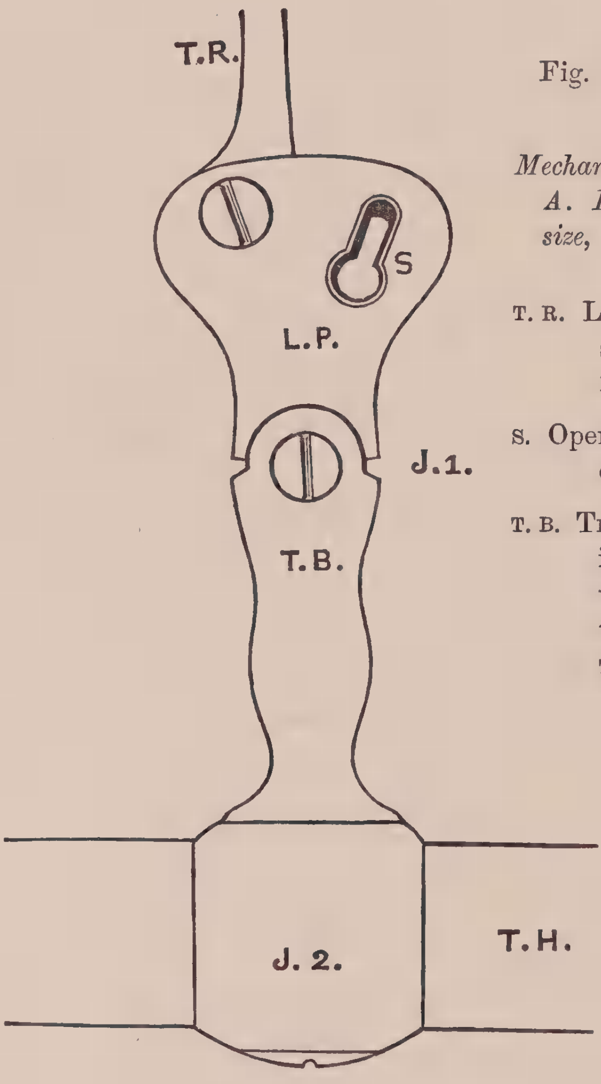


Fig. 3. (*After A. R. Simpson.*)

Mechanism of Traction-Apparatus in A. R. Simpson's Forceps. (Full size, after A. R. S.)

T. R. Left traction-rod attached by screw to flat "locking-plate," L. P.

S. Open slot for reception of button end of right traction-rod.

T. B. Traction-bar, attached to locking-plate by screw-joint, J. 1, which permits of lateral motion, and to traction-handle, T. H., by rotatory-joint, J. 2.

Over Tarnier's model Simpson's has the advantage of greater lightness and simplicity. The greater length of the traction-rods in the latter allows in every case of the necessary attachments being completed without hampering by the closeness of maternal parts. The locking arrangement is also simpler and more manageable. But, as often turned out by instrument makers, the locking is not sufficiently rigid or exact, so that the strain falls unequally upon the two rods during traction, while the right one is constantly falling out of its slot. To one who is not experienced in forceps operations, or who has not read Professor Simpson's directions, one of the chief objections to his forceps must be the feeling of entanglement during the introduction of blades permanently encumbered with the entire traction-apparatus. Not that there is any real difficulty, but that there is an apparent one, which is very apt to be regarded by many as real, and inclines them to leave advantages which they do not clearly understand in the hands of specialists. Professor Simpson specially advocates this permanent attachment on the grounds that axis-traction is *always* desirable in forceps cases, and that any separate portions of the necessary instrument may be forgotten or mislaid. If the separable part was a small one there would be much force in this argument. As it really is, however, no believer in axis-traction would be more likely to forget his traction-bar handle than he would one blade of his forceps or a catheter when going to a labour case. Besides, in designing a very useful instrument, neatness, simplicity, and portability, are not to be sacrificed to the detriment of many for the possible benefit of a few. So far as regards this feature in the forceps I certainly prefer the simple device of Professor Lusk, in whose modification the traction-rods are simply fitted into two open slots of a locking-plate, connected by a jointed bar with a traction-handle, these parts being separate from the rest of the instrument, as in Tarnier's.

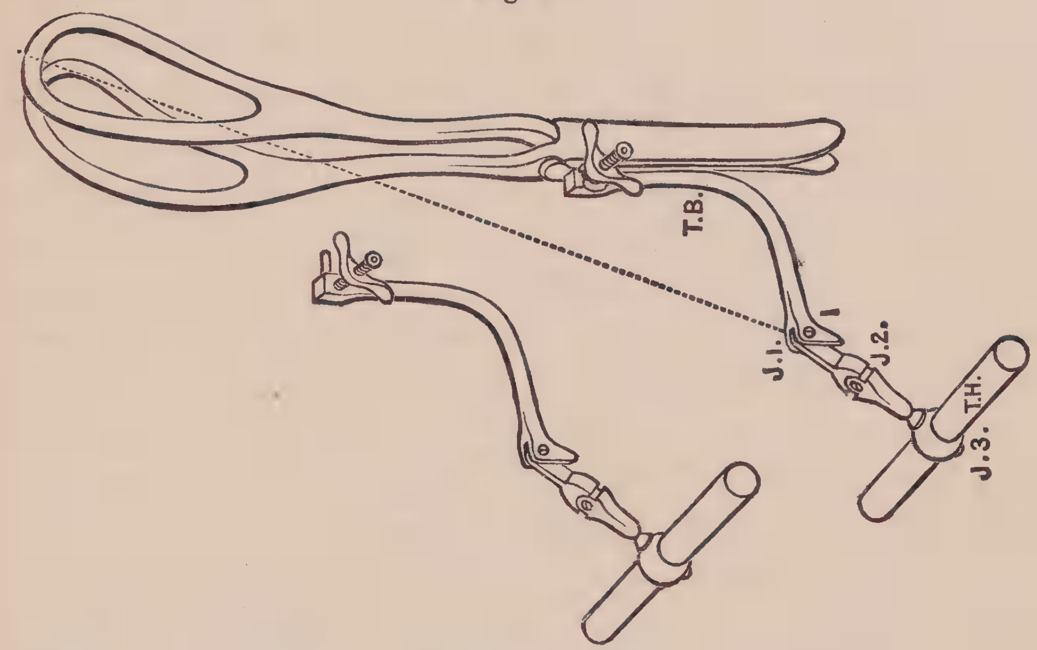
On first considering the mechanism of axis-traction forceps, I was much struck by their general uniformity. So far as I have been able to ascertain, all who have hitherto modified and improved Tarnier's forceps have been content to retain his device of making the traction force pass through steel rods united to the blades by pivot-joints immediately below the fenestræ. Yet it is easy to show that many disadvantages are inseparable from the presence of such traction-rods. They add to the complexity and increase the difficulty of introducing the instrument; to some extent they interfere with the pressing backwards of the shanks against the perinæum, which is essential if the blades are to be properly applied to the head situated at or above the brim of the pelvis, special care being needed in such cases so as to avoid the risk of injuring the anterior edge of the perinæum; and they introduce the main difficulty in such forceps—that, viz., of attaching the rods to the traction-bar by some device which shall be at once secure, simple, and easy to manipulate. It is quite true that such drawbacks as these, even when considered collectively, are of but trifling import when weighed against the decided advantages of these forceps, but they indicated an effort at least to devise some method by which the same good results might be achieved without them.

The question I proposed to myself was, then—Could an axis-traction forceps, with all the advantages of Tarnier's, be constructed without using the device of traction-rods? Theoretically, I thought I could see my way clearly enough to the solution of this problem, though I found the practical mechanism very difficult to work out successfully. I am quite sure that any intelligent practical mechanic—if I could have met such—would have saved a good deal of my time, and settled the question much more quickly and neatly than I have been able to do.

Messrs. Fannin & Co., of Grafton-street—to whom I am much indebted for the trouble they have taken in carrying out my plans—have constructed an axis-traction forceps, without traction-rods, upon the following lines:—

The general plan of the instrument will be understood by a reference to Fig. 4, which shows the forceps with its traction apparatus affixed. The forceps used may be a Barnes', Simpson's, or any other long double-curved forceps, according to individual fancy. I prefer the first-named because of its greater length, which allows the head to be easily seized, even when situated above the pelvic brim. A short way below the lock a re-curved steel bar (T B) is rigidly fastened to the handles by means of a special device presently to be explained. This bar curves backwards sufficiently far to reach the imaginary continuation of the axis of the blades, marked by the dotted line in the woodcut. At the point of intersection the bar turns downwards in the line of the continuation of this axis, and is immediately finished off as an indicator (I), which serves to show the proper axis of traction.[a] A pivot-joint (J 1), admitting of free motion in the horizontal plane, unites this traction-rod to another shorter one, which terminates in a screw-joint (J 2), so formed as to admit of motion in a plane perpendicular to that allowed of by the previous joint. From this joint a short bar continues the apparatus onwards to the transverse traction-handle (T H), with which it is connected, exactly as in Tarnier's forceps, by a joint allowing of a rotary motion (J 3).

Fig. 4.



When, with this forceps, traction is made through T H in the axis of the forceps—*i.e.*, in the axis of the pelvic canal (approxi-

[a] The direction of the indicator as shown in the woodcut is not quite accurate. It should point a little more downwards, continuing the dotted line representing the axis.

mately), as shown by the indicator (I), the force acts directly from J 1, exactly as if the traction were made from the extreme end of the handles of such a forceps as Aveling's. Mechanically, everything depends on the practical rigidity of the junction between the traction-bar and the forceps handles. So far as the indicator this forceps is practically the same as Hubert's second forceps, previously described, and from that on we have merely a device, modified from Tarnier's, for allowing mobility to the head while traction is being applied. The three joints admit of complete mobility, and the indicator shows the direction in which the tractions are to be made. It must be clear, however, that in practice such an indicator is scarcely needed, the proper axis of traction being otherwise sufficiently apparent. In the junction between the traction apparatus and the forceps handles I have adopted the device suggested by the employé of Messrs. Fannin, who made the first rough model for me. The traction-bar terminates in a T-shaped extremity, one end of the T being finished off as a screw, while a rounded stout pin projects downwards at right angles to the other end. The forceps having been applied as usual, the traction apparatus is then affixed to it as follows:—The rounded pin is first fitted into a hole specially bored for it in one of the handles (the left), and the screw end of the T is then rotated into position in a slot fixed to the other handle. The blades are then fixed by the nut attached to the screw. This device gives perfect rigidity when the forceps is so applied as to lock well with parallel handles, and is, moreover, a very simple one.[a]

For this forceps I claim that—

1. It gives all the advantages of Tarnier's over the ordinary double-curved forceps.

[a] This mode of junction I believe to be one of the best and simplest possible. Some even simpler one may, however, be found to work equally well. For example, the traction-bar might be simply attached below the lock to one handle only, the fixation-screw being separated from it. Traction would, of course, act all the same equally upon the two blades because of the lock and screw. Such an attachment could be made absolutely rigid under all circumstances.

2. It is a much simpler instrument, and more easy to apply than Tarnier's, or any of its modifications.

3. It avoids the disadvantages of traction-rods, and the traction apparatus is entirely outside the vagina, in sight.

4. The traction apparatus described can be easily fitted to any ordinary double-curved forceps.

5. The forceps can be used with or without the traction apparatus.

I am greatly indebted to Dr. Macan, Master of the Rotunda Hospital, for having practically tested the first model of this forceps in that institution, and am gratified to hear from him that it worked well. To another friend, Professor FitzGerald, F.T.C.D., I am also indebted for the advice he kindly gave me on the theoretical side of the mechanics involved; and I am strengthened by the opinion of such an authority that the forceps described has essentially the same mechanical power as Tarnier's.

I believe that, for the reasons given, this axis-traction forceps is superior to any with which I am acquainted; but, though attaining its results in a different and simpler way than that which, following Tarnier, has hitherto been constantly adopted, the objects aimed at are precisely the same. Whatever modifications have hitherto or may hereafter be made, with a view to perfecting the mechanism of these instruments, the credit must always remain with Tarnier of having been the first to formulate the essentials of a good forceps with scientific exactness, as well as of having shown in no crude form how the theoretical indications might be practically fulfilled. I do not doubt that, in doing so much, he has made an epoch in the history of the forceps, or that in one form or another the principles for which he contended—axis-traction and liberty of movement for the head during traction—will gradually be universally adopted. I hope that the instrument I have described may prove at least to be a suggestive simplification of those previously designed to admit of the practical application of these principles.

P.S.—Since writing this paper I have had some personal experience in the use of the instrument described. That experience, added to reports I have had from others who have also tried it, has been in every way satisfactory. One point concerning its practical application deserves notice. This is that care be taken to make tractions in—or nearly in—the direction shown by the indicator. Should the line of traction vary much from this fixed one, the result would be in part a tendency to displace the position of the blades on the child's head by either pulling the forceps handles backwards, or pushing them forwards. What is meant will readily be apprehended by referring to Fig. 4. Tarnier's forceps, and those with traction-rods, avoid, it is true, this possibility of a side strain on the forceps handles, and thus have, I think, their only point of superiority. Practically this point is indeed a small one, as, even were there no indicator, the slightest consideration would insure the traction being made sufficiently accurately.

ON DISPLACEMENTS OF THE OVARIES.

By THOMAS MORE MADDEN, M.D., F.R.C.S. Edin.;
President, Obstetric Section, Academy of Medicine;
Obstetric Physician to the Mater Misericordiæ Hospital.

[Read in the Obstetrical Section, January 8, 1886.]

Displacements of the ovaries, apart from those caused by ovarian disease, until recently were generally ignored by gynæcologists, and still attract less attention than their pathological importance demands. From clinical experience I have been long convinced that ovarian herniæ are of more common occurrence than is supposed even by Dr. Barnes, by whom this subject has been ably discussed in the *American Journal of Obstetrics.* In every-day practice a certain proportion of our gynæcological patients complain *inter alia* of some degree of dull, sickening, left-side pain, the situation of which, though not always clearly defined, is usually referable to the inguinal region. If further investigation be instituted in such cases we may, in not a few instances, be able to trace this pain to ovarian displacement, which, however, is too commonly passed over without recognition, owing to the greater prominence of other symptoms.

Ovarian herniæ may be found in the inguinal region, and may be either direct or oblique. In the former the tumour appears in the groin above Poupart's ligament; in the latter it follows the course of the canal downwards and forwards, and makes its way into the labium. Occasionally the displacement is observed in the femoral region, immediately below Poupart's ligament, and to the inner side of the femoral vessels. Still more frequently the ovary is displaced downwards into Douglas's space, and this prolapse may, for all practical purposes, be here considered as a form of ovarian hernia. In these cases the left ovary, as from its anatomical position might be anticipated, is that usually prolapsed into the recto-vaginal fossa, where, on examination, it may be discovered as a small oblong, elastic and highly sensitive tumour, bulging into the post-cervical vaginal *cul-de-sac.*

Although in some instances congenital, these herniæ most commonly occur in patients whose abdominal parietes have been relaxed, and viscera compressed, by repeated gestation. They may also be induced by similar immediate causes as other herniæ—such as the violent muscular efforts of the second stage of labour, lifting a heavy child, straining at stool, &c. But in the most frequent of all forms of ovarian displacement—namely, that downwards into Douglas's space—the causes of the protrusion are more commonly gynæcological, as, for instance, the *vis à tergo* of abdominal or uterine tumours, or the direct tension on the uterine appendages occasioned by displacements of the uterus.

Symptoms.—Ovarian hernia manifests itself by the sudden occurrence of a small ovoid tumefaction possessing certain distinctive characteristics, and making its appearance in either the inguinal or femoral regions, or in the labia, or directly downwards in Douglas's space. This tumefaction, as observed in its ordinary condition, is about the size of a large walnut, and when inguinal is usually very slightly sensitive. Before the menstrual periods, however, the extruded ovary becomes enlarged—in one instance recently under my care it increased to the size of a small orange—and then gives rise to a dull aching pain, which gradually subsides, so that shortly after the termination of the menstrual epoch the displaced organ resumes its previous condition, and generally ceases to give any active trouble until its functional activity is again stimulated by the approach of the next catamenial period. In some instances, however, these symptoms do not thus disappear in the interspace, the dull sickening pain remaining permanently, and the congestive hypertrophy of the displaced organ continuing to increase until relieved by suitable treatment.

Diagnosis.—That the differentiation of ovarian displacements was formerly very imperfect is, I think, evident from the scant notice of such cases by the older gynæcologists, by whom their existence was generally either ignored or confounded, when external, with enlarged inguinal or femoral glands, or, when labial, with other tumours in that situation; whilst ovarian protrusion into Douglas's space was apparently in many instances taken for pelvic abscess, subperitoneal pedunculated fibromata, hæmatocele, or the displaced fundus uteri. We at least have now no excuse for similar errors in the diagnosis of ovarian herniæ. These, whether inguinal or femoral, may be readily distinguished from enterocele by absence of the characteristic smoothness and globular form, gurgling on compression and resonance on percussion of the latter, whilst from epiplocele they may be differentiated by contrasting the firm, clearly-defined ovoid tumour observable, if it be ovarian, with the soft doughy feeling, and irregular ill-defined outline of the hernia, if omental. From enlarged inguinal or femoral lymphatic glands the ovarian tumefaction may be recognised by the smaller size and multiple character of the former. From pelvic, psoas, or other abscess, the distinction of an extruded ovary is obviously rendered easy by the history of the case, as well as by the presence or absence of fluctuation. Lastly, ovarian prolapsus into Douglas's space is distinguished from a posterior uterine displacement, or a fibro-myoma, by recto-vaginal examination and the use of the sound; whilst from the tumefactions in the posterior vaginal *cul-de-sac* that may be occasioned by cellulitis, rectocele, tubal, ovarian, or parovarian cysts, or abscesses, or pedunculated subperitoneal fibromyomata, ovarian prolapsus may be differentiated by the methods of examination just alluded to, which will enable us to determine not only the existence of any uterine mal-position, but also the character of the tumour and the presence of any fluctuation therein. If the uterus be thus found normal in size and position, if there be no fluctuation discoverable, and if at the same time in the posterior *cul-de-sac* we discover a small, well-defined, firm, ovoid tumour, enlarging regularly at each menstrual period, which tumour, on slight pressure, gives rise to a peculiarly sickening dull pain, we need have little hesitation in concluding that we have to deal with a case of prolapsed ovary.

Treatment.—The treatment of ovarian displacements is necessarily dependent on the situation of the extruded organ in each case; or, in other words, whether it be found at either of the abdominal rings, or in the labium, or in the recto-vaginal interspace. In the first of these, whether the ovarian hernia be above or beneath Poupart's ligament, an effort should in the first instance be made at its reduction by taxis. In the majority of cases, however, such herniæ are irreducible when seen by the gynæcologist, and even in those few instances in which reduction is possible, the retentive pressure of a truss is neither endurable nor effectual. In most cases of this kind we must, therefore, be content to protect the ovary if protruded externally from further extrusion or injury by a well-fitting hollow truss. But before this an attempt should be made to lessen the local hyperæsthesia of the generally hypertrophied displaced gland by sedative applications, and, if necessary, by leeching, &c., whilst the constitutional irritation almost always present in such cases should be allayed by suitable constitutional treatment.

When, however, these measures prove ineffectual in relieving the almost constant, worrying, dull aching pain which at each monthly period in these cases becomes accentuated into acute suffering—when, too, the patient's health is manifestly and seriously endangered by the gastric disturbance and constitutional irritation occasioned by this apparently trivial and too often neglected displacement—we should then be prepared, if it becomes absolutely necessary, to fall back on the extirpation of the dislocated and probably diseased gland as the only resource available under the circumstances.

In my own practice I have more than once been thus obliged, however reluctantly, to resort to this procedure. In one case the ovarian hernia occupied the right labium, the patient being an unmarried woman about forty years of age, who, when admitted into hospital, was completely broken down by dull, dragging pelvic pain, anorexia, and nausea. She was greatly emaciated, despondent, and hysterical. Menstruation was scanty and somewhat irregular, and physical examination failed to disclose any other local complaint than a tumour, about as large as a hen's egg, within the right labium, any handling of which occasioned severe pain and nausea. This swelling she had only noticed a few months before admission into hospital. The patient having been etherised by my colleague, Dr. Kennedy, we proceeded to remove the tumour, which was found to extend through the canal, to the walls of which it was firmly adherent in many places up to the abdominal ring, where it tapered off to a narrow pedicle. This was secured by double ligature and divided. In the adhesions some large vessels had to be secured, and subsequently from the surface a free venous oozing took place, which was arrested in the first instance by packing the cavity with styptic cotton, and, when this was removed, by bringing the parts together with wire sutures, and afterwards applying a roller and compress externally. It is needless to add that the operation was thoroughly aseptic. That night she slept fairly; the temperature 101° and pulse good. Next morning she had some retching, but was able to retain a little iced champagne and jelly, still she was extremely weak; the temperature was only 99° and pulse 120. That afternoon, however, she suddenly became collapsed and died, and we were not able to secure permission for a *post mortem* examination, which we were anxious to obtain. The tumour removed, on careful examination, verified our diagnosis, proving to be a greatly hypertrophied and disintegrated ovary.

In the next case of ovarian hernia that came under my observation the issue was more fortunate. In this instance the displacement was situated in the left inguinal region; the patient being a young lady who shortly after marriage commenced to suffer from dragging pelvic pain, irritability of stomach, loss of appetite, and consequent wasting and debility, further increased by menorrhagia. She also now became extremely hysterical and despondent, and for nearly two years before I saw her had been under almost continual gynæcological treatment abroad and at home—during this time having, *inter alia*, worn almost every form of pessary for the relief of some supposed uterine displacement. Ultimately a small ovarian hernia, which became very troublesome at each monthly period, was discovered in the left inguinal region, and being irreducible, after the failure of other treatment, its removal was proposed and agreed to. Accordingly I extirpated the dislocated and hypertrophied ovary, after which she made a rapid and complete recovery.

Last autumn another case of the same kind, occurring in a young unmarried woman, was under my care in St. Monica's ward; but as the patient declined operation at the time, she left, promising to return for this purpose as soon as the hospital re-opened for the winter session, but up to the present she has not done so. To these cases of ovarian hernia I might, did time permit, add the history of four or five cases of prolapse of the ovary into Douglas's space which have come under clinical observation in the hospital within the last seven years. In one of these oöphorectomy was found necessary, the others being sufficiently relieved by the application of a suitable air-pad pessary.

I need hardly observe that ovarian herniæ requiring removal of the ovary are very much more exceptional than cases of the displacement referred to; nor is the performance of oöphorectomy under such circumstances ever devoid of risk. Hence in no instance should this step be resorted to without urgent necessity, and until a fair trial has been first made of other remedial and palliative measures.

In considering the treatment of prolapsus of the ovary, its ætiology must be carefully borne in view. Thus, the extrusion may be due, as already pointed out, to the pressure from above of a uterine or ovarian tumour, or from the traction of a uterine displacement on the broad ligaments, and obviously these abnormalities must be removed or relieved before any successful re-position of the prolapsed ovary can be made. When this condition is due to some accidental circumstance, or to a relaxed state of the parts occasioned by constitutional causes, we may, with greater probability of permanent success, attempt to return and retain *in situ* the displaced viscus. For this purpose the patient—being first etherised in order to permit of the necessary manipulation of the generally highly-sensative ovary—should be placed in the left lateral semiprone position, when by firm but gentle, steady, bimanual pressure through the rectum and vagina, upwards and forwards, we may be able to lift the extruded ovary out of the recto-vaginal fossa and to push it up into its normal position, where it may then be retained by either a Greenhalgh's or an Arnold's glycerine pad-pessary. When, however, as too commonly happens in long-standing cases of this kind, the re-position of the ovary cannot be thus effected, the support of a well-fitting pessary will, in the majority of instances, be found effectual, not merely in preventing any further prolapse, but also in relieving the discomfort and suffering caused by such displacements. And finally, in some instances where these measures fail, and where the local and constitutional effects of the ovarian prolapse are urgent and are otherwise irremediable, we may, as happened in one of my cases, be obliged to resort to vaginal oöphorectomy.

TWO CASES OF FIBRO-MYOMATOUS TUMOUR OF THE UTERUS.

By WILLIAM J. SMYLY, M.D.; F.K.Q.C.P.;
Gynæcologist to the City of Dublin Hospital.

[Read in the Obstetrical Section, January 8, 1886.]

Case I.—*Interstitial Fibroma of the Cervix; Enucleation.*—In this case the tumour was situated in the substance of the cervix—a position in which fibroids are rarely met with. Their occurrence in this situation is interesting to obstetricians, however, not on account of their rarity alone, but also because of their liability to obstruct the passage of the fœtus during labour.

The patient from whom this specimen (exhibited) was removed was admitted into the City of Dublin Hospital in July last. She was forty-two years of age, and had several children. Her last confinement took place seven weeks previously, and delivery was effected, by her usual attendant, with the greatest difficulty; and it is evident that this tumour, which has been some months in spirit, and is still as large as a hen's egg, must, at the time of labour—for, subsequent to delivery, these tumours shrink very considerably—have been of such a size as to offer a very serious obstacle to delivery.

The growth had been detected during a previous confinement, but was then so small that it scarcely attracted attention.

Upon examination I discovered a portion of the tumour, about 1 inch in diameter, protruding through the os uteri, the cervix being thinned out and expanded over it, forming a ring between which and the tumour the finger could be swept around in a shallow groove. The idea that the case was one of inversion of the uterus was immediately corrected by the external hand, which easily discovered that organ displaced backwards and to the right, and the sound passed in the same direction to a depth of three inches.

The growth was very easily enucleated, though the hæmorrhage from the cavity left by it was somewhat profuse. Upon introducing my finger into this cavity I found that it passed through a hole in the left side of the cervix, and upon further examination I found that the internal os was above and to the right of this opening. Finding that injection of hot water failed to check the hæmorrhage, I plugged the cavity with iodoform gauze and the vagina with cotton wool. Recovery was rapid and complete.

CASE II.—*Interstitial Uterine Fibroid; Oöphorectomy.*—The lady who was the subject of this tumour consulted me first in June, 1882. She then informed me that her age was thirty-nine; that she had been married sixteen years, but had no children; and that she had been more or less an invalid since puberty, but was steadily getting worse. During all that time she had been almost constantly under medical treatment.

She complained of pain in the back and in the left ovarian region, with difficulty in walking. She was anæmic, and her strength had been much reduced by diarrhœa, leucorrhœa, and hæmorrhage. Menstruation was profuse, and was preceded and accompanied by severe pain.

Upon examination the uterus was found to be retroverted, and a tumour about as large as a small orange could be felt upon its posterior surface. The organ itself was tender to the touch, and the sound passed backwards to a depth of four inches.

I divided the cervix—an operation which Dr. M'Clintock had previously advised, but to which, at that time, she refused to submit. Subsequently, I lifted the uterus upwards, so as to take the pressure off the rectum, and supported it by means of a pessary. I also prescribed ergot at the periods. From this treatment she derived very marked benefit; the diarrhœa ceased, the pain in the back was relieved, and she could walk about without distress. After about six months, however, she began to disimprove, the hæmorrhages especially becoming more violent in spite of hot water injections and ergot. I then determined to dilate the cervix, and explore the uterine cavity. The cervix was so rigid that it required 48 hours to dilate it sufficiently, and it was then found that the tumour bulged forward into the uterine cavity as well as outwards towards the peritoneum. The mucous membrane covering the rest of the cavity was thick and pulpy, and this I thoroughly cauterised with fuming nitric acid. This was followed by some feverish symptoms, and subsequently the uterus was found to have become fixed posteriorly. Nevertheless, she derived some temporary benefit from this proceeding, the hæmorrhages becoming much less severe, and I was thus enabled to tide her along for two years more towards the hoped-for menopause. I need not detail all the palliative measures which were had recourse to during that time—including ergot, Ruspini's styptic, and hydrastis canadensis by the mouth; hot-water vaginal douches, and styptic and caustic injections into the uterus itself. However, in the summer of last year I was driven to more active measures, and I then determined to scrape out the pulpy membrane with the curette. In doing this I found no difficulty in denuding the anterior and lateral walls; but, owing to the bulging forward of the tumour, there was a pocket above it which I was quite unable to get at. The result of this proceeding was not satisfactory; the hæmorrhages continued to increase in frequency and severity, and the patient rapidly lost ground. However, I advised her to return home, in the hope that after a time the result might prove more satisfactory; but in this I was disappointed, and as her condition was becoming critical, I determined to remove the uterine appendages, and did so on the 30th of last November. The operation was a difficult one. The abdominal wall was very thick, and bled profusely. The right ovary, which was the only one that I had succeeded in palpating previous to operation, was easily found, but, owing to the rigidity of its ligaments, I could not succeed in bringing it outside the abdomen, and was, therefore, obliged to ligature and remove it in the depth of the wound. The left ovary, which was bound down by adhesions in Douglas's space, was found with very great difficulty, and but for Dr. Macan's assistance I doubt if its removal, together with the tube, would have been satisfactorily accomplished. The abdominal wound was then closed in the usual way, and dressed with sero-sublimate gauze and turf-moss.

Her convalescence was most satisfactory, and, although it is premature to speak of ultimate results, yet it is now eight weeks since the last menstruation.

Before closing this communication I may briefly criticise the methods of treatment employed. Firstly, as to the division of the cervix; this procedure appears to have been of undoubted benefit in a certain, but very limited, class of cases—those, namely, where the tumour is situated very low down. Secondly, as to the removal of the mucous membrane either by cautery or curette; since this is most frequently the source of the menorrhagia, its destruction appears to be good practice, but as the membrane rapidly re-develops, it can only benefit temporarily. Lastly, castration appears to be a really beneficial proceeding. It has, however, the great disadvantage that the tumour remains a source of danger and trouble. But in this case the removal of the latter would be attended with very great difficulty, and danger to life.

ON THE TREATMENT OF CHRONIC CYSTITIS IN WOMEN.

By THOMAS MORE MADDEN, M.D., F.R.C.S.Ed.;

President, Obstetric Section, Academy of Medicine; Obstetric Physician, Mater Misericordiæ Hospital.

[Read in the Obstetrical Section, April 2, 1886.]

Having recently had occasion to look through my hospital case books for the past seven years, I was somewhat surprised at the comparatively large number of cases of cystitis which have there come under observation. Hence I now submit a brief paper on this subject, in the hope of inducing a discussion on the management of a disease thus frequently met with in gynæcological practice, and the treatment of which is still far from being satisfactorily established. As met with in females, cystitis is not only more prevalent and more intractable, but, moreover, of greater pathological importance than the same disease in the male. In the former it generally presents itself in the chronic form, in many instances being consequent on the extension of vulvar or vaginal inflammatory conditions along the urethral canal to the bladder. In other cases it is due to direct mechanical causes, such as the pressure of the fœtal head during the second stage of labour. It also may result from structural, renal, or vesical disease, or from the effects of cold or local injuries, or from reflex irritation. By far the most frequent, however, of all the causes of this disease is that long delay in complying with the call to micturition to which women are so generally trained, or which may be occasioned by accidental circumstances, such as a long railway journey.

Symptoms.—Few of the complaints to which women are subject occasion greater suffering and are more difficult to deal with than well-marked subacute cystitis. Amongst its more prominent symptoms are, first, a peculiarly teasing, dull supra-pubic pain, extending down through the pelvis and into the rectum, a frequent desire to pass water, with no relief on each occasion, micturition being attended with pain, straining and inability to void the last drops of urine. In this way, in extreme cases, the patient becomes the victim of hourly-repeated torture, night and day, so as to render her life a positive burden to herself. In the first stage of the disease the urine is turbid and alkaline, and as the vesical irritation develops into structural disorder, the water becomes abominably fœtid, as well as scanty, and high-coloured, and, later on, generally tinged with blood and laden with muco-purulent matter, which becomes semi-gelatinous on cooling.

Treatment.—As a rule, cystitis in women is a secondary affection consequent on some diseased condition of the vulva, vagina, or uterus, to the removal of which our first attention must be directed. At the same time, if the pain and spasmodic irritation be acute, these may be generally relieved by clearing out the rectum, by enemata of warm water or thin gruel, after which a morphia or belladonna suppository should be introduced, or an injection with from twenty to forty drops of laudanum may be thrown up. In most cases opiates will be required; and of these, in such cases, probably the best is the old-fashioned Dover's powder, given in small doses twice or thrice daily. In all cases absolute rest in bed, with frequently-repeated, long-continued warm hip baths, and plenty of dilutents, such as barley water, flaxseed tea, or gum water, will be essential. In many instances great advantage will be found from the use of boracic acid, in ten or fifteen grain doses, three or four times a day, a practice for the use of which in such cases I am indebted to my friend, Dr. Duke.

In chronic cystitis, where the diseased mucous membrane secretes the thick muco-purulent exudation, already alluded to, local treatment is always indispensable, and in such cases great relief may be afforded by merely washing out the bladder with either a weakly-carbolised injection or with plain warm water two or three times a day. This, however, can only be regarded as an adjunct to the more active local treatment required—namely, the injection into the empty bladder, after free dilatation of the urethra, of a solution of boro-glycerine, or else of a couple of ounces of a sixty-grain solution of nitrate of silver. This generally causes intense pain for some hours after its use, and in milder cases occasionally proves effectual.

It sometimes, however, happens that these measures fail not only in curing, but even in relieving in any degree the sufferings and risk of subacute cystitis. In such cases we may have recourse to the formation of an artificial vesico-vaginal fistula, through which the urine may be drawn away as rapidly as secreted, and thus afford absolute rest to the diseased bladder, and give the patient a reasonable hope of respite from an otherwise inevitable death from the extension of the disease to the kidneys. This plan of treatment, although previously suggested by Dr. Parker, of New York, in cases of cystitis in males, was first successfully carried out by Dr. Emmet for the relief of cystitis in women, on the recommendation of the late Dr. Marion Sims, and since then has, on Dr. Emmet's high authority, been largely adopted by other gynæcologists.

In the large number of cases of cystitis which I have had occasion to treat, I have myself met with some instances in which the formation of a fistulous opening appeared to be called for. In most of these cases it was successful in its result, and was carried out in accordance with Dr. Emmet's directions, which may be here briefly recapitulated :—If resorted to before the disease has advanced so far as to extend to the kidneys, the operation is, says Dr. Emmet, "as free from risk as any in minor surgery. Even under the more unfavourable circumstances the risk of the operation is justifiable ; for by it life may be prolonged and a great degree of comfort obtained in obviating the persistent efforts to empty the bladder." The operation is to be performed under the influence of an anæsthetic when possible, with the patient on the left side, and the anterior wall of the vagina fully exposed by means of a large-sized Sims' speculum. A sound, somewhat abruptly curved an inch and a half from its extremity, must be introduced into the bladder and held by an assistant. While the point of this instrument is firmly pressed in the median line against the base of the bladder, a little behind the neck, the projecting tissue on the vaginal surface must be seized with a tenaculum, and divided by a pair of scissors directly on the point of the sound, until it can be passed through into the vagina. With the sound remaining in the opening as a guide, one blade of a pair of scissors should be passed into the bladder, and the vesico-vaginal septum be divided backwards in the median line. By this mode, especially where the vagina is of a natural size, the operation is extremely simple, and is completed in a few moments. The object of cutting on the point of the sound is to be sure that the bladder and vaginal surface are divided in corresponding incisions; for there is so much mobility of one surface over the other that it is exceedingly difficult to enter the bladder unless the parts are transfixed. The median line has been preferred for the location of the incisions, since it is not likely to include any large blood vessels, unless the opening be extended too near to the cervix uteri, or to the neck of the bladder. In theory, there is no necessity for an opening larger than that equal to the area of the two ureters; in practice, however, it is found that it must be greater at first than this, from the fact that, in spite of all the care that can be taken to prevent it, a large portion of it will close too soon. Moreover, at first it is a great advantage to have an ample opening through which the accumulated mucus in the bladder may be easily washed out. It is very seldom that much bleeding follows this operation unless, as just stated, the incision is extended too far in either direction. When a large vessel has been divided it will either be a branch of the circular artery of the cervix uteri or one given off from the subpubic artery. Bleeding in either direction is readily arrested by introducing a silver suture, so as to include a fair amount of tissue beyond the angle of the wound, and twisting it sufficiently tight."

Various means have been suggested for keeping the artificial opening patulous. In some cases Dr. Emmet uses a glass stud or eyelet, half-an-inch in diameter and not unlike a spool in shape, which is buttoned into the slit, while the vaginal rim prevents its slipping into the bladder. It will remain loose, and with sufficient play to prevent the parts from healing up too tightly around it.

If used, it should be made light, and only from the finest quality of Bohemian glass; for if the least amount of lead or any other impurity exists in the glass, it will in a few hours become encrusted with a sabulous deposit from the urine, and increase greatly the irritation. Hence, Dr. Emmet at first relies on the careful introduction of the finger night and morning; but, after a few days, when the irritation of the parts has somewhat subsided, the urine is in better condition, and the incision beginning to close, the glass stud may be used with greater advantage.

As the main difficulty in such cases is to keep the incision open after the use of the scissors or knife, Dr. Vulcan recommends in their stead the employment of the thermo-cautery for this operation. I am myself inclined to think that in the cases of cystitis generally met with in this country these operations are less often necessary than in America, as we may, I believe, in many instances obtain their advantages without their risks by the sufficient dilatation of the urethra, so as to produce a temporary incontinence of urine, and thus give rest and freedom from irritation to the inflamed endo-vesical mucous membrane. Various dilators have been from time to time suggested for this purpose, but of these I, myself, prefer that which I have now the honour to exhibit to the Society, and the efficacy of which I have practically demonstrated to my class in numerous cases of cystitis recently treated in the gynæcological wards of my hospital. I will merely add that to be effectual in any case this dilatation must be sufficiently extensive to overcome for some considerable time the contractility of the submucous muscular structures of the urethral walls, and should be accompanied by swabbing over the inflamed endo-vesical mucous membrane itself with glycerine of carbolic acid. By such treatment I have in several instances been successful in dealing with the most intractable cases of cystitis that have recently come under my care, and I can, therefore, recommend a trial of it in suitable cases to other practitioners.

USE OF THE CURETTE IN THE DIAGNOSIS AND TREATMENT OF DISEASES OF THE UTERINE MUCOUS MEMBRANE.

By W. J. SMYLY, M.D., Univ. Dubl.; F.K.Q C.P.;
Gynæcologist to the City of Dublin Hospital.

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Before entering upon the special subject of the use of the curette in the diagnosis and treatment of diseases of the uterine mucous membrane, permit me to guard against a possible misapprehension. I do not wish it to be supposed that because I have limited my remarks, as far as possible, to the use of a single instrument, that therefore I would limit our means of diagnosis to its use; on the contrary, it is against such exclusiveness that I wish to contend. It certainly seems strange that, although the sense of sight has long since been brought to bear upon the diseased conditions of the uterine mucous membrane, yielding, as might have been expected, most important results, we should almost have ignored this method of diagnosis, and have practically limited ourselves to the sense of touch. In proof of this I would refer to our text-books, or to Dr. Edis' paper, "On the Exploration of the Uterine Cavity in Cases of Metrorrhagia," read at the meeting of the British Gynæcological Society on the 9th of last December. There he advises—and all who joined in the subsequent discussion appeared to agree with him—"in all cases where hæmorrhage from the organ persists unnaturally," to dilate the cervix and explore the cavity with the finger. Now, my present object is to state my conviction, and, if possible, to prove that this routine practice of dilating the cervix for diagnostic purposes is unnecessary; and that if an intelligent use of the curette be substituted for it, dilatation will be required in exceptional cases only. I do not, however, deny that the use of the curette for diagnostic purposes is recommended in our text-books, or that it is occasionally employed for this purpose; but the teaching in these countries appears to be this—first to explore with the finger, and if this fail, then to try the curette; whereas the contrary course appears to me advisable—namely, first the curette, and, this failing, then the finger.

Let us now submit these two methods to the admirable tests adopted by Dr. Edis in the paper above referred to—first, efficacy; second, harmlessness; and thirdly, facility.

Let us apply them, in the first place, to the method of examination by the finger. Is it the more efficacious? No doubt, in a large uterine cavity, with soft and yielding walls, gross pathological changes, such as polypi, carcinoma, or pulpy and thickened membrane, can be easily felt. But the conditions are very different when the uterus is firm and unyielding, and when its cavity is but slightly enlarged. In such a case the tactile sense is greatly impaired by compression, and it will often be found impossible thoroughly to explore the cavity. But, even under the most favourable conditions, who would attempt to distinguish by touch alone such affections as retention of the decidua, fungous endometritis, malignant adenoma and sarcoma; or catarrhal, interstitial, and atrophic endometritis—affections our knowledge of which has been almost entirely derived from the use of the curette and the microscopic investigations of such men as Dr. C. Rugé of Berlin?

As to harmlessness, I should not wish to exaggerate the risks which attend upon the dilatation of the uterine canal; when properly carried out they are not very great, but in pre-antiseptic times this was not so; and considering the very loose ideas still prevalent as to the necessity for minuteness in antiseptic details in the minor gynæcological operations, I think that a rule necessitating the dilatation of the cervix, and the introduction of the finger in all cases of severe intra-uterine disease, has caused, and will cause, much suffering, and even loss of life. There is also—and this appears to me to be a point of much importance—a large class of cases in which the dangers attending this process are so great as almost to preclude its employment—as, for example, those in which there is inflammation in the uterus itself, or in the tissues surrounding it. In these cases the manipulations attending the dilatation and examination with the finger would probably lead to an aggravation or rekindling of the mischief. The case of a patient at present in the City of Dublin Hospital illustrates this very well. She was twice admitted into the Rotunda during the time that I was Assistant to the Master, suffering from severe menorrhagia. The uterus was dilated with sea-tangle and mopped out with fuming nitric acid, and with excellent results. But upon a subsequent occasion this was followed by a severe attack of inflammation, the results of which can still be felt in the neighbourhood of the uterus. Dilatation in this case would be hazardous, but I had no hesitation in using the curette, and with excellent effect.

That the methods of dilating the cervix are not altogether satisfactory may be gathered from the number in vogue at the present time, and the variety of instruments which have been devised for carrying them out. Some of these methods are difficult and some dangerous; some involve a bruising and lacerating of the tissues, and others intense suffering to the patient, and loss of time to the practitioner. Now, although these imperfections are not present to so marked a degree as to counterbalance the important advantages of correctness in diagnosis, were dilatation necessary for its attainment, yet they are quite sufficient to prevent our resorting to this proceeding where shown to be unnecessary for such a purpose.

If, now, the same tests be applied to the use of the curette, it will be found, in the first place, to be eminently efficient as a diagnostic agent. With this object it should at first be passed lightly over the surface of the membrane lining the interior of the uterus, whereby the condition of the latter can be readily appreciated, whether smooth and even, or rough and irregular, soft like velvet, or harsh and grating. I have never myself detected a polypus in this way; but Dr. Fritsch says "that it is impossible to discover and to remove small polypi without having felt them will only be believed by those who have no experience in this method" (*Die Lageveränderungen und Entzündungen der Gebärmutter*, page 1017). The cutting edge of the curette should now be used, and the entire mucous membrane removed by firm, bold strokes. As the instrument is occasionally withdrawn during this process, it will be followed by long stripes of mucous membrane, either thin and translucent, as in the normal and in simple catarrhal conditions ; or thick, succulent, and semi-opaque, as in hyperplasia. In malignant affections, hard, friable particles, often of a whitish colour, will probably come away. This alone is generally sufficient for the diagnosis, but occasionally recourse must be had to the microscope. It has been denied that the particles of membrane thus obtained are sufficient for the latter purpose, or that a diagnosis can be made in this way; and I have no doubt that it is often a very troublesome and tedious task, but that it can be done is certain. In nine cases published by Düvelius,[a] the diagnosis was made by this means alone ; in two it was carcinoma, in two malignant adenoma, in one sarcoma, and in four simple adenomatous endometritis. In all the organ was extirpated; in the four latter, on account of uncontrollable hæmorrhage, and the diagnosis confirmed by the subsequent examination of the specimens obtained. In two cases of my own, in which I felt doubtful as to the nature of the disease, I had the scrapings examined microscopically, and this important point determined. I have, therefore, no doubt that a certain diagnosis can be made in this way, provided the disease involves the mucous membrane.

Now, as to the second test—harmlessness. Certain dangers naturally suggest themselves as likely to attend this process; and especially, that the mucous membrane having been removed, it would be replaced by cicatricial tissue, and thus sterility would result. Reasoning from analogy, we should expect this to occur, but analogy does not hold good in this instance. The uterine mucous membrane differs from every other in its rapid and complete regeneration, as is well illustrated by two cases of Dr. Martin's. The patients, who were both approaching the climacteric, had been thoroughly and repeatedly curetted, and cauterised, for metrorrhagia, but without benefit. They were at last reduced to such an anæmic condition that, as a last resource, the uterus was

[a] Zeitschrift für Geburtshulfe und Gynækologie. Bd. X.

removed. In one of these cases the last scraping took place two, in the other, four months previously. The microscopic preparations obtained after extirpation showed no trace of the previous energetic curetting. The newly-developed membrane was related to the muscular and intermuscular connective tissue in the usual manner, nor was a cicatrix to be found anywhere in the mucous membrane, or between it and the muscle. This is what might be expected if the new formation grows from beneath and not from the sides. Schrœder believes that it grows from the remains of the glands which are left by the curette. Theoretically, therefore, there is no reason why nidation should be interfered with, but, on the contrary, it should rather be favoured by the removal of a diseased membrane and its replacement by another which we have every reason to hope may prove a healthy one. Now, as a matter of experience, it has been found that this is the case. Dr. Düvelius found that out of the patients operated upon during the four years between 1879–1883 by himself and Dr. Martin, 60 were known to have become pregnant. In 11 of these the operation was performed on account of incomplete abortion; in the remaining cases, for disease of the endometrium or for sterility. Sixteen times the diagnosis was confirmed microscopically. Out of 49 patients 32 had previously borne children; 17 had not. Out of the 60 pregnancies 6 ended in abortion. Four of these were women who were operated on in consequence of a previous miscarriage; and of these 3 conceived again after a second curetting. The exact percentage of cases in which pregnancy followed this kind of treatment could not, for obvious reasons, be ascertained. In addition to these cases, Dr. Benicke has published ten; and some of my own patients have borne children subsequent to the operation. It is, therefore, evident that, no matter how thoroughly the curetting may be carried out, the membrane will be regenerated; that this new membrane is not cicatricial in structure, and that the operation favours rather than hinders the occurrence of pregnancy.

Now, as to the third test, nothing could be much more simple than this trifling operation. The patient having been placed in the dorsal position and etherised, the thighs are flexed and fixed by

Sänger's apparatus, the cervix exposed by Simon's speculum and fixed with a bullet-forceps, the vagina and uterus washed out with antiseptic lotion, the sound introduced to ascertain precisely the direction of the cervical canal and uterine cavity, and then the curette is introduced and the entire mucous membrane removed; fragments and blood-clots washed out, a Braun's syringeful of strong solution of iodine or perchloride of iron is injected, and the superfluous fluid is washed out again.

Before leaving this part of the subject I shall speak of a few details. Sänger's apparatus is a modification of Clover's, and is better adapted to the needs of a gynæcologist. I have found it of the greatest advantage, especially in private practice, where the number of assistants is a matter of consequence. With this apparatus and an irrigator upon a convenient stand, an anæsthetist is the only assistant that is really necessary. For washing out the uterus I prefer the Fritsch-Bozeman catheter to any other. It is easily cleansed, insures a safe return of the fluids injected, and, owing to its conical shape, can be made to enter almost any cervix; but should difficulty be found in doing this, it is better to postpone the uterine douche until after the curetting, when, owing to a remarkable dilatation of the cervix which always accompanies this process, it will then be found to enter easily. The curette which I prefer is either a Sims' or that employed by Dr. Martin, of Berlin, which is, I believe, very similar to that originally introduced by Recamier. It is a powerful steel instrument, with a long, oval, dull-edged spoon at both ends. The membrane is removed with the side of the spoon, which is swept around the uterine cavity; the end is only employed for scraping the fundus. This instrument I prefer when the uterine cavity is sufficiently enlarged to give it free play; but in other cases I use the sharp steel loop. A very ingenious instrument has been introduced by Freund, which combines an irrigator and curette, but I have never employed it, being perfectly satisfied with the simpler forms. The best instrument is that which removes the membrane most completely. And therefore those which, like Thomas' dull-wire curette, have been devised especially to avoid doing this, are the least efficient. A point of some practical importance is, that when the mucous membrane has been removed, and the instrument comes down upon the firm uterine wall, a harsh grating is felt, and even heard, as it is drawn over it—a sensation very different from that communicated by the soft mucous membrane, and resembles the sensation and sound produced by drawing the instrument across the extended palm of the hand.

I think, then, that we may come to this conclusion, that in the diagnosis of diseases involving the uterine mucous membrane the use of the curette will be found more efficient, safer, and easier than dilatation of the cervix and introduction of the finger.

Let us now consider this instrument as a therapeutic agent. In the foregoing remarks its action in this respect has been anticipated. It depends upon the regeneration of the mucous membrane. Why should we spend months and years in making applications to a membrane which may be got rid of in a few minutes, with a certainty of its being speedily replaced by at least as good, but probably a very much better one? That the new membrane may be diseased is true, but this objection applies to every kind of intra-uterine treatment; and speaking generally, we may say that the success which has attended any therapeutic agent in this direction has been in direct proportion to its power of destroying the diseased membrane. This explains to us the marked benefit derived from the use of powerful caustics, especially the fuming nitric acid. It would be both unpatriotic and ungrateful were I to depreciate the worth of this agent; but there are three points in which it is inferior to the curette—1st. Its use necessitates a previous dilatation; 2nd. Its action is rendered uncertain by discharges, especially hæmorrhage; and 3rd. It destroys the tissues, upon which, in many cases, we have to depend for a diagnosis. In bringing forward some cases to illustrate the use of the curette in treatment, I at first thought of selecting some typical examples; but this method, I fear, would convey too favourable an impression of its results. I have, therefore, brought forward some cases in the order in which they were operated upon; and to avoid overtaxing your patience with a long list of ordinary affections, I have limited myself to twelve, which were thus treated during the summer and autumn months of 1884—a period sufficiently remote to judge of the results :—

Case I.—Mrs. B., aged thirty-one, married six years. Two children and several abortions. Present illness dates from the last miscarriage, four years previously. During all this time she had been under the care of a distinguished specialist, who saw her twice weekly, and made applications to the interior of the uterus. Under this treatment she had somewhat improved.

She complained to me of profuse menstruation, lasting from eight to ten days, attended by severe pain in the back and head, and expulsion of clots. There was also a distressing forcing-down feeling. Between the periods there was constant leucorrhœa. Upon examination the uterus was found enlarged three and a half inches in length, the cervix abraded, and a copious catarrhal discharge.

Believing that the ordinary methods had been fully tested during the previous four years, I determined to use the curette, and this I did on the 8th of July, 1884. After this her menstruation became regular and painless, and the leucorrhœa greatly diminished; and when I saw her a year after she looked well and strong, and told me that she never felt better in her life. She called upon me again two months ago and told me that menstruation was again becoming excessive in quantity, but never exceeded four days in duration.

Case II.—C. D., aged twenty-six, a servant girl; two children; the last two years previously. Menstruation regular, but profuse and painful. The patient was pale and anæmic, and complained that she was unable to work in consequence of constant pain in the sacral and ovarian regions. The uterus was found retroverted and enlarged; there was copious catarrh and an abraded cervix. I had this patient under treatment for several months—sometimes in hospital and sometimes as an out-patient. The uterus was replaced and a pessary adjusted; the metritis and endometritis treated in the usual way, and the general health attended to, but only with temporary benefit. On July 15th I curetted the uterus. Since then menstruation has been normal. She has no pain, and can do her work. I have only seen her on two occasions since to have the pessary adjusted.

Case III.—Mrs. O'C., four years married; four children; last pregnancy nine weeks previously; since then continuous hæmorrhage; is weak and anæmic. Having spent a short time in trying other remedies, I came to the conclusion that the case was one of retention of the decidua, and accordingly resorted to the curette for its removal. Menstruation became normal for three periods, when she became pregnant and completed her full period. She is at present in the fourth month of another pregnancy.

Case IV.—Mrs. B., aged thirty, twice married; five children by her first husband, the youngest five years of age. Her illness dated from that time. She complained of great pain at each menstrual period, of the flow being excessive and lasting from eight to ten days. On examination the cervix was found hypertrophied and fissured up to the vaginal junction on both sides, and the lips everted and eroded; the uterus soft and tender to touch, normal in position, three and a half inches in length. Many weeks were spent in this case in treating the conditions described. On Sept. 19th I scraped out the uterus, and on August 3rd performed Emmet's operation upon the lacerated cervix. I last saw her in March, 1885, and found her greatly improved; the cervix was normal in size and shape, and there was no catarrh. Menstruation, also, was less profuse.

Case V.—Mrs. W., aged forty, suffering from metrorrhagia; curetted on October 4th; made a good convalescence. I have not heard of her since.

Case VI.—Miss M'A., aged thirty-two. For last three years menstruation had been very irregular and painful; came on profusely one month and not at all the next. Has leucorrhœa between times, and pain in left ovarian region and back. On examination the uterus was found to be two and three-quarter inches in length, normal in shape and position. Having tried other remedies for months, I determined to resort to the curette, as much for diagnostic as therapeutic purposes. A few thin shreds of apparently normal membrane came away, and subsequently the symptoms remained unchanged.

Case VII.—Mrs. C., eleven years married, no children; complained of severe pain in the sacral and left ovarian region, greatly aggravated during the menstrual flow, which was regular as to time but very small in quantity. She had always suffered from dysmenorrhœa, which had greatly increased after marriage. She first consulted a general practitioner, who divided the cervix, but as this gave her no relief, she went to a specialist, who treated her with intra-uterine applications for nearly three years without any benefit. When I examined her the uterus was apparently normal in every way, except that the passage of the sound caused intense suffering. On October 13th I scraped out the uterus. Since then menstruation has been painless, but is still very scanty. This case puzzled me very much, and I was inclined to think that the cure of the dysmenorrhœa was due more to mental than physical causes; but on reading a most interesting paper of Prof. Schrœder on dysmenorrhœal endometritis, I found there an exact description of its symptoms; and what to me was peculiarly interesting was that the use of the curette was strongly recommended as the only means of cure. The fragments removed, when examined by Dr. C. Rugé, almost always showed inter-glandular development of fibrous tissue, and the discovery of this tissue establishes the diagnosis.

Case VIII.—Mrs. B.; one child in 1883; since then menorrhagia, leucorrhœa, and sterility; uterus retroverted. Having been replaced and pessary adjusted, routine treatment was adopted, with intervals, for some months. Uterus curetted Oct. 27th. Since then menstruation normal.

Case IX.—Mrs. G.; has had two children, youngest six years ago. Since then menstruation had been irregular and very profuse. She was in delicate health and very anæmic; complained of pain in the back and leucorrhœa; the uterus was retroflected, and the passage of the sound caused great pain. The attempt to treat this case by local applications failed, as each attempt brought on hæmorrhage, which lasted from a week to ten days. I therefore had recourse to the curette, and with very good results. When I saw her a few days ago she told me that she was completely cured.

Case X.—Mrs. S.; had three children and a series of miscarriages. She complained of menorrhagia, which was very severe, large clots being expelled with violent expulsive efforts, and a profuse leucorrhœa in the intervals. I had this patient constantly under my care since 1881, and had tried every means that promised relief, but only with temporary benefit. In 1882 I dilated the uterus, introduced my finger and explored the cavity, and then thoroughly mopped it out with fuming nitric acid; this caused a temporary improvement, but when the symptoms again became aggravated she absolutely refused to submit to the process of dilatation. On the 3rd of December I curetted the uterus, and since then she has improved much in health, and the flow has not been so excessive.

Case XI.—Mrs. T., aged forty-two; hæmorrhage from imperfect abortion; uterus scraped out December 18th; menstruation became regular for awhile, but is now very irregular—sometimes three to six months interval. She is evidently at the menopause.

Case XII.—M. N., aged twenty-one; one child seven months previously; since then menorrhagia; uterus curetted December 10th; menstruation became normal; have not seen her now for months.

Out of these twelve cases, seven dated from delivery and three from abortions. One was sterile and five had acquired sterility; two were not married, leaving four, one of which had reached the menopause; one has since borne a child, and two have been lost sight of; eight complained of menorrhagia or metrorrhagia—of these six were cured and two improved; three of dysmenorrhœa, all of whom were cured; all of leucorrhœa, which was less affected than the other symptoms.

In conclusion, I may state that when it is considered that most of these cases had been for months, and even years, under treatment before the use of the curette was resorted to, these results are, upon the whole, very gratifying.

ON OVARIOTOMY.

By LOMBE ATTHILL, M.D., F.K.Q.C.P.;
Ex-Master of the Rotunda Hospital.

[Read in the Obstetrical Section, April 2, 1886.]

THE abdominal section is now so frequently made for the removal, not alone of ovarian tumours, but also of the Fallopian tubes when in a state of disease, and even of the uterus itself, and at the present time engages the attention of surgeons to so great a degree, that no apology is needed for bringing the subject again before this section of the Academy of Medicine. Indeed, I feel it is a duty so to do, because there has been, and even still there exists to a certain extent, an impression that the operation of ovariotomy has not been nearly so successful in Ireland as in the sister island. This undoubtedly was some years ago well founded, but is now absolutely groundless. I propose in the following paper to detail briefly the particulars of a case I have recently operated on, which was in some respects interesting, then to refer to one or two others which possess points worthy of notice, and finally to discuss some matters connected with the subject which are not yet settled.

On the 1st of December last I saw a lady who had previously been under the care of Dr. Jackson Cummins, of Cork, who diagnosed the existence of a solid ovarian tumour. This opinion was confirmed by Dr. O'Sullivan, of that city, who saw her in consultation with him, but the existence of a large quantity of ascitic fluid, the result of a recent attack of peritonitis, induced them to advise the postponement of any operation till such time as the patient's condition should improve. This, under their treatment, having taken place, and the fluid having been in a great measure absorbed, she came to town and placed herself under my care.

The tumour, which was of the size of the fœtal head at full term, was very firm, and, indeed, so hard did it feel that a doubt arose in my mind as to whether it could be a uterine fibroid, the resemblance to which was increased by the existence on its surface of a well-marked nodule, which was the size of an egg. The tumour was, however, freely movable, and could be easily separated from the uterus, which lay behind it, and which was of its normal size. Menstruation had, moreover, been for some time scanty and irregular. This fact, coupled with the extreme mobility of the tumour, and the small size of the uterus, induced me to decide that the tumour was ovarian, possibly a dermoid one, and I anticipated very little trouble in removing it; but on this point, as the sequel proved, I was much mistaken—it proved a most troublesome operation.

I had put the patient under the influence of chloroform a few days previously to enable me to ascertain the exact relations of the uterus to the tumour, and she bore it so well that I decided on using that anæsthetic during the operation; but on this occasion the greatest difficulty was experienced in producing perfect anæsthesia. The moment the knife was used she moved, so we substituted ether, and though it was very freely given, she was never absolutely insensible for more than a few seconds at a time. She would every now and then hold in her breath and force the diaphragm down, the result being that, as the tumour was not in actual contact with the abdominal wall, the intestines were forced out through the opening, and I had to enlarge it greatly before they could be returned and kept *in situ* by means of a large sponge held over them. The anæsthetics were administered by Dr. Andrew Horne, whom I consider to be one of the best chloroformists I have met with. I was assisted by Professor Bennett and Dr. Corley.

The next difficulty I encountered was altogether unexpected, and one I had never previously heard of. I was aware that the tumour was solid, and I had resolved not to attempt to tap it or to lessen it. Accordingly, as soon as it was fully exposed, I seized it with a strong vulsellum, but on attempting to draw it forward it proved to be of so soft a structure that the claws of the instrument tore through it as though a mass of dough or putty. Cyst forceps did no better, and I was compelled to pass my left hand into the abdomen from above the tumour, to insinuate the fingers of that hand between it and the brim posteriorly, and so with great difficulty I tilted it upwards. Quite twenty minutes were expended on this seemingly easy matter. And be it remembered that there were no adhesions, but the tumour fitted the brim so accurately and was so firmly impacted, probably from atmospheric pressure, that great difficulty was experienced in getting the fingers down between it and the brim, while little or no help could be given in front. Here then was a tumour which before the operation seemed so hard that the possibility of its being a pedunculated fibroid presented itself, and yet, in fact, was so soft and friable that no form of vulsellum could retain a hold of it. I should have mentioned that its upper surface was coated with a soft, curdy substance, which seemed to be unorganised lymph, and that there was a considerable quantity of semi-purulent fluid in the abdomen. The nodule which was felt on its surface proved to be a cyst full of clear fluid; this ruptured, and was destroyed during the process of extraction.

The tumour was subsequently examined by Mr. Bewley. He reports that—"It is a sarcoma of the class commonly termed 'recurrent' by surgeons, and but rarely met with in the ovary. It is composed mainly of small spindle-shaped cells, lying in fibrous tissue; in some parts it is nearly altogether composed of these cells; in others a larger proportion of fibrous tissue exists. Its softness is due to the very large proportion of cellular structure present."

The tumour, when at last drawn forward, was found to be attached by a tolerably long and broad pedicle. The structure of this, too, was very soft; it was ligatured in sections, but even so, several vessels had to be separately tied, and there was some troublesome hæmorrhage.

After the usual precautions of thoroughly cleansing the abdomen, the wound was closed, and though the operation in all occupied two full hours, the patient's condition at its termination was very satisfactory. The pulse was good, as was also the colour of the face.

In the afternoon she complained of pain, and begged for opium, of which she had taken a good deal during the attack of peritonitis from which she had only recently recovered, but being convinced that patients do better without it, I refused at first to give any. At 10 p.m. the pulse was only 105, and the temperature 99·5°, but she was sleepless, restless, and complained so much of pain that I reluctantly gave her half a grain of morphia hypodermically. After this she slept for a couple of hours, and had a quiet night. In the morning neither the pulse nor temperature had risen, but she vomited a little dark-coloured fluid, and in the evening was so restless that I repeated the hypodermic injection of morphia, but it produced no sleep. She had a restless night, and next morning was troubled with the incessant regurgitant vomiting. This I suspected was due to the morphia, for her pulse was but 85, and temperature absolutely normal. So I merely forbade he rtaking anything by the mouth except a little ice, and as she complained of being weak, ordered 2 oz. beef-tea and ½ oz. brandy to be administered by the rectum every three hours. In the afternoon the vomiting ceased, and before the end of the week she was absolutely convalescent.

Two curious circumstances occurred during the course of her recovery—one, that at about twenty-four hours after the operation a pretty copious oozing of blood occurred from the wound, which I suspected must have come from the pedicle; it soon ceased, and, as the pulse was good and quiet, and her condition in all other respects satisfactory, I did nothing for it. The second was that, on the seventh day after the operation, serum oozed from the wound in such large quantities as to render it necessary to change the bed-clothes. Previously to this the wound seemed to have united perfectly, but during its occurrence I could, by pressing on the abdominal walls laterally, force out the serum through a very small opening near its centre; this closed the day following.

This case, in which recovery took place so rapidly, contrasts strongly with that of the lady on whom I operated some years ago, and in whom vomiting continued almost incessantly for more than a fortnight, being only restrained for a few hours at a time by the hypodermic injection of morphia; during all this time she was fed *per rectum.* There was enormous distension of the abdomen, and the bowels did not move for twenty-one days. This patient ultimately recovered perfectly, all these distressing symptoms having been due to enteritic inflammation, the cause of which I never was able to ascertain, but she was in very bad health prior to the operation, which was undertaken as a last resource.

This patient, as already stated, was fed for many days *per rectum*, $1\frac{1}{2}$ oz. beef-tea and $\frac{1}{4}$ oz. brandy having been injected through a large catheter every third or fourth hour. This proved amply sufficient to sustain life for a fortnight. On two or three occasions I administered enemata containing other substances—such as milk, beaten-up egg, &c., but they invariably caused discomfort, and after a few hours were expelled, nearly unaltered in character, while the beef-tea and brandy were always retained and absorbed.

A curious, and, as far as I am aware, unique sequence occurred in one of my cases—I think this was the forty-sixth in my list of operations. The patient was a delicate woman; the operation was an easy one, and all went on well, save that there was always a little oozing of semi-purulent fluid from the lower edge of the wound, and that the temperature kept up above the normal standard; her convalescence was consequently tedious. Finally, I sent her to the country, though there was still a little discharge from the wound. Quite two months after the operation she came to me in a state of much alarm, stating that "a worm had come out through the wound," and that she had brought it me in a bottle. On examination, this proved to be the ligature, which, instead of having become embedded, as we suppose is usually the case, had separated and been discharged through the fistulous opening in the wound, which, after this had occurred, closed rapidly. This lady has since given birth to a child.

Just a year ago I read a paper here giving the details of a very interesting case I had recently operated on, but at that time the patient had not recovered perfectly. I am now in a position to give some additional particulars of interest. The operation was undertaken when she was almost *in extremis*, the temperature in the morning on which it was performed was 103°, and the diagnosis of a suppurating cyst had been made, which the operation verified. The tumour was bound down by dense adhesions to the pelvic walls laterally and posteriorly, and in separating these I opened a large pelvic abscess, the existence of which had not been suspected, and indeed could not have been detected. The abdomen was soon full of fœtid pus and blood; after this had been sponged out, I poured into the abdomen a quantity of carbolic lotion, and sponged it out again. Subsequently, for many days I injected carbolic lotion into the abdominal cavity through a drainage tube, and sucked it out with a syringe. On the fifteenth day after the operation a fistulous opening formed in the rectum, and fæces were discharged through the wound, the lesser end of which was still open. This continued for months. On the 29th July, seven months from date of operation, she menstruated normally, save that she suffered a good deal of pain, and she has since done so regularly, but on the 25th October its appearance was preceded by intense pain, felt all over the abdomen, which terminated in a copious discharge of blood through the wound, though previous to this the fistulous opening had apparently closed. This lady is now enjoying good health, menstruates normally, and does not suffer from any difficulty in defæcation.

There are many matters bearing on the details of the operation of ovariotomy and abdominal section generally still unsettled, prominently that of the use of antiseptics. We find Sir Spencer Wells, Mr. Knowsley Thornton, and others, continuing to carry out Listerism in all its details with the most satisfactory results. On the other hand, Mr. Lawson Tait scoffs at it, and by the careful use of pure water obtains as good results; while Mr. Keith, of Edinburgh, once an enthusiastic upholder of the use of the spray, abandons it after having for years carried it out with most satisfactory results, and still obtaining marvellous success without its aid; he, however, employs all the other antiseptic precautions. Such facts must shake the faith of the most devoted follower of Lister as to the absolute necessity of his system, but they do not necessarily in any degree prove the fallacy of his theory; they prove, to my mind, that the use of the spray is unnecessary; that, by the strictest attention to cleanliness in every detail, antiseptics may be dispensed with, but they do not prove that it is wise to do so. Admit that with the greatest possible care no particle of septic matter remains on any instrument, or in any sponge, or material used for ligature, suture, or dressing, it does not follow that we do not add greatly to the certainty of obtaining absolute safety for our patient by immersing our instruments, sponges, &c., in a disinfecting fluid. Years ago, when the spray was in general use, I pointed out that the cold it produced on the exposed intestines and peritoneum was very objectionable, and I ceased allowing it to play directly on the patient. I believe now that it is best not used at all during the operation, and that it should be discontinued at its commencement; but I continue to wash my sponges and keep my instruments, ligatures, &c., lying in an antiseptic fluid. Nay, more, I carefully wash with my own hands all my instruments, prior to the operation in the same, and keep my sponges, after being thoroughly washed in cold water, lying for at least twelve hours in a solution of perchloride of mercury. But all these precautions are useless if the principles on which they are founded be forgotten during the performance of the operation, and yet they often are. I have been present at an operation in which the room was full of carbolic spray, all the instruments lying in deep trays full of carbolic lotion, but have seen the patient covered with a blanket so soiled that it must have recently been taken off some not over cleanly bed, and on this I have seen the surgeon repeatedly lay his knife, clips, torsion forceps—in fact every instrument he used, one after the other, to be taken up and used again, oblivious that thereby he was rendering nugatory all his previous precautions. In like manner, I have seen ligatures used without having been passed through the antiseptic solution. Can this be called Listerism? Is not the whole thing rendered a farce, and better altogether omitted. The use of antiseptics becomes in the hands of such a man not alone a delusion but an actually injurious practice, and one calculated, most unjustly, to bring the system into disrepute.

With respect to sponges, I use only a small number, and these I make use of over and over again. I find that ten small and three large flat ones are, as a rule, all that are needed. If you have a large number in use they are apt to be mislaid. I have seen the abdomen searched all over at the end of an operation for a sponge which had rolled unnoticed into a corner of the room, where, if only a limited number had been used, one, if dropped, would have been missed at once. But I always have two in reserve to replace any which, from having dropped on the floor, or from any other cause, may be deemed unfit for further use. The operation being over, I wash the sponges myself in pure water thoroughly, and then leave them soaking for 24 hours in a solution of the perchloride of mercury, of the strength of 1-1000; squeeze them out of this, and dry them thoroughly, and they again go through the same process before being used.

In many cases I find it better not to use the trocar at all. If the tumour consists of one large cyst full of fluid, it is often satisfactory to empty it through Sir Spencer Wells' trocar; but if it be multilocular nothing is gained by the use of the trocar. It is best to have the sides of the abdomen compressed by an assistant so as to bring the tumour well into the opening in the abdominal wall, and to make an incision into it; by this proceeding trouble is saved, and the tumour emptied much more rapidly and satisfactorily.

The after-treatment really should consist, as far as possible, in doing nothing. Absolute rest and the administration of nothing but ice for at least twenty-four hours should be all that is needed. Indeed, in general, it is better not to give anything but ice for two days. Of course the occurrence of severe pain or great restlessness may compel us to administer morphia hypodermically or per rectum, but those patients from whom it can be withheld do best. In the patient whose case I narrated at the commencement of this paper, the troublesome vomiting from which she suffered on her second day was due, I am satisfied, to the morphia which she insisted on having; but if vomiting sets in after the operation, and the use of ice fails to stop it, a hypodermic injection of $\frac{1}{3}$ or $\frac{1}{2}$ grain of morphia often acts like a charm.

On one other point there is some difference of opinion—namely, as to the size of the incision. I have always followed the example set by Sir Spencer Wells, and made as large an opening in the abdominal wall as would enable me to see what I was doing, holding that the size of the incision does not affect the chance of recovery. Of course it should not be larger than necessary; but I hold that it is not wise to force a semi-solid tumour through a small opening, and still more so not to leave space sufficient to enable you to see clearly any bleeding point. Mr. Lawson Tait, on the other hand, insists strongly that the incision should be as small as possible, and that a large one tends to favour the formation of a hernia. I cannot agree in this. I have now been watching cases for fifteen years; several of these have had children, and others have large pendulous abdomens, but in not one have I seen hernia to form through the line of the incision, and the fear of its occurrence will never deter me from making one sufficiently large for the object I have in view.

In this city the actual cautery is seldom used for the division of the pedicle. Still in many cases it is the most satisfactory method of treating it. If the pedicle be a thin one I do not think there is any risk in using the cautery in place of the ligature. Mr. Thomas Keith has used it extensively, and, as well as I can remember, had secondary hæmorrhage on only one occasion, and we all know that hæmorrhage from the pedicle after the use of the ligature sometimes occurs. In the case of a soft, unhealthy, vascular pedicle, such as in the case I have related at the commencement of this paper, the actual cautery would, I think, have been safer than the ligature; but in a thick pedicle, with large vessels in it, I should certainly prefer the ligature. The case I alluded to a few moments ago, in which the ligature separated, acted as a foreign body, and eventually came away through the abdominal wound, shows that, if possible, it is an advantage to omit the use of any ligature, except, indeed, those made of catgut, and I think in time the cautery will be more frequently used than at present.

LABIAL HÆMATOMA OR PUERPERAL THROMBUS.

By ROBERT FOSTER DILL, M.D.,
Professor of Midwifery in the Queen's College, Belfast.

[Read in the Obstetrical Section, May 7, 1886.]

When the immediate perils and dangers of a case of labour have been brought to a close, and when the accoucheur deems the case to have been conducted to a successful issue, is it not most mortifying for him to contemplate some terrible catastrophe which has occurred, and for which he is altogether unprepared? Such was the position in which I found a medical friend when called in to give him what asssistance I could, when, as he had believed but a short time before, he had guided his patient successfully through her first case of labour. I was introduced to our patient; she was young, not over twenty-one, and delivered but a few hours before, without artificial means, of her first, a male child. At this time she was suffering from severe shock, and from intense local pain. The pulse was very quick and feeble, and the thermometer indicated a high degree of temperature. There was an extreme nervous alarm over her regarding her condition and prospects; and she seemed to be on the brink of a hysterical seizure. Her attendant exposed to view a very large dark tumour, situated in the right labium, which had suddenly reached its present size, being now rather over the size of a child's head. The lady was extremely sensitive to touch, and an examination and a manipulation of the tumour caused much pain and great irritation. On the inner surface of the swelling—that is, a little within the vulva, which could only be reached with difficulty—the finger came on a thin membranous-like fold, which immediately gave way, and within was a cavity containing a quantity of fluid and clotted blood, which was soon removed. The blackness or ecchymosis extended round the buttocks and down the thigh, and over the abdomen above the umbilicus, and nearly as high on the right side as the axilla. Under such circumstances, was it to be wondered at to find Mrs. D. suffering from severe shock and intense pain? The cavity from which the blood came was immediately filled with a pledget of lint saturated with weak carbolic water; the tumour was covered over with lint saturated in the same carbolic water, made comfortably warm, and the whole was covered up with gutta-percha tissue. This, with frequent stupings or fomentations, alternated with linseed meal poulticings, constituted the main local treatment, and by this much ease and comfort were afforded. The vagina and uterus, I should add, were frequently syringed with antiseptic fluids. The other treatment consisted in mild aperients, enemata, five-grain doses of sulphate of quinine, three or four times in the twenty-four hours, changed after a few days for iron and other tonics. Catheterism, as might naturally have been expected from the swelling and inflammation of the parts, was found necessary for some days.

Within a fortnight we had the satisfaction of witnessing some improvement appearing in the case, which improvement steadily continued, and the lady afterwards made a rapid and a good recovery from this very serious complication of the puerperal condition.

Although I have frequently seen in practice hæmatoma, thrombus, and pelvic hæmatocele, yet I had never before seen such a large blood-tumour as the one I have now the honour of bringing under the notice of the members of the Obstetrical Section of the Academy of Medicine, and I am not sure that I have seen recorded more than one or two cases in which the size of the tumour was so large or the sufferings and symptoms were more aggravated than those found in the case thus briefly described.

The diagnosis is very easily made, for when the tumour has acquired a considerable size it presents a very dark—indeed a black—colour, and when so large as in this case, the vagina appears to be completely filled up, and the entrance at the vulva seems almost in a state of occlusion. The rapid growth, great hardness, and intense pain, appear to be the peculiar characteristics of a blood-tumour.

The prognosis is not so easily made—indeed it becomes sometimes serious—and must be so (1) because of the large amount of blood which has become so extensively infiltrated through the tissues and beneath the skin, and (2) shall I also say within the lining membrane of the abdominal cavity? and (3) because we find that in such cases as are on record the mortality is reported as very high—that is, more than one in three. Death, as might naturally be expected, comes either by shock, exhaustion, bursting, hæmorrhage, gangrene, or by suppuration. But, luckily for us, the case just described terminated by resolution and the cavity closed by granulation.

The folds of skin and mucous membrane in the neighbourhood of the vulva enclose numberless small vessels—veins and arteries—and these are embedded in a copious supply of cellular substance. In pregnancy these vessels become larger, are often varicose, and in the last struggles of labour their rupture may take place, and the blood finds its way, is infiltrated, and occasionally accumulates in large quantity in the loose cellular tissue. It is in this way that such blood-tumours are formed, and it was thus that the puerperal thrombus, hæmatocele, or labial hæmatoma just described was formed.

ON THE SO-CALLED LAPAROTOMY EPIDEMIC.

By THOMAS MORE MADDEN, M.D., F.R.C.S.Ed.;
President, Obstetric Section, Academy of Medicine;
Obstetric Physician to the Mater Misericordiæ Hospital.

[Read in the Obstetrical Section, Friday, June 4, 1886.]

I. INTRODUCTION.

In the hope of inducing the discussion of a subject which urgently demands the consideration of the Obstetrical Section of our Academy, previously to its adjournment for the session, I desire before vacating this chair to submit some observations on what has recently been elsewhere described as "the Laparotomy Epidemic." With this we are distinctly concerned, for if that term be rightly applied it is our province here to oppose as far as possible any extension in this country of operations thus characterised. But if, on the other hand, the operations included in this term be, as is asserted, important steps in the recent progress of our art, it is no less obviously our duty to encourage their employment in appropriate cases.

To determine these points, therefore, it will be well for us to take counsel together, and with this object I shall endeavour to place before you some aspects of the laparotomy question from my own point of view. At the same time, I would also venture to hope that in the ensuing discussion all sides of the question may be fully, fairly, and temperately debated, and that whatever conclusions are arrived at thereon may bear the imprint of the sound professional judgment and common sense generally characteristic of the teachings and practice of the Dublin School of Medicine.

In the following observations the term laparotomy is employed in its commonly accepted, rather than in its strictly accurate sense, and is applied to intra-peritoneal operations on the uterus or its appendages. Amongst the latter, however, what is now more generally described as ovariotomy is not here included, although some reference must be made to so-called "normal ovariotomy." Nor indeed is there any further occasion for debate with regard to an operation such as ovariotomy, the value of which has been demonstrated beyond any possibility of controversy.

The most important points for our present consideration are the expediency of imitating in our practice here that frequent recourse to laparotomy which is elsewhere in vogue in affections of the uterine appendages, or connected therewith, and also in the treatment of fibro-myoma and other diseases.

With regard to the frequency of the complaints which laparotomy for removal of uterine appendages is resorted to, so far as from reading and experience I can form any opinion on this subject, I would say that the ovarian and tubal diseases referred to have been long recognised, although of late years their frequency seems to be somewhat greater than was formerly the case, and at the same time their diagnosis has been facilitated. Secondly, with regard to treatment, it appears to me that in some instances those ovarian and tubal disorders for which laparatomy is advocated may be treated without recourse to the operative procedures included in this term. At the same time it must be admitted that in certain cases these operations afford a means of saving life, or of relieving suffering formerly regarded as beyond remedy.

No one acquainted with ancient medical literature will question the continually recurring influence of fashion on medical opinion and practice in every age; nor can it be gainsaid that in successive epochs various forms of disease and methods of treatment come into and go out of vogue. Thus, at the present time it is apparently rather the fashion to ascribe obscure female complaints to ovarian and tubal diseases, as five years ago it was the fashion to attribute somewhat similar symptoms to uterine displacements and flexions, or ten years before to set them down to uterine inflammations and ulcerations.

II. INCREASE OF LAPAROTOMY.

To exemplify this I subjoin a few extracts which, although possibly *ex parte* and capable of satisfactory explanation, are yet interesting as indicating the importance at the present time attached to the laparotomy question itself, and the desirability of discussing it without prejudice, and apart from any personal or local controversies that may have been elsewhere imported into it, and concerning which we have here no special cognisance and are not competent to offer any opinion. Hence, whilst now expressing my own views on general grounds on the laparotomy question, and, in exemplification, referring to particular statements elsewhere published, I shall very willingly accept any correction found necessary in them. I have always held that in controversies concerning any suggested means of relieving human suffering or prolonging life, all personal cousiderations should be rigidly excluded, although I have myself on former occasions had some reason to think that this view is not universally accepted. But, as "hard words break no bones," and as strength of language is not synonymous with force of argument, I have weathered the storm formerly raised by my discussion of these questions, and, whilst maintaining my own opinions, hope to do so with due deference and respect for the views of those who—and as I am sure they think rightly—differ from me.

The increasing frequency of laparotomy in recent gynæcological practice—as well as the fact that this procedure is no longer invariably regarded as one only to be adopted with cautious deliberation in extreme and exceptional cases, but that, on the contrary, it is now sometimes recommended for diagnostic as well as therapeutic purposes—seems established. Thus, in the last number of the *British Gynœcological Journal*, under the heading "Explorative Laparotomy," prominence is given to the following citation:—

"There is yet room for missionary work before men—and good men too—can be induced to come out of their shell of conservatism so-called, and with a bold front to help to break down the prejudices and misgivings based on an ill-founded fear of the peritoneum and its behaviour under the knife. But true conservatism consists in conserving or keeping the lives of those entrusted to our keeping. In the hands of the true surgeon prompt measures of relief will naturally follow certainty of diagnosis, and none the less so because the latter may involve the carrying out of the advice that Mr. Tait is said to have given to Professor Lusk on one occasion, when grave intra-peritoneal conditions demanded a positive diagnosis—'Cut the patient open and find out'—advice based on common sense, sound principles, and conviction, growing out of an exceptionally extensive and varying experience in the surgery of the abdomen."[a]

Within the past few months the laparotomy question has been brought prominently forward in medical journals and societies. For example, in the *Provincial Medical Journal* of 1st February, it is said—"Thus oöphorectomy is suggested as the panacea for all the ills from which unfortunate women suffer, and their ovaries are removed with as little impunity as the butcher spays his sows, and with the same consideration for the wishes of the patient. An ethical question arises here on which we have a very pronounced opinion. The ovaries should not be removed without the consent of the patient. It is always easy to explain to the patient what is proposed to be done. This is only fair and just." In another journal we are told—"It is more than hinted that women are spayed without being told what the nature of this operation of spaying is, and the position in which they will be placed by it."

In a recent discussion on this subject, from which limited space prevents my quoting *in extenso*, some remarkable statements may be found (as reported in the *Medical Press and Circular* of February 14th) with regard to the frequency of laparotomy operations, and the circumstances under which, it is there asserted, they are occasionally resorted to. Thus, *inter alia*, it was mentioned by Dr. Carter that, in one hospital, "no fewer than one hundred and eleven women had been deprived of one or both ovaries during the year 1885." In some of the cases referred to during this discussion, it was stated that the ovaries were removed "without the patients'

[a] George R. Fowler, M.D., New York Medical Journal.

knowledge of the condition to which they were thus reduced," although, it should be mentioned, that this statement as well as the accuracy of some of the statistics then referred to was controverted.

III. LAPAROTOMY FOR UTERINE FIBRO-MYOMATA.

In my Inaugural Presidential Address here, "On the Recent Progress of Obstetric and Gynæcological Medicine," I referred briefly to this subject. As, however, that Address will not appear in our "Transactions," to which I trust this paper may be admitted, I will recapitulate a few observations bearing on the topic under consideration. One of my reasons for so doing is, in accordance with my promise, to place on record in the "Transactions"—in the last volume of which a communication of mine was published, in reference to which a complaint has been since made—this evidence of my desire to clear up a point on which some misunderstanding took place between Mr. Lawson Tait and myself. In that inaugural discourse, alluding to the enthusiasm prevailing with regard to abdominal surgery, I said:—"In a paper read here last session, on Uterine Fibro-myomata, I endeavoured to show that in their treatment abdominal section, although in some instances necessary, was by no means invariably indispensable. I pointed out that such tumours might occasionally be removed by enucleation per vaginam; that in other cases they could be kept in check or their symptoms obviated by purely medical means; and that in others, again, they call for no active treatment whatever. In so doing I referred to the statistics published by several eminent specialists, one of whom (Mr. Lawson Tait) has since complained of being misrepresented in a passage in which, alluding to certain incomplete operations, I said—'Of these incomplete operations Mr. Lawson Tait thinks that "he may speak with a certain amount of satisfaction," though from whence he derives this contentment I am at a loss to understand, as his mortality in them was 50 per cent.' He says—'Of one group I think I may speak with a certain amount of satisfaction, that is the group which includes thirty cases of incomplete operations, even if all that I can say is that three per cent. of incomplete operations is not a large proportion, and that I have the satisfaction of knowing that it is still on the decrease, as my experience grows.'"

In reply I pointed out to Mr. Tait that in the passage to which he takes exception I was referring to the rate of mortality in one particular group of cases, which was stated to be 50 per cent., and that I saw no unfairness in my not having quoted those words, of the omission of which he complains, as they do not bear on the stated mortality in that group of operations, but referred to the diminution of incomplete operations in his practice, and not to their result. In my paper and elsewhere I have fully acknowledged Mr. Tait's skill and ability, and as I feel free from any consciousness of having misrepresented his statements, I regret that he should be under the mistaken impression that I desired to do so.

My views on the treatment of myoma have been more than once controverted, more especially with respect to the general necessity of constitutional as well as operative treatment, and the comparative advantages of laparotomy and vaginal operations, and as the efficacy of my methods of dealing with such cases has been disputed, I shall—since the question comes directly within the scope of this communication—again summarise my opinions as confirmed by my present experience on these points. This I am induced to do with the object of having my statements here discussed in the presence of colleagues who are cognisant of my practice, and who have kindly assisted me in the treatment of such cases, some of which are now in my ward in the hospital. Under these circumstances, I may again point out that whilst in certain instances of fibro-myoma laparotomy may be useful, in other cases myomata may be advantageously dealt with by medical treatment; in some instances they can be treated surgically without abdominal section, and in others, again, they appear to require no special treatment whatever. I have myself seen cases in which these tumours have, in the course of time, become notably reduced in size, and instances of their complete subsidence in this way have been recorded by Dr. Kidd and others.

With regard to oöphorectomy, as well as hysterectomy and myotomy, it appears to me that one of the causes of the frequent employment in recent practice of these operations is the fact that in the minds of many practitioners the very successful results attained by modern ovariotomists have probably influenced the extension of abdominal surgery in this as well as in other directions. It should be borne in view, however, that there can be no parity in the treatment required for ovarian and uterine tumours—the former being, as a rule, though not without exception, rapidly progressive in their development and eventually fatal, whilst the latter, however much discomfort and impairment of health they may occasion, seldom directly destroy life, and in many instances become arrested in their development at the menopause and in the course of time.

The mortality of hysterectomy and myotomy should, as a general rule at least, in my opinion, exclude them from consideration as measures of election in the treatment of uterine tumours. Dr. Keith has said :—"The proportion of cases of uterine fibroid, in which interference of any kind is at any time warrantable, is extremely small. It is not perhaps greater than five per cent. of all cases. I have no hesitation in saying, from what I know, that operations for fibroids are far too often performed, considering the fatal nature of the operation. At present there is a speculation abroad for abdominal section, and a woman with a movable tumour in her abdomen has in these days a small chance of escaping the said section. As the operation stands at present, its mortality is perhaps greater than that of any other surgical operation. It ought not to be undertaken without some strong necessity, for not one fibrous tumour in twenty gives the woman any trouble, or scarcely any, during the whole menstrual life, and a death directly from one is extremely rare."[a]

In reference to oöphorectomy, which has been largely employed of late years not only for the prevention of hæmorrhage, but also for the arrest of the development of uterine tumours, it appears to me that further evidence is needed of the general necessity of this procedure in such cases. That it can be carried out with safety in a large number of instances there is no question, or that it may be effectual in some cases of actively-developing fibromata not

[a] Edin. Med. Journal. May, 1885. P. 969.

removable by the vagina, as a means of checking the growth of the tumour and arresting hæmorrhage, more especially in young patients, who might not otherwise reach the menopause. Under other circumstances, however, the removal of the ovaries merely for arrest of hæmorrhage appears to me inexpedient until other methods of checking it have been tried.

In this connection I must further refer to the alternative measures which, according to my experience, may in many cases be substituted for the procedures just alluded to with a fair expectation of a successful result.

First, then, with regard to enucleation. This, although commonly restricted to submucous tumours, and even excluded in such cases by some authorities, is, as I have elsewhere proved, applicable to many cases in which the size and position of the tumour is such as to render it capable of removal through the vagina.

With regard to medical treatment, I can still speak confidently from frequent experience of its value in the management of cases of fibro-myomata such as I have now in hospital, in which for various reasons operative treatment is not deemed necessary, expedient, or feasible. By appropriate treatment of this kind we may, in many instances, keep the hæmorrhage, pain, and other symptoms of fibro-myoma sufficiently under control, so as to enable patients to await the menopause with a fair amount of comfort, even if we cannot hope to thus exert any direct influence on the uterine tumour itself. With this view our treatment should, in the first instance, be directed to the arrest of the metrorrhagia resulting from the myoma by the free use of liquor ergotæ, which I prefer to ergotine, and employ in large doses hypodermically as well as by the mouth, and which in my practice not only seldom fails to check such hæmorrhage, but also in some cases produces, by its long-continued administration, a well-marked diminution in the size of the tumour. To aid in thus diminishing the congestive hypertrophy that attends the development of uterine myomata, iodide of potassium may also in many cases be freely employed with the happiest effect, when administered in suitable cases, in efficient doses, and for a sufficient length of time.

Lastly, I shall here reiterate the opinion I have elsewhere expressed more than once, and which is founded on long observation and experience—*i.e.*, that in cases of myomata in which operative interference is not expedient, we may possibly succeed in arresting the progress of the disease and prolong the life of the patient by sending her to one of those suitable iodated or bromated spas which are described in my work on "The Health Resorts of Europe and Africa."

IV. LAPAROTOMY FOR EXTIRPATION OF CANCEROUS UTERUS.

The extirpation by abdominal operation of the entire uterus in cases of cancer might also be included within the scope of this paper. And although it would be impossible to discuss the subject within our present limits, it may, at least, be remarked that it has been clearly shown by Dr. W. A. Duncan that whilst the mortality in 137 cases of abdominal extirpation was 99, or at the rate of 72 per cent., in 276 cases of vaginal extirpation of the uterus there were only 79 deaths, being a death-rate of 28·6 per cent. It was forcibly observed, however, by Dr. Priestley, in the discussion on this subject in the London Obstetrical Society:—"Cancer was so pitiless and distressing, and a disease so much dreaded, that all must welcome a remedy, however severe it might be, if such a remedy gave a chance of recovery at the time and of immunity for the future. And it might well be said that the operation for extirpation of the uterus, although perilous, was not out of proportion for the gravity of the disease for which it was proposed. It was not a great operation for a minor ailment, which the satirist had characterised as apparently undertaken more for the benefit of the surgeon than for that of the patient."[a]

V. LAPAROTOMY IN OVARIAN AND TUBAL DISEASE.

Turning now to the consideration of the pathological importance and treatment of diseases of the uterine appendages, which are held to require operative interference, it may not be unprofitable to refer briefly to some of the earlier writers on this subject. This

[a] Obstetrical Transactions. Vol. XXVI., p. 98. London, 1886.

is, I think, the more desirable, inasmuch as—from the manner in which the attention of the profession has become directed to ovarian and Fallopian tube diseases—it might be supposed that these complaints have only recently become recognised. In this respect the history of these disorders is of interest as illustrating the commonly evinced tendency in medical literature to confound new names with new facts. For it is unquestionable that inflammation, and the consequent pathological changes resulting therefrom affecting the ovaries and Fallopian tubes, were recognised by the older pathologists and gynæcologists long before the more scientific names by which they are now described had been made as familiar as household words by their successors. With regard to ovarian disease this fact is, I believe, admitted, and hence I need not refer further to it; but with regard to Fallopian tube disease I am not clear that this is so. The evidences of Fallopian tube disease leading to serous exudation or purulent collections therein, or, in other words, of hydro- and pyo-salpinx, are recognisable clinically as well as on pathological investigation. Now, however wanting in other respects our gynæcological predecessors may by some be perhaps supposed to have been, they do not appear at all deficient in accurate clinical observation, nor do the writings of the great pathological teachers of a former age exhibit any trace of their inability to recognise such obvious pathological changes as those under consideration, and, as a matter of fact, such cases are referred to or described by Blundell, Davis, and other practitioners, as well as by pathological writers.

Thus Morgagni, in that neglected treasury of pathological knowledge, his unrivalled work, "De Sedibus et Causis Morborum" (Epist. XLVI., section 27), describes a very remarkable case of pyosalpinx. His great English rival, Dr. Baillie, in his "Morbid Anatomy," relates a case of hydrops tubalis, or, as we now term it, hydrosalpinx. Mason Good, in his "Study of Medicine," in 1829, devoted a chapter to its consideration, and refers to a case related by Munick, in which the distended tube contained a hundred and ten pints of fluid; and Dr. Blundell, in his "Obstetric Medicine," also refers to this subject; Harder cites a case in which the fluid measured one hundred and forty pints; and Cypriani another, in which it amounted to one hundred and forty pints" ("Epist. Hist. Fœtus ex Tubâ Excisi," Leiden, 1700). Worms also describes an instance of this kind ("Abhandl. einer ungewöhnlichen Krankheit, &c." Rastadt, 1785). Dr. Davis, in his "Obstetric Medicine, 1828" (Vol. II.), narrates a case of pyosalpinx, in which the Fallopian tube contained a pint and a half of pus. Another similar case is referred to by Portal, in which the tube was distended by eighteen pints of purulent matter.

These references are here given merely with a view of proving that, in the works of the older authorities cited, such cases are described, though in their experience their occurrence was infrequent. It would, therefore, be of interest if any one would afford us some explanation of the causes of the apparently increasing frequency of grave cases of disease of the uterine appendages, for which, in modern practice, operative measures are so often deemed expedient.

Having referred to some of the earlier writers on this subject, and as the name has been mentioned in connection therewith of Dr. Blundell, of whose kindly approval of one of my earliest publications I treasure the memento, I have again looked through his generally neglected "Principles and Practice of Obstetric Medicine," and would commend his observations on abdominal surgery, of which he was a pioneer, to the consideration of any one not familiar with his works. In the "Physiological Researches" says Dr. Blundell, "we will find, together with some other memoirs, a paper on the subject of abdominal surgery, in which I put together the principal facts which had come to my knowledge—all concurring to prove that it is possible to lay open the abdominal cavity more or less extensively, not without danger (for that I would never assert publicly or in private), but without necessarily destroying life in the way that some of our established surgeons seemed to imagine, especially in this country. This principle has now received further corroboration from further observation on the human body; in cases in which the abdomen has been laid open more or less extensively, and where the patients have not died. . . . In obstetrics, everywhere to intermeddle is bad; in obstetrics on all occasions, our operations are an evil; and hence in this, as in every other case, it becomes us to ponder duly whether the remedy or the malady is to be regarded with greater apprehension." [a]

In reference to the foregoing citations it may, perhaps, be said that the older medical literature quoted can have no bearing on the laparotomy questions of the present day. I think otherwise, for, as I have shown, some of those diseases of uterine appendages for which laparotomy is now employed, have long ago been discussed; and if, as Dr. Johnson says, "no use is to be made of the labours of past ages, the world must always remain in the infancy of knowledge. The discoveries of every man must terminate in his own advantage, and the studies of every age be employed on questions which the past generation had discussed and determined."

Be this as it may, however, returning from former writers who have referred to this subject to the more familiarly known authorities of the present time, we find not only in recent medical journals, but also in modern text-books, abundant evidence that the frequent necessity or expediency of laparotomy for removal of uterine appendages is by no means universally accepted. In proof of this, it may be noted that Sir Spencer Wells, in the last edition (1885) of his classic work "On the Diagnosis and Treatment of Abdominal Tumours," mentions the particulars of only four cases in which he removed the Fallopian tubes for any disease; and adds: "Considering how very frequently I have performed the operation of ovariotomy, it seems remarkable that these four cases are the only examples I have met with where either one or both Fallopian tubes have been so diseased as to require their removal." Another eminent writer, Dr. Atthill, speaking in the last edition of his "Clinical Lectures" on the subject of oöphorectomy, in cases in which the mental and neurotic element predominates, says:—"As the result of my own experience, I believe the number of cases in which it is justifiable is very few, and I protest against the frequency with which it has been performed by some surgeons."

In the "Proceedings of the New York Academy of Medicine," it

[a] Dr. James Blundell. Op. cit. P. 822.

was recently observed by Dr. H. C. Coe:—"There are not a few women now attending the various clinics in New York who have had their ovaries and tubes removed, and yet who complain of precisely the same pain as before; in fact, I can recall cases in which, although the menstrual disturbance is wanting, the pain is more severe than it was before."

If Fallopian tubal and ovarian diseases were always prevalent in these countries, and if they can be properly treated only by the removal of the uterine appendages, it may be well asked here what became of patients so affected before the recent introduction of laparotomy operations as a rule of practice in such cases? If such patients survived these diseases without the operations now deemed essential, it is surely a strong argument, and one which I would ask the Section to weigh well against the alleged general necessity of the operations in question. If, on the other hand, such patients died largely from these Fallopian tube and ovarian diseases before the present method of dealing with them was employed, it would be as strong an argument in favour of laparotomy. Therefore, as evidence of any formerly more frequent occurrence of deaths from these causes should, probably, if forthcoming, be discoverable in our pathological museums and in the successive mortality reports of the Registrar-General, I have searched in these for proof of this, and, as far as my comparatively limited researches have gone, I have not yet found it in either.

In the course of my own gynæcological experience, now extending over several years, I have from time to time met with a good many cases of disease of uterine appendages. Comparatively few, however, of these were of any great pathological importance. Nor in my earlier experience as a Demonstrator of Anatomy did I encounter the evidence of such disease in the dissecting room, except in one instance of conjoint pyosalpinx and large ovarian abscess. I therefore think I am justified in the conclusion that serious cases of tubal or ovarian disease, excepting of course cystic disease, do not appear to be commonly met with in this city in either gynæcological practice or in *post mortem* examinations.

In those cases of salpingitis and pyosalpinx which have come within my own observation, the most frequent cause of the disease was the extension of subacute inflammatory action from the uterus through the patulous Fallopian orifice after parturition. The next most frequent causes of either condition were gonorrhœal infection and catarrhal inflammation, and in one instance it was obviously traceable to sexual irritation. In these cases the symptoms which I regarded as characteristic of tubal disease were localised pain and tumefaction in this region, with, in chronic cases, dysmenorrhœa, congestive hypertrophy extending to the uterus, constitutional disturbance, and reflex mammary irritation. These symptoms sufficiently point out not only the seat of the disease, but also indicate the general line of treatment required.

A chief source of danger in case of inflammatory action in the uterine appendages is the risk of the products of such inflammation, whether serous or purulent, and whether ovarian or tubal, rupturing into the peritoneal cavity, and this fact has probably influenced the early operative treatment of the diseased appendages in such cases. It should not be altogether lost sight of, however—first, that some cases of this kind may terminate by resolution; others are curable by medical treatment; and in others, again, such abscesses and exudations may burst into adjoining hollow viscera, and so escape *per vias naturales*. Of the latter fact I have had experience in cases of parovarian abscess occurring shortly after parturition, the evacuation of the matter in this way being followed by the recovery of the patient in those cases. In my own practice I have found some cases of inflammation of the uterine appendages acute and chronic, and their consequences amenable to treatment as other similar conditions in other parts; therefore, I am at a loss to understand incredulity with regard to the possibility of treating such diseases, at least in certain instances, by medical measures, and fail to see the necessity of resorting to operative procedures, involving serious consequences, until the less serious resources of our art have been exhausted in vain.

Sir Spencer Wells, in the last edition of his work "On the Diagnosis and Treatment of Abdominal Tumours," 1885, p. 170, speaking of Fallopian tube disease, has put his views on this point in words which should be well weighed by us. He says:—"No doubt the tube occasionally becomes the seat of gonorrhœal inflammation; but, whatever may be the experience of others, my own observation would lead me to believe that these and other cases of so-called salpingitis, or pyosalpinx, usually recover under ordinary care and rest, without surgical treatment. It would appear to me as rational to perform castration in every case of gonorrhœal orchitis, as to remove the Fallopian tubes simply because they are inflamed or the seat of suppuration."

In this connection I may remark that the preference now so generally given to chirurgical, or operative, over medical treatment in these cases appears to me to be, in some measure at least, the outcome of that exclusive reliance on local measures on which I have commented on a former occasion as one of the characteristic features of modern gynæcological practice generally.

At the same time I need hardly add that I recognise the importance of local treatment when necessary in gynæcological cases, as well as the expediency of the removal of the diseased uterine appendages in those urgent cases of ovarian and Fallopian tube disorders in which other less heroic measures have been vainly tried, and in which the patient's life is manifestly endangered by the course of the disease, or in which, failing the removal of the affected parts, she would be subjected to a life of hopeless suffering and uselessness. But I have yet to learn from my own experience that such cases are of frequent occurrence in this city.

VI. LAPAROTOMY AND ITS ALTERNATIVES IN NEUROTIC AND CONSTITUTIONAL OVARIAN COMPLAINTS.

Dr. Battey's views with regard to the advantages of so-called "normal ovariotomy" have, in my opinion, been carried in some instances to somewhat extreme lengths on both sides of the Atlantic. Having already expressed my conviction of the advisability of endeavouring to impose some limitation to operative zeal in this direction, I may be reasonably asked what alternative treatment I would suggest in certain of those cases in which others regard the removal of the uterine appendages as the most rational *modus medendi*? It reply to this question, even at the risk of reiterating views before expressed, I may observe that the cases referred to appear to me to be divisible into three classes—viz., those, firstly, in which operative treatment may possibly be dispensed with; secondly, those in which it is obviously essential and indispensable; and, thirdly, those in which the question as to whether local or constitutional treatment, or both, is indicated, is debatable. In the latter category may be included cases of ovarian hyperæmia or chronic inflammation of the uterine appendages, as well as all those obscure reflex cerebro-nervous disorders associated with ovarian irritation, and in the remote causation of which the strumous diathesis is a very important, though generally neglected, factor. This I first pointed out several years ago, when in a former paper I called attention to the circumstance that a large proportion of patients brought under observation in my hospital practice were women of well-marked strumous diathesis, or in many instances actually suffering from scrofulous glandular disease. In such cases I have frequently been able to trace the influence of that constitutional taint in the symptoms of the local pelvic, peri-uterine, or ovarian disease for which they came under gynæcological treatment. The accuracy of this observation has been subsequently confirmed by my more extended clinical experience.

Besides struma, other constitutional taints or cachexiæ—such as gout, rheumatism, neuralgia, and secondary syphilis—must also in the same way be recognised as occasional causes of chronic ovarian diseases, as well as of chronic uterine complaints.

The symptoms, as well as the history of such cases, point to the necessity of something more than merely topical treatment. In nearly every instance of this kind there is marked derangement of the general health associated with the local complaint. The patient's appetite is generally impaired, capricious, or voracious; the bowels are torpid, and flatulency and nausea are seldom absent in such cases. Cardialgia, palpitation, left submammary pain, and frequent attacks of intense headache, are also usually present in the majority of instances of chronic ovarian irritation and hyperplasia, and a similar connection between utero-ovarian irritation and ophthalmic disease has been pointed out by Dr. C. E. Fitzgerald.

In these cases, too, the mind generally evinces its sympathy with its tenement by the concurrence of nervous and mental disorders with ovarian as well as uterine troubles. Of the certainty of this association of reflex hysterical affections with nearly every form of gynæcological complaint, there can be no question. Thus, in one of the hospitals to which I am attached, upwards of thirty per cent. of the patients under my care, within the last nine years, suffered from some nervous derangement, which was found traceable to utero-ovarian reflex irritation, on the removal of which the secondary nervous disturbance generally subsided.

In such cases, as already observed, the ablation of the uterine appendages, or "normal ovariotomy," has been recommended and practised. Yet I cannot but repeat that I am more and more convinced by clinical experience that under those circumstances our primary care would often be better directed to the allayment by constitutional treatment of the existing ovarian irritation or hyperplasia, of which the nervous disorder is symptomatic, rather than to the removal of the affected parts. From the same experience I have long learned that in many instances the primary local complaint, together with the reflex, or secondary, nervous consequences, may be remedied by constitutional measures, more especially by the various bromides and other nerve sedatives, by which the abnormal molecular activity of the nerve centres in hysterical cases may be allayed.

In cases of hysteria connected with ovarian amenorrhœa, ferruginous tonics, and more particularly some of the natural chalybeate waters, are obviously indicated. If the patient's circumstances admit of it, these latter should be used at their source, and thus the patient will be afforded all the conjoined advantages not only of the chalybeate, but also of that change of climate, mode of living and occupation, which are so potent in the cure of all those nervous complaints so commonly associated with chronic uterine disease.

In such disorders the physician must rise above a narrow gynæcological specialism. As already said, he must look to any local pelvic or ovarian trouble, but in doing this he must guard himself against the possibility of increasing the existing local hyperæsthesia by topical treatment not absolutely indispensable. He must further, in these instances, strive to act on the moral as well as on the physical constitution of his patient. He must insist on healthy occupation of mind as well as of body, and fit the latter for this by the constitutional remedies called for by the special exigencies of each case. If the hysterical condition be consequent on disordered menstruation, this must be corrected; if it result from undue stimulation of the sexual functions, the physical and moral evils consequent on such abuses must be clearly pointed out. Besides the strumous diathesis various other constitutional taints frequently underlie, and are the starting-point of certain forms of chronic utero-ovarian disease, and hence the importance of the recognition and treatment of these constitutional conditions in many gynæcological cases in which topical measures are too generally exclusively relied on.

It has been already observed that no class of remedies are so generally appropriate in the cases referred to as mineral and thermal waters, when employed at their sources, and thus conjoined with change of air and altered modes of living and occupation. In such complaints the most important of these are the various iodated and bromated waters, of which Wildegg and Kreuznach may be taken as examples.

Next in importance to these in this connection are saline and simple chalybeates—*i.e.*, Spa, Ems, and Schwalbach, Tunbridge Wells, Cheltenham, and the Stahlbrunnen of Homburgh; thirdly, should be mentioned the arsenical thermal springs of Royat, Mont Dore, and St. Nectaire, and the warm "glairine" waters of St. Sauveur in the Eastern Pyrenees; and, lastly, as remedial agents of unquestionable efficacy in cases of abnormal ovarian irritation, we should here allude to those non-mineralised, or feebly mineralised, so-called plain thermal waters, of which in this country we have a good specimen in Bath, which, like Pfäffers and Schlangenbad, when properly employed in suitable cases, have a potent sedative influence in many instances of this kind, and are no less useful in cases of ovarian dysmenorrhœa, and in the protean forms of reflex cerebral disturbance consequent on ovarian hyperæsthesia and hyperæmia.

VII.—GENERAL ASPECTS OF THE LAPAROTOMY QUESTION.

In its ethical and social aspects, as well as from a therapeutic point of view, the question of the increasing performance of operations for removal of the uterine appendages affords much matter for consideration. This recently increasing frequency of such operations being, I believe, admitted, if their general employment is to be regarded as the measure of their expediency, it must follow that before long a considerable number of women will be necessarily, completely or partially, unsexed or deprived of a portion of their distinctive sexual or reproductive organisation. Before such a plan of treatment is here commonly adopted by us, however, it should be carefully considered and discussed in all its aspects—ethical, medico-legal, and social as well as medical.

"The *New York Medical Record* appears to think," somewhat sarcastically observes a recent writer,[a] "that the laparotomists need an apology, which it proceeds to deliver in the form of the following argumentative eulogium. We suppose the sceptics will be silenced thereby:—

"'The attempt of the New York Academy of Medicine to check the practice of spaying is evidently an ill-considered one, and is entirely antagonistic to the progressive instincts of the day. The history of the world shows that the practice in question has always been one of the crowning ornaments of the best types of civilisation, from the Chaldean to the Roman age. Now that, after many centuries of gloom, it is again brought into beneficent existence, its critics should beware of thoughtless and shallow opposition. The trouble probably lies in the fact that there still lingers an impression among crudely educated minds that the ovaries are organs of social necessity and economic importance. This, however, is a serious mistake. These organs are, it is true, useful for a short period in the existence of a portion of woman-

[a] Medical Press and Circular, June 2nd, 1886.

kind for the perfunctory propagation of the race. Aside from this, however, they are not only of no service, but are a source of racial, domestic, and individual distresses of the greatest magnitude. Philosophers of the present day have ascertained several facts which place this view upon a solid and impregnable basis. No woman wants more than two children, many only one, and a large per cent., including all the unmarried, not any at all. But, in fact, the population is increasing at a seriously rapid rate, and the modern economist has had to revive and readopt the views of Malthus. In this exigency, when society's needs are antagonised by infant multiplicity, the laparotomist steps in, as a kind of modern saviour from the threatened polypædic catastrophe. The woman has her child, the ovary swells, the learned touch of the gynæcologist detects a pyosalpinx, and in a twinkling out comes all the source of woman's labours and man's unsought paternities. The laparotomist is plainly society's best friend. Like all benefactors of the race, he must endure opposition and calumny for a time; but his noble work of radically removing the sources of over-population will go on, and we calculate that, at the present rate of increase, in fifty years some thirty-five per cent. of women will be permanently relieved of all the worry of maternal anticipation.'"

Seriously speaking, however, the facts cannot be eliminated from consideration in this discussion—viz., that the ovaries are to women much what the testes are to men, and that by their complete removal in either case the individual is thereby unsexed or deprived of all reproductive sexual power. New words will not alter old facts. And hence the more euphonious phrases by which the removal of the uterine appendages are now referred to, or the more scientific and successful method in which this is accomplished, do not render the ultimate consequences of such operations now less important than when they were more tersely, if less scientifically, described. These secondary consequences are, *inter alia*, to render those on whom such procedures have been completely carried out, by the removal of both ovaries or Fallopian tubes, incapable of fulfilling what in every Christian community has been generally regarded as one of the chief functions and primary objects of woman's married life—namely, that of child-bearing.

I have already expressed my belief in the occasional necessity of such operations as the only available means of saving life in certain urgent cases. But I need hardly add that, in my opinion, even in these they should not be resorted to without the patient's previous concurrence and full knowledge of their consequences.

With regard to that deterioration of feminine appearance, alteration of voice, growth of hair on the face, loss of sexual instincts, and arrest of menstrual vitality, which are enumerated by Dr. Wiglesworth, and controverted by other eminent authorities, as being consequences of removal of uterine appendages, I do not myself think the possible occurrence or non-occurrence of such circumstances are of sufficient importance—at least as compared with others already alluded to—to materially influence our judgment either for or against the operations in question.

It may, perhaps, be held that one who brings forward no statistics of his own to controvert the great amount of statistical evidence which has been elsewhere published to demonstrate the utility and safety of the removal of the appendages in large numbers of cases, is not entitled to question the expediency of adopting such operations generally. I cannot, however, acquiesce in this view, as I regard this subject as one also to be considered on broad general principles. I have not questioned either the accuracy of those statistics, or the fact that the uterine appendages may be removed with as much safety by a skilful operator as might follow his removal of the mammary gland in a similarly large number of cases. But I do not see that such results, however successful in many cases, *per se* prove the general necessity of adopting such operations in other instances, without reference to the special circumstances of each individual case.

In conclusion, I shall merely reiterate the hope that the practical importance of the subjects included within the scope of this communication, to only a few of the more salient of which have I been able to glance in the course of the present hurriedly prepared paper, may induce discussion thereon, and thus assist in ascertaining to what extent, and with what limitations, the operations referred to should, in the opinion of the Irish Academy of Medicine, be here adopted or avoided.

THE TREATMENT OF PRESENTATIONS AND PROLAPSES OF THE FUNIS.

By WILLIAM C. NEVILLE,
M.A., M.D., and Master in Obstetric Surgery, Univ. Dubl.;
Physician to Pitt-street Institution for Diseases of Women and Children.

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In bringing this subject under notice, I have been guided by considerations which, to my mind at least, seemed satisfactory. These considerations are:—The practical importance of the subject; the length of time since it has been discussed amongst us; and the conviction to which, whether rightly or wrongly, I have been led both by experience and reflection, that the methods of treatment usually taught and practised are gravely defective.

From the title of the paper it will be seen that I distinguish between presentations and prolapses of the funis. The distinction, though commonly alluded to, has not been consistently adhered to in standard works upon obstetrics. Inasmuch, however, as it is one of great convenience and practical importance, it will be strictly kept in view throughout the following paper.

The funis will be considered as *presenting* when it can be felt in front of or beside the presenting part of the fœtus *through intact membranes;* as *prolapsing* when it occupies a similar position *after rupture of the membranes.* Prolapse of the funis may follow as a sequel of its presentation, or it may occur for the first time after the membranes have ruptured.

The subject is an important one—of practical rather than of curious interest; deserving the most serious attention, both on account of the comparative frequency of these accidents, and of the grave fœtal mortality which attends them.

These displacements of the funis are by no means infrequent, though statistics on the subject are even more than usually at variance. Thus Manzoni met with one or other displacement as often as twenty times among 450 patients (1 : 22½) in a lying-in hospital at Florence,[a] while another observer[b] met with only one case in 1,897 labours. Churchill found that out of 188,730 recorded labours, collected from various sources, the funis presented or prolapsed 816 times, or once in 231½ labours. Charpentièr's still more extensive statistics (342,929 labours) give an almost identical frequency—viz., once in 227 labours. Such statistics as these are chiefly useful as furnishing a broad rather than an exact basis from which to draw conclusions. In considering them, certain points must be borne in mind. In the first place, while they cannot overstate, many reasons lead us to think that they may considerably understate, the frequency with which these displacements of the funis occurred in the particular cases recorded. All they prove is that the accident was observed and recorded in so many cases; and their exactness must depend upon the care and capacity of the observer. From these and other statistics it would seem that it is more frequent in Germany than in England or France—a fact which Simpson attempted to explain by the position in which labour-patients are placed (on their backs with shoulders raised), during the first stage at least, in that country. May not the difference—which is quite a marked one—be also partly explained by the greater exactness and zeal with which German obstetricians examine their patients? A large proportion of the labours collected in these statistics occurred in connection with hospital practice in which, the immediate care of the patients being in the hands of subordinates, it is but natural to suppose that many less marked cases of these accidents passed unnoticed and unrecorded. Included also in the statistics under consideration are found the records of some old observers, not specially compiled to elucidate the point at issue. I was much struck by the fact that, according to these older statistics, the frequency of the

[a] Vide Charpentièr, "Traité des Accouchements." Paris, 1883. Tome II., pp. 442.

[b] Bland, quoted by Lusk, Science and Art of Midwifery.

displacement appears to be very markedly less than it would seem to be from more recent records—partly, perhaps, for the reason just given, partly because the old observations were less accurately made or kept, and also because, in the presence of other graver complications as regards the mother, these may not always have been noted. Thus M. Boivin, quoted by Churchill, recorded 20,357 labours in which this complication is stated to have occurred 39 times, or once in 522 labours; and M. Lachapelle, quoted by Charpentièr, recorded 37,895 cases with a frequency of one presentation or prolapse in 592 labours. We may fairly conclude then, by making the necessary allowances in the statistics of Churchill and Charpentièr, that these displacements of the funis occur about once in 200 cases of labour.

The question of the fœtal mortality which attends these cases is alluded to as follows by Dr. Playfair, in a paragraph which is quoted in full as embodying standard conclusions:[a]—" With regard to the danger attending prolapsed funis, as far as the mother is concerned, it may be said to be altogether unimportant; but the universal experience of obstetricians points to the great risk to which the child is subjected. Scanzoni calculates that 45 per cent. only of the children are saved; Churchill estimated the number at 47 per cent.; *thus, under the most favourable circumstances, this complication leads to the death of more than half the children. Engelmann found that out of* 202 *vertex presentations only* 36 *per cent. of the children survived. The mortality was not nearly so great in other presentations;* 68 *per cent. of cases in which the child presented with the feet were saved, and* 50 *per cent. in original shoulder presentations.* The reason of this remarkable difference is, doubtless, that in vertex presentations the head fits into the pelvis much more completely and subjects the cord to much greater pressure; while in other presentations the pelvis is less completely filled, and the interference with the circulation of the cord is not so great. Besides, in the latter case the complication is detected easily, and the necessary treatment sooner adopted."

[a] Playfair, Science and Art of Midwifery. 5th edition. Vol. I., pp. 403. [I italicise those portions of the quotation to which I wish to direct particular attention.]

These conclusions, universally accepted, are extremely significant—far more so, I think, in their practical bearings than has been hitherto perceived. That such striking facts should have been generally observed and alluded to merely as of curious, instead of practical, interest surprises me greatly. Is it possible for facts to plead more strongly against the practice of allowing a vertex presentation to remain as such when once the funis is discovered to present? By changing a vertex into any other presentation—a change easily and quickly accomplished without rupture of the membranes in a large number of cases—we at once decrease the fœtal mortality by from 15 to 30 per cent.

We are now ready to consider the treatment of presentation of the funis. I will assume that the patient is seen at an early period of labour, that the cord has been felt through the membranes, and that by its pulsations, or by hearing the fœtal heart and establishing, by external examination, that there is only one child to which it can belong, we are satisfied that the child lives. How now are we to act? To take the simplest and least dangerous case first :—

If the Breech presents.—The treatment usually taught is as follows :—Preserve an expectant attitude so long as the cervix remains incompletely dilated and the membranes remain intact. Guard in all possible ways against premature rupture of the membranes, keeping the patient in bed, forbidding any attempts at bearing down, and, above all, by exercising extreme caution and gentleness during vaginal examinations, which should only be made for necessary purposes. From time to time try the knee-chest position, maintained for as long as possible, and the lateral semiprone position, with hips well raised above the level of the shoulders. By attention to these postural methods the cord may recede spontaneously from its position of danger. Thus far the management is the same, as commonly taught, for all cases. The cord, if it does not recede, will finally prolapse, when the membranes rupture spontaneously. Then repose the cord as well as possible with the hand, and bring down one foot at the same time. Rapid extraction should follow if the cord does not then remain reposed.

The alterations which I would suggest as improvements in the above practice are as follows:—If the cord has not receded under the influence of postural treatment, when the os is $\frac{1}{2}$–$\frac{2}{3}$rds dilated and dilatable, we should wait no longer, but proceed at once to rupture the membranes, introduce the hand into the uterus, repose the cord, and bring down a lower extremity. In order to prevent any sudden outrush of liquor amnii, the patient should lie, during the operation, in the semiprone position, with her hips well raised above the level of her shoulders, and the membranes should be ruptured when the uterus is perfectly quiescent. In steady pluriparæ, with fairly roomy vaginæ, the operation may be very well performed, with the patient in the knee-chest position, which minimises the difficulty of dealing with the cord. Chloroform, administered so as to produce complete relaxation, may be necessary in primiparæ or nervous patients, who could not otherwise be trusted to remain quiet during the operation, which should, however, be a very quick one. At its conclusion the thigh of the child should so fill the cervix as to prevent the possibility of the funis prolapsing by its side. The labour should then be left to progress naturally, occasional traction being made with a view to stimulating and aiding uterine contractions, and at the same time guarding against any space being left through which the cord could prolapse. The fœtal heart must subsequently be watched, any decided irregularity or slowing pointing to the need for accelerating delivery.

The only difficulty in this proceeding consists in bringing down the limb without allowing the cord to prolapse. It is better to bring down a knee than a foot, as the thigh, which immediately follows in the former case, rapidly fills the uterine outlet and secures us against prolapse. But if prolapse do occur, a fillet should be immediately put round the fœtal limb, which can then be delivered by traction from without, while the internal hand keeps guard over the funis. Some dexterity is needed to surmount this difficulty in all cases, yet the difficulty is less than it is likely to be later on in labour. After the waters have escaped at the close of the first stage, the uterine outlet is so fully dilated as to be blocked with difficulty, and we may also have to deal with a cord prolapsed, from the very beginning of the operation, altogether out of the vagina. The timely operation is also a less risky one for the mother than the later one, because of the presence of liquor amnii, and more hopeful for the child, because it avoids the dangers which are entailed even by the most temporary disturbance with the oxygenation of its blood. And even if the advisability of taking action so early as I have recommended be questioned, on the ground that the subsequent delivery of the after-coming head may thereby be made more difficult, yet, at least, there can be no question as to the wisdom of acting—since at some time we must act—before, rather than after, rupture of the membranes. Indeed, the cardinal point in my contention is that we should avoid, if possible, the spontaneous rupture of the membranes, as a result of which the danger gets ahead of us, and the necessary operation is made more difficult and dangerous, and less likely to be of service to the child. Indeed, I can conceive no gain whatever from allowing the membranes to rupture of themselves when once the os is fully or almost fully dilated. Better far is it to take and keep the command of the situation from the very beginning, than to be forced to act hastily under the constraint of a pressing and immediate danger.

When the Shoulder presents.—We need not delay over this case, the treatment of which is usually stated to resolve itself into that of the fœtal malposition. But we must also consider the most favourable method and moment for interference, in view of the displacement of the cord. The interests both of mother and child will, I think, be best conserved by adopting the same line of treatment as that first recommended for cases in which the breech presents. During the earlier period of labour try and secure spontaneous recession of the cord by attention to postural treatment, and take precautions against premature rupture of the membranes. Then, when the os is $\frac{1}{2}$–$\frac{2}{3}$rds dilated, introduce a hand into the uterus, repose the cord if still presenting, and block the outlet by bringing through a lower extremity. Labour may then be allowed to proceed naturally, being hastened only when the fœtal heart-beats show by their character that the child is in danger.

When the Head presents.—The usual teaching (that, for example, of Lusk, a thoroughly representative authority) is as follows:—During the first stage preserve the membranes very carefully, and try to secure recession of the cord by the various postural devices. The common teaching, in fact, as regards the treatment during the first stage is the same whether the head or the breech presents.

Supposing now, as is most likely, that the cord prolapses when the membranes rupture; then, if dilatation is complete, the pelvis roomy, and the pains strong and effective in a pluripara, the case may possibly be left to nature, while we attempt to protect the cord from pressure by guiding it into the comparative shelter of one or other sacro-sciatic notch. If the pains are not strong enough, use the forceps to quicken delivery. But if the head remain above the pelvic inlet, the alternatives are version or reposition—the latter, as the milder alternative, being first tried. If successful in reposing, re-prolapse must be guarded against by fixing the head in the brim, either by external pressure or by the use of the forceps. Efforts at reposition cannot be long continued without danger to the child, and if not quickly successful recourse must be had to version.

It is a matter for some surprise that so many authorities should be content to follow the routine of a treatment sufficiently discredited by the terrible fœtal mortality which has attended its application in practice. For, promising as this treatment may appear, the fact that the mortality attending it—in the hands, too, of skilled specialists—has averaged about 60 per cent., is a remorseless commentary on its inadequacy. Suppose that the mortality were maternal instead of fœtal, is it conceivable that so unsuccessful a treatment should so long have been acquiesced in? It may be said, however, that the want of success is due to the gravity of the accident, and that the treatment is as good as it can be.

There is, at least, an alternative treatment radically differing from the usual one, which I am anxious should obtain an extensive trial. Its main feature consists in utilising the first stage of labour for altering the head presentation into one which experience has unmistakably proved to be much less dangerous to the child. As early an opportunity as possible should be taken to turn the head—face, or brow—into a breech or even shoulder presentation. In pluriparæ with the head presenting above the pelvic brim, this version is easy, and can often be effected by simple external manipulation. In primiparæ, on the other hand, external version is seldom easy, the head-pole of the child's body being already intra-pelvic, and thus incapable of being acted upon from without. But for this very reason presentation of the cord is only quite exceptionally encountered among primiparæ where the pelvis is normal and the head presents, though slight contractions of the brim, which interfere with the descent of the head previous to labour, are frequently enough to be credited with presentations of the cord in this class of patients. Version—in these cases doubly indicated—may then be performed through the abdominal walls, and this kind of version is really much more easy than those who have not yet tried it are apt to think. Indeed, from the casual way in which external version is treated of in most works upon obstetrics, it is scarcely to be wondered at that so few put trust in this most useful operation. Some members of this Academy will, I know, readily support my statement that version through the abdominal walls, under fairly favourable conditions, and towards the commencement of labour, is often an easy, safe, and rapid operation. Should it not succeed, however, from any of the causes which render its performance difficult—*e.g.*, scanty liquor amnii, with more or less fixation of the fœtal body, irritable uterus, or tense and incompressible abdominal walls—we should content ourselves with keeping the patient at rest, and trying postural methods until the os is well—say two-thirds—dilated. We should then proceed to turn by the bi-polar method of Braxton Hicks, avoiding rupture of the membranes if possible, until the feet are near the os. The cord should then be manually reposed, and a lower limb brought through so as to block the outlet as before explained. If from any cause the operator is unable to turn in this way, he must have recourse to the ordinary podalic method, though this involves much more extensive introduction of the hand into the uterus. Anyhow, the risk to the mother is distinctly less now than if the same operation is performed compulsorily during the second stage—one of the commonest sequels of the ordinary practice—while the child's chances also seem better, the cord never being subject to any pressure, unless, in bringing through the leg, the operator's want of dexterity allows it to prolapse. In the latter event a noose must be put round the limb, which can then be brought down by traction from without, the cord being kept out of the way by the inner hand, or reposed by one of the devices afterwards to be described. The subsequent management must be as after the similar proceeding when the breech is the originally presenting part.

Not rarely, when there is a slight excess of liquor amnii, external or combined internal and external version is so easy that the fœtal body can be easily turned round and round inside the womb. In such cases we may find that the presenting cord disappears after one or more rotations. In one case—the only one in which I had an opportunity of trying this device of repeated turnings—it succeeded perfectly after I had altered the fœtal position three or four times. In head presentations I would always turn early during labour, if possible by external version, without troubling about postural treatment—because (1) the mere turning may cause the funis to recede; and (2) because the recession of the cord under postural treatment is not always permanent, prolapse being likely to recur when the membranes rupture. Spontaneous recession will sometimes occur without any postural treatment; but this, again, is not likely to prove permanent. So, again, if turning can be accomplished without rupturing the membranes, by Braxton Hicks' method, I would perform it for greater security, though the cord had, for a time at least, receded. But if turning could not be performed without rupture to the membranes, I should prefer to trust to the cord remaining reposed than to perform the old operation of version, when not absolutely needed. But, as previously observed, when the cord has once been noted as presenting, prolapse is very apt to occur along with spontaneous rupture of the membranes, and sudden escape of liquor amnii. Hence the rule which should be adopted in these cases—to avoid spontaneous by artificial rupture, the patient being in the knee-chest posture, and the uterus flaccid. A small puncture rather than an extensive tear is also advisable.

I may now briefly recapitulate the line of treatment I have recommended in cases of funis presentation :—

1. In all cases the patient should be strictly confined to bed.

2. In breech or footling presentations the various postural methods of reposition may be tried. If the funis become reposed under the influence of such devices we should wait until the os is nearly fully dilated, when there is danger of sudden spontaneous rupture of the membranes. Then place the patient in the knee-chest posture, and in the interval between two pains rupture the membranes artificially, making as small an opening as possible so that the liquor amnii may escape slowly. Keep the patient in the knee-chest position until the bulk of the waters has escaped, and the breech has descended so as to fill the inlet and block it as much as possible against a prolapse. Then change the patient's position to the usual side one.

If the funis does not recede under the influence of postural treatment, wait until the os is one-half to two-thirds dilated, and then, introducing a hand into the uterus, repose the cord, and bring down a leg, so as to block the outlet against prolapse. This operation is best done, if the patient be a steady pluripara, in the knee-chest position. In primiparæ or nervous patients chloroform may be required, and the patient may then be placed in the lateral semiprone position, with her hips well raised during the operation. After its conclusion, unless immediate extraction be indicated by the unsatisfactory nature of the fœtal heart-beats, delivery should be left to nature, aided from time to time by tractions on the leg, which will help to keep the uterine outlet blocked against the extrusion of the funis.

3. When the shoulder presents, if the fœtal body be fairly mobile within the uterus, external version can usually be performed with

ease, and the breech brought over the os. The case may be treated as an original breech presentation; if not, when the os is about two-thirds dilated, introduce a hand into the uterus, repose the cord, and bring down a leg as above recommended.

4. When the head (face or brow) presents, external version should, if possible, be performed early in labour; after which, if successful, the case may be treated as one of breech presentation. If unable to turn by external manipulation, try and do so by the combined method of Braxton Hicks, without rupture of the membranes when the os is one-half dilated. If successful, we may find that with the version the cord has disappeared, in which case labour should be allowed to progress uninterfered with until the time arrives for artificial rupture of the membranes with the patient in the knee-chest posture—*i.e.*, when spontaneous rupture is close at hand. If, however, the cord still presents, we should try postural methods for a short time—failing which, the membranes must be ruptured, the cord manually reposed, and a leg brought down until the outlet is blocked by the thigh or breech. If the membranes rupture accidentally while attempting to turn through them by Braxton Hicks' method, the version must be completed at once either by that or by the ordinary podalic method, and, the cord being manually reposed, a leg brought down. The only effect will have been to precipitate matters somewhat, while depriving us of the chances of spontaneous recession, or of recession under postural treatment. In head, face, or brow presentations, even though the cord may recede spontaneously or with the aid of postural treatment, its tendency to re-present or prolapse in the course of labour makes version always desirable if it can be accomplished without rupture of the membranes. In some cases external version is so easy that the position of the child can be altered almost *ad infinitum* through the abdominal walls. In such cases recession of the cord can often be secured by adopting the device of repeated turnings. It is always desirable, however, in view of the chance of its again presenting, that the final presentation of the child should be one of the breech. After successful version with intact membranes a binder should be applied, as otherwise the child is very apt to revert spontaneously to its original position.

Two main principles underlie all the above recommendations—viz., firstly, to prevent the presentation of the funis passing into its dangerous sequel of prolapse; and, secondly, to obtain the presentation of the fœtus, which experience and reflection alike prove to be attended with least danger to the child.

We must now consider the treatment of prolapse of the funis.

Prolapse most commonly occurs as a sequel to presentation of the funis, but it may also occur independently of presentation after rupture of the membranes, and more especially during the outflow of the liquor amnii. In either case the membranes may have ruptured and the cord have prolapsed before the patient is seen. The treatment of such cases is much less promising than that of presentation—for, apart from the chance that the child may have been asphyxiated before any opportunity has been afforded of trying to save it, it will most probably have suffered considerably from the disturbance of its placental respiration. It may be already dead, or its condition may be one of such semi-asphyxiation as must greatly lessen its chances of surviving still further and necessary risks.

If the prolapsed cord be pulseless when the patient is first seen, nothing can, of course, be done. It is necessary, however, to make sure that the pulselessness is permanent, and not due to any temporary cause, such as the compression exerted on the cord occurring during the continuance of a "pain."

When the cord is found to be prolapsed and pulsating, the treatment must altogether depend upon the individual circumstances of each case.

1. *If the prolapse occurs when the os is only very slightly dilated*, the funis must be secured from the risks of compression, while steps are taken to provide for the quickening of labour. These ends may be obtained by—(1) reposing the cord, (2) altering the position of the fœtus should it be presenting by its head end, and (3) employing water dilators for the dilatation of the os.

If the cord be much prolapsed with weakened pulsations, the first step must consist in its reposition, which can best be effected, while the patient is in the knee-chest position, by one of the special devices presently to be mentioned. But if only a small loop is prolapsing, and its pulsations be strong, it is preferable to postpone this step until after the alteration of the child's position, which is advisable when it presents by its head end.

By turning under these circumstances we do much towards lessening the risk of funic compression which must continue once the liquor amnii has escaped, so long as the hardest and bulkiest portion of the child is allowed to press most directly against the very region of the uterus into which the cord has gravitated. The combined bimanual version at this early period is also much easier than it would be later on in labour, when the uterus may have moulded itself tightly around the body of the child; and once performed, it puts us in a position to proceed at once to extraction, should this be necessary, after the cervix has been sufficiently dilated. The version at this stage must, of course, if at all, be performed by external manipulations, or by the combined method of Braxton Hicks, which can scarcely fail in skilled hands when the liquor amnii has not escaped for too long a time. The great point consists in removing the head from over the os, substituting for it a breech or shoulder. The latter, indeed—as the least likely to cause compression—might be temporarily the most desirable, if we could be sure that it would not quickly revert into its original presentation. A slight prolapse may disappear during such version.

The third step consists in introducing a suitable-sized water dilator, which must in due time be replaced by a larger-sized one, dilatation being thus proceeded with until the os is sufficiently dilated (about one-third) and dilatable to introduce a hand and bring through a leg. The replacing of the water dilators is best done with the patient in the lateral semiprone position, with her hips very well raised; and the dilator in situ should not be removed until the new one has been pushed up to the cervix, ready to take its place immediately. In the case of a steady multipara I would prefer to introduce the dilators while she was in the knee-chest posture, the trouble attending re-prolapse of the cord being thus best guarded against. Sufficient dilatation can almost always be attained by one change of dilators.

After a leg has been safely brought through the os we are fairly secure against a recurrence of prolapse. Labour should then be helped by occasional tractions made synchronously with the uterine contractions. The fœtal heart must be watched, and quick extraction proceeded with should it appear necessary in the interest of the child.

2. *When the os is fairly dilated* version may be at once performed if required to bring the lower extremity over the os, the hand then introduced into the uterus, the cord manually reposed, and a leg brought down, &c.; or simple podalic version may be had recourse to directly. The same proceeding is also most advisable when the os is fully or almost fully dilated, and the head or breech is at the inlet.

3. *When the patient is in the second stage of labour*, the presenting part (head or breech) being in the pelvic cavity, if this be tolerably roomy in a pluripara, it is best to deliver quickly, doing what we can in the first instance to guide the cord into a position as much out of the way of pressure (*e.g.*, into one or other sacro-sciatic notch) as possible. Thus the forceps can be applied to the head, while, if the breech cannot otherwise be quickly delivered, one leg may be brought down. Quick delivery in these cases is really safer, causing less delay and opportunities for compression than an appeal to the unsatisfactory proceedings for reposing the cord. Even when we happen to be without a forceps we may be able to complete delivery very rapidly when the head is in the pelvis by impressing upon the patient the absolute necessity for her making the utmost of her "pains," while we assist them by a vigorous and intelligent use of the method of "expression." I have been more than once struck by the remarkable efforts which a woman can make when told that upon the strength and duration of her pains the life of her child depends. It is seldom, indeed, that a patient in labour puts forth the whole force of her voluntary efforts with the intention of rapidly terminating her labour; and when stimulated by fears or hopes into doing so, the effect is sometimes quite astonishing.

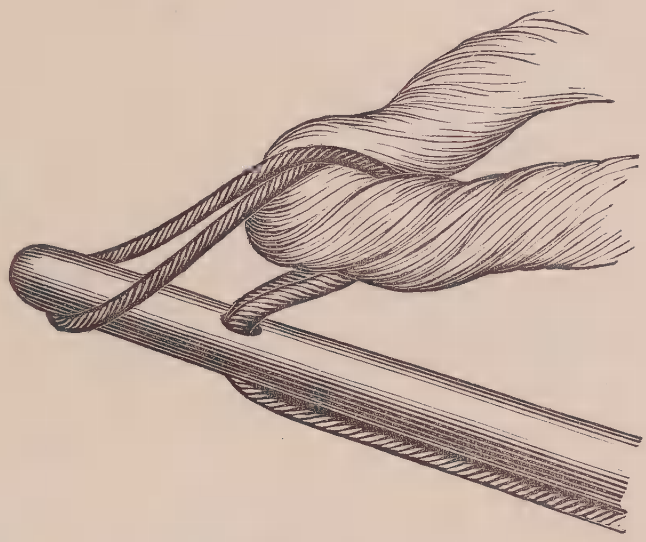
It only now remains briefly to allude to the methods and uses of reposing a prolapsed funis. The idea of reposition is a natural one, and some of the special devices for effecting it are fascinating in their ingenuity. Experience, however, proves that reposition is a most tantalising and unreliable operation. As fast as one loop of the cord is replaced another comes down, and when we do at length regard the reposition as an accomplished fact, we may only have to wait for the next "pain" to discover how premature were our hopes. The unsatisfactory results which have generally marked the treatment of prolapsed funis are, I believe, largely to be credited to a groundless belief in the efficacy of reposition as the "be all and end all" of its treatment. The child suffers much from excessive handling and chilling of the funis, apart altogether from the repeated risks of its compression when prolapse recurs often. It should be an accepted rule in the treatment of these cases always to have regard to the possibility of the prolapse recurring, and therefore only to regard reposition as a necessary preliminary of the treatment, immediate subsequent measures being taken to block the uterine outlet in such a way as will keep the cord from again descending. The methods of effecting this imprisonment of the cord under various circumstances have been sufficiently indicated in the directions given as to treatment.

Reposition of the cord may be effected posturally, manually, or instrumentally. Postural reposition is of most service when the membranes are unbroken, or as an aid to the other methods of reposition. I have also alluded above to the advantages of rupturing the membranes artificially with the patient in the knee-chest position in cases where the cord had presented at an earlier stage of labour.

Manual reposition I have recommended in those cases of presentation or prolapse in which the dilatation or dilatability of the cervix was such as to make it advisable to bring down a leg, and thus block the outlet against recurrence of the accident. This would be, therefore, the selected treatment in those cases of funis presentation seen from an early stage of labour.

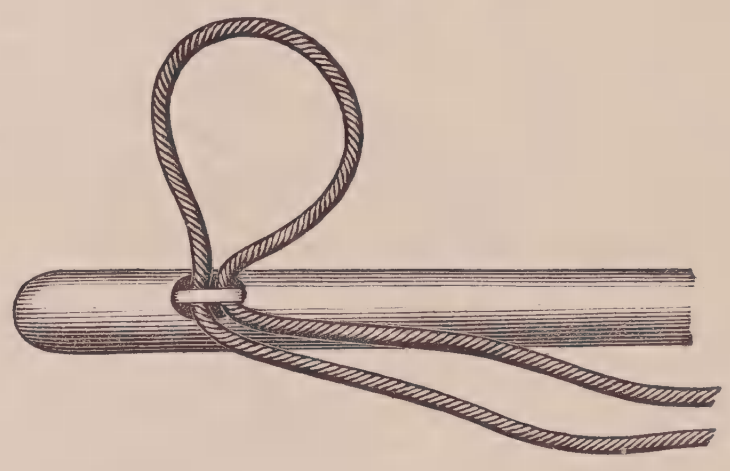
Instrumental reposition is necessary only under two sets of conditions. The first is when prolapse is associated with premature rupture of the membranes, the os not being sufficiently dilated to repose manually and bring down a leg. The second is when the prolapse co-exists with a head, face, brow, or breech presenting in the upper part of the pelvis during the second stage of labour, the general conditions being such as make it desirable to secure reposition before proceeding to extraction.

Fig. 1.—Braun's Repositor.



An endless variety of devices exist for the purpose of effecting reposition. Of special repositors the simplest and best is that of Braun (Fig. 1), an excellent substitute for which can readily be made out of a long (14–18 in.) flat stick of whalebone, perforated by a good-sized hole about two inches from its extremity. The noose is best made by some soft material—*e.g.*, floss silk, narrow tape, or worsted. The woodcut sufficiently indicates how the funis is intended to be caught prior to its reposition. When prolapsed completely out of the vagina, it is preferable to embrace in the noose an entire loop of the funis, which should be made as long as possible by pushing the noose well up over it into the vagina. Braun has, however, himself practically discarded this repositor in favour of one formed by an ordinary full-sized male gum-elastic catheter. The latter has the advantage of being universally available, while it can also, according to circumstances, be used in a variety of ways. When greatly prolapsed a coil of the funis may be embraced in a worsted noose formed at the eyelet in one of the ways shown in Fig. 2 or Fig. 3. The former is figured as the quickest way of making a catheter serve as a repositor, but it has the disadvantage of depending entirely upon the stylet, which cannot be withdrawn if it is desired, as is generally the case, to keep the cord up by leaving the catheter in the uterus after reposition. A noose may, however, be almost as quickly made by cutting off

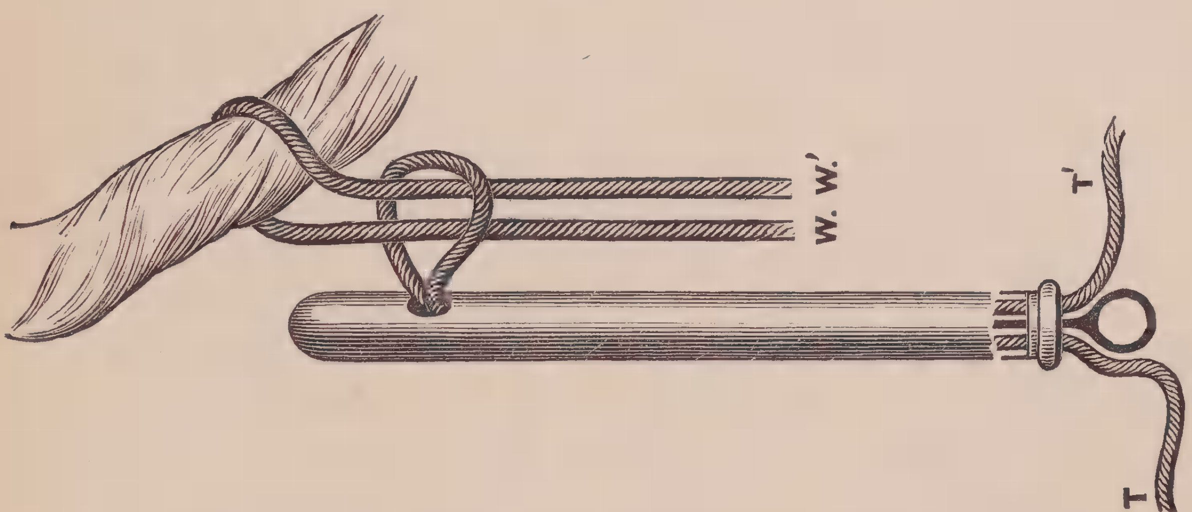
Fig. 2.



the end of a catheter, then doubling a long piece of narrow tape and passing its folded end in through the eyelet and out of the open end of the catheter. This way of forming the noose is that which I would myself adopt; it does not depend on the stylet, as is the case with the noose formed as shown in Fig. 2, and is much more rapidly made than that shown in Fig. 3. When the funis is not sufficiently prolapsed to allow of its being snared in the ways mentioned, it is usually advised that it should be further drawn downwards by traction. This results, however, in making the prolapse greater than ever, and increases the subsequent difficulty of successful reposition.

For such cases I have devised and successfully practised the following method:—A piece of tape or other suitable material (W W′ in Fig. 3) is first carried over the loop of prolapsed funis, after which its two free ends are encircled by a noose formed in any of the various ways just explained. The point of the catheter is then to be pushed upwards into the vagina until in close apposition to the prolapse, when by tightening the two ends (T T′) of the cord which forms the noose, and uniting them around the mouth of the catheter to the straightened ends (W W′) of the tape, the funis is secured without any risk of compression. When only a very small loop of the funis is prolapsed at the roof of the vagina the tape or worsted should be carried over it by means of a catheter, sufficiently curved and provided with a stylet. Carrying the curved end of the catheter over the loop of funis, we can then catch the end of the tape in our fingers, and, keeping the catheter *in situ*, draw it downwards without danger of dragging on the funis at the same time. The universal applicability of this method—which I have not seen elsewhere described—and the freedom it gives from any danger of accidental compression of the cord by the means used to secure it, make it, in my opinion, preferable to any of the methods usually described.

Fig. 3.



When the funis is secured—whatever method be employed—the patient should be placed on her hands and knees, and the repositor pushed as high up into the uterus as possible. If a catheter be used it should then be left there, as, softened and made flexible by the heat and moisture of the passages, it cannot interfere with labour, or the subsequent use of water dilators, should these be required. The stylet should be withdrawn, as its continued presence would prevent the catheter being perfectly moulded against the sides of the genital canal. I have twice succeeded in permanently keeping up a prolapse occurring at a very early stage of labour by shoving the catheter entirely into the uterine cavity, and then using the dilators.

It must, however, be remembered that one loop of the cord reposed, another is only too likely to take its place. Hence the rule that, immediately after reposition, we should always take measures to block the uterine outlet against a further prolapse. I have previously shown that instrumental reposition is only rarely needed—firstly, when the membranes have ruptured at the beginning of labour, in which case further prolapse is secured against by the use of water dilators, &c. ; secondly, when the prolapse occurs at the beginning of the second stage, and when quick delivery cannot be relied on, in which case reposition is only a preliminary to the use of the forceps, or other measures for hastening delivery.

I am prepared for two principal criticisms upon the treatment advocated in this communication. The treatment, it may be said, is too active and aggressive. Presentation does not necessarily pass into prolapse of the funis, nor is prolapse itself, even when left alone, always fatal to the child. Is it not more advisable therefore to forbear from active interference until what may prove to be only a potential becomes, if it does become, a positive and actual danger ? In answer to such an argument it is only necessary to point out that here, as elsewhere, we have to deal with and provide for average rather than for exceptional results; that the average consequences of presentation of the funis are such as make active interference necessary at one time or another; and that the results hitherto obtained from the practice of waiting until imminence of the danger imperatively calls for interference have been extremely unsatisfactory. My own experience leads me to the conclusion that much better results can be achieved by at once recognising the evil tendencies of funic presentation, and by attempting to forestall or to minimise them by adopting the line of treatment above advocated.

Secondly, it may be urged that some of the proposals upon which I lay most stress are largely impracticable, inasmuch as we seldom see labour-patients until towards the termination of the first stage, and very commonly not until after the membranes have ruptured.

On this point each one must answer for his own practice. For my own part, believing that there are many cases, unrecognisable by an average nurse, in which skilled aid is very desirable if not absolutely essential, during the first stage of labour, I always ask to be sent for as soon as labour has set in. I do not, of course, maintain that it is our duty to remain in constant attendance upon our patient—a primipara, perhaps—throughout the length of a protracted first stage, but that we should make it a rule to see her as soon after its beginning as possible, and afterwards at sufficiently frequent intervals to satisfy ourselves that nothing is left undone which could contribute to her or the child's safety. If so much be not done, whenever possible, there is a failure in duty towards the patient; and if it be done, a presentation of the funis must be recognised betimes, and treated promptly.

PATHOLOGICAL SECTION.

PATHOLOGY OF LEAD-PARALYSIS.

By WALLACE BEATTY, M.D. B.Ch. Univ. Dubl.;
Assistant Physician to the Adelaide Hospital;
Lecturer on Pathology, Carmichael School of Medicine.

[Read in the Pathological Section, November 6, 1885.]

Having had an opportunity of examining the spinal cord of a man who suffered from lead-paralysis, I think it may be of some interest to detail to the Academy the result of my examination, especially as the question of the primary cause of this affection is still uncertain, an uncertainty due to two facts—namely, that the number of cases hitherto recorded in which a careful pathological investigation has been made is small, and that the results of the investigations have not been by any means constant.

Before recording my case I propose briefly to detail the changes that have been found by others, and to discuss the various theories which have been put forward to account for the paralysis.

The pathological changes that have been found affect the muscles, the nerves, and the spinal cord.

1. Changes in the muscles:—

There have been found simple diminution in volume of fibres, with conservation of transverse striation; granular and fatty degeneration of fibres; proliferation of nuclei, thickening of the sarcolemma, increase of connective tissue between the fibres, with atrophy of the contractile substance. The changes in the muscles are described by some as atrophy, by others as myositis terminating in atrophy.

2. Changes in the nerves:—

(*a*) In posterior nerve-roots.—Nothing abnormal has been discovered.

(*b*) In anterior nerve-roots.—Atrophy of some nerve-roots with granular degeneration of fibres has been observed. Duplaix found in a case he examined a localised change in the nerve-fibres, some being diminished in volume, some presenting a swelling of their medullary sheath and axis-cylinder; while in places empty tubes were seen whose nuclei had multiplied.[a] In the majority of cases no change was discovered in these roots.

(*c*) In the peripheral nerves.—The musculo-spiral nerve has been found affected in many instances with atrophy and granular degeneration of fibres, proliferation of nuclei, and increase of connective tissue between the fibres. Some describe the affection as neuritis, others as atrophy. Westphal found in the radial nerve regenerated nerve-fibres, along with others that were atrophied. Dr. Robinson noticed in a case he examined that the walls of the small arteries in the nerve sheaths had undergone a hyaline change, similar to changes which he met with in the vessels of the spinal cord.

(*d*) In the intra-muscular nerve-fibres.—Atrophy of fibres with multiplication of nuclei and increase of connective tissue were found in some cases.

3. Changes in the spinal cord:—

There have been found by different observers changes in three distinct structures.

(*a*) In the ganglion cells of the anterior horns.—Some cells atrophied and devoid of processes. Vulpian found colloid degeneration of some cells.

(*b*) In arterial walls.—Dr. Robinson found in a case he examined hyaline degeneration of vessel-walls in the gray and white substance. Oeller found hæmorrhages in the gray and white substance of the cord, which he believed followed upon arterial degeneration.

(*c*) In neuroglia.—Increase of the neuroglia has been observed by Duplaix.

[a] *Cf.* paper by Dr. Robinson in Brain. January, 1885.

Before proceeding to discuss the question of the primary cause of lead-paralysis—whether it be an affection of the muscles, of the nerves in any part of their course, or of the spinal cord—it will be well to state briefly the leading symptoms, in order that the conditions which a theory worthy of acceptance must fulfil may be kept in view. The leading symptoms in typical cases of the disease are paralysis of the extensor muscles of the forearm—a paralysis (as Remak has pointed out) affecting groups of muscles associated in their action, and not specially those supplied by a single nerve—wasting, reaction of degeneration on electrical testing, this occurring early, in a case recorded by Erb before either paralysis or wasting; no loss of sensation. It is further to be noted that when other muscles, whether those of the upper or lower extremity, are affected, it is usually those functionally related.

One theory which has been held is that the primary cause of the paralysis is in the muscles, the nerve changes being secondary. This theory, however, fails to explain two symptoms—the fact that muscles associated in their action are affected, and the electrical conditions. The muscle theory does not, I believe, meet now with many supporters.

Some view the disease as the result of a primary affection of the intra-muscular nerve-endings. This theory satisfies all the symptoms except the grouping of the paralysis; it fails to explain how nerve-fibres belonging to muscles functionally related, without relation to definite nerve trunks, are attacked. In this connection I may mention an ingenious suggestion of Dr. Robinson. Arguing from the researches of Onimus, who found that the extensor muscles and nerves lost their excitability after death sooner than the flexor muscles and nerves, and those of the upper extremity before those of the lower, he suggested that the extensor muscles possess a less power of vital resistance than the flexor muscles, and consequently they are the first to succumb under the action of a general poison.

A theory that would refer the paralysis to the trunk or larger branches of a peripheral nerve or nerves (mixed nerves) is opposed by the fact that sensation is usually unaffected, as well as by the localisation of the paralysis.

Two views remain to be considered—(1) that lead-paralysis is due to an affection of the anterior nerve-roots, and (2) that it is a central disease.

Two difficulties stand in the way of accepting the former of the two theories. In the majority of cases no change has been found in these roots, even when the peripheral nerves were markedly atrophied. One of two alternatives follows. Either changes have been present in every case, but have been overlooked, which is hardly likely; or the nerve-roots may be functionally affected and yet give rise to obvious degenerative changes in the periphery—a view which will, I think, scarcely meet with general acceptance. Again, it is improbable that special nerve roots should be attacked which are derived from centres presiding over special functions, the centres themselves remaining healthy.

The theory of a central cause remains to be considered. If we regard the disease as caused by localised change in special groups of ganglion-cells of the anterior cornua, possessing either a special affinity for lead or a special susceptibility to its injurious influence, this theory alone of all the views that have been advanced accounts satisfactorily for the symptoms usually present. It explains the loss of power—the loss of power affecting groups of muscles functionally related, and so under the control of special nerve-centres—wasting from loss of the trophic influence of the ganglion-cells, and the electrical relations. The difficulty in accepting the central theory is the absence of definite changes in the spinal cord in many cases examined by competent observers. De Watteville very reasonably argues that this does not necessarily upset the theory, for either the changes may have been so localised and slight as to have escaped detection, or they may be molecular rather than molar. He argues from analogy of other poisons, such as mercury and arsenic, which may give rise to nervous symptoms without producing any evident lesion to explain them, and he also instances Landry's paralysis in which no definite central changes have been found. He adds that "we must choose between the greater of two probabilities—(*a*) between toxic effect of the metal manifesting itself by interfering through a hitherto unexplained process with the function of a certain independent centre, and (*b*) saturnism setting up a peculiar localised neuritis strictly limited to a certain number of motor fibres of some nerves, picking out precisely those originating from that centre." "The latter alternative," he says, "it will be granted, has some features which makes its acceptance far from easy."

Case.—The man whose spinal cord I have had an opportunity of examining was admitted into the Adelaide Hospital in October, 1884. He was thirty-one years of age; had been seventeen years a painter. Was well up to August, 1883, when he was attacked with colic and constipation, for which I treated him at the Adelaide Hospital Dispensary; he soon got well. He had a second attack in August, 1884, that is, a month before admission. On admission he had characteristic drop-wrist, weakness of the legs so as to be unable to stand, marked cachexia and albuminuria. He died of uræmia.

I removed his spinal cord, and with the kind assistance of Dr. Alfred Scott, to whom I am greatly indebted, I have prepared microscopic sections, which I exhibit this evening.

There appears to be a marked change in the internal and anterior groups of ganglion cells of the anterior cornua in both the cervical and lumbar enlargements, especially in the latter. The cells are both fewer in number—some sections showing only one or two—and smaller than natural; some stain badly, and in many the processes are not distinct. The ganglion cells of the lateral groups are well represented and appear quite normal.

Round the central canal in both the cervical and lumbar enlargements there is a large collection of cells, especially abundant in the lumbar region. The number of these cells appears to be considerably beyond the normal.

Dr. Scott made sections of some of the anterior nerve roots. No abnormality was observed either on cross or longitudinal section.

ON A MILKY FLUID FROM A CASE OF ASCITES.

By GEORGE F. DUFFEY, M.D. Dubl.;
Physician to the City of Dublin Hospital;
Lecturer on Materia Medica, Carmichael College of Medicine.

[Read in the Pathological Section, December 4, 1885.]

Cases in which serous effusions, instead of presenting the usual physical characters of such fluids, are of a milky appearance, have been recorded by several observers. Such an occurrence has been noticed in pleuritic, pericardial, and in mediastinal effusions, in ascites, and in the fluid of a hydrocele. In the latter the milky appearance is in some, but not in all, cases due to the presence of seminal fluid from a ruptured seminal duct.[a] Chylous ascites, similarly, is due to the escape of chyme into the peritoneal cavity from a rupture of the thoracic duct, or to obstruction and congestion of the lymphatic ducts, by the pressure of a tumour, or to rupture of several lymphatic capillaries.

Although indubitable instances of such cases of true chylous ascites have been reported—notably one recently in the *British Medical Journal* of May 30th, 1885, by Dr. Whitla, of Belfast—they are of rare occurrence. I cannot, however, help thinking that it is possible some such cases may have been regarded as instances of chylous ascites in which there was no ground for such an appellation beyond the circumstance that the fluid removed resembled milk in its appearance. Such an appearance was presented by the fluid withdrawn by tapping, and found in the abdomen after death, in the case I now bring under the notice of the Section. But an examination of the fluid, which I exhibit, shows that it does not contain chyle; and the autopsy proved that none of the causes of true chylous ascites that I have mentioned was present. In speaking

[a] For a recently reported case in which it was not due to the latter cause, see the *Australian Med. Gaz.*, July, 1885, p. 249.

of the different pathological forms of milky fluids met with, it would be an oversight not to mention chyluria. Notwithstanding the remarkable discovery of the filaria sanguinis hominum in such cases by Timothy Lewis, no more satisfactory definite relation has been established between the presence of that hæmatozoon in the urine, blood, and tissues of patients suffering from that disease, and the occasional milky appearance of their urine, than has any anatomical proof been furnished of the chyle entering the urinary passages by any abnormal communication. In this connection, the statement that the filaria sanguinis hominum have been occasionally found in the milky secretion in conjunctivitis is a notable one.

Case.—The patient who furnished the morbid specimens now exhibited was an unmarried Scotchwoman, aged fifty-two, who came over to Dublin from Glasgow last July to reside with a sister living in this city. Soon after her arrival she had a severe attack of diarrhœa, which lasted for ten days. She attributed this attack to change of diet from Scotch to Irish fare. Although not a very strong woman, she never had had any previous serious illness, but annually for the last nine or ten years her legs used to swell for about three months at a time, and then get well again. Her family history was good. A few days after the attack of diarrhœa had ceased it recurred, and continued from that time up to October 20th, the date of her admission to the City of Dublin Hospital. Her bowels were generally, she said, moved seven or eight times in the twenty-four hours. Six weeks before admission her abdomen commenced to swell, and she got thin and weak. On admission she was observed to be pale, weak, and emaciated. Her abdomen presented the usual signs of ascites. There was but slight enlargement of the superficial veins. The legs, especially the right one, were a little œdematous. The skin was dry; the urine very scanty and slightly albuminous, and there was a systolic cardiac apex murmur. There was little or no tenderness on palpation of the abdomen. At first no pain was complained of; and there was no fever nor vomiting at any time. There were no abnormal pulmonary symptoms nor physical signs. Diarrhœa was a prominent symptom upon her admission, but subsequently it became less so. Mr. A. H. Benson kindly examined the eyes, and reported them normal.

On the 23rd October she was tapped, and 145 oz. of a clear, milky fluid drawn off through a Southey's tube. This fluid was alkaline; sp. gr. 1013; it had a sweetish smell and coagulated on being heated. Microscopic examination showed a number of large granular cells in it, but no fat corpuscles were seen. When shaken up with ether there was also no indication of the presence of fat, only a thin pellicle formed between the two fluids. After standing for several hours, and even days, there was no coagulation or subsidence of any deposit, nor did any layer resembling cream form on the surface. It was, therefore, not chylous. Filtration made no change in the colour of the fluid, and no perceptible deposit was left on the filter. It remained sweet for a long time. Relief was afforded by the tapping, but the fluid rapidly re-accumulated, and at her own request paracentesis was again performed on November 3rd. A similar fluid to that removed on the first occasion came away. But after 40 oz. had flowed through the tube a little blood was noticed, and my resident pupil removed the cannula. On standing a short time, the blood fell to the bottom of the glass vessel containing the fluid. After this operation more abdominal pain was complained of than before, and she grew weaker. The fluid again rapidly accumulated, and again, at her own request, she was tapped, and 80 oz. of milky fluid removed on the 13th November. This, the third operation, was followed, unfortunately, by erysipelas, which commenced at the site of the wound, rapidly extended over the lower part of the abdomen, and finally assumed a gangrenous appearance. She died on November 18th.

At the meeting of the Medical Section on Nov. 20, I exhibited, by card, several morbid specimens obtained from this case, which were then quite recent, and which many of the Fellows and members of the Academy examined. As it may be questioned whether the *post mortem* appearance observed had or had not any influence in causing the peculiar appearance of the ascitic fluid, I may be permitted to again exhibit and briefly describe the lesions observed; and I would then ask the Section its opinion—I. As to their nature; II. As to the nature of the fluid and the cause of its colour; and III. As to the connection, if any, between the anatomical lesions and the peculiarity presented by the fluid.

The cavity of the abdomen contained a large quantity of milky fluid similar to that removed during life. The serous membrane was studded over with grayish-white nodules, and the parietal peritoneum was thickened and opaque. The omentum was greatly thickened, and sprinkled over with small miliary deposits, some of which had spots of hæmorrhage, while others showed a slight depression, on their summits. In the gastro-hepatic omentum were several large glands, some of which surrounded the vena porta, but did not seem to press much upon it. The first stage of the duodenum was very vascular, and the mucous membrane for some inches below the pylorus presented numerous minute white spots, possibly swollen villi. The jejunum was congested, and its serous covering was in places whitish, as if spattered over with a thin paste. The pancreas was enlarged and hard; it weighed $3\frac{1}{2}$ oz. The spleen weighed $11\frac{1}{2}$ oz., and was large, soft, and flabby. The liver also was large. It weighed 3 lbs. 9 oz., was soft, friable, pale yellow, and very fatty. In the substance of the left kidney were a few isolated fibrous nodules the size of a pin's head.

In Douglas' pouch there was a considerable deposit of nodules, which in some places had coalesced and looked like caseous masses commencing to soften. There were no adhesions between the coils of distended intestine or shreds of lymph in the cavity. The mesenteric glands were not enlarged. The thoracic duct, which was carefully dissected, appeared normal.

An examination of the cæcum showed a considerable amount of disease. There were numerous small ulcers situated in it and in the adjacent portion of the colon; and in some places—on the mucous surface—there were hard isolated nodules similar to those observed on the serous envelope of the intestines. Both segments of the valve were thickened; the upper segment, which was especially so, was also ulcerated. There was an enlarged gland in close proximity to the vermiform appendix.

In the thorax there was a plentiful deposit on the diaphragm and costal pleura similar to that on the peritoneum, but there was no effusion, and the lungs appeared healthy. The heart was soft and flabby; it weighed 9 oz. There was a good deal of atheroma in the aortic arch. The left ventricle and mitral orifice seemed dilated, but there was no disease of the valves.

Are these appearances due to general tuberculosis or to a general carcinosis, or are they dependent upon some other condition causing a fibrous deposition of a different type? I have to thank Prof. Purser for his kindness in examining the fluid, as well as portions of the ileo-cæcal valve, kidney, and peritoneal lesions microscopically. He also saw the viscera when in a recent state. Prof. Purser informs me that the portions of the specimen he examined presented no typical characteristic of either tubercle or carcinoma, but merely those of a chronic inflammatory nature.

There was nothing to favour the idea of tuberculosis in the ætiology or mode of origin of the case, neither was there any apparent pulmonary disease. The age and sex of the patient, and the course of the disease as characterised by uniform apyrexia, rapid wasting, and anæmia, would perhaps be more in favour of carcinoma. But as far as the naked eye appearances go, and from the fact that the microscopic examination gave evidence in favour of it, I believe the case to have been one of chronic tuberculosis. I am aware that all caseous matters do not represent tubercle, but it is likewise true, I believe, that a fibrous condition may also be the final change in tubercular growths.

The effect of a chronic peritonitis in causing the so-called chylous ascites has been recently brought forward by Dr. Letulle in a note in the *Revue de Médecine* for Sept., 1884. Certainly the morbid change which he looks upon as essential in this condition, and the presence of which is exemplified in the history of the eight cases he has collected—viz., a chronic peritonitis—existed in my case.

In Dr. Whitla's case there was a distinct perforation of a dilated portion of the thoracic duct; and, as in my case, chronic tubercular peritonitis and ulceration of the colon. He dubs as "fanciful theories" and "startling propositions" the views M. Letulle has advanced. But even from one clearly demonstrated case, Dr. Whitla himself is hardly justified in advancing the theory, which he appears to consider will "answer for all time to come," that in such cases as these "there is probably a small aperture in some of the minute lacteals, which, though not visible, can still permit the direct escape of the chyle." Such a theory is not original, nor, in view of the negative result of careful autopsies, at all satisfactory.

[NOTE.—Soon after the above communication was read, a second paper by Dr. Letulle, in the *Revue de Médecine* for November, 1885, p. 960, reporting another case of chlyliform effusion into the abdomen, came under my notice. In this paper he gives a chemical analysis of some of the fluid removed by paracentesis, and reiterates his opinion that a more or less chronic peritonitis of some kind, is, in the great majority of cases, the cause of this peculiar effusion. The fluid itself he classes apart from other pathological fluids as a "residual liquid;" and he ascribes its lactiform appearance to various phases in the degenerative changes which take place in the exudative products of the chronic inflammation. In some cases the result of such changes resembles a granulo-fatty emulsion, which may be one of the causes of the characteristic opalescent aspect of the liquid.—G. F. D.]

TUMOUR OF INFERIOR SAPHENA VEIN.

By J. K. BARTON, F.R.C.S.;
Surgeon to the Adelaide Hospital.

[Read in the Pathological Section, December 4, 1885.]

J. I., a carpenter, sixty-four years of age, was admitted to the "Bolton" ward, Adelaide Hospital, under Mr. Barton's care, on the 12th of November, 1885. Six months previously the patient had for the first time noticed the varicose condition of the veins of his right leg. He had not taken any means to prevent the increase of this state, and about three months ago he became aware that there was a prominent swelling on the vein which ran along the inside of the knee; this, when first noticed, was the size of a small hazel nut, but at the time of his admission it had increased to the size of a pullet's egg—round, smooth, firm, and somewhat painful in being handled. Its size could not be diminished by any moderate degree of pressure.

This tumour was evidently formed by an expansion of the coats of the vein at this spot, the entrance of the vein at its lower part and exit at its upper being plainly seen, but pressure on the vein on either side made no difference in the size or tension of the tumour. The superficial veins of the leg below this were all varicose—but none of them was expanded into any new defined tumour such as this.

The skin over the tumour was thin, and it appeared probable that a very slight increase of pressure would cause it to give way—and, indeed, when the patient walked for a very minutes this result seemed to be imminent.

A few days' rest in bed made a great difference in the fulness and tension of the varicose vein of his leg, but did not make any difference in the size of the saphenic tumour, showing that the circulation in it was completely arrested, and that the vein and sac were filled with a firm clot. Such a condition is almost always followed by inflammation of the vein thus blocked, so no further time was lost, and, on the morning of the 19th of November, the tumour, with a piece of the saphena vein as it entered or left it, was removed. Antiseptic precautions being taken, about two inches of one of the most dilated of the varicose veins of his leg were removed at the same time. The wounds healed kindly, suppuration being prevented. The temperature had shown there was no inflammatory fever. The patient is now up, and leaves hospital to-morrow.

Examination of the tumour shows that it is, as was surmised, a dilatation of the coats of the vein—a true aneurysm, which is filled with a clot so firm and adherent to the lining membrane as to be with difficulty separated from it. The only cause assignable for the production of this localised dilatation is the trade of the patient. He was accustomed to stand at his carpenter's bench all day working, and it seems probable that as age increased his circulation became feeble, and the strength of the tissues diminished, and thus a varicose state of the vein of his right leg, which probably existed previously to some slight extent, was greatly increased, but why at this spot the coats of the vein yielded so much I am at a loss to say, unless the flexure of the joint, at the side of which it was, may have retarded the feeble circulation going on in the vein so much as to allow of the formation of clot, which, increasing, dilated the vein into the sac which we see here.

SPECIMENS FROM A CASE OF BRIGHT'S DISEASE.

By ARTHUR WYNNE FOOT, M.D., F.K.Q.C.P.;
Physician Meath Hospital; Professor of Medicine, Royal College of Surgeons

[Read in the Pathological Section, December 4, 1885.]

The specimens which it is my privilege to lay before the Pathological Section illustrated in a typical manner some of the pathological events in Bright's disease.

The specimens are a pair of granular kidneys, symmetrically diseased, an hypertrophied heart, and the occurrence of pericardial inflammation as the terminal accident in the downward series of changes.

The kidneys are obviously of the kind described by Richard Bright as granulated; their surface is mottled by being broken into minute areas of a pale colour surrounded by more pinkish—because more vascular—tissue; they are contracted in all their measurements; weigh together, free from their adipo-cellular envelope, six ounces; are firm, tough, and, on the whole, brownish-red in colour; in fact, they are in the recent state, just as represented in the excellent drawing done, while they were fresh, under my superintendence.

The heart weighs thirty-two ounces. The hypertrophy chiefly concerns, and is most conspicuous in, the left ventricle. There is neither valvular disease nor atheromatous change in the aorta beyond a trifling degree, so that the enlargement may fairly be assumed to have been due to increased capillary resistance. The evidence of recent pericarditis is plain to be seen on the surface of the heart in the deposition of a sheet of reticulated exudation. The fluid parts of the exudation have escaped in the opening of the sac. There are two points of clinical interest about the case which are deserving of notice. This man, who may be stated to have been fifty years of age, and by employment a steam-raiser—*i.e.*, a man who has to have the fires lighted and steam up for the drivers of locomotives—was admitted to hospital for hæmatemesis, which had been of daily occurrence for three weeks. The quantity of blood daily discharged from the stomach, while he was under observation, varied from three to fifteen ounces. A history of intemperate habits had, somehow or other, accompanied him into hospital; and the hæmorrhage was regarded as due to hepatic causes. However, examination of the liver, stomach, or spleen, afforded no explanation of its occurrence. He had no dropsy or œdema, and made no complaint of defective vision. His colour was pallid, but not more so than would be accounted for by such a loss of blood as he had been subject to. His urine was examined for the purpose of ascertaining if it would present the characters called "hepatic," and was discovered to be albuminous. The hæmatemesis having subsided in four or five days under the use of ice and ergot of rye, he was moved to another part of the hospital, and, in his transference, seems to have caught cold, because the day after his removal he complained of pains across the upper part of front of the chest, which led me to examine that region for aneurysm, on the possibility of such a disease existing and leaking into the œsophagus. Two days after he complained of pain in the left inframammary region, and on auscultation over the seat of pain I heard nothing to suggest inflammatory action in the pericardium. After twenty-four hours of great restlessness from this date he expired quietly. At the autopsy at least eight ounces of turbid serum, occupied with flakes of lymph, was found in the cavity of the pericardium. The peritoneal and pleural cavities were free from effusion, as was the subcutaneous cellular tissue—the pleuræ were more or less agglutinated by recent adhesions, dry, fleecy, and easily detached. The liver was smooth, and natural in shape, colour, and consistence. The stomach was examined with some care as the seat of the bleeding, for which he had been admitted. Its contents had no appearance of blood—some of the rugæ on its posterior wall presented an arborescent vascularity, but there was no obvious breach of surface. This case had an interesting clinical feature in the fact that the primary—in fact the principal and only—hæmorrhage was from the mucous membrane of the stomach. If called upon for an explanation of this, I can only say that it must have been the *locus minoris resistentiæ*, or offer the usual explanations for the hæmorrhagic attacks in contracted kidney—viz., friability of arteries, increase of arterial tension, and deficient coagulability of the blood. Why, in one case, the hæmorrhage should be an epistaxis; in another, a hæmatemesis; and, in a third, should take place from the female sexual organs (as West has pointed out); why, again, it should be sometimes retinal, sometimes intracranial, is difficult to explain, and I make no attempt to do so. The complete absence of dropsy in this case appears to have been due to the powerful action of the hypertrophied heart.

Pericarditis is the especial characteristic of the granulated kidney, and furnishes one of the most frequent means by which the fatal result is accomplished. It is induced by the most trivial exciting causes, lasts but a short time, and is almost invariably fatal. The first of Richard Bright's cases, and that which furnishes perhaps his best drawing of the granulated kidney, died with pericarditis. It has been calculated (Tyson) that pericarditis occurs in twenty-five per cent. of cases of granular kidney. The non-discovery of pericarditis, when my attention was drawn to the region by the complaint of pain, the day before his death, I can only explain on the supposition that the effusion was sufficient to intervene between the opposite surfaces; the lymph was also of a soft and buttery consistence, such as is recognised as not productive of loud friction. Theoretically it might be assumed that the murmur produced by a hypertrophied heart in pericarditis should be both loud and extensively diffused. I have met with cases where this expectation has not been borne out by facts. These specimens are such common results of granular kidneys, that I was at first unwilling to occupy the time of the Section with their exhibition otherwise than by card, and feel called upon to apologise for offering such trite subjects to the consideration of the members of the Section.

OSTEOMALACHIA.

By E. H. BENNETT, M.D., F.R.C.S.;
Surgeon to Sir P. Dun's Hospital; Professor of Surgery, Trinity College.

[Read in the Pathological Section, December 4, 1885.]

These specimens were taken from a patient who had been under my observation for several years before her death, and during the entire period of her disease. The case was exceptional in that while the woman was comparatively young, being at her death about thirty-nine years of age, pregnancy had not occurred as the starting point of the disease—nor were the conditions of life such as could be assigned as the cause. She had lived in the capacity of upper nurse in most comfortable situations and in various climates, in England, Ireland, and in France. The chain of events in the progress of the disease was spontaneous fracture of the left clavicle; after union of this, its refracture; a fracture of the shaft of the humerus at its upper extremity, followed at intervals of several months by successive fractures of the same bone in its upper half, each fracture uniting well and in the usual time for healthy bones. During the treatment of the last of these accidents, just as the repair was completed, the shaft of the femur on the same side broke in its upper half as the patient turned in bed. At this time she suffered severe pains in the lower limbs and back, and particularly in the right thigh—such that I began to expect the occurrence of fracture of that bone also. She could not bear the restraint of a Liston splint, and milder means were adopted, but the patient suddenly died from failure of the heart. Her fingers had become remarkably clubbed at the ends, while the nails were corrugated, points which raised a suspicion of pulmonary phthisis, but of this there were no signs during life and no evidence *post mortem*. The bones which had been the seat of pain, as well as those that were broken, presented a red mottled appearance, and were so softened that a scalpel could easily be thrust through their tissue, even through the shaft of the unbroken femur. The medulla was, however, fatty and free from red colouring. The kidneys were studded everywhere with fine grains of dull white particles of gritty sand, against which the edge of the knife grated in cutting the cortical and medullary tissues, but there was no free sand or calculus in the hilum, ureters, or bladder. Chemically examined these grains were found to be composed of mixed phosphate and carbonate of lime—to be, in fact, bone-earth. Structurally they were amorphous. The liver and spleen were free of disease. The heart was very small, thin, and soft, and evidently fatty.

This case differs in many essential points from typical osteomalachia—in its occurring without the recorded predisposing conditions; in its remarkable selection of the left side, and of such bones as the clavicle and humerus for its first development; and in the great readiness it showed for the development of callus and complete union.

In this last character the case falls more into the category of senile osteoporosis, but from this again it is widely separated by the characters I have noted as distinguishing it from typical osteomalachia. From the rickets of childhood it is even more widely separated, for there is no evidence here of such abnormal developments of bone as this disease shows. If a longer course had been run, it is clear that the unsymmetrical character would have been lost, for already the right femur was in a condition to break. Had the disease been more acute its selection of the one side of the body might have been unnoticed. With all these exceptional characters I can find no place for the disease except in the group indicated by the name "true osteomalachia."

A CASE OF DENTIGEROUS CYST.

By ARTHUR W. W. BAKER, M.B., Ch. M., Dublin;
Surgeon to the Dental Hospital of Ireland.

[Read in the Pathological Section, January 15, 1886.]

Under the term dentigerous cyst come two very distinct classes of tumours connected with teeth—1st, those cysts which arise from a slow form of inflammation at the roots of perfectly developed teeth; 2nd, cysts arising in connection with teeth which are still unerupted from the jaw. It is to a case of the latter description that I wish to direct your attention.

Case.—J. E., a boy, aged sixteen, was sent to me by my colleague, Mr. Story, on account of a swelling on the right side of his lower jaw. Mr. Story had previously operated on this patient at St. Mark's Hospital for strabismus; he also was treated by Mr. Arthur Benson for some aural trouble.

On examination I found a large swelling on the right side of the lower jaw, extending from where the first molar had been extracted to the lateral incisor; this to all appearance was a perfectly solid tumour. On digital pressure there was neither fluctuation nor parchment-like crackling. The permanent canine I perceived was wanting on the right side, and its milk predecessor, which was loose, was standing in its place; this latter tooth fell out a few days afterwards, and was brought to me by the patient, the root showing the usual absorption.

This swelling, which caused the patient no pain, commenced about six or eight months previously to my seeing him, by his feeling something crack on that side of the jaw while eating his lunch. Since then it had increased in size, and latterly he noticed some slight discharge oozing from the external surface of the gum.

I came to the conclusion that most likely it was a dentigerous cyst containing the missing canine, and in order to verify the diagnosis suggested an exploratory puncture. In operating I had the advantage of Mr. Story's and Mr. Benson's assistance. I made an exploratory puncture into the wall of the tumour with the dental drill; this showed us that the tumour was a cyst with very thick walls. I then, with a pair of specially-constructed bone forceps, with sharp points, cut an opening into the cyst, starting from the hole made by the drill. We were then able to explore the cyst, which contained a foul-smelling, whitish, colloid-looking substance, but we were unable to detect a tooth; at this I was not surprised, for I mentioned to my colleagues before commencing that we were unlikely to find it until sometime after the cyst was opened.

The interior of the cyst, which was lined with a thick and vascular membrane, measured one and a half inches long, by one broad and one deep. It will, perhaps, give a better idea of the size of the interior of the cyst when I say that I was able to plug into it a strip of lint nearly an inch wide, and about three-quarters of a yard long.

At Mr. Story's suggestion I subsequently used iodoform in dressing the cavity, which rendered it perfectly pure. I found that by using a saliva ejector I was able to wash the cyst out perfectly. This instrument, or a modification of it, would be of great use in washing out any cavity where it is impossible to get a dependent opening. A week later I discovered the tooth lying transversely in the anterior portion of the cyst, underneath the lateral incisor; and exactly eight weeks after the operation, by using a fine-bladed forceps, I succeeded with some difficulty in removing the tooth. The difficulty in dislodging it was accounted for by the dilaceration of the root—that is, during the latter stages of the formation of the tooth, when the crown and a portion of the root were calcified, they from some accident got twisted at an angle to the immature pulp, which latter, subsequently calcifying, produced an irregularly-formed tooth. I can only find one other case of a dilacerated tooth occurring in a dentigerous cyst, and that is in a case mentioned by M. Jourdain.[a]

The importance of forming a correct diagnosis in these cases may be gathered from the fact that in a case almost identical with mine, which occurred in the practice of the late Mr. Fearn, of

[a] Salter's Dental Pathology.

Derby, who deserves much credit for his honesty in making it public, the cyst was supposed to be a solid tumour of the jaw, and in consequence the left half of the lower jaw, from the symphysis to the articulation, was removed.[a]

The main points relied on for the purpose of diagnosis in these cases are—1st, the crackling sensation of the thin bony cyst wall; 2nd, absence of a permanent tooth without any history of its removal; 3rd, the presence of a cystic fluid on tapping; this last is used to decide doubtful cases.

The history of my case, and also of Mr. Fearn's, shows that the first sign, the parchment-like crackling, may be completely absent. The absence of a permanent tooth cannot be relied on as a sign, for it frequently happens that a certain tooth may never make its appearance in several members of the same family; also, the permanent teeth may be all present, and yet the cyst be due to the retention of a supernumerary tooth, or the rare case of a cyst developed in connection with a temporary tooth may occur. I believe there are only two cases of this last kind on record—one mentioned by Mr. Salter, and the other by my friend, Mr. Stack. The third point is the diagnostic value of the cyst contents, which may be determined by an exploratory puncture. Referring to Mr. Eve's lecture on cystic tumours,[b] he says—"The diagnosis between a follicular cyst and a multilocular cystic tumour would, I apprehend, in certain cases present considerable difficulties. The lobulation in the latter disease, from the presence of numerous cysts, the incompleteness of its osseous covering, and the presence of fluctuation at several points, would afford valuable aid to diagnosis. Exploratory puncture would, in the case of multilocular cystic tumour, give exit to either a colloid or a brownish serous fluid in different parts of the same tumour, whereas the fluid within the follicular cyst is usually clear and serous."

Now, in the case I have brought under your notice, the matter contained in the cyst was colloid, and did not in any way resemble the usual cystic fluid.

[a] Heath's Injuries and Diseases of the Jaws.
[b] British Medical Journal, Jan. 6th, 1883.

The facts I have mentioned will, I think, show that dentigerous cysts are not as easily diagnosed as it would seem from reading most text-books. The generally accepted view as to the origin of these cysts is, that on the completion of the enamel there is normally a small quantity of clear fluid between the enamel and the follicle, and that in those cases where the eruption of the tooth is delayed, this increases in amount and expands the tissues around it.

Majitot states that dentigerous cysts, or, as he calls them in his classification, cysts of the coronary period, have an epithelial lining. But Mr. Eve has pointed out that this statement, if true, would have a very important bearing on the origin of these tumours, for it would point to there being an expansion of the enamel organ, otherwise it would be very difficult to account for the presence of epithelium, unless Dr. Majitot's explanation be correct, and I think that many will dissent from it—viz., that the epithelial lining is a new formation. Mr. Eve states that he was unable to verify this epithelial lining in any of the three cases which he examined very carefully.[a] I regret that I omitted to examine my case for the presence of epithelium ; I also regret that I neglected keeping any of the colloid matter which oozed out of the cyst when I opened it, as a microscopic examination of it might have thrown light on its origin, which I feel at a loss to account for, unless it was the remains of the enamel pulp, and similar to the contents of a multilocular cyst. This view would involve a contradiction, in this case at all events, of the fact that the enamel organ disappears as soon as its task of forming the enamel is completed.

The injury or shock which dates the beginning of this cyst would also, I think, coincide with the period at which the dilaceration of the root took place ; in this way, I imagine, the additions to the root forced the tooth in an abnormal direction, and, preventing its eruption, caused the cyst.

[a] M. Malassey has recently put forward the view that masses of epithelium exist normally round the roots of adult teeth, and thus account for certain epithelial intramaxillary new formations. A translation of his communication on this subject appeared in the Journal of the British Dental Association for June, 1885.

ULCER OF THE STOMACH, OPENING INTO THE LEFT VENTRICLE OF THE HEART

By J. MAGEE FINNY, M.D., Dub., F.K.Q.C.P.;
Vice-President King and Queen's College of Physicians in Ireland, and Clinical Physician to Sir Patrick Dun's Hospital.

[Read in the Pathological Section, January 15, 1886.]

The specimen which I exhibit is one illustrating a disease of very great rarity, no similar case having been recorded in the United Kingdom, and demonstrating features, in the clinical and pathological history of gastric ulcer, of unusual interest.

It is a specimen of an oval ulcer of the stomach ($1\frac{1}{4} \times \frac{3}{4}$ inch), situated on the anterior wall, two and a half inches from the cardiac orifice, which perforated the gastric wall and the diaphragm, opened into the left ventricle of the heart, and caused death by hæmorrhage directly from the heart into the stomach.

The clinical history of the patient from whom the specimen was removed is as follows:—

Case.—A farm labourer, aged nineteen, was admitted, under my care, into Sir Patrick Dun's Hospital, at the end of October, 1885, suffering from articular rheumatism of a subacute type, of a month's duration. He was well nourished though somewhat pale; the knees were painful and swollen, and a pericardial friction-sound, limited to the sternum, was discovered. In a few days the rheumatic symptoms disappeared, and the pericardial inflammation abated. The friction-sound was not followed by evidences of serous effusion, nor accompanied by endocardial valvulitis.

It was thought, therefore, that the pericardial inflammation had ended in resolution or adhesion. The febrile disturbance (99° to 101°) which existed at the time of admission diminished, but never disappeared, during the five weeks of the patient's life in hospital. He felt, however, so well, the first fortnight after admission, that, at his urgent request, he was allowed to sit up for a day or two;

and his diet was sufficiently liberal, including eggs, bread and butter, and puddings—meat was not allowed, though asked for.

There were no symptoms pointing to disease of the stomach, with, perhaps, one exception. There was no pain in the epigastrium or back, either before or after food, nor any sickness of stomach; and there was a complete absence of any history of his ever having vomited or passed blood, or of ever having had any gastric distress. The only symptom which might have awakened a suspicion of what was going on in the stomach was a pain, which he referred to a small area, corresponding to the cartilage of the sixth rib on the left side, and also to the region near the coracoid process, under the clavicle, on the right side. This pain, he stated, he had suffered from during the last five years at varying intervals; at times it was so severe as to necessitate his giving up his work and lying down. Strong pressure with the folded arms always relieved it. It was seemingly in no way connected with the introduction of food, and was not accompanied or followed by vomiting.

While he was under observation in hospital he had three or four attacks of severe pain, such as I have described, and on two occasions they were relieved by morphine, but during the week preceding death he had much less pain than before, and seemed to be comfortable. The fever, however, was much higher, and assumed a hectic type, so that it was thought that probably some latent tuberculosis was its cause, and to this idea confirmation seemed to be lent by some imperfect respiration and slight crepitus audible near the root of the left lung. Albumen for the first time appeared in the urine during this pyrexial period.

During the night of December 8th the patient had but little sleep, and in the morning of the 9th, about 8 a.m., he went to the night-chair, complained of feeling very weak, and fainted when he was put back to bed. The dejecta consisted of liquid blood. The patient became greatly blanched, and, in spite of immediate assistance, died in half an hour. There was no vomiting.

At the necropsy there was marked anæmia of the body generally, and of the viscera. There was a considerable amount of adipose tissue. The left pleural cavity contained some serous fluid, and the layers of the pleura over the diaphragm were adherent. The bronchial glands were enlarged, and one close to the bifurcation of the trachea was caseous and calcified. The pericardial sac was obliterated; the adhesions over the anterior surface of the heart were vascular, recent, and could be separated by the finger; but those over the posterior surface, and around the apex, were old and dense, and could not be broken down. The valves and muscular structure of the heart were healthy, and nothing abnormal could be seen on examining the cavity of the left ventricle.

The stomach and intestines down to the anus were full of liquid blood, the former alone containing about two quarts. The anterior surface of the stomach was adherent to the diaphragm for an area of two inches; the adhesions were not of a dense nature, and were readily separable. On examining the inside of the stomach, when emptied of its contents, it was seen free from disease, except where it was adherent to the diaphragm, and here situated on the anterior wall, two and a half inches from the cardiac orifice, and two from the lesser curvature, there was an ulcer ($1\frac{1}{4}$ inches by $\frac{3}{4}$) which had perforated all the coats of the viscus, the tendinous portion of the diaphragm, and the obliterated pericardial sac. The floor of the ulcer was rough and granular, and was found to be the muscular structure of the under surface of the left ventricle of the heart not far from the apex.

No sac or abscess existed between the stomach and heart; the communication was direct, and the size of the exposed part of the heart was very little smaller than the gastric aspect of the ulcer. The cause of the fatal bleeding was not evident at first; but, on passing a probe, it was seen that a channel, of the size of a No. 5 catheter, existed in the ventricular wall, passing upwards and backwards, and opening into the left ventricle behind, hidden by a musculus papillaris attached to the posterior curtain of the mitral valve.

The muscular structure of the heart was perfectly healthy and free from fatty degeneration, except at the seat of the ulcer; and here the fibres were granular, rough, and friable, with several interstices, through one of which the probe passed. Microscopically, the fibres were converted into granular matter, devoid of fatty globules, striation, or outline.

A careful review of the life-history and pathological conditions suggests the following as the probable order of events :—(*a*) Simple ulcer of the stomach, of unknown duration, with periods of quiescence and activity, situated where it was but little exposed to gastric digestion or the irritation of ingesta. (*b*) Rheumatic pericarditis. Old adhesions of former attacks obliterated the posterior and under-part of the sac, and recent adhesive inflammation attacked the anterior surface. (*c*) Activity of the gastric ulcer within a fortnight of patient's death; perforation of the gastric walls; adhesive peritonitis around it, preventing extrusion of the gastric contents into the peritoneal sac; perforation of the diaphragm; perforation of the pericardium, and exposure of the muscular structure of the heart to the action of the gastric juice; softening of the exposed muscles; and, finally, through the interstices, and especially through the larger canal, the cavity of the left ventricle was tapped.

The last act of this sad drama occurred during the night preceding death. If the course of events be as I have supposed, it is a curious coincidence that adhesive pericarditis should have preceded the perforation of the stomach. At first I was disposed to attribute the pericarditis to the gastric ulcer, but the following considerations influenced me to alter my opinion in the direction indicated :—

1. The adhesions over the posterior and under-surface of the heart were evidently of old standing, as they were thick, dense, and fibrous, and the layers of the pericardium could not be separated.

2. The adhesions between the diaphragm and stomach were recent, soft, and readily broken down.

It is, moreover, more than probable that, had the gastric perforating ulcer penetrated the tendinous portion of the diaphragm, pneumo-pericarditis, and not adhesive pericarditis, would have been set up, and the left ventricle would not have been opened. Such an occurrence happened in a case published by P. Gutmann (*Berl. Klin. Wochenschr.*, April, 1880). As such cases are very rare, and, as in my case, there was no appreciable symptom to point to the presence of a round ulcer of the stomach, which, however, as Gutmann remarks, must have existed for a long time, I think it well to refer briefly to it.

A young man, aged thirty-six, was under treatment for a right pleurisy for about a week, when suddenly a metallic sound, synchronous with the movements of the heart, was audible at a distance of some feet from the patient's bed, while great displacement of the lungs, cyanosis, and a pulse of 130, caused no doubt of pneumo-pericarditis, having occurred. The patient died on the third day. The necropsy revealed a pericardium, dilated to a great extent with air, and containing a few ounces of non-putrid pus. On the posterior (? anterior) wall of the stomach, and the lesser curvature near the cardiac orifice, an oval opening existed, and, at thè lowest part, a perforation led into the pericardium.

A. Mathieu (the reviewer in the *Archives Générales de Méd.*, 1880, Vol. 2, p. 224) stated that he was aware of but two other similar cases reported (those by Sœninger and Rosenstein), but he unfortunately gave no references by which they can be traced.

Reference to a recent article, by Moizard, entitled "Observations on pneumo-pericarditis, consequent on gastric ulcer," has come under my notice. It is published in *Bull. et Mém. de la Soc. Méd. des Hôp. de Paris*, 1885, 3, s. II., 180–183, but, to my regret, and presumably loss of valuable information, our Dublin libraries do not possess it.

Graves records a case of pneumatosis of the pericardium, in which the air, though derived from the stomach, was due to an abscess of the liver, which opened into both the stomach and the pericardium (*Dublin Journal of Medical Science*, Jan., 1869).

The pericardium is liable to perforation from the œsophagus, by foreign bodies, as in Parkes' ("Path. Trans. Lond.," Vol. II., p. 40) famous case of the juggler, who, while attempting, once too often, to swallow a sword, passed it into the pericardium, instead of the stomach, and set up a fatal pericarditis; and, as in the case of a set of artificial teeth which were swallowed, and, lodging above the cardiac orifice of the stomach, ulcerated through into the pericardium (*Medical Times*, May, 1858).

I have not been able to find references to more than two instances in which the pericardium was opened, and pneumo-pericarditis set up by an ulcer of the œsophagus (Dr. Trotter, "Trans. Path. Soc., Lond., Vol. III., p. 316; and Dr. Forsyth Meigs, *Amer. Jour. Med. Sciences*, 1875).

Dr. G. F. Duffey presented the Museum of the Royal College of Surgeons in Ireland with a rare specimen of ulcer of the stomach, perforating the diaphragm, and opening into the left lung, not far from the pericardial surface, and causing gangrenous abscess of the lung.

Perforation, therefore, of the pericardium from the stomach, would, I imagine, be followed rather by pneumo-pericarditis than by adhesive pericarditis.

Instances of ulcer of the stomach perforating the pericardium must be extremely rare, as there seems to be no recorded instance of it in the medical literature of the United Kingdom, and inasmuch as no more than a bare allusion to such a possibility is found in any of the standard works on the heart or stomach—for example, Ziemssen's "Cyclopædia of Medicine;" "The French Dictionary of Medicine;" or Pepper's or Fagge's recent works; or B. Bramwell's "Disease of the Heart, &c." Brinton, "On Disease of the Stomach," p. 175, writes:—"The pericardium is very rarely opened;" but he gives no instance of such an occurence—an omission excusable only on the grounds of his knowing of none. Dr. Habershon, whose attention, more than that of others, has been for many years directed specially to gastric disorders, informs me, in kind reply to my inquiry, that he has never met with such a case, nor is he aware of any recorded instance. Our pathological museums here contain no such specimens; and Mr. C. Stewart, the curator of the Museum of the Royal College of Surgeons of England, tells me of a similar absence there. Professor Turner courteously informs me that the only specimen in the Anatomical Museum of the University of Edinburgh, which at all bears upon the subject, is "one (No. 932) of a cancerous ulceration of the œsophagus and cardiac end of the stomach, and from the centre of the cancerous mass a small channel led into the pericardium, the entire serous layers of which were covered with recent lymph; no history is attached."

When I came across my case, I thought it was unique; but, thanks to my friend Dr. G. F. Duffey, I have discovered that three similar cases of perforation of the heart, from gastric ulcer, are recorded; each observer, like myself, thought his case the first reported.

Owing to the interest attaching to the subject, and the advisability of collating all such cases, I subjoin the following abstracted reports:—

Case I.—The first case is one presented by Professor Chiari,[a] in May, 1880, to the Vienna Medical Society. The patient, a woman, aged seventy-one, had died shortly after her admission to hospital, with the symptoms of gastric ulcer—namely, hæmatemesis and passage of blood from the intestines. At the necropsy a round hole, two centimètres in diameter, was found in the lesser curvature of the stomach. The opening led into a sac as large as a wallnut, formed by cicatricial tissue; the sac extended through the diaphragm, pericardium, and wall of the left ventricle of the heart, and presented at its apex an ulcerated opening large enough to admit an ordinary probe into the left ventricle. The heart was closely adherent to the pericardium, over a space eight centimètres square; the muscular tissue of the heart was pale and friable, and had undergone moderate fatty degeneration; the endocardium was thickened for a distance of one centimètre around the perforation. The stomach and intestines contained a large quantity of fresh blood. The body generally was very anæmic. In the walls of the sac, close to the opening into the stomach, a hard brittle mass, two millimètres long and about one millimètre thick, was found—probably a piece of glass that had been accidentally swallowed at some time. Dr. Chiari attributed no special importance to this foreign body, as he thought that it had merely found a lodgment in the sac, subsequent to the formation of the latter. He stated that he had been unable to find in medical literature any analogous case of the opening of a round ulcer of the stomach into the cavity of the heart.

The other two cases are reviewed in the *American Journal of Medical Science*, Vol. LXXXIII., 1882, p. 560, and the authors were not aware of Chiari's case.

[a] Chiari, H.: Fall von Perforation eines runden Magengeschwüres in den linken Herz-ventrikel, und Demonstrirung des bezüglichen Präparates (Anzeigen der kl. Gesellsch. der Aerzte in Wien, 1880, pp. 161–163. Abstracted from *Allg. Med. Cent. Zeit.* into the *New York Medical Record*, September 4th, 1880, p. 262).

Case II.—The first is published by Brenner,[a] and is that of a woman, aged fifty-five, who was subject for years to attacks of cardiac pain, occasionally accompanied by vomiting. Six months before death she had an attack of pleurisy with violent pains radiating to the epigastrium. A few days before death she vomited blood, had severe cardiac distress, and passed black tarry stools. The necropsy revealed a circular perforation in the lesser curvature of the stomach, which communicated with an opening in the wall of the left ventricle.

Case III.—The other (*Wiener Med. Blätter*, No. 52, 1881) is by Oser, where the necropsy of the patient, a woman aged seventy-one, revealed a round ulcer of the stomach, which had opened into the left ventricle. The communication was established between the two organs by a long narrow canal. No air was found in the heart or in the arteries. The perforation had occurred three days before death, and was indicated by vomiting of bright blood, and by tarry stools.

My case may be considered as a supplement to these, and in some respects differing from them, for all the three cases to which I have referred occurred in aged females. My case was in a youth, aged nineteen. Vomiting and hæmatemesis were absent from my case, while invariably present, and a marked symptom, in the others. Brenner's case and mine resemble each other closely in their pathology, as in Oser's there was a long narrow canal through which the communication between the stomach and heart was maintained; and Chiari's, in like manner, had an intervening abscess or sac, and was due, in my opinion, to the irritation of the impacted glass.

It needs no great insight to recognise the reasons why such an occurrence as a communication being set up between the stomach and heart or pericardium must be one of the rarest pathological conditions met with.

Independently of the fact that, in 80 per cent. of all cases of gastric ulcer, it is situated on the posterior wall and near the pylorus, there are three important physiological and anatomical

[a] Brenner, F.: Perforation eines runden Magengeschwüres in den linken Herzventrikel. (*Wien. Med. Wochenschrift*, 1881, No. XXXI.)

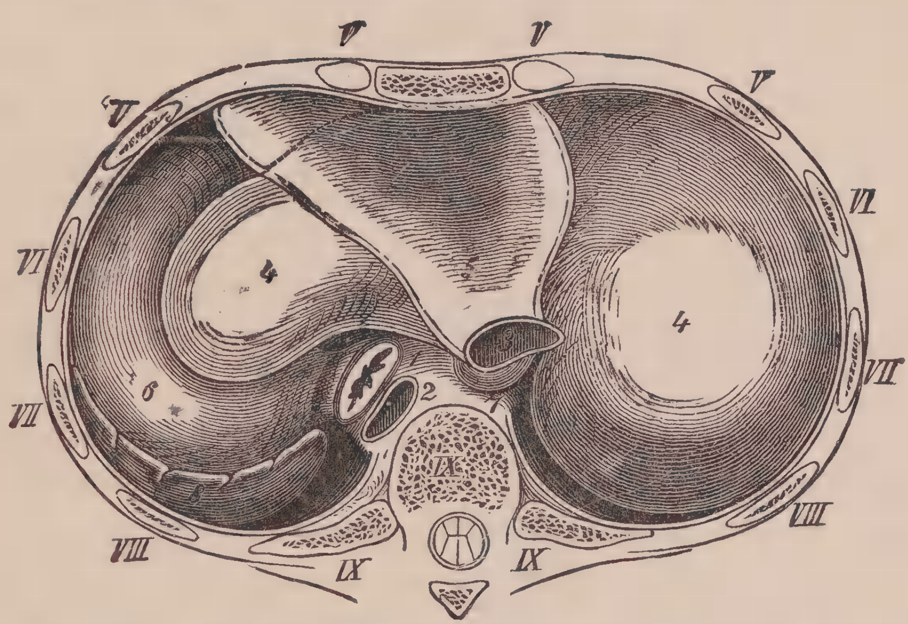
facts which go to explain this great rarity—(1) the very small part of the stomach which can possibly be in contiguity with that portion of the diaphragm upon which the heart rests; (2) the greater movement, during the processes of digestion and the acts of respiration, of the anterior surface of the stomach, so that, were an ulcer situated in this locality, adhesion between the stomach and diaphragm would be prevented; (3)—and perhaps the most cogent—the intervention of the left lobe of the liver between the stomach and diaphragm.

This last point is one of no little interest, as it is one upon which there seems to be some diversity of opinion among the most distinguished anatomists—a diversity dependent, I believe, upon anatomical differences in the topography of the abdominal viscera presented by different bodies which have been the subjects of examination.

In the majority of subjects opened for dissection, which I have examined, the liver, by its left lobe, largely fills the left vault of the diaphragm, and entirely separates the tendinous portion from the stomach; and this is portrayed—and by all accounts true to life—in the beautiful models of the viscera in their relationship one to the other, prepared by His, a pupil of Braune.

Quain (Anatomy, 7th edition, p. 867), writing of the relations of the stomach and liver, says:—" When the stomach is quite empty, the left part of the under-surface of the liver may overlap the cardiac end of that viscus;" and though he refers to the stomach, when inflated, being in contact with the diaphragm, it is plainly not with that part which lies near to or under the heart.

Professor Cunningham, to whose kindness I am indebted for my information on this subject, and who enabled me to investigate it in the anatomical rooms of the School of Physic, Trinity College, Dublin, is in favour of the teaching of Braune, and sets forth, in his manual of Dissections, that the stomach does come into contact with the anterior portion of the left vault of the diaphragm just inside the left mammary line, and below the apex of the heart. Thanks to Professor Cunningham, I reproduce, in the adjoining topographical diagram, the identical plate of Braune's, taken from his frozen sections; and it will be seen that the only part of the diaphragm upon which the heart rests (5), which can come into contact with the stomach, is the small triangular portion marked off by dotted lines. This corresponds in its thoracic aspect to the under-surface of the left ventricle, and on its abdominal to the anterior wall of the stomach, two inches and a half from the cardiac orifice, and two from the lesser curvature—points which strikingly correspond with the situation of the two pathological conditions which exist in the case I have recorded.



1. Œsophagus. 2. Aorta. 4. Liver, right and left lobes. 5. Pericardial layer of diaphragm. 6. Stomach. [Section at the level of the fifth rib in front. Braune.]

I had an opportunity of verifying this anatomical relation in one recent subject. A pin, passed through the chest-wall, in the left fifth intercostal space inside the nipple, and directed downwards, traversed the pericardial aspect of the diaphragm, and impinged on the anterior wall of the stomach in the situation pointed out.

It would, therefore, appear as a necessary anatomical condition that the left lobe of the liver should be smaller and narrower than it is known to be in the majority—or any way in a very large number—of instances, before an ulcer of the stomach can perforate the pericardium or heart.

A CASE OF VILLOUS DISEASE OF THE KIDNEY, FOLLOWED BY HYPERTROPHY, DEGENERATION, AND RUPTURE OF THE HEART.

By WILLIAM THORNLEY STOKER, M.D., F.R.C.S.;
Surgeon to the Richmond Hospital.

[Read in the Pathological Section, February 12, 1886.]

The details of the following case are interesting, not alone because they afford an example of a train of pathological events of some moment, but because the initial circumstance of villous disease of a kidney is one of such rarity that I have found no previous record of it.

If I fall short in the history of this case, it must be remembered that I only made my acquaintance with it when I was called on to make a *post-mortem* examination for the purpose of setting at rest the vexed question of the cause of death. No regular notes having been taken during life, I have had to depend for my account of the symptoms on the statements of friends, on a diary kept by the patient, and on particulars kindly given to me by Dr. James Little, who was occasionally consulted during the attacks of renal pain, and who rightly regarded the kidneys as the seat of disease:—

About ten years before his death, which took place on December 12, 1885, at the age of seventy-two years, the subject of the communication, a gentleman of a full, vigorous habit, consulted a leading Dublin physician for acute renal pain and hæmaturia, which occurred at irregular periods up to August, 1885, when the last bleeding took place. The hæmorrhages came on at irregular periods, at times a few days only intervening between them; at other times some weeks of interval were observed. The bleedings at first lasted only for a day, but, as time went on, increased their duration to a week or ten days. The pain was the characteristic one of renal colic, and was usually on the left side, but sometimes on the right. He had never pain without hæmaturia, but he often had hæmaturia without pain.

He knew when an attack was coming, by the fulness of his temporal arteries, and by a languid feeling which developed itself for a day or two before. The earliest appearance of blood was in the shape of a few clots, followed by a flow of bloody urine. The bleeding nearly always commenced during the night, and diminished during the day until after dinner, when it increased again until next morning. During these attacks pain was often felt, and on occasions all the symptoms of the passage of a renal calculus showed themselves—once, for example, in August, 1883, when the first really profuse hæmorrhage took place, and again in August, 1885, when the last bleeding occurred. The urine passed on the latter occasion he described as being like thin stirabout. The loss of blood during his attacks was at times enormous, and left him so completely exsanguine that even his gums were blanched. He showed great power of reparation, and each time made a rapid recovery, until his final attack in August, 1885, after which he always complained of a dull pain in the lumbar region, and showed loss of strength, coldness of the extremities, defective memory, sleeplessness, irritability, and low spirits. Latterly he complained of itching and fidgets, which came on when the bleeding had ceased, and were relieved the moment it reappeared. These symptoms were removed by cold and aggravated by heat. He formerly suffered from bleeding piles, but had not been troubled by them lately.

He suffered from chronic bronchitis, and on the Sunday preceding his death complained of an oppression behind the sternum, which, he said, felt as if a person were sitting upon his chest. On the next night (Monday) when getting out of bed, he got a sudden pain in the left side of the chest, which for the moment caused dyspnœa. The pain then came on in paroxysms, and shot through to the back and down the left arm, causing a numbness in the fingers. Sometimes the pain started at the wrist and extended upwards. A sense of constriction of the chest preceded each attack of pain. This condition lasted for two days—until Thursday—when he fell into a partially collapsed state, out of which he could easily be aroused. On Friday night after a little exertion he fainted, and when he recovered consciousness was restless and irritable, his breathing irregular, pulse intermittent and compressible, and the cardiac impulse scarcely perceptible. The following morning (Saturday) he felt better, asked for a cup of tea, and whilst swallowing it turned on his left side, and died instantly without a struggle.

During his whole illness he only complained of severe cardiac pain on the two occasions referred to shortly before his death. He was full-blooded, but not florid, and stout but not unwieldly; was a moderate liver and of active habits. He had only two attacks of syncope—one in 1883, and the other the night preceding his death. He did not suffer from giddiness. His radials seemed atheromatous, the second cardiac sound was very weak, his abdomen was usually flatulent. The urine was repeatedly examined. It contained no abnormal element except blood-cells and a few small octahedra of oxalate of calcium.

The story of this case will readily account for the great difference of opinion which had existed as to the nature of the disease, some of the gentlemen consulted having considered the kidneys as its seat, while others had referred it to the bladder. Villous disease of the bladder and renal calculus were among the conditions said to exist.

The *post mortem* examination was made on December 14th, 1885, about forty-eight hours after death. Decomposition had commenced. The body was very stout, large deposits of superficial and deep fat were present, and most of the organs were in a state of fatty degeneration. With this exception, no evidence of disease was discovered, save in the heart and kidneys, and in the larger blood vessels, which were slightly atheromatous. The bladder was attenuated, but otherwise normal.

The pericardium was filled with a large clot, accurately moulded around the entire surface of the heart, which had evidently formed at the time of death, and was consequent on a rupture of the left ventricle. The heart was very large, weighed 1 lb. 6 ozs., and was loaded with sub-pericardial fat. At and below the junction of the middle and lower thirds of the left ventricle there was an irregular reddish brown discoloration, the size of a crown piece, due to the engorgement of the tissues with blood. A rupture near the centre of this was the immediate cause of death, and permitted the ready passage of a probe into the cavity of the left ventricle.

The diseased conditions of the vascular system were evidently secondary to kidney disease. The left kidney was considerably enlarged, it was the seat of chronic nephritis, and its sinus contained two facetted calculi, as large as hazel nuts, composed of oxalate of calcium and colouring matter. The pelvis of the ureter was enormously dilated, as a consequence of the valve-like action of these bodies on its opening.

The right kidney weighed about 18 ozs., the renal substance had almost disappeared, and was replaced by a number of spaces, filled with blood-clots in different stages of formation, some being old, well formed, and laminated, while others were recent and scarcely solidified. The pelvis of the ureter was filled with gruel-like matter, which proved to be disintegrated blood. The sinus, infundibula, and calyces contained extensive villous growths, presenting the usual dendritic character, which had evidently been the source of the most serious hæmorrhages.

The heart and kidneys having been submitted to Dr. M'Kee, the Curator of the College of Surgeons' Museum, for microscopic examination, he has furnished me with the following complete and interesting report, which quite accords with my own examination of the case and with the facts as observed during life:—

" *Heart.*—The descending branch of the left coronary artery may be distinctly felt through the fat as it passes towards the area of engorgement. On removing the fat from the vessel, its wall is found occupied by patches of calcification; and on cutting into it, and following it in a downward direction, an embolus is discovered riding upon the angle formed by its division into two smaller branches and projecting into each of these. One of the branches supplies the engorged area, and is completely occluded by the embolus. The engorgement is, in fact, due to the embolism, and the area in question is the seat of hæmorrhagic infarction.

" The fact that ecchymoses have sometimes been observed around ruptures of the wall of the heart seems to make a few words of explanation necessary. Microscopic examination of the muscular tissue of the left ventricle revealed fatty degeneration and some brown atrophy of its elements. Advanced fatty changes being by far the commonest cause of rupture of the heart, were it not for the existence of embolism we might regard the infiltration as an ecchymosis produced by a slow penetration of blood during the progress of the rupture, which would then have taken place gradually. Since, however, Hyrtl proved, by the failure of all attempts to inject the branches of one coronary artery by material thrown into the other, that these vessels are, in the strictest sense, terminal arteries, it cannot be doubted that the heart may be the seat of hæmorrhagic infarction. Accordingly, not only are such cases on record, but this condition has come to occupy an acknowledged place amongst the rarer causes of cardiac rupture. As regards the left ventricle, then, the course of events was probably somewhat as follows :—Hypertrophy (due to the renal affection), sclerosis of left coronary, fatty degeneration, dilatation, *thrombosis*, embolism of the descending branch of the left coronary, infarction, rupture. The embolus must have been derived from a thrombus formed in the left side of the heart, but as the specimen had been exposed to the action of spirit for some time, it was not easy to say which of the small clots occupying the apex of the left ventricle were formed *ante* and which *post mortem*. One or two small fragments, however, are so closely adherent to the wall that there is little doubt that they may safely be regarded as portions of a thrombus.

"The aortic and mitral valves are thickened, owing to chronic endocarditis. On the right posterior segment of the aortic valves a fibrous nodule is situated, and the corresponding sinus of Valsalva is dilated. The wall of the aorta immediately above the origin of the right coronary artery is occupied by a calcified plate of sclerosis. There was no insufficiency of the aortic valves. The chordæ tendineæ attached to the anterior papillary muscle appear slightly thickened and shortened, but there is no obvious mitral insufficiency. The left auricle is somewhat dilated; the right ventricle is dilated and very thin-walled; its musculi papillares are atrophied; the tricuspid and pulmonary valves are normal.

"*Left Kidney.*—Microscopic examination of the left kidney reveals the group of changes characteristic of so-called partial interstitial nephritis. Patches of connective tissue are seen here and there throughout the section; the epithelium of the tubules situated within their compass is atrophied through pressure. Collections of leucocytes occur, chiefly in the vicinity of the dilated venæ stellatæ. Many of the glomeruli are in a condition of sclerosis brought about by pericapsulitis. A few small cysts may be seen, due to obstruction of the tubules; they contain hyaline substance, and are surrounded by epithelial cells, which are flattened by pressure from within the cysts. Owing to the advanced state of decomposition of the organ, and consequent failure in hardening it properly, it is difficult to pronounce an opinion as to the existence or non-existence of other changes in the renal epithelium during life. The most important alteration in the kidney is, however, the thickening of the walls of the arteries, which, despite the poorness of the sections, is very evident. Clearly this opens up the question of the influence of this condition as a possible cause of the ventricular hypertrophy.

"*Right Kidney.*—The right kidney presents a condition which appears to be hitherto unrecorded. The organ is enormously enlarged. The fibrous capsule is excessively thick, owing to perinephritis. The section shows that the renal tissue, with the exception of a narrow portion at the lower extremity of the organ, is replaced by what appear to be cysts filled with old and recent blood-clots. These apparent cysts communicate with the pelvis, which is also filled with coagula, and are, in fact, nothing but enormously-dilated calyces. As the result of the gradual dilatation of the calyces, the renal tissue has been more and more encroached on, so that ultimately the calyces have come into contact, the approximation of their walls giving rise to dense, thick fibrous septa running throughout the organ. The lower half of the kidney is occupied by a single immense cavity containing several old clots and a recent one. It is possible that one or more of the fibrous septa may here have disappeared from atrophy or necrosis caused by the pressure of the effusion. That a succession of hæmorrhages occurred is proved by the laminated character of the clots.

"A general dilatation of the calyces can be due to one cause only—namely, an abnormal increase of tension within the pelvis of the kidney; and accordingly a softened coagulum was found occluding the ureter some two inches below its commencement. It appears highly probable that the hæmorrhage was the primary event; that obstruction of the ureter was occasioned by a clot; that the dilating force was supplied by the joint pressures of the hæmorrhage and the urine which would at first continue to be secreted; and that, lastly, the clot in the ureter, after undergoing softening, was swept on into the bladder to be discharged with the urine. After an interval the same series of occurrences would be repeated; there would arise a fresh obstruction of the ureter, a renewed elevation of tension in, and dilatation of, the pelvis and calyces, and a new lamina would be added to the first coagula. This hypothesis perfectly accords with the intermittent character of the hæmaturia as observed during life.

"As regards the source of the bleeding, it is improbable that an intermittent hæmorrhage of this kind could be of arterial or venous origin, and indeed a very obvious source of capillary rupture is presented by the new formation seated on the pelvis a little above the origin of the ureter, and existing also in some of the calyces. A naked-eye inspection shows this to belong to the class of villous growths so common in the urinary bladder and so notorious for their bleeding propensities. The diagnosis is confirmed by the microscope."

The matter of primary interest in this case is the occurrence of villous disease in a situation so rare or so unique as the calyces, infundibula, and sinus of the kidneys. While these dendritic growths have been described in the gall bladder and in the urinary bladder, I can find no record of their invasion of the chambers of the kidneys.

Whether the changes in the vascular system had their initial seat in the right kidney or the left—in other words, whether the disorder due to hæmorrhage dependent on villous disease, or that arising from renal calculus, was the first factor in the train of events, is beside the main interest of the case. Without doubt the hypertrophy, ending in infarction and rupture of the heart, might have had its origin in the disease of either kidney, and it is likely that, latterly at all events, both organs contributed to it. The examination after death removes any difficulty of diagnosis which existed during life, and, with the exception of the doubt as to the priority of disease of one kidney over the other, puts the pathological theory of the case beyond the regions of conjecture.

That renal clots, like calculi, can, during their passage into the bladder, produce the characteristic colic, is a matter well known in connection with diseases—such as cancer—involving the renal passages and producing hæmorrhage. That the pain in the present instance was sometimes due to the passage of clots, is borne out by the observation that it was often relieved by the discharge of the "stirabout" or gruel-like matter such as was found after death in the right ureter, and which examination showed to be disintegrated blood. This is still further evidenced by the occasional occurrence of the pain on the right side, where villous disease produced bleeding, as well as on the left, where calculi were present.

The spheroidal shape of the cysts in the right kidney, and the theory as to their mode of formation, is in exact accord with authority. Rindfleisch, speaking of retention-cysts, says:—"The simplest, yet least common, mode is that by plugging of the duct. The secretions accumulate; the longer this lasts the more does the original form of the isolated cavity give place to a spherical or spheroidal shape; a cyst is formed whose walls are identical with those of the original duct." Here a clot was the plug, and the urine, with successive hæmorrhages, the agents of pressure.

The conditions of embolism of the coronary artery and rupture of the left ventricle, which were here found, are so absolutely typical of what is stated by the author I have just named, that the description which he gives of a focus of fatty-softening preceding rupture might be, word for word, applied to the present case.[a] The gradual giving way of the fibres of the heart, probably beginning on the surface where the convexity is greatest, just as breakage occurs in the hoop of a barrel, was the probable cause of the two attacks of pain and dyspnœa on the Monday and Friday preceding the Saturday on which the death took place. I have no doubt that such a partial rupture may precede the final and fatal one.

The dual points of most interest in this history of a very unusual case are—

1st, the occurrence of the villous disease in the chambers of the kidney; and,

2nd, the complex chain of events which caused during life so much uncertainty as the nature of the complaint, and the *post mortem* explanation of which may be of use in the elucidation of some future enigma of disease.

[a] Pathological Histology. Rindfleisch. Sydenham Society. Vol. I. Page 274.

ON CANCER OF THE LIVER.

By JAMES LITTLE, M.D., F.K.Q.C.P.
Physician to the Adelaide Hospital.

[Read in the Pathological Section, February 12, 1886.]

On the first of January a lady's nursetender, aged forty-four years, was brought to the Adelaide hospital suffering from profuse hæmatemesis. She had been attending a lady in Molesworth-street, and went to bed with the new-born infant. Soon afterwards she fell sick and vomited, as she said, half a chamber-full of blood. From the quantity she brought up after her admission into hospital it was probable that this was not an exaggeration. On seeing her I inquired into her previous history, and found that she had been always a delicate woman, and during the past year particularly so, the chief ailment from which she suffered having been pain in the epigastrium, brought on by eating solid food. She mentioned three or four physicians whom she had consulted. The pain went between her shoulders, and particularly under the right shoulder blade; and it was always brought on by eating solid food, but was occasionally relieved when she took liquid food. She had also suffered from nausea, particularly in the morning, and had sometimes vomited, but had never vomited blood until the occasion mentioned. My diagnosis was that she had an ulcer of the stomach. She mentioned that whenever she took aperient medicine it greatly aggravated the pain; and also that when her bowels were about being moved, and also for some time afterwards, she suffered from a great feeling of soreness all through the abdomen. The existence for twelve months of the epigastric and interscapular pains, and the circumstances of their having grown worse during the last three months, and of their having been particularly bad on the day preceding the hæmorrage, seemed to me sufficient to justify the diagnosis of an ulcer in the stomach. Another circumstance tending to the same conclusion was that the blood was very bright in colour, and not of the dark colour usually observed in blood coming from the stomach. She only lived twenty-four hours after her admission, and had several profuse hæmorrhages before her death. A *post-mortem* examination was made by Dr. Bewley, who has given me the following report of it:—

"On *post-mortem* examination the stomach was found to be quite healthy, with the exception of a patch near the lesser curvature, about the size of half-a-crown, where the veins were greatly dilated and varicose. There did not, however, appear to be any breach of surface.

"The intestines contained a large quantity of blackened blood, but their walls appeared healthy; the œsophagus was quite normal, and no breach of surface in any part of the gastro-intestinal tract could be found. The liver weighed 4 lbs.; almost the whole of the right lobe was occupied by one large mass, which presented in places a white and in places a yellowish-brown colour, and which was very firm and dense, cutting almost like cartilage. This mass was bounded by an abrupt line of demarcation from the rest of the liver. The remainder of the liver, consisting mainly of the left lobe, was very tough, and presented the characters of cirrhosis. The vena porta was filled with a soft dark thrombus.

"On microscopic examination the mass in the right lobe of the liver was found to be cancerous in nature, consisting of loculi of firm fibrous tissue, completely filled with polygonal-shaped cells. At its margin this mass pressed on the liver cells, causing them to atrophy and disappear. Many of the branches of the portal vein are filled and rendered impervious by a growth of the cancer in them. The left lobe of the liver shows the microscopical characters of cirrhosis. In both lungs were several nodules, about the size of a small cherry, which have exactly the same structure as the mass in the liver. All the other organs in the body appeared healthy, as far as they could be examined.

"The course of events in this case seems to me to have been as follows:—The cancer in the liver grew into the branches of the portal veins, gradually obstructing more and more the blood-flow through them. At last the blood, not being able to circulate, clotted in some branch, and from this the thrombosis spread through the portal veins in the liver, thus completely, or almost completely, checking the flow of blood in the portal vein. This caused so great congestion of the stomach and intestines that their over-full capillaries poured out the blood in the enormous quantities which were vomited.

"That the abdominal viscera were greatly congested is shown by the fact that, in spite of the great loss of blood, the spleen was found, after death, to be very full of blood, and to weigh 10 oz."

The case then was one of cancer of the liver occurring under unusual circumstances, and not presenting the characters of ordinary Farre's tubercle of the liver. It was a mass of hard cancer occurring in a liver which had been previously the seat of cirrhosis, and the extensive hæmorrhage was evidently due to sudden obstruction of the portal vein.

SPONTANEOUS RUPTURE OF THE HEART.

By ARTHUR WYNNE FOOT, M.D., F.K.Q.C.P.;
Physician Meath Hospital; Professor of Medicine, Royal College of Surgeons.

[Read in the Pathological Section, February 12, 1886.]

My contribution to the subject of rupture of the heart derives any interest which it may present from the fact of the rupture having occurred in bed during sleep. In all other respects it accords with the majority of reported cases—that is, it happened in a man, in one over sixty years of age; the heart was in a condition of fatty degeneration, and the rupture was in the left ventricle on its anterior aspect. Rupture of the heart—under no circumstances a very common event—is still less frequently met with independent of physical effort or mental excitement. The subject of the event was a car-driver, sixty-six years of age, admitted to hospital exactly a fortnight before his death, complaining simply of weakness and short breathing. He said he had been ailing but for a week before admission. At the time he came under my observation his feet and legs were œdematous, the action of the heart weak, slow, and unequal, his colour pale, and skin of a satiny smoothness. His appetite was good, and urine free from albumen; and under the use of rest, meat, and wine, with tonic doses of digitalis and nux vomica, he frequently said he was daily gaining strength, but never showed any inclination to leave his bed. He slept a great deal, and often had to be aroused when the class came round. His sleep was never of a lethargic or comatose variety. On the morning of the 9th February, 1885, he was found dead in his bed. There was no indication of any discharge from his stomach or bowels having taken place, nor was there any disturbance of his bed-clothes; his face was quite placid; he was such a quiet man, and usually slept so much that his not moving in the morning was unnoticed by the other patients; he had made no noise during the night. The distended pericardium, when slit open, gave exit to blood-stained serum, and contained 12 oz. of jet black coagulum, part of which was connected with a longitudinal rent in the anterior surface of the left ventricle, midway between the apex and base, and close to the septum ventriculorum.

The pale, soft, and greasy muscle of the heart had obviously been the seat of fatty degeneration, the condition which, above all others, predisposes to rupture. In 62 cases of rupture, Quain found it present in 25; and in the cases more recently collected by Barth, it was so in 19 out of 24.

I do not think the rupture can be attributed to the use of digitalis—a possibility which has been suggested by Lauder Brunton—the digitalis by the energy of the systole causing rupture of the degenerated fibrils—because the digitalis was given in small doses, was unattended by nausea, anuria, or marked slowing of the pulse, and he expressed himself as feeling an almost daily improvement under its use.

MITRAL AND AORTIC DISEASE.

By WALTER G. SMITH, M.D., &c.;
Physician to Sir P. Dun's Hospital.

[Read in the Pathological Section, February 12, 1886.]

THESE viscera were taken from a man who died of cardiac disease. The patient, aged thirty-seven years, was admitted into Sir P. Dun's Hospital, January 15th, 1886, and died on the 21st instant. He was a ship-chandler, and had drank hard. Twenty-three years ago he suffered from rheumatic fever, but remained in good health up to two years ago, when he began to suffer from shortness of breath and pain over the heart. In December, 1885, he sought advice for cough, dyspnœa, pain in chest, and insomnia. Upon examination the heart was found to be much enlarged, and several murmurs were heard; at the base of heart, systolic and diastolic murmurs, and at the apex a localised systolic murmur, and a doubtful presystolic one. Pulse small and weak. Bronchial râles over right lung; dulness over back of left lung. Viscid muco-purulent sputum, tinged with blood. Feet swelled; no albumen in urine. He rapidly declined in strength, and died suddenly, January 21st.

Post mortem.—Right pleura normal; left much thickened. Both lungs in a state of marked brown induration. A considerable hæmorrhagic infarction in lower lobe of right lung. Heart weighed 17 ounces. Endocardium thickened and opaque in right and left cavities. Dilatation of right cavities. Pulmonary valves healthy. Walls of left cavities, especially ventricle, thickened, and very firm in texture. Mitral valve constricted, scarcely admitted little finger; warty growths and cauliflower excrescences, chiefly on its auricular aspect; curtains of valve adherent and blended into a cone-shaped funnel. Aortic orifice—warty vegetations on all the segments, which were thickened and adherent, commencing atheroma in aorta.

The abdomen contained several pints of fluid; a large pale infarct in spleen; a small infarct in right kidney, and minute hæmorrhages in pelvis of each kidney; substance of kidneys firm and tough; liver $2\frac{1}{2}$ lbs., very hard and tough; nodulated on surface; section very dark and mottled. Walls of intestines thickened and œdematous; minute hæmorrhages scattered over the mucous membrane; stripes of ecchymosis along great curvature of stomach, and in lower end of œsophagus.

A CASE OF MALIGNANT ENDOCARDITIS.

By J. M. PURSER, M.D.; F.K.Q.C.P.;
Professor of Institutes of Medicine, Trinity College; Physician, Sir P. Dun's Hospital.

[Read in the Pathological Section, November 6, 1885.]

Case.—E. M'D., a carpenter, aged twenty-two years, was admitted into Sir Patrick Dun's Hospital, October 22nd, 1885. Some years ago he had rheumatic fever, from which he completely recovered. His habits were steady and temperate until two years ago. Since then he has been very drunken. On the evening of October 17th he came home drunk and violent. The next day he was very ill, and complained of pain in his head. Since then he has been delirious, sleepless, and violent.

Oct. 23rd.—He is a small, slightly-made man. He lies in bed moaning, and in an apparently semi-unconscious condition. When spoken to loudly he puts out his tongue, which is dry and brown in the centre, red at the edges; says he has pains in his head and ears. There is no discharge from the ears or other sign of disease in this part. Temperature, 104°; pulse regular, and of fair strength and volume; respirations irregular. No evidence of disease in heart or lungs can be discovered by physical examination. Abdomen rather hard and moderately distended; spleen distinctly enlarged; urine retained—when drawn off found to be highly albuminous. He swallows well. Bowels not moved since admission. Pupils are rather dilated, but respond to light. There is no paralysis. He objects to being touched or moved. There is no eruption on the skin. There is the scar of a very extensive burn, suffered in childhood, on the lower part of the chest and upper part of abdomen. On the dorsal surface of the right hand, over the metacarpo-phalangeal joint of the middle finger, is a small oval scab, about $\frac{1}{4}$ in. × $\frac{3}{16}$ in. in size, surrounded by a circle of loosened epidermis, looking like a collapsed vesicle or pustule. The whole patch is about $\frac{1}{2}$ inch in diameter. It is not surrounded by any inflammation. There is no inflammation of lymphatic vessels or glands.

The patient was restless last night, and violent. He had a hypodermic injection of morphia, after which he got some sleep.

On the following day his condition became worse. The unconsciousness became greater, so that he made no response when spoken to. He was absolutely sleepless; he moaned and muttered continually. His lips were dry, face pale and sunken, abdomen not much distended, but hard. Bowels moved by enemata; urine still retained. Pulse continued of good strength; respiration irregular. An inspiration was made, the breath held for some time and then expelled by a long and noisy expiration—such noisy and irregular respiration alternated with periods of quiet breathing. Physical examination failed to detect any abnormal sound in heart or lungs. There was no eruption. *Tache cèrébrale* not present.

25th.—Sleeplessness and delirium continue. Swallows with difficulty. He holds the drink in his mouth for some time, and then ejects it violently. Face more pale and sunken. Pulse still of good strength. *To-day a distinct systolic blowing murmur heard over a limited region in the mitral area.* Since admission the temperature has varied between 103° in the morning and 104° in the evening. An enema of egg and milk, with quinine, gr. xx., and chloral, gr. xv., was given and repeated. Both enemata were retained, but had no effect in reducing the temperature or in causing sleep.

26th.—Morning temperature, 104·5°. Strength manifestly failing. Delirium persistent. To-day several small bluish ecchymoses were seen—one over left eye, others on back of right forearm, on left hand, and on other parts.

Repeated doses of antipyrin, gr. xv., were given, at first by the mouth and subsequently by the rectum. The first dose was followed by very profuse sweating, and the exudation of a large quantity of bloody froth from the mouth. The temperature fell within an hour more than four degrees, but soon rose again to its previous height. The subsequent doses were almost without effect.

27th.—Strength failing. Urine and fæces passed in bed. Swallows with great difficulty. In afternoon the air passages began to fill with fluid.

28th.—Died at 3 a.m., without convulsions or marked change in symptoms. The cardiac murmur persisted to the end unaltered.

The body was examined eight hours after death. Extreme lividity of depending parts. Ecchymoses as noticed during life. Rigor well marked. No œdema.

Calvarium heavy, dense, and firmly adherent to *dura mater*, which was otherwise normal. *Sinuses* contained fluid blood and some small loose clots. On the convex surface of the *brain* were three hæmorrhages into the subarachnoid tissue. The amount of extravasation was not sufficient to cause any pressure on the brain. Each hæmorrhage was circular in shape, and about an inch in diameter. There was one on the anterior part of each frontal lobe, and the third was situated over the posterior end of the first left frontal convolution. At the anterior portion of the right temporo-sphenoidal lobe there was an ochre-yellow coloured patch, oval in shape, $1 \times \frac{3}{4}$ inch in extent, and with ill-defined edges. The tissue here was firmer than normal. This patch extended in depth through nearly the whole thickness of the grey matter. In the middle there was a small depression, apparently a loss of substance. The microscope showed numerous orange-coloured pigment grains of varying size, singly and in groups. The Pacchionian granulations along the longitudinal sinus were numerous and large. Pia mater transparent, and not unduly adherent to the brain. Vessels at the base normal. Ventricles normal. Sections through the brain showed numerous congested vessels, but beyond this nothing abnormal. Cerebro-spinal fluid clear, and not remarkable as to amount. Bones of the base of the skull, and dura mater covering them, healthy. *Optic nerves* normal. In each *retina* a small punctiform hæmorrhage.

Subcutaneous adipose tissue scanty in amount; muscles dark-coloured, dry and firm. No hæmorrhages were found in any of the muscles examined. Transverse colon much distended with gas; peritoneum contained no fluid; no peritonitis. On cæcum and some of the coils of small intestine there were subperitoneal ecchymoses, unaccompanied by any sign of inflammation. On the right side, the diaphragm reached the level of the fourth rib; on the left, the upper edge of the fifth rib.

Thorax.—Lungs collapsed badly; a few old adhesions on right side. *Pericardium* contained about two ounces of clear fluid. Numerous punctiform ecchymoses under the epicardium. Well-marked white patch on the right ventricle and another on the outer and posterior surface of the left auricle. Cavities of the *heart* on the right side almost empty. The left auricle and ventricle contained dark fluid blood and some clots. The heart was flaccid; the arterial valves supported the water column. On the right side the valves were healthy. The aortic valves were slightly thickened. The anterior curtain of the mitral valve was somewhat thickened, as were the chordæ tendineæ; on the auricular surface, near the free edge, some small subendocardial ecchymoses, and a small raised rough spot of yellowish-white colour. The whole of the auricular surface of the posterior curtain was rough, and covered by a raised, warty, yellowish-white, diphtheritic-looking layer, with more prominent looser masses near the free edge. At this part there were some small hæmorrhages. The raised diphtheritic patch extended for about half an inch above the attached edge of the valve, and involved the posterior and outer wall of the left auricle.

There were numerous punctiform hæmorrhages under the endocardium of both ventricles. The muscular tissue of the heart was rather pale, but firm. On cutting into it numerous small linear ecchymoses were found, many of which had a minute pale point in the middle, but there was no actual suppuration. The coronary vessels were healthy. The heart, with the origin of the great vessels, weighed 11½ oz.

The Lungs were heavy. The lower lobes were dark in colour, and friable; on the right side quite airless, and yielding on pressure a large quantity of fluid. On the left side the lower lobe contained some air. There were numerous dark purple spots, apparently hæmorrhages, spots of catarrhal pneumonia, and much œdema. The upper lobes of both lungs were œdematous, but contained air.

The trachea and bronchi were reddened and contained frothy mucus.

There were numerous subpleural ecchymoses in both visceral and parietal layers.

The bronchial glands were large, firm, of dark purple colour, and contained black pigment.

Aorta rather thin. At the convexity of the arch there were a few small calcareous plates under the intima. There were a few opaque, slightly raised, patches in the intima of the ascending part. Measurements of the aorta—just above the valves, 6·5 cm.; just below the diaphragm, 4·5 cm.; immediately above the bifurcation, 3·2 cm.

Œsophagus healthy—contained some brown grumous fluid, regurgitated from stomach.

Abdomen.—Spleen large; weight, 13½ oz.; very soft. Some dark purple spots seen through the peritoneum on surface. On section these were seen to be the bases of more or less conical patches, of firm consistence, for the most part of a dead yellowish-white colour and surrounded by a dark purple border. There were also numerous smaller dark purple spots, apparently hæmorrhages. The pulp of the spleen was soft, of a grayish-red colour. On section it did not bulge above the level of the incision. Scrapings showed a few blood-corpuscle-holding cells, and numerous free blood corpuscles, mostly distorted in shape.

Kidneys.—Capsule separated with difficulty, and carried with it portions of the cortex. On the surface of the kidneys were some ecchymotic spots, which formed the bases of conical infarcts—firm, colourless in centre, and surrounded by a dark purple zone. Other similar spots did not reach the surface. In the pyramids were some very minute linear white streaks, about 1 mm. in length, not surrounded by inflammation or vascularity. The general cortex was of greyish-pink colour, opaque, radial markings indistinct. Scrapings showed congested Malpighian tufts—tubes with coarsely granular (partially fatty) swollen epithelium.

The renal vessels, ureters, and bladder were healthy. The bladder contained some ounces of dark-coloured turbid urine. Prostate and vesiculæ seminales healthy. Rectum contained some yellow fæces of pasty consistence. The mucous membrane presented numerous round or oval ulcers 5–8 mm. in diameter, with sharply-cut edges and smooth floor; they were very superficial, and were not surrounded by inflammation or vascularity.

The *duodenum* contained bile-stained mucus. Parts in lesser omentum normal. Bile duct pervious. Bile dark reddish-brown and viscid.

Stomach contained dark grumous fluid. Mucous membrane extensively ecchymosed.

Pancreas healthy.

Liver rather soft, opaque, and of yellowish-brown colour; a few bluish spots beneath the peritoneum, corresponding to partially discoloured patches with surrounding purple zone. Weight, 4 lbs. 7 ozs. *Gall-bladder* healthy; contained a few drachms of dark bile.

Small intestine contained yellow mucus above—fæcal matter in lower part. Coats presented numerous ecchymoses, some visible internally, some externally, and some in both directions. In the centre of some of these a lighter-coloured point could be seen. They were most numerous in the lower part of the jejunum. They occurred also in the cæcum and vermiform appendix. Peyer's patches and the solitary follicles were not enlarged.

The *large intestine* contained a good deal of light-coloured pasty fæces.

Mesenteric glands enlarged, firm, purple in colour.

Inferior vena cava contained fluid blood; coats normal.

Microscopical examination.—Vertical sections through the thickness of the mitral valve showed that the white mass which covered its auricular surface was entirely composed of micrococci; these formed in many places a layer 0·25 mm. in thickness. They were very small round or short oval bodies, and where the section was very thin it could be seen that they formed long chains. The free surface of this mass was uneven. At the deep surface the micrococci could be seen penetrating into the tissue of the valve, and extending in long tracks between the bundles of fibrous tissue. The endothelium seemed to be absent where the layer of micrococci was thick and continuous; but where the micro-organisms formed smaller detached colonies, the endothelial cells could be seen swollen, loosened, and in places apparently being forced off the subjacent structure by the growth of the micrococci. Underneath the layer of micrococci the tissue was necrotic. The outlines of the elements were indistinct, and the nuclei did not stain in logwood or other nuclear dyes. This necrotic layer was not of any great depth; beyond it there was a reactive inflammation, as shown by an infiltration of small round cells. In a few of the sections some of the blood-vessels of the valve were plugged by emboli composed of micrococci. This condition, however, was not general, and where it occurred no continuity could be traced between the micrococci in the vessels and those on the free surface.

In the discoloured patches in the myocardium the smaller vessels were found to be very extensively plugged with emboli composed of micrococci. In some places considerable tracts of vessels were completely filled, so as to resemble an anatomical injection. In the neighbourhood of these places the muscular fibres were granular, and their nuclei did not stain. There was hæmorrhage between the fibres, and an inflammatory infiltration of round cells was commonly present.

In the embolic patches in the spleen the small arteries were very extensively occupied by masses of micrococci. Large tracts were gorged with blood corpuscles, and stained diffusely with blood pigment. Here no nuclei could be stained by logwood.

The kidneys were in an early stage of chronic Bright's disease—the connective tissue throughout the organs was increased, and the epithelium of the tubes and the glomeruli showed degenerative changes. The Malpighian capillaries, and those between the tubes of the cortex, were very extensively plugged with micrococci, so that in places the kidney looked like an injected organ. In some cases the micrococci were found not only in the Malpighian vessels, but free in Bowman's capsule. Necrotic and inflammatory changes were marked in the neighbourhood of the plugged vessels.

In the lungs, hæmorrhage into the alveoli, with swelling, proliferation and desquamation of the alveolar epithelium, were the principal changes. In the sections examined no micrococci were found.

The section through one of the reddened patches in the ileum showed extensive hæmorrhage confined to the submucous coat. The vessels were filled with blood corpuscles, but in the sections no micrococci were seen.

Corresponding to the ulcers in the rectum there was a complete disappearance of glandular tissue. The floor of the ulcer was composed of a tissue of small round cells, resembling a granulation tissue.

In the discoloured patches in the liver the lobular capillaries were extensively occupied by plugs of micrococci; reactive changes about these were not well marked. Throughout the liver Glisson's capsule was infiltrated with round cells.

In this case the diagnosis was at first extremely doubtful, but became clear on the supervention of the mitral murmur, and still more so when the ecchymoses appeared in the skin.

The thickening of the mitral valve and of the chordæ tendineæ was evidently of old date, and originated probably in the attack of rheumatic fever, of which there was a history. The absence, however, of symptoms of cardiac disease, and the repeatedly confirmed absence of physical signs in the earlier period of the illness, show that the valve, although in an unhealthy condition, was competent to perform its functions in a satisfactory manner until the growth of the micrococci prevented its closure.

The statement made some years ago by Köster and Klebs, that all cases of acute endocarditis are of bacterial origin, has not been confirmed; and it would be inconceivable that in this case bacteria could have lain latent for a long time in the diseased valve, and have then suddenly taken on a rapid growth. It is, however, by no means easy to discover the door by which the parasite found an entrance into the body. The only probable portal was the pustule on the hand.

Recent experiments by Wyssokowitsch[a] are of great interest in connexion with this case. It had been found by previous experimenters that mechanical injury of the valves of the heart was well borne by rabbits, and was not usually followed by inflammation. It was also found that certain micro-organisms which occur in pus and in pyæmic and septic affections in men, could be injected into the blood of rabbits without effect, although some of them were fatal when introduced into the bodies of other animals. Wyssoko-

[a] Virchow's Arch., CIII. 301.

witsch found that when he injured the aortic valve by a sound passed from the carotid, and subsequently injected a pure culture of certain micrococci into the vein of the ear, he produced an inflammation of the valves which, in its anatomical characters and in the secondary changes it caused in other parts (embolism, necrosis, suppuration, &c.), resembled the cases of bacterial endocarditis observed in man. He obtained positive results with streptococcus pyogenes, staphylococcus pyogenes aureus, and the coccus sepsis of Nicolaier. Micrococcus tetragonus, bacillus pneumoniæ, and a bacterium accidentally introduced during the operation, failed to cause inflammation in the injured valve.

Ribbert[a] has produced endo- and myo- carditis resembling those seen in men by injecting staphylococcus pyogenes aureus into the vessels of rabbits without previous mechanical injury of the heart. In his experiments, however, the coccus was grown on potato, and the injection contained particles of the potato, which, no doubt, acted as mechanical irritants, and gave here the predisposition which was furnished in Wyssokowitsch's experiments by the direct injury, and which made it possible for the micro-organism to take root and develop.

Wyssokowitsch found that injections of cocci into the air-passages, or subcutaneusly, were inactive in causing endocarditis in injured valves; so that the entrance of the parasite into the human body is still, in most cases, surrounded by some obscurity. But the apparently well-established fact that, besides the entrance of the germ, a local predisposition is necessary in order for its growth, is of the greatest interest, and seems well borne out by the present case.

In the absence of culture experiments it is impossible to name positively the actual coccus in the present instance. In its appearance it resembled most streptococcus pyogenes. This is further made probable by the abundant growth and by the tendency to anæmic necrosis rather than to suppuration, for Wyssokowitsch found that these were peculiarities of streptococcus, while staphylococcus grows less luxuriantly, and causes intense reactive inflam-

[a] Fortschritte d. Medicin., 1886, I.

mation of the tissues, with suppuration. It is most probable that in cases of bacterial endocarditis in man the parasite is not always the same. In the present instance there can be no doubt that the main growth of the parasite took place on the mitral valve, and that the masses found in other parts were detached from this and carried to the different organs by the blood. The failure to find cocci in the intestine and lung is, of course, no proof of their absence in these parts. The recent hæmorrhagic spots in the brain were, no doubt, due to recent vascular disease, probably embolism. The necrotic patch over the temporo-sphenoidal lobe was of much older date, and it is not easy to state its exact nature. The very imperfect history which could be obtained threw no light on the matter.

The commencing endarteritis in the aorta, which is unusual in so young a person, may be due to the renal disease, which, together with the commencing hepatic cirrhosis, was probably attributable to the very intemperate habits of the patient.

ULCERATIVE ENDOCARDITIS LIMITED TO THE RIGHT SIDE OF THE HEART.

By WALTER G. SMITH, M.D.;
Physician to Sir P. Dun's Hospital.

[Read in the Pathological Section, Friday, March 12, 1886.]

The viscera I exhibit were taken from a man, aged forty-four years. He had served in India for thirteen years, and enjoyed good health, except for several attacks of ague during the last two years of his stay there. He was temperate. December 23rd, 1885, he was seized with rigors, attended with cough, dyspnœa, loss of appetite and disturbed sleep. He was admitted into Sir Patrick Dun's Hospital on December 29th, in a drowsy, apathetic condition, and presented the physical signs of pneumonia of the left lung. T. 103·2°; R. 30; P. 100. Urine contained a little albumen, was rich in urobilin, and deficient in chlorides at first. Heart's sounds normal. Convalescence from the acute attack apparently set in on the ninth day, but four days later the temperature rose to 104·6°, and evidence of a fresh pneumonic attack in the left lung was found. Then a few days subsequently pleuro-pneumonia of the right side declared itself, with abundant, tenacious, rusty sputum. Diarrhœa afterwards set in; he became extremely weak, and died quietly on Feb. 20th. Eleven days prior to death a systolic blowing murmur developed towards the apex of the heart and persisted, and the existence of an ulcerative endocarditis was conjectured.

Post mortem examination:—Liver, 71 oz., nutmeggy; spleen, 15 oz., almost diffluent; several red infarcts along thin edge; section speckled with numerous red and dark spots; kidneys pale, full size, and apparently healthy; several pints of fluid in right pleura; right lung, 38 oz.; left lung, 24 oz.; left pulmonary artery, at its bifurcation, blocked by a large yellow clot, intimately adherent to the wall of the vessel; numerous firm thrombi in the smaller branches of the pulmonary artery in each lung; several lumpy patches of consolidation in each lung, and one or two sharply-defined infarcts; no trace of pericarditis; heart, 13 oz.; left chambers healthy; aortic and pulmonary valves competent and normal in appearance; tricuspid valve extensively diseased—it was covered with enormous ($1\frac{1}{2}$ inch) cauliflower excrescences and vegetations, some hanging by a narrow pedicle close to the free edge of the valve; close to one curtain of the valve was a ragged cavity in the heart muscle, about half an inch in length; surface rough and uneven; a patch of granular exudation upon the endocardium of right ventricle; no disease of pulmonary artery; a number of small firm thrombi were entangled in the recesses of the musculi pectinati of right ventricle.

The case was obscure in its origin and clinical course, and the limitation of endocarditis to the right side of the heart is noteworthy.

ENDOCARDITIS WITH LOCALISED PERICARDITIS.

By C. J. NIXON, M.B., LL.D., Dubl.; F.R.U.I.;
Senior Physician to the Mater Misericordiæ Hospital.

[Read in the Pathological Section, March 12, 1886.]

The heart exhibited was taken from the body of a boy, aged twenty, who had been admitted into the Mater Misericordiæ Hospital on the 20th of December, 1885. Six months previously he had had an attack of rheumatic fever in which his heart was affected. Afterwards he complained of some difficulty of breathing, especially on exertion; but it was only a fortnight before his admission to hospital that serious symptoms presented themselves. He then got a rather sharp attack of hæmoptysis, and came to the hospital for treatment. He was remarkably anæmic; had considerable dyspnœa and orthopnœa; his urine was small in quantity and slightly albuminous; and he had general anasarca and a moderate amount of ascites. On examining the heart the usual signs of well-marked mitral regurgitation were noted. Occasionally, in addition, a feebly-pronounced presystolic murmur was heard. In the course of the case the symptoms that usually developed towards the late stages of mitral disease were present, the most marked symptom that called for treatment being sleeplessness. Towards the end the boy passed into a condition of drowsiness which merged into coma, and he died on the 30th of December, ten days after his admission.

The *post mortem* appearances found, as regards the heart, were as follows:—It was generally enlarged, weighing over 14 ounces. There were present well-marked evidences of mitral valvulitis of an extensive character. The anterior mitral segment was almost completely disconnected from its musculi papillares, and at the extremities of its ruptured chordæ tendineæ a precipitation of fibrine had taken place. At one place this precipitation was attended with calcification. On opening the left auricle it was found that its entire posterior aspect was covered by a lymphy exudation resembling in appearance coarse velvet pile or plush. This exudation, on microscopic examination, presented the appearance of ordinary embryonic tissue; there were no traces of micrococci in it. The auricular surface of the mitral valve had upon it some thickened nodules sufficiently large to explain the occasional presence of a direct mitral murmur. The right side of the heart was dilated, and the walls of the right ventricle somewhat hypertrophied. The condition of the epicardium found was peculiar. On the posterior wall of the right auricle, as far as the base of the auricular appendix, there were signs of pericarditis; very considerable roughness, thickening and opacity of the visceral layer of the serous membrane, but no traces of inflammation on the corresponding part of the parietal layer. The appearances noted were not like the usual milk spots so frequently met with on the anterior aspect of the heart; they were obviously the result of a limited pericarditis. I have noted this condition of pericarditis, with pasty exudation, strictly limited to the posterior aspect of the right auricle in cases of death occurring in the purulent infiltration stage of pneumonia; it is an almost constant condition in such cases. The patient had had before admission a severe attack of hæmoptysis, and this had recurred whilst under observation in hospital. The lungs, as had been anticipated, showed well-marked signs of hæmorrhagic infarction, and the branches of the pulmonary artery leading to the infarcts were found plugged.

The two special points of interest in the case to which I desire to direct attention are—1st, the evidences of localised pericarditis which existed; and, 2ndly, the remarkable immunity which, judging from the history obtained, the boy had from distressing symptoms until a short time before his death. I have been in the habit of teaching that the symptoms of chronic valvular disease of the heart are paroxysmal in character. A certain time elapses during which there is an immunity from distressing symptoms.

Then supervened a period when the patient complained of dyspnœa and orthopnœa, palpitation, sleeplessness, partial suppression of urine, and perhaps dropsy. These acute symptoms usually, in the early stages of the affection, subside, and the patient resumes his avocation under fairly good conditions of health. The signs of obstructed circulation again show themselves, and the former history is repeated. I think the most rational explanation of the paroxysmal character of the symptoms in chronic endocarditis is that at certain periods during the course of the disease attacks of acute endocarditis supervened upon the chronic valvular lesion, and that as long as the acute or subacute inflammation lasted, the symptoms also remained acute. How far any implication of the myocardium existed in such cases it is difficult to say. It is not unlikely that, as in acute peritonitis, the muscularis mucosæ was affected, so also the muscle of the heart was involved by direct contact with the inflamed epicardium. The influence which the inflammation exercised upon intra-cardiac nervous mechanism should not, of course, be overlooked.

RUPTURE OF A PELVIC CYST.

By CHARLES B. BALL, M.D., F.R.C.S.;
Surgeon to Sir P. Dun's Hospital; University Examiner in Surgery.

[Read in the Pathological Section, April 9, 1886.]

This specimen was removed from the body of an unmarried lady, aged sixty-eight years, who died shortly after the performance of a very trifling operation. She stated that at the time of her menopause a tumour formed in her abdomen, which was attended with considerable menorrhagia; but subsequently it ceased to grow or give her any inconvenience. A year ago I saw her for the first time for some trifling complaint, and she then directed my attention to this abdominal tumour, which, for twenty years, had not given her any uneasiness. It was at the middle line, very firm to the touch, and absolutely painless. I did not then consider that any interference with it was called for. About six weeks ago she consulted me for piles, from which there had been considerable bleeding. She had been operated on for them before; and I operated by clamping two or three small piles. For two days she went on as well as such cases usually do; and then, about forty hours after the operation, she was suddenly seized with sickness of the stomach, and a state of intense restlessness followed. Her abdomen was somewhat distended, but was not tender, and there was no very definite evidence of peritoneal mischief. The rectum had the appearance ordinarily presented after such operations, and there was no inflammation about the seat of the operation, or evidence that anything was going wrong there. The restlessness increased, and Professor Smith saw her with me on the evening of the same day; but she died in ten hours after she presented these symptoms, none of which was referable to the operation. I was enabled to make a *post mortem* examination, and was prepared to find a fibro-myoma attached to the uterus. The first part of that diagnosis turned out to be correct. The tumour was a fibro-myoma; but it was attached not to the uterus, but by a broad pedicle to the neighbourhood of the right ovary. Section of the tumour showed that it corresponded with ordinary uterine myomata, and microscopic examination confirmed that view as to its nature. Dr. Purser, who made the microscopic examination, told me that there was great difficulty in distinguishing between fibro-myomatous tumours and spindle-celled sarcomata; and if I had not had the benefit of Dr. Purser's opinion I should have concluded from the microscopic appearances that it was a spindle-celled sarcoma. The peritoneum was found to contain a considerable quantity of bloody fluid; but beyond a slight congestion of the vessels there was no evidence of peritonitis or adhesions to the viscera. Coming to the broad ligament on the same side as the tumour we found a ruptured and collapsed cyst, which, when distended, would have been about the size of a small orange. On the surface of the uterus were one or two small sub-peritoneal fibro-myomata, and penetrating into its cavity were some small tumours of a similar nature. The left broad ligament was occupied by a cyst, evidently similar to the one on the right side, which had ruptured. The most interesting feature about the cysts was the peculiar nature of their contents, which very closely resembled pasty fæces; and at first I thought the matter might have proceeded from a ruptured intestine, but that proved not to be the case. The material had no fæcal odour, and it filled the cysts in both broad ligaments. The rectum exhibited the remains of the piles, and these showed no evidence of inflammation, or thrombi in the hæmorrhoidal veins, beyond what one would expect to find in such a case. Consequently I concluded that death was due to the rupture of the cyst in the right broad ligament. The material which the cysts contained was semi-pultaceous, and proved on microscopic examination to be composed of large plates of cholesterin and fat granules. The ovary on the left side was well above the cyst and unconnected with it. The material in the cysts might be due to the formation of dermoid cysts, which occur sometimes in that region, or to an antecedent hæmorrhage which had become changed. The pathological evidence was in favour of the latter of these propositions, because I have examined a number of slides of the material, and in none of them is to be found anything that could be distinguished as epithelial *débris* of any kind. So that I conclude these tumours to be old extravasations of blood—pelvic hæmatoceles which have undergone change.

LARYNGEAL CHANGES SUBSEQUENT TO LARYNGO-TRACHEOTOMY.

By E. H. BENNETT, M.D., F.R.C.S.;
Surgeon to Sir P. Dun's Hospital; Professor of Surgery, Trinity College.

[Read in the Pathological Section, April 9, 1886.]

THIS specimen consists of the larynx and trachea of a man on whom I performed laryngo-tracheotomy fourteen years ago. During the building of the Spencer Dock this man, who was then between twenty-five and thirty years of age, was employed at the excavations. He had previously suffered from typhoid fever, from which he had had a tedious recovery; and pressed by the necessities of life, he undertook work sooner than he should have done. In a fortnight after leaving the Whitworth Hospital, where he had been treated for the fever, he was admitted into Sir Patrick Dun's Hospital suffering from extreme dyspnœa. His distress was so great that no detailed examination was possible; and I opened the air passage by an incision through the crico-thyroid membrane and down through the cricoid cartilage. Immediate relief followed, and a tracheal tube was introduced. It was an ordinary double tracheotomy tube, with a loop-shaped orifice cut in it to facilitate respiration through the larynx. Very soon the mucous membrane projected into this opening; and at each change of the tube a sort of shearing of the edges of the membrane took place, accompanied with hæmorrhage and pain. Consequently that tube was abandoned, and another one of more suitable pattern introduced. The injury to the mucous membrane occurred two or three times in consequence of the withdrawals of the inner tube. The man left the hospital cured; and during many years afterwards I constantly saw him. I used to observe him at cuttings in the street opposite my own house, which were made for the purpose of testing the gas works, and from which very peculiar odours were emitted. The man would sit smoking at the edge of the cutting; and he said it was immaterial to him what the odours were, because he breathed entirely through the tube, and consequently smelt nothing. One of his amusements consisted in trying to see how high he could spit through the tube which he wore in his trachea. During his convalescence after the operation a laryngeal examination was made, from which it was discovered that the dyspnœa had been caused by suppurative perichondritis of the larynx. All the tissues were swollen and infiltrated; and the laryngoscope showed that on the left-hand side of the vocal chords there was an opening, through which an abscess had penetrated into the larynx. No necrosis of any part of the larynx ensued. The point of interest was, why it was that in such cases as this, in which laryngeal operations were performed on adults for syphilitic ulceration or for any other affection, the tube had to be always worn afterwards. In several cases I have withdrawn the tube, and the result was that the opening contracted and I have had to perform a second tracheotomy and introduce a fresh tube. Did the laryngeal changes result from the mode of operation, or from what other cause? Looking at the present specimen I am inclined to think that the mode of the operation had something to do with it. The first tracheotomy I ever performed was a laryngo-tracheotomy with an incision in the crico-thyroid space. I maintain that Boyer's mode of performing the operation is the easiest and safest, and much to be preferred to the low operation. In this, as in all the cases of old tracheotomy, the thyroid, cricoid, and other cartilages of the larynx, were all ossified. The whole region of the cricoid was shrunk to a minimum, and the larynx was shrunk to the condition of a child's larynx. I am indebted to the President for having called my attention to observations made on this subject by Dr. Pilcher, of New York, and recorded in the *Annals of Anatomy and Surgery*. I believe the experience of every Dublin surgeon is that if tracheotomy is performed in an adult, and a tube kept in for so long as three or four weeks, it would have to remain in for the patient's life. As I have said, I have always performed the high operation, because I consider it the easiest, and have never done the low one except for the purpose of extracting a foreign body.

In the only case in which I ever divided the trachea below the isthmus for the purpose of extracting a foreign body, I found it necessary to pass up through the thyroid and through the cricoid cartilages, passing the knife between the vocal chords, and I took away the tube within three weeks after the operation. It took a fortnight to get the foreign body away, but after that I was able to let the opening heal up. The subject was a boy, six or seven years of age, whose mother lived at Milltown; and two years ago, happening to be in that neighbourhood, I saw the boy, who had grown to be a young man, and had entirely recovered. He suffered from none of the changes in question, and laboured under no defect of the larynx; so that these changes could not result from the mere section of the larynx, but must be due to the presence of the tube and the chronic irritation caused by it. Anyone looking at the trachea I now exhibit would see that it would have been impossible to remove the tube from it. I knew another case of a man who was able to go for three days without his tube, but at the end of that time he was always obliged to get it in.

RED GRANULAR KIDNEY.

By M. A. BOYD, F.R.C.S.;
Physician to the Mater Misericordiæ Hospital.

[Read in the Pathological Section, April 9, 1886.]

I EXHIBIT the kidneys of a boy, aged twenty, showing the peculiar granular changes associated with what Dr. Mahomed calls the red granular kidney. The case was admitted to the fever ward of the Mater Misericordiæ Hospital as one of typhoid, many of the symptoms of this fever being present—viz., vomiting, tympanitic abdomen, diarrhœa, delirium, and a dry brown tongue, with bronchitic râles over the chest. The temperature was not that of typhoid, being subnormal, and continuing so up to the time of his death, four days after his admission to hospital. The following history was obtained. For two months prior to admission he was suffering from chilliness, loss of appetite and memory, and occasional vomiting, a puffy, livid appearance of his face, and frequent bleedings from his nose, and total inability to attend his usual occupation, which was that of grocer's assistant. He had had scarlatina ten years previously, but no symptoms of kidney disease followed it, and his habits were temperate and his family history was good. No history of rheumatism.

In addition to the typhoid symptoms mentioned above, an examination of his chest after admission showed congestion of base of right lung, pain in the ankles and knees, with effusion into the right knee; hypertrophy of left heart, with full pulse, but no cardiac murmur; his face and eyes had a swollen, puffy, congested appearance, and he answered questions in a dull, stupid manner. He also complained of pain in the back, and in the left side of his head, and his kidneys had not acted for twenty-four hours before admission. A small quantity of urine drawn off by catheter showed albumen.

From the history and above symptoms case was considered one of uræmia. His drowsy condition gradually passed into coma, and he died four days after admission. No convulsions.

The *post mortem* showed the kidneys considerably reduced in size, the capsule peeling off with difficulty but not adhering, and the cortex showing granular and contracted through it. A section showed the area of the cortex very much narrowed, with numerous points of hæmorrhage scattered through it, and its tissue granular and light in colour in places. Microscopic sections exhibited showed the vascular changes in a marked degree.

LUNGS FROM A CASE OF PYO-PNEUMO-THORAX.

By WALLACE BEATTY, M.D.;
Assistant Physician to the Adelaide Hospital.

[Read in the Pathological Section, May 14, 1886.]

The specimens I exhibit this evening were taken from a case of pyo-pneumothorax, secondary to phthisis.

Case.—The patient, a young man, aged twenty, a gardener by occupation, was under my care in the Adelaide Hospital last July for nine days. He had at that time phthisical symptoms, and an examination of the chest revealed crepitation anteriorly below the clavicles. He was re-admitted on Oct. 27th, suffering from pneumothorax of the left side. He stated that ever since he left hospital in July he had been troubled more or less with cough, and that on Oct. 24th, four days before re-admission, in the morning, when in bed, a violent fit of coughing came on, as he was turning from his right side to his left, and he was seized with a severe pain below the left clavicle, causing a sensation as though his left lung was breaking in two. Shortly afterwards he fainted. Cough for the next few days caused much pain, so he was obliged to get his sides supported when he coughed.

On admission to hospital the characteristic signs of left pneumothorax were present—enlargement of the side, the heart displaced to the right, percussion note tympanitic, and amphoric phenomena. After a few weeks fluid was effused into lower part of pleural cavity, as was evidenced by percussion dulness at left base, and succussion splash. There were physical signs of excavation in the upper lobe of the right lung.

For a time the patient got on wonderfully well; he was able to go out a little on fine days, but for the last couple of months of his illness he was confined to bed, and died a slow and distressing death.

Dr. Bewley made a *post mortem* examination. On opening the abdomen, the left half of the diaphragm was found depressed, convex downwards and extending $1\frac{1}{2}$ inch below the lower border of left costal arch—it had doubled the left lobe of the liver upon itself. The right half of the diaphragm was in its normal position. Gas escaped from the left pleural cavity on its being opened; that cavity contained about five pints of odourless pus; the pleura was much thickened; the left lung was very small, was pressed up against its root, with exception of the upper part, anteriorly, where it was bound by pleural adhesions to the chest wall; the lung was carnified. At the inner surface, in its upper part, a circular opening was found, where obviously the perforation had originally taken place. This opening, or ulcer, was in diameter about the size of a sixpence, shallow in its posterior half, where a minute pin-hole perforation can be seen; in its anterior half it was $\frac{1}{4}$ inch deep, and subdivided by bands of lung tissue.

The upper lobe of the right lung presented numerous cavities, the lung tissue between the cavities being consolidated.

The only breathing surface the patient had was that of the lower lobe of the right lung.

The following points seem noteworthy:—

The long time that elapsed between the occurrence of the pneumothorax and death—viz., six months.

The fact that the pus in the pleural cavity was odourless.

The extraordinary extent to which the disease advanced in the right (the only active lung) before death took place.

PERICARDITIS.

By J. MAGEE FINNY, M.D. Univ. Dubl., F.K.Q.C.P.;
Vice-President and Censor, King and Queen's College of Physicians;
King's Professor Practice of Medicine, School of Physic.

[Read in the Pathological Section, May 14, 1886.]

The specimen of acute pericarditis which I exhibit is one very typical of the disease, which occurred in a case of commencing granular disease of the liver and kidneys. It was taken from the body of a man, aged fifty-two, addicted to alcoholic excesses, who died in Sir Patrick Dun's Hospital, May 12th, 1886. He came into hospital five days before death, and on admission was in a state of collapse, the extremities cold, pulseless, and the face, ears, &c., deeply cyanosed, with much distension of the jugulars.

From this state he rallied partially, but the coldness of the extremities and the cyanosis did not leave him, and the heart's sounds could barely be made out with the greatest care. The area of cardiac dulness was widened, but did not assume the usual triangular form of pericarditis with effusion, and did not extend above the third left intercostal space. No friction sound could be detected under any change of posture, nor was there any impulse to be felt over any part of the thorax. The difficulty of diagnosis was increased by the fact that neither over the aorta nor elsewhere could either sound of the heart be made out—a point of diagnostic value between pericardial effusion and a weak dilated heart.

In the present instance the pericarditis had passed into the stage of effusion before admission to hospital, and had occurred, so far as the history of the case could be depended upon, without any marked symptoms prior to the state of collapse.

An examination of the heart showed that the whole surface of the membrane was inflamed, and that bands of adhesive lymph were forming between the adjoining surfaces of the pericardium, while the greatest amount of lymph was behind and around the right auricle and the auricular appendix. The heart itself was covered with a thick coating of fat, particularly over the auricles and the front of the right ventricle, and on section the muscle of the heart was undergoing fatty metamorphosis. There were no evidences of valvular endocarditis on either side of the heart. The kidneys and liver were cirrhosed, and contraction was commencing. It would appear, therefore, that the case was one of fatty heart undergoing dilatation, which was further weakened by pericarditis with effusion; and the difficulty of diagnosis of the latter was due to (1) the absence of subjective symptoms, and (2) the fatty weakness of the muscle of the heart.

ZONULAR CATARACT AND DENTAL MALFORMATIONS.

By JOHN B. STORY, M.B., F.R.C.S.;
Surgeon to St. Mark's Ophthalmic Hospital, Dublin.

[Read in the Pathological Section, November 6, 1886]

It is more than twenty years since Professor Horner, of Zurich, first called attention to the connection between the occurrence of zonular cataract and certain dental deformities, which he attributed to the influence of infantile rickets. Prior to the publication of Horner's observations, Professor Arlt, of Vienna, had noticed that individuals with zonular cataract presented generally a history of infantile convulsions, and he based upon this fact a theory as to the etiology of this peculiar form of cataract—viz., that in the convulsions some solution of due physiological contact took place between the denser central portion and the more fluid peripheral fibres of the lens, and that this purely mechanical lesion manifested itself as a stationary zonular opacity. Horner also observed the frequency of infantile convulsions in these cases, but noticed in addition that these patients exhibited cranial deformities, defects in intelligence, and a peculiar malformation of the teeth, all of which defects he traced to the action of rickets. Of sixty-five cases observed by Arlt and Horner collectively, forty-eight possessed a history of infantile convulsions, and in Horner's thirty-six cases he noticed twenty-five with dental deformities, sixteen with malformed crania, and four with defective intellectual development.

The dental deformity in question must not be confounded with that ascribed by Hutchinson to congenital syphilis. It is, on the contrary, precisely similar to what Hutchinson has attributed to the use of mercury in infancy—Hutchinson in this matter also bringing in convulsions indirectly—the convulsions lead to the use of mercury, which, by setting up stomatitis, produces the dental abnormality. Horner's "rachitic teeth" are, as a rule, thicker and coarser than the normal; the neatly-formed incisor appears somewhat cubical and shapeless, though the shape can in many cases very closely approximate to the normal. The enamel, instead of gradually thinning away on the neck of the tooth, terminates abruptly in a swollen ridge. The delicate horizontal furrows to which the enamel owes its satin-like appearance becomes so enlarged as to be even visible to the naked eye, and sometimes, especially towards the cutting edge, a horizontal row of round holes marks the position of one of these excavated grooves. The body of the tooth terminates in a convex border at the cutting edge. The junction of the labial and lingual surfaces of the enamel runs as an irregular zigzag line over the surface of the tooth. Sometimes the enamel is quite absent in the grooves, the floor being formed by the discoloured dentine.

In corroboration of Horner's observations, the following nine cases of zonular cataract seem worth recording:[a]—

Case I.—Nannie B., aged nineteen. (No. 387, Nov., 1885.) Eyes always defective; had convulsions in infancy. Teeth with "rachitic" markings; not very characteristic on upper incisors, but typical on first molars; mouth and jaws very small, and curious abnormally shaped head. The eyes are myopic, $V = \frac{6}{24}$ with each eye and Jæger No. 1. The central portion of both lenses occupied by typical zonular cataracts, that in the right eye somewhat larger than in the left, but both blocking up the pupils, except when the illumination is pretty dull. There is a second faint but distinct zone of opacity in each lens outside the principal cataracts.

Case II.—Margaret O'C., aged fifteen. (Nov. 388, Nov., 1885.) Eyes bad as long as she can remember; had convulsions badly when a child. Teeth markedly "rachitic." Right eye, $V = \frac{6}{24}$ and Jg. 8. Left, $V = \frac{6}{36}$ and J. 16. Zonular cataracts in both eyes.

Case III.—Bernard C., aged twenty-one. (No. 565, Mar., 1886.) Sight defective "ten years." Never had convulsions; had whooping-

[a] Four of the cases were exhibited at the meeting on November 6, 1885. Three others only came under observation subsequent to the writing of this paper.

cough and measles at age of two, and scarlatina and smallpox at about age of four. No dental peculiarity visible. Right eye, V=fingers at 6m. and Jg. 18. Left eye, $V=\frac{6}{36}$ and Jg. 16. The centres of both lenses occupied by dense opacities, which seem to be nuclear and not zonular, for no illumination can be got through them, and their density seems to be greatest in the polar axis. Outside these nuclear cataracts, however, there is in each eye an irregularly shaped zone of fainter opacity, which must be regarded as a distinctly lamellar cataract.

Case IV.—Hugh M., aged thirteen. (No. 156, Jan., 1885.) Failing sight for many years; speaks quite unintelligibly, and so appears deficient in intelligence. Had convulsions at age of five months. For seven months was so bad with "spurious" croup that his life was despaired of. Teeth markedly "rachitic." Double zonular cataracts. Only counts fingers with each eye.

Case V.—Mary M., aged thirty-three. (No. 4,481, Feb., 1886.) Sight failing two years; sees better in a dull light. Incisors normal, all first molars carious beyond examination. Right, $V=\frac{6}{60}$ and Jg. 16. Left, $\frac{6}{60}$ and Jg. 16. Zonular cataract in each eye.

Case VI.—Clara S., aged twenty-three. (No. 3,100, Nov., 1882.) Married, has two children; suffers from pains in forehead all her life "like a knife scraping the bone;" latterly it has come into the eyes, and she got glasses four weeks ago, which have made a marked improvement in the eyes. $V=\frac{6}{24}$ in both eyes. Right with -1 D V=a few letters of $\frac{6}{6}$. A zone of dotted opacities in both lenses about half way between the centres and the equators, not a typical case of lamellar cataract, but to some extent an approximation thereto. Teeth normal.

Case VII.—William R., aged four. (No. 3,141, Dec., 1882.) Always healthy; never had convulsions, but had whooping-cough and measles. Mother has had two still-born and two living children. Right eye, well-marked zonular cataract. Left, a faint haze of the whole lens, but no definite zonular band visible. No note of teeth.

Case VIII.—Wm. L., aged seventeen. Always been short-sighted. $V < \frac{5}{50}$ with -11 $D=\frac{5}{30}$ right and left. Both eyes read Wecker No. 2. Had convulsions as an infant one and a half years old; none of the rest of the family had them. So-called "rickety" teeth beautifully represented. An elder brother of Mr. L.'s had also the "rachitic" teeth typically shown. No definite opacity could be seen in his lenses, and he had not had convulsions as a child.

Case IX.—Michael C., aged twenty-four. (No. 107, June 4th, 1886.) Sight always defective; patient always healthy; has a well-shaped head. All the first molars typically "rachitic;" none of the other teeth abnormal. Right eye, $V = \frac{6}{18}$ and Jg. 1. Left, $V = \frac{6}{18}$ and Jg. 1. In both eyes rather small central cataracts of the zonular variety, but not typical examples. Both cataracts are triangular rather than circular in shape, and have neither the peripheral dull line nor the less degree of saturation in their centre that are usually observed in zonular cataracts. In the left, however, there can be seen a second nuclear opacity inside the fainter more peripheral opacity, so that the latter must be regarded as distinctly lamellar. Refraction not noted, but fundus recorded as exhibiting "myopic crescents."

In these nine cases the condition of the teeth is noted eight times, and in only two of these cases (Nos. 3 and 6) were the teeth normal. In the other six the "rachitic" deformity was always present except in one (Case V.), when the incisors were normal, and the first molars, the teeth commonly most affected, too much decayed to be worth examination.

While in accordance with Prof. Horner's terminology the epithet "rachitic" is applied to the teeth in these cases, it is by no means to be understood that this deformity is in all cases produced by rachitis, nor even that rachitis is the commonest cause. All the evidence is in favour of the view that the dental defect is due to an arrest of development of the teeth, and it must be admitted that such an arrest of development could be produced by many other conditions besides rickets. Any constitutional or local lesion affecting the nutrition of a tooth during the period of growth of the enamel organ may conceivably produce the characteristic defect. I say the enamel organ because, although I learn from my friend Dr. Stack that the dentine is also implicated, the typical defect is to be found in the enamel. Now the period of growth of the enamel organ of any tooth is of only limited duration, and we may fairly conjecture that in time further observations will enable our odontological confrères to determine what are the conditions which result in producing this peculiar defect in the enamel. Tomes, quoting Majitot, places the growth of the enamel organ between the sixth month ($6\frac{1}{2}$) and the ninth. An accurate life-history of the child during these months should enable us to ascertain what are the possible causes of the dental defect.

It is, of course, very uncertain that the defect in the lens is due to the same cause as the defect in the teeth. The great frequency of the so-called "rachitic" teeth, and the exceeding rarity of zonular cataracts (unless we conclude that all or nearly all juvenile cataracts are in their commencement zonular) render this conclusion on the face of it improbable; but the evidence in the case of the lens points to a temporary interference with its growth as the cause of the cataract, and there is such a close similarity between the lens and the dental enamel in their origin and development that it is natural to expect that they will be affected by the same pathological agents. Both are formed by an involution of the epithelium from the surface of the embryo, and in both the growth proceeds from the deeper layer of this epithelium when separated from the rest of the epiblast by the interposition of a mesoblastic layer. In one respect alone does there seem to be a noteworthy difference between them. The enamel organ has only a brief functional existence. It forms the enamel, and after a few months disappears as a functional organ, but the growth of the lens proceeds during many years, if not during the whole period of life.

SUB-SECTION OF STATE MEDICINE.

THE DUTIES OF MEDICAL OFFICERS OF HEALTH IN ENGLAND AND IRELAND CONTRASTED.

By D. EDGAR FLINN, F.R.C.S.;

Surgeon, St. Michael's Hospital, Kingstown;
Late Medical Officer of Health, Brownhills, East Riding Staffordshire Urban Authority.

[Read in the Sub-Section of State Medicine, February 4, 1886.]

I PROPOSE in the short paper I have the honour to bring before this Sub-Section to-night, to speak of the difference there exists in the duties of medical officers of health in England and Ireland, and to lay before you some points as to the administration of the Sanitary Acts in Ireland; likewise briefly to show the disadvantages that the Irish health officer labours under in the discharge of his duties.

The working of the Public Health Act in Ireland has, in great measure, well-nigh become a dead letter, and one of the principal causes of the slow progress of sanitary reform in this country is, that there are no local boards nor combinations of districts. The Boards of Guardians are the sanitary authorities for the Irish rural districts, and are, as a rule, generally opposed to any measure of sanitary reform in the mildest acceptation of the term. They are careful never to move in any measure of sanitary work that will cause any outlay, and are thankful to their medical officers of health who do not trouble them with verbose reports.

A noticeable fact that has impressed me is, that seldom if ever the reports of medical officers of health appear in the public prints in Ireland. They seem to be carefully put aside and ignored, and consequently the inhabitants of a particular town or district are kept from day to day, and year to year, in total ignorance of the state of the health, mortality, and other vital statistics of the immediate district wherein they reside. The reports of the medical officers of health are regarded as documents of a mere formal nature, that hardly deserve a place on the agenda, and require no special notice or comment from the sanitary authority, though the medical officer may have reported an alarmingly high death-rate, that infectious diseases have been prevalent, that some of the local courts and laneways are in a filthy, unwholesome condition, and that there are overcrowded tenement houses. These little items have no charm for the Irish Poor Law Guardian—he is of opinion that they merely concern the enthusiastic scientist. A medical officer of health may report that a certain defined portion of his district requires sewering; his attention may likewise have been drawn to nuisances that exist which are dangerous to health, and it is very possible that the parties who cause these nuisances, and whose property is liable to be taxed with the cost of constructing sewers and making sanitary improvements, are members of the sanitary board, and the medical officer is, as has been frequently the case, reproved for attempting to perform the duties appertaining to his office. In England the recognised primary duty of the urban or rural sanitary authority is the health of the people. The report of the medical officer of health is an all-important document. It is duly discussed, and the suggestions offered for the improvement of the health of the district are promptly acted upon.

It is impossible that the Public Health Act in Ireland can be carried out satisfactorily under the existing system. In Ireland the dispensary medical officers are the medical officers of health. They receive a miserable salary of ten or fifteen pounds a year. These gentlemen are generally the medical attendants of members of the Boards of Guardians, who very probably are owners of property in their respective districts, and it is obvious that sanitary work is neglected. Again, how could this neglect be otherwise when we regard the pay of the medical officer? Can it be expected that for such a paltry stipend he is to devote his attention and labour to sanitary work? The fact is apparent that the Local Government Board in Ireland, in sanctioning the payment to Poor Law medical officers of such inadequate salaries, are palpably hindering the efficient administration of the Public Health Act. In England one of the main reasons why the Public Health Act is so efficiently carried out is due to the fact that a considerable majority of districts are combined, able to command the services of a medical officer of health who does not practice his profession, whose services are availed of at a fair remuneration, and whose duties are strictly limited to sanitary work. This official is consequently unfettered and independent of the public around him, and can discharge his duties without fear or favour.

The union medical officer of health in Ireland is, on the contrary, most inadequately remunerated; his interests and his duties are in direct antagonism to each other; it is simply impossible for him to engage in an independent discharge of his duties—by so doing he is keenly alive to the fact that he would gravely offend some of the best and most influential of his *clientèle*—and thus the administration of the duties of officers of health are neglected, and as a consequence we see around us the slow progress of sanitary reform in Ireland, and this condition of things will exist so long as the union medical officers are *ex-officio* medical officers of health.

There must be a clean sweep of this *ex-officio* system in our Irish sanitary administration, and each union should appoint one or two medical officers of health specially trained in sanitary work at a fairly remunerative salary, who would devote all their time to the duties of the office, and render them independent of private practice.

The formation of county boards in Ireland, which probably may be in the near future, will, to an extent, become an impetus to the progress of health legislation. The class of men who might be called upon to administer the laws, sanitary or otherwise, would be an improvement on the present Boards of Guardians, who systematically hinder and obstruct medical officers of health when desirous of carrying out any work of sanitary reform, and who dislike nothing so much as independence, ability, and zeal in their medical officers.

In Ireland, again, the officers of health are without any definite rule for obtaining available knowledge of prevailing sickness, and frequently cases of infectious disease occur in their districts without their knowing of its existence, and they are not informed (universally at least) of the deaths as they occur.

The importance of the early notification of cases of infectious diseases to the health officer is of the very highest importance, and England is not much better off in this regard than Ireland. Efforts have been made from time to time to induce successive Governments to take up this subject, but without avail. The local authorities of some towns in England have moved in the matter—for example, Bury, Bolton, Northampton, Nottingham, Leicester—but with comparatively little success. It was sought to compel medical men to report cases of infectious diseases occurring in their own practice to the sanitary authority. At Bury the attempt was a failure. At Bolton the health authorities succeeded, under an Improvement Act, in attaching a fine of ten pounds to the failure of medical men to report cases of infectious disease occurring in their practice. At Leicester there was strong opposition to the compulsory notification of infectious disease. Undoubtedly the proper person to make the notification is either the householder or the nearest relative attendant on the patient, but considering the importance of early information to the protection of the community and the saving of life, and possible prevention of epidemics, it seems reasonable to rely to some extent also upon the services of medical practitioners generally. I have made this little digression in order to court the opinions of the Members of the Academy as to the method and means by which the medical officer of health should be assisted in the detection and isolation of cases of infectious disease.

The duties of health officers in England contrast with advantage when compared with the duties of health officers in Ireland, and, as a rule, the medical officer in England finds comparatively little difficulty in carrying out the various duties of his office; his reports are always read and carefully considered; his urban or rural sanitary authority regards his monthly communications as important and valuable records, and whatever suggestions he may recommend for the improvement of the health of his district are, as a general rule, acted upon; his reports are not misinterpreted and misconstrued; he has no fear that his monthly or other communications to his board may bring down the wrath of the squire, or embitter the farmer and shopkeeper against him; his position and remuneration make him independent, and consequently he carries out the duties of his office without fear or favour. How fares the Irish health officer, or, rather, the Irish dispensary medical officer of health? His reports (which he takes special care not to make very frequently, being fully aware that over-zealousness might make his position embarrassing and untenable) are regarded as a mere formality—as documents that are of no importance, that contain nothing worthy of remark or notice. These reports are very frequently not even read, and the clerk's waste-paper basket is most probably their resting-place.

It is a disheartening prospect to a medical officer of health in Ireland, who takes an interest in his work, to see that time after time his reports are unheeded, marked as read without, perhaps, being even opened, and yet he is aware that nuisances dangerous to health exist in the very midst of his district; that infectious diseases are prevalent. So far as I have been able to ascertain, it is the universal opinion of the officers of health themselves that the Public Health Acts are not being carried out, and in some districts not even any attempt is being made to carry them out.

In Ireland the working of the Public Health Act seems to be encumbered by a plurality of executive officers, whose duties appear not to be very clearly defined. First, we have an official termed a sub-sanitary officer; next, an executive sanitary officer; then, a medical officer of health; and finally, a mighty functionary in the person of a superintendent medical officer of health.

There seems to be a difficulty in exactly defining what are the special duties of these particular officers. Sir C. Cameron, in speaking at the Sanitary Congress in Dublin in 1884, stated, in reference to these appointments under the Irish Public Health Act, that he knew one gentleman who bore the high-sounding title of "Superintendent Medical Officer of Health," but had no one to superintend, and he had been promoted from the position of "Consulting Medical Officer of Health," which office also carried with it no duties. This gentleman had made no reports since his promotion, because his reports as consulting medical officer were never heeded nor attended to.

In England there is no such plurality of officers, or such ponderous machinery required to set the sanitary wheel in motion, and carry out the provisions of the Public Health Act. There a whole county is under the supervision of two, three, or at most four medical officers of health, who, with the aid of the inspectors of nuisances in each district, are found fully equal to the task of working the details of sanitary legislation, and that in a thoroughly practical and efficient manner.

In Ireland each union has its own set of medical officers of health, with a superintendent, and a whole myriad of sub-sanitary and executive sanitary officers. Take one county alone—Kildare. In that county there are no less than nineteen medical officers of health, with three consulting medical officers, making in all twenty-two medical gentlemen empowered to carry out the provisions of the Public Health Act. Is it any wonder that the Act, as carried out in Ireland, is a complete failure? Having held the appointment of medical officer of health in a district in Staffordshire comprising a wide area, opportunities have been afforded me of knowing how the Public Health Act should be worked, and I trust I may be excused when I state that I regard the administration of the Public Health Act in Ireland as sadly deficient. There seem to be no method, no defined line of action, no interest taken in the sanitary condition of the country. Of course I except Dublin and Belfast, where of late years the Act is being vigorously and effectively carried out.

It is yearly becoming more evident that the Irish Local Government Board will have seriously to consider the question of districts and unions being combined, for the better administration of the duties of medical officers of health, for it is impossible that the Public Health Act can be efficiently worked under the existing system. In lieu of there being a whole company of medical officers of health in each district, with a consulting officer, there should be one, or at most two, who should be men free from the cares and anxieties of practice, with special sanitary knowledge, and whose pecuniary interests would not be perpetually at war with a fearless discharge of their duties. The Local Government Board of Ireland would be doing good and useful work were it to give its attention to this amalgamation of districts. It has been found practicable and to work very well in England, and why should not the same condition hold good in Ireland? There might possibly be a difficulty in carrying out such a scheme in Ireland, on account of the smaller rateable value; but I contend it is worth the trial, and any scheme would be an improvement that would provide for medical officers of health being independent of the prejudices and whims of local bodies, and that would limit the duties in each union to one or two officials, who could give all their time and attention to the duties of their office. Then, possibly, we might have some marked improvement in the sanitary condition of our Irish country towns and districts, and some interest thrown into the working of the Public Health Act.

Improvement is necessary, and the sooner it comes the better for the status of the Irish medical officer of health. It may be in the early future, but it must come some day, and dispensary medical officers must either be relieved of their functions as medical officers of health, or they must receive adequate remuneration for the important health duties they are expected to perform.

As year after year rolls by, improvements of a social and sanitary nature demand a supply of skilled knowledge for their most effective administration, and the Irish medical officer of health must don his garb of labour, and must not remain in the background of sanitary inefficiency and *inertia*.

ON SOME CONSIDERATIONS IN REFERENCE TO THE SANITARY CONDITION OF THE DUBLIN DAIRY YARDS.

By C. F. MOORE, M.D., F.R.C.S.;
Medical Officer No. 3 Dispensary, South Dublin Union.

[Read in the Sub-Section of State Medicine, April 8, 1886.]

In acceding to a request that I should bring something bearing on the subject of Public Health before this Section of the Academy of Medicine in Ireland, I have endeavoured to show the need for further improvement in the state of our Dublin dairy yards in this paper.

The domestic ox—bos tauros—has been recognised as most important to man even prior to the Flood. As Youatt pointed out, sheep and oxen were presented by Pharoah to Abraham one hundred and eighty years before the horse was mentioned.

The intimacy, so to speak, between the natives of many parts of Africa and the ox is well known to travellers. They act as able servants and protectors to the Caffre; and many curious instances are known of the sagacity and usefulness in the United Kingdom of the ox; while the buffalo and too often the cattle of our own country have shown themselves most formidable foes.

The great importance of maintaining, in a state of health and cleanliness, the cattle on whom we and our children depend so largely for meat and butter and milk, must be self-evident. It is, however, but too apparent that our cattle, especially in cities, towns, and their suburbs, are often so badly kept as to be productive of injurious effects on the health of those who depend so much on them for food. The records of pleuro-pneumonia, as well as of other diseases to which cattle are subject, show how the animals themselves suffer from the condition of their surroundings.

Dr. Littlejohn, speaking of the cowsheds or byres, as they are called in Edinburgh, mentions the following as points to which he paid special attention when examining them:—

1st. Their proximity to human dwellings.

2nd. Their condition as to cleanliness.

3rd. Their overcrowding and want of ventilation; and

4th. The state of the court where the manure is accumulated.

In comparison with the country byres or cowsheds, the eminent medical officer of health of Edinburgh observes that those situated in a crowded city district are veritable nuisances—this is specially the case when near dwellings—and he adds that, wherever situated, much depends on the manner in which the byre is cleaned, whether thoroughly or otherwise.

Dr. Littlejohn goes on to point out that the class of men employed in dairies in Edinburgh (at least some years since) are not remarkable for care and cleanliness in the way they kept the dairies, and we cannot report more favourably of the same class in Dublin. He further observes that the dairyman in the modern Athens, instead of invigorating his stock by dryness, cleanliness, and judicious ventilation, and thus warding off disease, flies to the opposite extreme, and actually creates the danger he is seeking to avoid—he refers to keeping the cattle warm in ill-ventilated byres.

The chief public complaints, he adds, that arise as to byres, refer principally to the manure heaps which are to be found, in the case of stable lanes, immediately in front, or, where the byre is detached, in a vacant space or court. As regards cows there is a large quantity of liquid refuse, and this along with the washings of the byres speedily accumulates, and where the paving is imperfect and the drains are not properly constructed, the whole area of the court becomes saturated and cannot be kept in a cleanly state. In dry weather the smell is offensive, but at all times we have the manure fermenting in receptacles generally of a faulty construction, either too small for the requirements of the place or with such defective fall that the drainage is inoperative. It is said, continues Dr. Littlejohn, that the quality of the manure is thereby improved, and

I have been informed on good authority that dairymen have actually obstructed the drains so as to prevent the escape of the liquid refuse. This may be quite allowable in the country, where the population is thinly scattered, but in densely-peopled localities it should not be tolerated. The Report goes on to point out that landlords should not let premises for the purposes of a dairy unless suitable provision could be made for sufficient accommodation as to room, drainage, water-supply, cleanliness, &c. The Report also points out the injurious effects of such a state of things in the frequency of the occurrence of pleuro-pneumonia in the dairies of Edinburgh twenty years ago.

Reference is made by the above-named writer to the early action of the authorities of Paris in the matter of cowhouses, which have there been long since placed under police inspection in the third class of insalubrious occupations.

Finally, the author above quoted advised the registration of all byres in the parliamentary district of Edinburgh, with their dimensions, the number of cows being determined accordingly; also the regular inspection of the byres, with a view to compel their proper paving, and provision of suitable receptacles for manure, and the weekly cleansing of the latter; proper water-supply; and also the notification of each death occurring in a byre, with inquiry as to the cause and the disposal of the carcase, with a record of same; also an inspection of all butcher meat.

The scope of this paper precludes my entering on the important matter of pleuro-pneumonia carcase meat—the too common result of keeping animals in ill-ventilated, dirty sheds. This matter is, however, treated at the length it deserves in the able report on Edinburgh, which I have quoted above.

Scotland.—The Fortieth Annual Report of the Board of Supervision for the Relief of the Poor and of Public Health in Scotland (1884–85) contains an Order in Council relating to dairies, cowsheds, and milk shops, dated 19th June, 1885, the purport of which is very nearly exactly the same as that quoted subsequently in this paper as in force in Ireland. The orders in force in Scotland are, however, more explicit in some important particulars than those in force in the South Dublin Union. Thus, paragraph 10 in the former order gives very stringent instructions for the prevention of any communication or ventilation into or between any water-closet or other sanitary arrangement, and any dairy or any room used as a milk store or milk shop.

Under the heading of "Construction and Water-supply of New Dairies and Cowsheds," the authorities in Scotland require one month's notice before the occupation of any building for such purposes, and they require very ample provision for the lighting, ventilation, air-space, cleansing, drainage, and water-supply of the same during its occupation as a dairy or cowshed. The same requirements are also made in respect of all dairies and cowsheds occupied at the commencement of this order.

It is unnecessary, I think, to advise the shutting up of all cattle-yards in confined localities—a matter which, during much of the year, would not be attended with difficulty or hardship—and if their permanent removal out of the city could not be effected for some time, arrangements in accordance with modern agricultural and hygienic progress could be effected in such yards as would not be considered, for the present at all events, very ill-situated.

I have to express my thanks to the Public Health Department of the Corporation for enabling me to see the very excellent provisions of the Contagious Diseases (Animals) Act, 1878, as well as for a copy of the Regulations of the Guardians of the South Dublin Union in reference to the Act just cited, and the several orders of the Privy Council, Ireland, of 1879, '80, and '83, on the subject of dairies, cowsheds, and milk shops, which I shall now quote:—

Cleansing of Milk Stores, Milk Shops, and Milk Vessels.—Contamination of Milk.—Par. 18. "It shall not be lawful for any person following the trade of cow-keeper, dairyman, or purveyor of milk, to allow the milk-vessels in such dairy or cow-keeping premises to be used for any purpose whatsoever which is incompatible with the proper preservation and cleanliness of the milk-vessels, nor in any manner likely to cause contamination of the milk therein."

Par. 19. "The following are the particulars required to be registered under Regulation 8:—

"1st. Name of owner.

"2nd. Address or residence of owner.

"3rd. Locality in which dairy, cowshed, or building is situate.

"4th. Nature of trade or business; whether cow-keeper, dairyman, or purveyor of milk.

"5th. Number of animals in cowshed, yard, or building.

"6th. Dimensions of cowshed, yard, or building.

"7th. Description of ventilation of cowshed or building.

"8th. Whether water supply is good or sufficient.

"By order,

"THOMAS PHELAN,

"Clerk of the Local Authority."

"Notice is hereby given, that legal proceedings will be taken against all persons carrying on the trade or business as herein defined who shall be found unregistered after one month from the date of this notice, or for any act in contravention of the provisions of the 41st and 42nd Vic. cap. 74, or of any Order in Council, or of a Regulation of this Local Authority, to recover the penalty under the statute, not to exceed £20, or if the offence is committed with respect to more than four animals, to a penalty not exceeding £5 for each animal.

"By order,

"THOMAS PHELAN,

"Clerk of the Local Authority.

"Dated this 5th day of June, 1884."

The Regulations oblige every cowshed to be so constructed as to be at all times well lighted and ventilated, properly and adequately supplied with water, the floor concreted, asphalted, flagged, or bricked, set in cement, and drained, &c. Also each animal to have not less than six by three superficial feet, exclusive of channel passage, crib, trough, and manger.

If the cowshed be not open throughout the entire length of one of its two longer sides, it shall be so constructed as to give each animal a cubic space of not less than 400 cubic feet for the purpose of ventilation.

Then follows in the Regulations—

Due provision for the proper storing of grains when used for feeding cattle in dairy-yards or cowsheds, for daily cleansing, and limewashing twice yearly, or oftener, as deemed requisite by the Inspector, the limewash to have mixed with it one pint of carbolic acid to five gallons of limewash; also for the registration of cow-keepers, dairymen, or purveyors of milk, and their business premises.

It is also ordered that no pigs are to be kept, or reared in, or allowed into, any cowshed or dairy-yard.

Also that any sick animal shall be speedily removed from the healthy cattle, and reported to the police.

Moreover, the shed which may have been occupied by such diseased animal shall be thoroughly cleansed, scraped, and swept, and disinfected with a solution of carbolic acid and water, or chloride of lime.

The dairy inspector or other officer of the Local Authority is authorised to visit and report on the state of any lands, dairy, cowshed, milk store, or milk shop, or other place or building to which the regulations apply.

Paragraph 18, treating of the subject of the contamination of milk, has been already quoted above; the regulations under this heading also provide that if, at any time, disease exists among cattle in a dairy or cowshed, or other building or place, the milk of a diseased cow shall not be mixed with other milk, and shall not be sold or used as human food, and shall not be used or sold as food of swine or other animals, unless and until it has been boiled.

It should not be forgotten that all ailing cattle are to be removed with "all practicable speed" from the healthy, as enjoined in the regulations quoted elsewhere.

On the occurrence of any infectious disease in or amongst the family of any cow-keeper, dairyman, or purveyor of milk, or in or amongst any of the attendants engaged in or upon the premises licensed for the sale of milk, the said premises shall be immediately closed, and notice of the disease shall be forthwith given to the Clerk of the Local Authority by the cow-keeper, dairyman, or purveyor of milk, or other occupier of the premises, and the said premises shall be closed against the sale of milk therein until the Local Authority shall by order otherwise determine. It is also ordered that these rules shall apply to any "milker" or any other person employed in the dairy, or in the distribution of the milk as assistant, &c.

I shall now read extracts from the morning paper, the *Daily Express*, of the 5th and 6th of the present month :—

"The first business in the House of Commons at Tuesday's sitting will be Mr. Duckham's motion as to the Cattle Diseases Act. Mr. Duckham and other agricultural experts are convinced that disease is spread in England by cattle brought from Dublin, and his proposal is that, in order to stamp out pleuro-pneumonia, in the general interests of the nation at large, it is necessary that the permissive powers given under clause 21, sub-section 2, of the Contagious Diseases (Animals) Act of 1878 should be rendered compulsory, and that the Act should be further amended, in order that the delegation of powers to the local authorities by the Privy Council might be so regulated as to promote a greater uniformity in the orders for the movement of animals, and in giving effect to the general provisions of the Act."

"Pleuro-pneumonia.—The official return of pleuro-pneumonia amongst cattle in Ireland, for the 27th ult., shows that twenty-five animals became affected with the disease, and were slaughtered. Twenty-four of the cases occurred in Dublin, as usual, and for the first time one case appears in Connaught, at Carrick-on-Shannon."

The official returns show that, during the week ending 13th March, 41 cattle in Ireland became affected with pleuro-pneumonia, and that they were all slaughtered. Dublin and its vicinity continue to form the home of the disease, the rest of Ireland remaining generally quite free from the disorder. Seven of the cases occurred in the North Dublin Union, and 34 in the South Dublin Union. No effort apparently is made to stamp out the disease.—(Dublin Morning Paper, 22nd March, 1886.) Again, on the 26th March, we read in the Report of the Royal Agricultural Society of the previous day, "the question of the existence of pleuro-pneumonia, which is certified in the *Dublin Gazette* by Government statistics once a week for years past in full detail as existing in and about Dublin, was the subject of a resolution, which was passed on the motion of Mr. James Robertson, to ask the Lord Lieutenant to inquire if the disease does really exist, and, if it exists, to have it stamped out, as it is reported to be injuring Scotch cattle." It occurs to me that the loss of so many Irish cattle, and of so much Irish rates to be expended in the shape of compensation to the owners of the slaughtered cattle, and extracted from the pockets of the ratepayers, are not less important matters.

Few of my hearers will doubt the connection between manure collections and sickness; it may, however, be well to point out corroborative evidence on the point. The *British Medical Journal* of March 20th last mentions an article, published in the *Lyon Médical*, in which M. Ferrand traces some relation between manure heaps and epidemics of diphtheria—a disease more frequent in rural districts than in towns and cities. Statistics in Scotland and Prussia show that the rate of mortality from diphtheria is highest in rural districts. In Lyons, the outskirts and surrounding country suffer most. Manure heaps do not exist in the urban districts, but are plentiful in the suburbs and adjacent country.

These heaps consist of various obnoxious and infectious kinds of residue. Klebs, of Zurich, has observed the deadly influence of these manure heaps. He states that diphtheria, on one occasion, appeared on the day following a general street-cleaning.

It may be safely concluded that the accumulation of dirt and refuse, known as manure heaps, are formidable factors in the etiology of diseases among rural populations. M. Ferrand urges that the authorities in agricultural districts should enlighten the peasants on this subject. Manure should, he adds, be kept in closed wells made of stone and glazed with bitumen, so constructed that all fluid filters away from the solid matter. Dr. W. E. Steavenson, in London, corroborates the above from his experience of the frequency of diphtheria in the children of families who live in rooms over the stables in the great Metropolis. Happily, in Dublin, we do not see much of diphtheria, but of other diseases we have a very serious amount, and in very many instances I have traced the connection between disease of various forms from debility, loss of appetite, general malaise, proneness to catarrhs and bronchitis, to pneumonia, and zymotic diseases, and derangements of the alimentary canal to proximity to manure heaps, as well as to foul sewers, filth-sodden premises, &c.

It is by no means uncommon to find a manure heap placed in an angle of a yard, one or more of the walls enclosing the manure being those of a dwelling-house, or cattle shed, or stable, or abutting on a street or lane.

If circumstances permitted, it would be much better altogether to avoid keeping cattle in towns of any considerable size, and I think Dublin, which is so conveniently situated as to suitable farm land on most sides, could easily dispense with the housing of cattle within the borough boundary.

This, in my judgment, is a matter which requires very great consideration at the hands of the Imperial Parliament.

Indeed, I would hail with much satisfaction an effort on the part of our Irish members of Parliament in this direction, and it reflects upon us as a nation when I find English and Scotch members drawing public and Parliamentary attention to our city and suburbs as the centre whence spreads throughout the United Kingdom the disease so well known as pleuro-pneumonia.

I regret to say that I am not surprised to find that our city has attained to such unenviable notoriety.

When I walk through certain parts of it, evidences of an unmistakable nature meet the eye, of a very unwise and very injurious state of things. I am, however, glad to say that some improvement has been effected in parts of Dublin, but much still remains to be done.

If you ask shortly for a statement of the means of preventing cattle disease and of promoting what is, in my judgment, the complement of it, better health of the people, I would say in a word—Cleanliness.

The late Lord Palmerston used to say there was no such thing as dirt, it was only matter in a wrong place, and in this every observer must agree; so long as the present state of existence continues, matter will accumulate if not speedily removed to the right place—namely, to the land.

Some years' experience of farming on a small scale enabled me to learn the great importance of this most elementary, but most important, truth. My experience was had in an open country district, and I found the great advantage of a combination of pasture and tillage with—in the rather long winter of the North of Ireland—stall-feeding, the principle that guided me being the entire absence of waste of any kind, the separation and collection of solid and liquid refuse, and the immediate application of the latter to the tilled or grass land at the proper season, or its preservation with the other refuse until the season when it would be required for the tillage.

Now, if you visit many of our back streets where cattle-yards abound, even though the gates of the latter may be jealously locked and barred against intruders, whether official or other, the very walls and adjoining roadway, &c., show the waste of matter out of place permeating masonry and pavement, and disfiguring these, whilst rendering the localities odorous and pre-eminently unhealthy to man and beast.

Foremost among the changes I would suggest should be the obtaining full power to remove all slaughter-houses from the city to the abattoir, constructed with so much cost and with so regrettably little foresight as to the necessary legal powers for enforcing its use.

Surely, to remedy this error and to enable the citizens to benefit by so excellent an improvement, is not attended with insuperable difficulty.

The next step should be the prevention of the retaining of any refuse whatever longer in winter than one week, and in summer than four days, within the borough boundary. To show the need of this measure, it will suffice to state that I have seen a bill of an auction to be held in a thickly-inhabited part of Dublin, which stated that 200 tons of manure were on the premises for sale.

This should apply to cowsheds, or dairy-yards, piggeries—too many of the latter are still allowed in Dublin—stables, and to any works, factories, workshops, slaughter-yards, so long as they are allowed to exist, as well as to the Corporation depots, and any other places where refuse is now allowed to accumulate.

I know a "registered dairy," where the manure heap occupies the highest ground within the precincts of the premises, and from it constantly meanders, at least when the cattle are in their winter quarters, a stream, anything but limpid, down to the ground occupied by the cowshed, the dwelling-house, and the dairy.

The recommendations of Dr. Ballard, of the Local Government Board of England, in reference to the well-ordering of cowsheds and yards, deserve attention; these recommendations are also similar in many respects to the advice of a practical farmer in a recent work.[a] We find in the Fourteenth Report of the Local Government Board, England, 1884–5, p. cv., that 10·009 samples of milk were examined, and of these 1·761, or rather more than one-sixth, were condemned. Many specimens of watered milk, it is thought, escaped condemnation on account of the difficulty as yet experienced in determining whether poor milk had been watered or was naturally poor.

It was found that as supplied to the retailer it was much poorer than after passing through the hands of the latter. Especially this was the case at Portsmouth.

The Dublin citizens have to thank two energetic companies, at least, for much improvement in the milk supply of Dublin. In both these undertakings pure country milk is supplied with every guarantee of purity and freshness, with the addition, at least in the case of one, of a constant supply of fresh cream and butter.

It is hardly necessary to add that I refer to the Educational Dairy Company and the Dublin Pure Milk Company.

There is one point in reference to the former that I think of

[a] Farming for Pleasure and Profit. Fourth Section. Stock Keeping and Cattle Rearing. A. Roland. Lond. 1880.

considerable importance to the poor—namely, the large daily supply of "separated milk" at a very moderate cost.

Too much praise cannot be given to the special features of the Educational Dairy Company, which adds instruction of a high order in this branch of industry, which is so very important in this country.

As I have above stated, it is to be hoped that a way may shortly be found for the removal of all slaughter-houses—of which in 1879 there were 104 in the city, as stated by the late Mr. Boyle, the Executive Sanitary Officer of the Corporation—and that the abattoir, lately constructed, shall be the only place for slaughtering cattle for the Dublin markets. As Mr. Boyle then stated before the Royal Commission, should that abattoir prove insufficient for the city and suburbs, another should be erected.

I am also glad to observe that the Commission referred to drew attention to the propriety of removing the cowsheds from the city.

It is most easy to wish to do what is right in sanitary improvements, but in practice vested interests and other difficulties arise. At the present time, however, I would claim special attention to the observations I have made, founded on the writings and statements of highly competent observers and my own experience.

The evidence of Mr. James Gamgee, on the importance of cleanliness and ventilation, without draughts of cold air, of all cattle sheds, is most important. The example afforded by Dutch farming, and the thrift of the Chinese in saving refuse matter for the land, should be carried out, but not in a town or city. Such matters should be, as in China, at once removed to the country.

The Report of the Massachusetts Milk Inspection, as quoted in the *Lancet* (March, 1886), deserves attention. It shows how, under a very thorough system of inspection and analysis, the number of cases of adulteration rapidly fell off.

Our dairy-yards should be greatly improved in cleanliness, and those in confined localities, crowded with unhealthy tenements, should be removed to suitable places, as should also the dairies or offices for the sale of milk.

The collection of manure should be absolutely forbidden and prevented in any part of the city, saturating, as it now does, the soil and the surroundings where it is permitted, as well as polluting the atmosphere.

Noxious emanations from offensive factories and workshops should be prevented.

Knacker's yards should be abolished, as well as lime-kilns, and removed outside the borough boundary.

Great attention should be paid to domestic sanitation and the cleansing of the sewers, and to the state of the Liffey foreshores.

The Corporation depots of scavenging should be removed outside of the city.

We have just at the present moment no great epidemic amongst the people and a diminution in the number of cases of pleuro-pneumonia amongst cattle; we have, too, a spirit of active inquiry in the sister isle as to the cause of cattle disease in Dublin, with a view to active measures against it; and I do trust that whatever division of opinion may exist amongst our people, that we all shall do our utmost to stay the ravages of disease amongst our cattle, and in this, as well as in every other way in our power, exert ourselves for the benefit of the health of the community.

If these measures were adopted, with the opening up of new and wide thoroughfares, and the construction of more dwelling-houses for our working and middle classes, to let at moderate rents, improved street-paving and cleansing continued, and cleanly and healthy habits in connection with the new baths and wash-houses inculcated, there would be good hope for a better state of health in our city.

THE WORKING OF THE CONTAGIOUS DISEASES ACTS.

By ARCHIBALD H. JACOB, M.D., Univ. Dub.; F.R.C.S.
Chairman of the Sub-Section of State Medicine.

[Read in the Sub-Section of State Medicine, April 8, 1886.]

In the discussion of the Army and Navy estimates during the Parliamentary session, 1885, the party which devoted itself to the repeal of the Acts for preventing the dissemination of venereal disease succeeded in carrying a vote, the effect of which was to withdraw the State subvention for the execution of those Acts; and on the 25th of March, 1886, Mr. Stansfeld, who has since been placed at the head of the Local Government Board of England, carried by a narrow majority the second reading of a Bill to repeal those Acts altogether. On the first of these occasions the Liberal Government supported the vote of money in so lukewarm and undecisive a way that many of their party voted against the grant, notwithstanding that Lord Hartington and other ministers assured the House that the Acts were of the greatest value, and ought not to be repealed. On the recent second reading of Mr. Stansfeld's Bill, the same Government assented to the measure, and the Bill has since become law; but notwithstanding that the question has been settled adversely, I think it right to address to you this evening a few words as to the results to the public health which have accrued from the operation of this law, in the hope that the verdict may be reversed on calmer consideration.

The Contagious Diseases Acts were passed in the years 1866, 1868, and 1869. They were preceded by the first Contagious Diseases Act, which was passed in 1864, and repealed in 1866. This statute provided in substance that on information being laid before a Justice of the Peace by the police or a medical practitioner, showing that he had good cause to believe that a woman was a common prostitute and infected with venereal disease, the Justice could order her to be taken to a certified hospital for examination. Provision was also made for voluntary submission to examination on the part of women, and on certificate that the woman was diseased, the Justice should issue an order for her detention in hospital. This Act did not, like subsequent legislation, actually enforce examination, but it imposed penalties on women who refused examination or quitted hospital without being duly discharged. It also imposed penalties on owners of houses for permitting prostitutes known to be affected with a contagious disease to resort to such houses, &c., for purposes of prostitution.

The Admiralty or War Office were empowered to provide hospitals, and to appoint the visiting surgeons and inspectors.

To make the provisions of the Acts intelligible, I shall give a simple narrative of the method by which they were worked. As the directly utilitarian purpose of the Acts had special reference to our army and navy, and as their effect could be tested with greater accuracy amongst a population respecting whose comings and goings everything was known, the operation of the Acts was confined to the following stations, at which our troops are massed in considerable numbers :—

Aldershot, Canterbury, Chatham, Colchester, Dover, Gravesend, Maidstone, Plymouth and Devonport, Portsmouth, Sheerness, Shorncliffe, Southampton, Winchester, Windsor, Woolwich, The Curragh, Cork, Queenstown.

These stations are divided into districts, sometimes one or more constituting one district—as, for instance, Cork and Queenstown.

To these stations are attached Lock Hospitals for the detention and cure of females suffering from veneral diseases, each of which is maintained wholly by Government, and is administered in accordance with the Acts, differing thus from the voluntary lock hospitals which exist in Dublin and elsewhere. Each district is also provided with one or more examining stations, to which women may present themselves for examination; and the hospitals are under the control of the chief medical officer and matron, a compounder, with nurses, cook, &c., and to each hospital are attached chaplains, with suitable accommodation for holding divine services. It is the duty of the superintendent to keep under observation all women who are known to be prostitutes within the district, as well as those who enter the district from the nearest large town, and those who are suspected of clandestine prostitution, though not actually pursuing that method of livelihood. It may be conceived that the knowledge of these people which the superintendent possesses is very complete, especially as the great majority of his constituency are well known and fully recognised—make no pretence of virtue, and submit themselves to the requirements of the Act without any hesitation.

For illustration, however, we will take the case of a woman who has come under observation for the first time, either having only just appeared in the district, or having only recently lapsed from a virtuous life. The police in their rounds note this woman, and report her presence to the superintendent. She is kept under observation for a few days until circumstances are observed which strongly confirm the diagnosis of the police, yet their suspicion cannot be acted upon until it amounts almost to certainty, because the superintendent knows that to take any step in reference to the woman will involve his proving her guilt before his superior officers. Having obtained what seems to him convincing evidence that she is engaged in habitual prostitution, the superintendent accosts her on the first opportunity, tells her that she is strongly suspected, and asks her whether she is ready to sign a "Voluntary Submission."

In offering this paper for signature the superintendent is bound to inform the woman of the nature of the document and of the examination, and to remind her that, should she sign it, she will be recognised as a regular prostitute. Being thus given the option, the woman may refuse or she may agree to sign. If she signs she repairs to the examining station as soon as possible, and is there examined, and, if found free of the disease, is allowed to depart, being bound to return fortnightly for the next three months, and being thenceforward under the surveillance of the police.

But we will suppose that the woman indignantly repudiates and resents the suspicion against her. In that case she is allowed to go as she sees fit, and nothing further can be done unless under the operation of the penal clauses of the Acts. Should she return to the paths of virtue, or supposing that, possibly, the superintendent has done her an injustice, she is entirely free from molestation, and the suspicions against her are nowhere recorded, saving in the memories of herself and the superintendent. But, supposing that she persists in her method of life, it becomes necessary to restrain her from disseminating disease, and, for this purpose, to apply the penal clauses of the Acts. The police continue their observation of her until their suspicions are realised into proofs; and it will readily be conceived how difficult it must be to produce evidence of an act to which there are no witnesses. The superintendent knows that he must prove his case to the satisfaction not only of his superior officer, but of the magistrates; and he does not, therefore, venture upon making a charge until he can produce proof positive, amounting almost to actual detection in the act. Having obtained that proof, he must first report the exact circumstances to his officer, who examines the evidence before he gives permission to lodge formal information. That permission being given, the superintendent makes his charge that he has good cause to believe that —— is a common prostitute, and is resident within the limits of a place to which the said Acts apply. Upon this, the woman may attend either in person or by attorney, and the case must be proved against her, just as a larceny or other offence; and lest, from the publicity of this hearing, an innocent person might suffer, it is expressly provided by section 37 of the Act of 1866 that the evidence shall be heard and judgment delivered with closed doors. Supposing that the charge of habitual prostitution is considered by the magistrates to have been fully proved, an order is then and there made for the woman to attend, at a given time, for examination; upon which order she is bound to attend once a fortnight as long as she is in the district, and engaged in prostitution; but she may at any time, under the 9th Section of the Act of 1869, free herself from the necessity for so attending by giving up her immoral life or leaving the district. The order for examination having been made, the woman goes, herself, to the examining station; but should she omit to do so, she may be arrested and brought there by force. If, on being examined, she be found diseased, she is committed to the Lock Hospital for detention and cure, by authority of the certificate of the visiting surgeon. This certificate is the authority for the woman's detention, and therefore one copy is served on her, a second handed to the superintendent, and the third sent to the medical officer of the hospital, as his order for her admission and treatment. There she is detained for three months, unless previously cured; but, if still not well, the certificate may be renewed, under section 24 of the Act of 1866, for two further periods of three months each. If cured before the nine months have elapsed, she is handed a discharge-paper, which certifies her free from disease; but, lest she might make use of this certificate to ply her trade, it is, by virtue of the 8th section of the Act of 1869, taken from her as she leaves the hospital gate. If the case is refractory or incurable, and remains unrelieved at the end of the nine months, or if it be considered desirable to send her elsewhere for treatment, she may be discharged, being served with a notice that she is "still affected with a contagious disease," and may go where she pleases, so long as she does not return to her former life; but should she do so, and then commence to spread the disease, she is liable, under section 31 of the Act of 1866, to imprisonment, with hard labour.

Finally, her improper detention in hospital is provided for by the 25th section of the Act of 1866, which enables her to appeal to the magistrates at any time.

Such is the machinery of the Act, and I must leave it to the public to judge how far it is likely or possible that its provisions should be oppressively employed against virtuous women, as has been charged by the agitators against the Acts.

Before the end of 1869 a formidable opposition to the Acts had arisen, supported, among others, by several ladies, who resented this legislation as insulting to their sex, and tending to the deterioration of public morals.

A Royal Commission was thereupon appointed, consisting of 25 members, of which Mr. Massey was president, and, in sittings which occupied 45 days, examined between 80 and 90 witnesses.

A Report, signed by 23 members of the Commission, was the result of the inquiry. The Commissioners considered, though the numerical results of the statistics must be inconclusive, the evidence before them appeared to testify to a general impression on the part of the medical officers of both services, that the Acts had operated beneficially on the health of the men. The Report stated that the Commission had come to the conclusion that although the periodical examination of common prostitutes was the most effectual mode of dealing with venereal disease, it would be difficult, if not practically impossible, to make the system general.

In June, 1879, a fresh committee was appointed, but in consequence of the General Election they were re-appointed four times before they arrived at a Report, having sat 68 days and examined 71 witnesses. It was thought desirable to divide the inquiry into two branches:—

1st. The hygienic effect of the Acts, especially on the health of the army and navy; and

2nd. The constitutional, moral, and social aspects.

The extent of the subject induced the Committee to exclude from their hygienic inquiry any special investigation into the navy, and to direct their attention principally to the army, which affords adequate basis for observation and inference.

The Committee approved of the establishment of hospitals at the public charge, and of police regulations enforced under central authority, within such districts. Seven of the Committee dissented to certain portions of the Report, especially to the recommendations that the periodical examinations should be given up. After pointing out the diminution of disease effected by the Act of 1866, the seven dissentients testify to the good moral effects which these calumniated Acts have produced, and which, in our opinion, far outweigh any moral objections which have been, or can be, alleged against them.

I think I can do no better than epitomise the conclusions arrived at by the Committee after this most exhaustive inquiry, and to state those conclusions in their own words :—

SEVERITY OF VENEREAL DISEASE.

Under the head of venereal diseases are included syphilis and gonorrhœa. The Medical Department of the Army divides syphilis for statistical purposes into primary venereal sores and secondary syphilis. Some of the evidence laid before the Royal Commission of 1871 and the Committee was directed to prove that syphilis had lost much of its virulence, and was no longer so great a physical evil as in the past. But the testimony to its severity, and to the injuries inflicted by it on the person who contracts it, and on his or her innocent descendants, was irresistible. It has lost little of its virulence when prompt and adequate measures for its treatment are not taken. It is still, when local, a painful and disabling malady, and in its constitutional form the cause not only of the numerous diseases and deaths generally attributed to it, but, in the opinion of eminent and experienced physicians, of much sickness and premature mortality, ordinarily set down to other sources, not only in those who contract it, but in their innocent offspring.

For the purposes of the inquiry comparisons were instituted—

(*a*) Between 14 subjected districts and all unsubjected stations.

(*b*) Between the same 14 subjected districts and 14 unsubjected stations, namely—

Isle of Wight, London, Warley, Hounslow, Pembroke Dock, Sheffield, Manchester, Preston, Edinburgh, Fermoy, Limerick, Dublin, Athlone, and Belfast.

EFFECT OF LORD CARDWELL'S ORDER.

A disturbing element which considerably affected the statistics came into force in 1873. An order, known as Lord Cardwell's Order, was issued, which had the effect of stopping the pay of men under treatment for primary syphilis and gonorrhœa. This led, in all military stations, to considerable concealment of both these forms of disease, to an apparent but unreal diminution of their prevalence, and to an apparent and real increase of secondary syphilis, the result of concealed and neglected primary sores. This order ceased to operate late in 1879.

In the absence of strong evidence to the contrary, it might fairly be presumed that a measure which has the effect of secluding while in a diseased condition a class of persons admittedly instrumental in propagating a contagious disease, must tend in some degree to diminish the disease. The Committee stated that the evidence taken fully bears out this presumption.

Syphilis, like certain other maladies, fluctuates under the operation of causes not yet brought completely within the range of science. Its intensity will increase for one period, and diminish for the next, without any palpable reason. This circumstance must be borne in mind in estimating the effect of the Acts. To guard against any error from this source, it is necessary to contrast the condition of unsubjected stations before, with their condition at periods after, the passing of the Acts.

At all unsubjected stations the annual rate per thousand of admissions from 1861 to 1866 of primary venereal sores was 103·0; from 1867 to 1872, 93·6. This shows a fall of 9·4, equivalent to 9 per cent. of the former ratio. In the subjected districts the ratio per thousand from 1861 to 1866 was 109·7, and from 1867 to 1872, 65·4. This shows a fall of 44·3, equivalent to 40 per cent. of the former ratio. The reduction of 9 per cent. in the unsubjected stations being the result of natural fluctuations and ordinary causes, and thus indicating a general abatement of the disease through the community, an equal reduction must be assumed to have been effected by the same causes in the subjected districts. It must, therefore, be deducted from the total diminution found in the latter, and the difference—namely, 31, is, in the opinion of your Committee, attributable to the application of the Acts.

The ratio of primary sores in unsubjected stations from 1860 to 1863 was 116·3 ; from 1870 to 1873, 86·0, showing a reduction of 26 per cent. on the former ratio. The ratio in the 14 subjected stations for the period from 1860 to 1863 was 129·8; for the period from 1870 to 1873, 52·5, showing a reduction of 60 per cent. Deducting from this the reduction of 26 per cent. in the unprotected stations, there remains a reduction of 34 per cent. due to the influence of the Acts when they had been brought into full operation, and before the disturbing influence of Lord Cardwell's Order was felt.

An examination of the figures showed that in some instances between 1866 and 1877, the ratio of admission for primary sores was higher in some subjected districts than in certain unsubjected stations. But on comparing the condition of the district and stations in question before the Acts with their condition when the Acts were in operation, it was found that in far the larger number of cases the reduction at the subjected stations was much greater than the reduction at the unsubjected ones. This circumstance pointed clearly to the effectiveness of the Acts in diminishing the disease. The mean percentage of reduction in admissions for primary sores between the two periods, 1861–1866 and 1870–73, when the Acts were in full operation in the 14 unsubjected stations, was 5; in the 14 subjected stations it was 52, showing an excess of reduction in the subjected district of 47 per cent.

The unsubjected districts showed a sudden increase in '67, and a further considerable increase in '69, maintaining a higher rate of disease than the subjected districts, and deviating enormously from the course of diminution which, if the theory of general improvement were true, they should have followed. Meantime, the subjected stations have shown, with some irregularities, a gradual and considerable diminution. Starting in 1866, with a rate slightly higher than the unsubjected districts, they experienced in 1867 a slight increase, trifling in comparison with that exhibited by the unsubjected. The Act of 1866 now began to be effective; in 1860, while the unsubjected districts were still rising, the subjected fell very considerably.

Secondary Syphilis.—In the subjected stations the ratio per thousand from 1861 to 1866 was 30·7; from 1867 to 1872, 27·2, showing a reduction of 5 per cent. In the subjected stations the ratio from 1861 to 1866 was 37·4, and from 1867 to 1872, 24·6, showing a reduction of 34 per cent. If 5 per cent., the reduction arising from fluctuation and ordinary causes, be deducted, the balance, 29 per cent., will represent the reduction effected by the Acts.

Turning to the periods between 1860 and 1863, and between 1870 and 1873, the ratio per thousand of admissions for secondary syphilis in unsubjected stations was, in the former period, 30·5; in the latter, 27·5, showing a reduction of 10 per cent. In the subjected districts the corresponding ratios were 40 and 20·3 per thousand, a diminution of 49 per cent., from which, if the natural reduction of 10 per cent. in the unsubjected districts be subtracted, there remains, as the result of the operation of the Acts, a diminution of 39 per cent.

Gonorrhœa.—The rate of admissions per thousand for unsubjected stations from 1861 to 1866 was 108·2; from 1867 to 1872, 105·4, a reduction of 3 per cent. The ratios for the same periods in the subjected districts were respectively 125·1 and 114·6, a reduction of 8 per cent.; attributing 3 per cent. of this to the causes outside the Acts, the latter are to be credited with a reduction of 5 per cent.

The contrast of 1860–63 and 1870–73 shows ratios in the unsubjected stations of 116·1 and 95·0, proving a general reduction of 18 per cent. The corresponding ratios in the subjected districts were 134·6 and 100·6, involving a reduction of 25 per cent., and leaving as the result of the Acts, after the subtraction of the general reduction, a reduction of 9 per cent.

Comparisons as to change of conditions with respect to primary venereal sores and gonorrhœa in the above periods were also drawn at an early stage in the inquiry of your Committee between the 14 subjected stations and 14 above-mentioned unsubjected stations, each of which contain 500 men or upwards. The conclusions deducible from these comparisons as to primary syphilis and gonorrhœa were even more favourable to the effects of the Acts than those derived from the broader comparison drawn between the 14 subjected and all unsubjected stations as to those diseases, and showed a considerable reduction in the case of the former disease, and a moderate one in the case of the latter affected by the Acts.

I cannot detain you to repeat the same comparison between the unsubjected and subjected districts, under all circumstances showing, uniformly, the same conclusions.

EFFECT OF THE ACTS ON THE EFFICIENCY OF THE ARMY.

The Committee examined carefully into the question how far the Acts have operated to influence the efficiency of the army. After carefully eliminating all discoverable inaccuracies and sources of error, the Committee were satisfied, contrasting the 14 subjected with all unsubjected stations, that during the period between 1870 and 1873, when the Acts were in full operation, they saved 5·38 men per thousand daily to the army. The daily loss from venereal disease in the 14 subjected stations from 1860 to 1863 was 24·01 per thousand; in the unsubjected stations, 19·75 per thousand, or 4·26 less than in the subjected. Starting with this disadvantage, the subjected stations in 1870–73, a daily loss of 11·31 per thousand against a daily loss in the unsubjected of 13·73. The difference, 2·42 per thousand, does not show all that has been effected in the way of saving by the Acts.

A difference of 5·38 per thousand, representing the true saving effected by the Acts, is the result. This, it is to be borne in mind, means, that out of 16·69 per thousand, who would probably have been daily withdrawn from the efficient strength per thousand of the army in the subjected districts, if they had not been under the Acts, 5·30 per thousand have been daily saved to the efficient strength of the army by the operation of the Acts.

To this saving ought to be added the gain to the service derived from the increased immunity of the men from the various debilitating and incapacitating disorders which, though not classed as venereal diseases, not unfrequently result therefrom.

A remarkable instance of the effect of venereal disease in diminishing efficiency in unprotected districts was presented to the Committee in the third year of their inquiry. It appeared that within ten months of the arrival of a regiment in Dublin, over 43 per cent. of its men had been incapacitated from duty by venereal disease.

EFFECT OF THE ACTS ON THE CIVIL POPULATION.

The Committee had evidence tending to show that the Acts have diminished venereal disease in the civil population. The variations in the number of deaths from syphilis in one division was contrasted with the variations in another division which were almost entirely outside the region of the Acts. The divisions in which the Acts were in force for the three quinquennial periods, 1865–9, 1870-4, 1875–9, showed a reduction from first to last of 14 per cent. in the deaths from syphilis in the civil population. The other divisions showed over the same period an increase of 16 per cent.; proceeding still northward, others showed an increase of 37 per cent., while the most northern part showed an increase of 15 per cent.

The Committee go on to record, in connection with this part of the subject, that they cannot overlook the very general opinion of the medical profession, both in and outside of the subjected districts, who on hygienic grounds strongly advocate the maintenance of the Acts.

Finally, the Committee express the opinion that the Acts have successfully served the two objects to which they were directed—the diminution of venereal disease, and the increased efficiency of the army. It is to be remembered that the Acts have had in reality but an inadequate trial. In the earlier years, from 1866 to 1869, they were not in full efficiency.

The three following years were not sufficiently long periods to develop their full influence, and their utility thenceforward has been diminished, up to 1879, by the action of Lord Cardwell's Order. The benefit conferred since 1866 is great, but it is only an earnest of what the Acts may be expected to do hereafter for the health and efficiency of the army.

So far I have recapitulated the effects of the Acts from the point of view with which the Academy is most concerned—*i.e.*, the hygienic and professional. The agitation for their repeal has, however, been conducted rather upon religious and moral grounds, with which it is not my function in this Academy to deal. I will admit that if it could be shown that these Acts were not clearly justified by results of unquestionable public value they ought not to exist. I am prepared to agree that, even if such results did accrue, the gain from the Acts might be dearly bought if it involved injustice and injury to innocent people; but if I am told that it is wicked to acquire immense public advantages by means of a law which recognises the sin of prostitution, I must decline to answer the question. As to the fact of the beneficial action of these Acts, I have said enough, except that I may with advantage add that, having read much of the argument put forth against the Acts, I am totally unconvinced. As to the risk of insult or injury to innocent persons arising from the operation of this law, I need only quote a verbatim report of the Select Committee:—

"The fifth objection to the Act is of the gravest kind, and if it could be sustained would be fatal to their maintenance. They have, therefore, thought it their duty to spare no labour in probing it to the utmost.

"The Committee call special attention to the fact that in the course of sixteen years not a single case has been brought before the Committee in which any woman alleged to have been wrongfully brought under the operation of the Acts has brought an action or taken legal proceedings against the police authorities in respect of any act done by them under the Contagious Diseases Acts. Considering the large funds at the disposal of the various associations for the repeal of the Acts—the zeal and activity with which their operations have been carried on, and the opportunities for the exposure of the alleged vices of the system, which such proceedings, even if unsuccessful, would afford, the Committee cannot but regard this fact as a significant testimony to the conduct of the police.

"On the whole, therefore, the Committee are not satisfied that in a single case the action of the police has been marked by carelessness and misconduct. The Committee, therefore, are of opinion that the charges of misconduct against the police have broken down, and they desire to record their concurrence in the opinion unanimously expressed by the Royal Commission—'That the police are not chargeable with any abuse of their authority, and that they have hitherto discharged a novel and difficult duty with moderation.'"

CREMATION: ITS SANITARY AND JUDICIAL ASPECTS.

By H. C. TWEEDY, M.D., Univ. Dub.;
Diplomate in State Medicine; Physician to Steevens' Hospital.

[Read in the Sub-Section of State Medicine, April 21, 1886.]

The subject of cremation has of late years been occupying an increasing share of public attention in England as well as on the Continent, and as it has now become a recognised mode of disposing of dead bodies, it may not be uninteresting to the members of this Academy to hear something of the present position of the question, the advantages claimed for this process by its advocates over other forms of burial, the arguments by which it is assailed, and the relation in which it stands to the law of England.

The subject is a large one, and might be discussed from many standpoints; but I propose to confine my observations chiefly to its sanitary and judicial aspects, as coming more especially within the domain of State Medicine.

Let us first take a glance at the right of burial as usually practised in these lands, and see what influence it may have on the health of the community.

Nearly every writer and speaker on the subject of cremation makes a point of introducing the well-known words of Lucan, quoted by Sir Thomas Brown in his celebrated "Hydriotaphia"[a] (Urn-Burial):—

> "Tabesne cadavera solvat
> An rogus haud refert."

"Whether decay or the funeral pyre destroys dead bodies, it matters not." Meaning thereby, we may presume, that in either case the body must be resolved into carbonic acid, water, and ammonia, and

[a] Published in 1658.

the mineral elements of which it is composed. This, of course, must be granted; but is that all? Is it absolutely immaterial whether this inevitable change takes place by the slow and gruesome process of putrefaction, or by some process more safe and rapid, such as cremation? That the public mind began to harbour misgivings as to the safety of the ordinary mode of burial may be gathered from the fact that in 1849 the General Board of Health organised a Commission, consisting of Dr. Southwood Smith and others, for the purpose of instituting an examination into the state of burial-grounds in London and large provincial towns. As a result of this inquiry, such horrifying details were brought to light that Acts of Parliament were passed prohibiting intramural interment. It was then proved[a] "that the placing of a dead body in a grave, and covering it with a few feet of earth, does not prevent the gases generated by decomposition, together with putrescent matters which they hold in suspension, from permeating the surrounding soil and escaping into the air above and the water beneath;" and the Report further stated "that there is abundant evidence that cholera was usually prevalent in the immediate neighbourhood of London graveyards." We now know more than this—not merely that the general health of those living in the vicinity of graveyards almost inevitably deteriorates, but that distinct infective organisms when buried in the earth not only retain their vitality for years, but even multiply under favourable conditions of moisture and temperature, and when brought to the surface, either by means of earth-worms[b] or during the process of opening a grave, can mingle with the air we breathe or the water we drink, thus conveying the specific disease of which they are the germs.

Pasteur has now demonstrated how districts in France have been poisoned by the interment in them of animals that have died

[a] Report on a General Scheme for Extramural Sepulture. (Clowes and Sons.) (Signed)—Carlisle, Ashley, Edwin Chadwick, T. Southwood Smith.

[b] See Darwin's paper, read at the Geological Society of London in 1837, and quoted by Sir Spencer Wells in a paper entitled "Cremation or Burial?" read at the meeting of the British Medical Association at Cambridge, August, 1880.

of "charbon" or splenic fever, and he has shown that the one way of guarding against the spread of that disease is by burning or otherwise destroying the animals that die of it. And again, Dr. Friere, of Rio Janeiro, has shown that the same thing is true of yellow fever, the germs of which multiply to a prodigious extent in the soil of burial-grounds, and that the disease can be produced by water in which the earth of such places has been washed; and, once more, Dr. Koch has proved that cholera itself is due to a similar organism, and that this fell disease can spread under like conditions. But even supposing the absence of infection, what a slow and tedious process is the natural dissolution of the body under the most favourable circumstances. In the "earth to earth" system advocated by Mr. Seymour Haden, he states that a body buried in a porous wicker basket decays away in about six years. This process is much lengthened when the dead are buried in impermeable coffins, under circumstances in which the air cannot act upon the body to complete its combustion and allow it to pass freely into its gaseous constituents. This is a plain unexaggerated statement of facts regarding ordinary burial and its results.

The body buried is reduced to its constituent elements, no doubt, but the process is a lengthened one, extending over several years, and attended with all the loathsome accessories of putrefactive changes, as well as fraught with dangers to the living, not only by contaminating air and water, but in many cases by storing up and fostering those subtle germs of infection which, when set free, go forth each to spread the disease of which it is the peculiar ferment.

How favourably does the process of cremation contrast with this picture. Instead of dragging over a period of years, the resolution of the body is completed in a couple of hours, the gases are driven off without any offensive odour, all infective organisms are completely destroyed by the excessive heat employed (about 2000° Fahr.), and nought remains but a purified sublimate, free from every trace of organic matter, and less than one-twentieth part of the weight of the original body.

Through the courtesy of Mr. Eassie, C.E., Secretary to the Cremation Society of England, I am enabled to describe the Gorini Furnace, which is the pattern crematory chosen by the Association, and upon which the Crematorium now in use at Woking has been modelled.[a] This crematory consists of a receiver, a furnace, and a chimney. The body is introduced into the crematory chamber by a door at one end. This door is then closed, and when the door at the opposite end, which separates the fuel-combustion chamber from the crematory chamber, has been drawn back, the flames from the surface play upon the body, which is placed with the head to the flames, and these and the products of both combustions descend by a flue to a chamber underneath, from whence they ascend by side flues to the chimney, and, after the resulting gases have been purified by passing through a coke fire near the base of the chimney, they pass innocuously into the open air unaccompanied by smoke.

In Italy cremations in the public crematories built on this system occupy from one and a half to two hours, in which is included the time allowed for the cooling down. The Gorini apparatus is that mainly used in Italy, and up to December 31st, 1884, 356 cremations had been effected by it in Milan, Lodi, Cremona, Varese, and Rome.

Another system, invented by the engineer, Joseph Venini, is also much used in Italy, and is highly spoken of. In Germany there are crematoria in Dresden and Gotha constructed on the Siemens principle. Various other forms of apparatus have been used in America and elsewhere; but those mentioned are at present the most highly approved.

In the presence of facts such as these I think we may fairly concede that all sanitary objections to cremation, when properly conducted, are now effectually disposed of. There are other objections, however, of a judicial or *quasi*-judicial nature, of so serious a character that they demand more than a passing word.

The first of these is the possibility of premature cremation during

[a] Taken from a paper entitled "Cremation," read at the International Health Exhibition Conference, by W. Eassie, C.E.

a trance. The second, the danger that cremation may destroy traces of violence or poisoning, and thus defeat the ends of justice.

The first objection is easily disposed of. Let us hear Sir Henry Thompson, President of the Cremation Society, speak on the subject. He says:[a]—"There is a source of very painful dread, as I have reason to know, little talked of, it is true, but keenly felt by many persons at one time or another, the horror of which to some is inexpressible. It is the dread of premature burial; the fear lest some deep trance should be mistaken for death, and that the awakening should take place too late. Happily, such occurrences must be exceedingly rare, especially in this country, where the interval between death and burial is considerable; so the fear is almost a groundless one. Still the conviction that such a fate is possible, which cannot be altogether denied, will always be a source of severe trial to some. With cremation no such catastrophe could ever occur, and the completeness of a properly conducted process would render death instantaneous and painless if by any unhappy chance an individual so circumstanced were submitted to it. But the guarantee against this danger would be doubled, since inspection of the entire body must precede the act of cremation, no such inspection being possible under the present system."

Several answers may be given to the second objection.

1st. A medical examination of the body previously to cremation would in the case of a large number of poisons—the minerals and mineral acids, at least—raise a strong suspicion that death had been compassed by foul means. Death from injury or from wounds would in like manner be discovered.

2nd. As far as mineral poisons are concerned,[b] "direct experiments, instituted by M. Cadet, and repeated by MM. Dourvault and Wurst, have proved that the salts of arsenic and all other metallic poisons, except mercury, which is completely volatilised, can be detected in the ashes after cremation."

[a] Contemporary Review. January, 1873. "Cremation: the Treatment of the Body after Death." Sir Henry Thompson.

[b] *Vide* Dr. Cameron's speech. Transactions of the Cremation Society. 1885. Pp. 29.

3rd. In cases in which there was the least doubt as to the cause of death it would be possible to remove the stomach and a portion of the viscera, and to preserve them for future examination should the necessity for such examination arise.

But, independently of this, it seems scarcely fair to raise objections to cremation which may also with justice be brought against ordinary burial. For instance, most, if not all, of the organic poisons are destroyed by putrefaction, though, of course, more slowly than by fire; and again, as Dr. Cameron very justly remarked when moving the "Disposal of the Dead (Regulation) Bill, 1884"—"It is childish to think of baffling a rich and skilful criminal by prohibiting cremation, so long as you permit him to remove the viscera of his victim and preserve him by the aid of arsenic and corrosive sublimate under pretence of embalming his remains. It is childish to think that any law can put an effective obstacle in his path as long as you permit him to transport the body of his victim to foreign countries in order that he may there bury him or burn him altogether uncontrolled by your laws." But it may be urged, in cremating a body you destroy all possibility of such re-examination as could be made were the body buried and subsequently exhumed. Granted. But what is the real value of such an examination? In the first place, decomposition would speedily obliterate all traces of injury to the soft parts of the body. Furthermore, in many cases the difficulty of identifying a particular corpse would be extreme, more especially if it were necessary to search for it in a crowded graveyard, where scores and hundreds of bodies were buried, huddled together promiscuously; and, lastly, recent researches have shown that in the process of putrefaction cadaveric alkaloids—termed, for want of a better name, ptomaines—have been found. Little is known of these, to be sure, but the knowledge that such alkaloids exist would naturally make us chary of forming conclusions based upon experiments made on animals with matters taken from the viscera of bodies in an advanced state of decomposition.

Let us now see what is the state of the law as regards cremation. It is simply permissive. A test case occurred two years ago in Wales. A man named William Price was charged with a misdemeanour under the following circumstances:—A child five months old, said to be his own, died in his house. As he did not register its death, the coroner gave him notice on a Saturday of his intention to hold an inquest on the body on the following Monday. Mr. Price, on the Sunday afternoon, took the body of the child to an open space, put it into a cask of petroleum, and set the petroleum on fire. A crowd collected; the body of the child, which was burning, was covered with earth, and the flames extinguished, and Mr. Price was brought before the magistrate, and committed for trial. The trial came on in February, 1884, when, after a very able and exhaustive charge to the Grand Jury, Mr. Justice Stephen [a] summed up his remarks by saying:—"After full consideration I am of opinion that a person who burns instead of burying a dead body does not commit a criminal act, unless he does it in such a manner as to amount to a public nuisance at common law. The reason for this opinion is, that upon the fullest examination of the authorities, I have been unable to discover any authority for the proposition that it is a misdemeanour to burn a dead body, and in the absence of such authority I feel I have no right to declare it to be one."

This decision led to the issue of an important circular by the Cremation Society of England, from which circular the following is an extract:—

"A recent decision of Mr. Justice Stephen declares that the cremation of a dead body, if effected without nuisance to others, is a legal proceeding. Under these circumstances the Cremation Society feel it a duty to indicate, without delay, those safeguards which they deeem it essential to associate with the proceeding in order to prevent the destruction of a body which may have met death by unfair means. They are aware that the chief practical objection which can be urged against the employment of cremation consists in the opportunity which it offers, apart from such precautions, for removing the traces of poison or other injury which are retained by an undestroyed body.

[a] Mr. Justice Stephen "On the Law of Cremation." Smith, Elder, and Co.

"The following, therefore, are the conditions on which the employment of the crematorium will alone be permitted by the Council:—1. An application in writing must be made by the friends or executors of the deceased.[a] 2. An ordinary medical certificate of death must be sent in by one qualified medical man at least, who attended the deceased until the time of death,[b] and one also by a second medical practitioner,[c] both unhesitatingly stating that the cause of death was natural, and what that cause was. When no medical certificate is enclosed an autopsy must be made and certified by a medical officer appointed by the Society, and at the expense of the applicant or of the estate of the deceased. These conditions being complied with, the Council of the Society reserve the right in all cases of refusing permission for the performance of the cremation, and, in the event of permitting it, will offer every facility for its accomplishment in the best manner."

These rules are excellent and comprehensive, but at present the law has no power to enforce them.

Dr. Cameron's Bill, before alluded to, included the following important provisions:—

(*a*). "That places used as crematoria should be licensed, and

[a] *Application from relative, executor, or friend of deceased.*—I, [Name] [Address] [Occupation] hereby request the Cremation Society of England to undertake the cremation of the body of and I certify that the deceased expressed no objection (orally or in writing) to being cremated after death. A Medical Certificate of the cause of death is enclosed. [Signature.]

[b] *Medical Certificate of the Cause of Death.*—To the Cremation Society of England. Certificate No. 1. I hereby certify that I attended [Name] [Address] [Profession or Occupation] aged , that I last saw h on 18 , that died on at and that the cause of death was as hereunder written. Cause of death—(*a*) First, (*b*) Second. [The time for each form of disease or symptom is reckoned from its commencement.] Time from attack till death. Signed [the first General Practitioner will sign here.] Professional Title . Address . Date . This Certificate must be signed by a Registered Medical Practitioner.

[c] *Certificate No. II.*—I certify that I have, in relation to the expressed desire that the deceased should be cremated, carefully and separately investigated the circumstances connected with the death. I declare that there are no circumstances connected with the death which could in my opinion make exhumation of the body hereafter necessary. Signed [the Second General Practitioner will sign here]. Professional Title . Address . Date . This Certificate must be signed by another registered Medical Practitioner.

that it should be a crime to dispose of a body by burning in any place not so licensed.

(β). "That it should be unlawful to cremate any body without a special official permit, to be issued only on receipt of a medical certificate (founded either on personal attendance during life or on a *post mortem* examination) that death undoubtedly resulted from natural causes, that the cause was so and so, and that there was no reason whatever to believe that death was caused or accelerated by foul play."

This excellent and comprehensive Bill was seconded by Dr. Farquharson, and supported by L. Playfair, Sir G. Campbell, Mr. Findlater, and others, but it was unfortunately thrown out on the second reading. Notwithstanding its fate, however, one cannot but feel that were it or some enactment constructed on similar lines to become law, with the addition, perhaps, of a clause recommending (as suggested by Sir Henry Thompson in 1873) the appointment in every district of a properly qualified medical inspector, with duties corresponding to those of the French "Médecin-Vérificateur des Décès,"[a] an official without whose written permission no burial can take place in Paris—one cannot but feel, I say, that we might then honestly, and without further hesitation, cast in our lot with the advocates of cremation, certain that in doing so we were outraging no law human or divine, animated with the confidence that we were benefiting our fellow-man by removing one most prolific source of disease and death, and at the same time fully satisfied that we were not opposing the course of nature, but merely anticipating by a little, and for the good of our kind, the time she herself takes in carrying out her own eternal and immutable law of "Dust to dust, and ashes to ashes."

[a] The following is the text of the French law :—Code Napoléon, Article 77.—"Aucune inhumation ne sera faite sans une autorisation, sur papier libre et sans frais, de l'officier de l'état civil, qui ne pourra la délivrer qu'après s'être transporté auprès de la personne décédée pour s'assurer du décès, et que 24 heures après le décès, hors les cas prévus par les règlements de police."

REMARKS ON THE CLIMATE OF DUBLIN, BASED UPON TWENTY YEARS' OBSERVATIONS.

By JOHN W. MOORE, M.D. Univ. Dubl.; F.K.Q.C.P.;
Physician to the Meath Hospital and Co. Dublin Infirmary;
Lecturer on Practice of Medicine in the Carmichael College of Medicine, Dublin.

[Read in the Sub-Section of State Medicine, April 21, 1886.]

Observations on the Weather and on the chief climatic elements in Dublin are by no means of modern origin. In the Report on the Tables of Deaths in the first volume of the Census of Ireland for the year 1851 will be found an interesting historical sketch, presumably from the pen of Sir William Wilde, one of the Census Commissioners, of the weather records which had been kept in Ireland, and in Dublin specially, from time to time. At page 648 of *The Philosophical Transactions* for 1676, there is a letter addressed to the Royal Society of London, giving an account of the weather in Dublin in the previous year, 1675. The next notices are preserved amongst the records of the Dublin Philosophical Society, established in 1683.[a] The MS. Proceedings and Transactions of the Medico-Philosophical Society of Dublin, founded in 1756, afford, down to the year 1784, several useful notices of the weather and diseases, particularly in the metropolis of Ireland. Dr. Rutty's observations were embodied in a monthly diary compiled, no doubt, from daily observations on the temperature, the wind, and the rain; but only a few of his tables have been preserved. The first accurately recorded weather observa-

[a] See The History of Periodic Medical Literature in Ireland, including Notices of the Medical and Philosophical Societies of Dublin.—Dubl. Quart. Journal of Med. Science, Vol. I. February, 1846. This paper was also written by Sir William Wilde, who was at the time editor of the Journal.

tions in Dublin were those made in 1788 and succeeding years by Mr. Richard Kirwan, President of the Royal Irish Academy. His researches were continued until the year 1808. Medical writers in the early part of the present century, including successive generations of Physicians to Cork-street Fever Hospital, depended for their meteorological information on records diligently kept from 1805 to 1841 by Dr. Thomas Herbert Orpen, and which are preserved in MS. in the Library of the Royal Irish Academy.

From 1829 to the present time an admirably systematic and, for the most part, scientific series of observations have been made at the Ordnance Survey Office, Mountjoy Barracks, Phœnix Park—a station which, however, by no means represents the climatic conditions under which the population of the city, or even its suburbs, in general live. These observations were commenced by the late Sir Thomas Larcom, and have been continued by the several Royal Engineer officers, who have been subsequently in charge of the department from time to time. At present the Observatory of the Ordnance Survey is in a state of high efficiency under the supervision of Col. Sir Charles W. Wilson, K.C.B., K.C.M.G., R.E., to whose able administration I am gratified to be able to bear this testimony. But city observations were not wanting during this time. The late Rev. Humphrey Lloyd, D.D., Provost of Trinity College, Dublin, based his classical "Notes on the Meteorology of Ireland"[a] to a large extent on a many years' series of observations, taken under his direction, in the magnetic observatory of Trinity College. The Royal College of Surgeons in Ireland, at the instance of Professor Apjohn, also instituted a series of observations, the results of which were for a long time after 1839 published week by week in *The Dublin Medical Press.* The observer appears to have been John Evans, a porter in the employ of the college. Mr. S. Yeates, of Grafton-street, subsequently kept a record of the weather, which was published weekly during many years in *Saunders's News-Letter.* Within a recent period the Glasnevin records, taken in the Botanic Gardens of the Royal Dublin Society, have been revised and improved.

[a] Transactions of the Royal Irish Academy. Vol. XXII., p. 411, *et seq.* 1854.

Although it lies outside the scope of this paper, which has reference to Dublin solely, I would like to mention the admirable weather records kept for many years at Portarlington by a respected member of the profession, Dr. M. W. Hanlon. That his observations possess scientific value is proved by the fact that his rainfall returns have been made use of by Mr. G. J. Symons, F.R.S., in compiling official tables of the rainfall of the British Islands for the Meteorological Council of the Royal Society.

This brief historical summary would be incomplete, and I would be wanting in filial duty, were I to omit mention of the observations taken by my father, Dr. William Daniel Moore, M.R.I.A., at his residence in South Anne-street, in the very heart of the city. His MS. records are still in my possession, and, with a few interruptions, cover the period between October, 1849, and the same month in 1860, when my own series of observations commenced. These, to the taking of which my father's example first inspired me, have been continued uninterruptedly to the present—thanks to the help from time to time most cheerfully afforded me by various members of my family. At first the observations were of a primitive kind, consisting solely of a daily morning record of the temperature, the atmospherical pressure, and the direction of the wind, with a few notes as to the prevailing weather. Afterwards the maximal and minimal temperatures began to be recorded, as well as the rainfall each twenty-four hours. Then a second daily observation was made, and in March, 1869, the dry and wet bulb thermometers were added to the observatory in the garden of my present residence, 40 Fitzwilliam-square, West. The instruments in use since that time were nearly all supplied, at my own expense, by the Meteorological Office of the Royal Society, London, and they were duly certificated at Kew Observatory.

The thermometers are hung in a modified Stevenson-stand, with louvre-work sides, which faces N.N.E., and is fixed on a raised mound near the southern wall of the garden. The barometer—of the Kew pattern, first designed by Mr. P. Adie, of London—is securely fastened to a book-case in the Library, the cistern being exactly 60 feet above sea level (Ordnance datum) at Liverpool.

My "station" is situated in latitude 53° 20′ N., longitude 6° 15′ W., at a height of 51 to 54 feet above mean sea level. It is annually inspected by Mr. Robert H. Scott, F.R.S., the Secretary of the Meteorological Office, who thus comments upon it in the Annual Report of the Meteorological Council to the Royal Society, for the year ending March 31, 1884:—"This station (Dublin, Fitzwilliam-square) fully maintains its character, as being as good as is possible for a town situation." He sums up his remarks on the three Dublin stations—Mountjoy Barracks, Fitzwilliam-square, and Glasnevin Botanic Gardens—with the words: "On the whole, it may be said that Dublin is as well represented, climatologically, as any city in the United Kingdom." I mention these facts in order to bespeak attention to the results I shall now endeavour to bring out.

From a hygienic point of view, the meteorological factors of greatest importance are—(1) Temperature; (2) Rainfall; (3) Number of Rainy Days; and (4) Relative Humidity. Of somewhat less importance are—(5) Mean Atmospherical Pressure; (6) Amount of Cloud; (7) Direction and Force of the Wind.

In the present paper I will deal chiefly with the former group; but Table I., reproduced from "the Supplement to the Seventeenth Report of the Registrar-General for Ireland on Marriages, Births, and Deaths," contains an abstract of the complete observations taken during the ten years (1871–80 inclusive). In this Table the average[a] "mean atmospherical pressure" has been obtained from daily readings of the barometer at 9 a.m. and 9 p.m., corrected and reduced to 32° Fahrenheit at the mean sea level. The average "mean temperature of the air" given is the arithmetical mean of the readings of the dry bulb thermometer in a Stevenson stand taken daily at 9 a.m. and 9 p.m. The "tension of aqueous vapour" (expressed in terms of inches of mercury), "relative humidity," and "amount of cloud," are the result of observations taken daily at

[a] Throughout this paper the term "average" is employed to denote *the arithmetical mean of a series of means;* thus, by average atmospherical pressure, we understand, in the present instance, the arithmetical mean of a series of ten years' annual mean pressures.

9 a.m. and 9 p.m. The "rainfall" is that measured at 9 a.m. each day and entered to the preceding day. A "rainy day" is one on which at least one-hundredth (·01) of an inch of rain falls within the twenty-fours hours from 9 a.m. to 9 p.m. The number of days of rain, snow, hail, thunderstorms, clear sky, overcast sky, and gales are calculated from observations at 9 a.m. and 9 p.m., and the same applies to the results obtained as to the direction of the wind.

The *average mean height of the barometer* during the ten years embraced in the Table, 1871–80, was 29·898 inches. The annual mean varied from 29·737 inches in 1872, to 29·964 inches in 1875 and 1880—a difference of nearly a quarter of an inch (·227 inch). The monthly average ranged from 29·996 inches in May to 29·847 inches in October. The extreme monthly means were—highest, 30·307 inches in January, 1880; lowest, 29·344 inches in December, 1876. I may state that the absolute extreme readings of the barometer at any time taken by me were—maximum, 30·935 inches, at 10 30 p.m., of January 18, 1882; minimum, 28·150 inches, at 2 15 p.m. of January 26, 1884, on which memorable occasion the probably unprecedented readings of 27·400 inches at Aberdeen, and 27·332 inches at Ochtertyre, near Crieff, in Perthshire, were recorded. No such reduction of pressure had taken place in the British Islands for at least 120 years previously.

The extreme observed range of atmospherical pressure in Dublin within the past quarter of a century or so was, therefore, 2·785 inches—a little more than two inches and three-quarters.

Of the 185 snowy days which occurred in the ten years under discussion, 104 were found in the first quarter, 15 in the second, none in the third, and 66 in the fourth. Of the 309 days on which hail was observed, 114 fell in the first quarter, 77 in the second, 23 in the third, and 95 in the fourth.

TABLE I.—*Yearly Abstract of Meteorological Observations taken at* 40 *Fitzwilliam-square, West, Dublin, during the Ten Years* 1871–80, *by* J. W. MOORE, M.D. Univ. Dub., F.K.Q.C.P., F.R. Met. Soc.

Long. 6° 15′ W.; Lat. 53° 20′ N. Height above Mean Sea Level, 51 feet. Thermometers 4 feet above ground. Rain-gauge 3 feet 4 inches above ground.

Year	Mean Pressure	Air Temperature									Tension of Vapour			Relative Humidity		
		9 a.m.	9 p.m.	Mean	Means of		Absolute Min.		Absolute Max.		9 a.m.	9 p.m.	Mean	9 a.m.	9 p.m.	Mean
					Min.	Max.	Temp.	Date	Temp.	Date						
1871	Ins. 29·886	° 50·5	° 49·5	° 50·0	° 45·3	° 55·5	° 25·9	Dec. 5th	° 78·3	August 10th	Ins. ·305	Ins. ·306	Ins. ·306	% 81·5	% 85·0	% 83·2
1872	·737	50·0	48·9	49·5	45·0	55·3	26·0	Jan. 21st	77·3	July 4th	·295	·297	·296	80·8	84·2	82·5
1873	·930	49·4	48·6	49·0	44·4	55·1	24·0	Feb. 3rd, 4th	79·3	July 20th	·291	·290	·291	80·0	82·0	81·0
1874	·944	50·0	49·3	49·7	44·8	55·6	25·2	Mar. 11th	77·5	July 18th	·292	·294	·293	80·0	83·0	81·5
1875	·964	50·3	49·5	49·9	45·1	55·5	26·7	Dec. 9th	74·3	July 29th	·305	·306	·306	81·9	84·4	83·2
1876	·863	50·2	49·3	49·7	44·9	55·4	25·4	April 13th	87·2	July 16th	·296	·299	·298	80·0	83·4	81·7
1877	·852	49·7	48·7	49·2	44·3	55·0	27·4	Feb. 28th	72·5	July 30th	298	·296	·297	81·5	83·8	82·7
1878	·921	49·4	48·6	49·0	44·5	54·6	14·0	Dec. 24th	75·1	July 17th	·306	308	·307	82·9	85·8	84·3
1879	·923	47·2	46·7	47·0	42·4	52·2	21·2	Jan. 12th	73·6	July 28th	·278	2	·279	82·1	84·5	83·3
1880	29·964	49·6	49·0	49·3	44·7	54·9	20·1	Jan. 22nd	74·8	Sept. 4th	·303	·306	·305	81·8	83·9	82·9
Averages and Extremes of Ten Years	Ins. 29·898	° 49·6	° 48·8	° 49·2	° 44·5	° 54·9	° 14·0	Dec. 24th, 1878	° 87·2	July 16th 1876	Ins. ·297	Ins. ·298	Ins. ·298	% 81·3	% 84·0	% 82·6

TABLE I.—*continued.*

Year	Amount of Cloud			Rainfall			Weather							Wind								
							Number of Days of							Number of Observations of								
	9 a.m.	9 p.m.	Mean	Total	Max.	Date	Rain	Snow	H il	Thunder Storms	Clear Sky	Overcast	Gale	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm
				Ins.	Ins.																	
1871	5·8	5·0	5·4	25·368	1·043	Sept. 26th	191	12	24	12	51	75	9	49	42	66	70	83	94	194	90	42
1872	5·8	5·1	5·5	35·566	1·276	Mar. 21st	238	12	26	7	54	74	9	42	40	52	90	72	97	200	87	52
1873	6·1	5·4	5·8	23·820	1·181	Sept. 13th	189	17	27	16	104	200	12	47	49	62	43	58	98	224	92	60
1874	5·9	5·4	5·7	27·186	2·482	Aug. 13th	186	11	30	9	26	85	16	34	36	62	55	51	110	248	87	47
1875	6·9	6·3	6·[illegible]	29·950	1·355	Oct. 26th	205	14	35	9	15	114	13	20	61	100	76	57	84	197	71	64
1876	6·6	5·7	6·2	32·663	1·828	Dec. 2nd	195	25	34	7	35	96	14	40	39	87	75	71	93	175	93	59
1877	6·8	5·7	6·2	32·146	1·150	Mar. 24th	229	18	30	8	15	101	13	31	34	62	53	68	110	231	97	44
1878	6·6	5·8	6·2	28·262	1·220	May 10th	202	26	28	11	28	108	15	43	37	52	69	58	82	212	113	64
1879	6·9	6·5	6·7	28·858	1·615	Aug. 5th	208	32	48	8	21	132	21	47	50	91	80	46	97	200	76	43
1880	6·4	5·7	6·1	34·512	2·736	Oct. 27th	188	18	27	15	33	108	17	41	54	95	56	45	99	201	72	69
TEN YEARS	6·4	5·7	6·0	298·331	2·736	Oct. 27th 1880	2,031	185	309	102	382	1,093	139	394	442	729	667	609	964	2,082	878	544

As regards the *direction of the wind*, 7,309 observations were made with this result—N., 394; N.E., 442; E., 729; S.E., 667; S., 609; S.W., 964; W., 2,082; N.W., 878; calm, 544. The preponderance of westerly (S.W. to N.W.) over easterly (N.E. to S.E.) winds is very striking—the figures are, 3,924 and 1,838 respectively, more than 2 to 1 in favour of westerly winds. But the great excess of due W. winds is still more remarkable. They number 2,082, or more than double the number of S.W. winds, 964. Partial deflection of S.W. winds by a range of mountains, with summits of 2,000 feet and upwards, to the southward of the city, in some measure accounts for this, and a further explanation is to be found in the frequent occurrence of light westerly land breezes during calm, cold weather. Correlated to this class of westerly winds are the light easterly and south-easterly sea-breezes of the daytime, which so materially modify the heats of summer in Dublin, and go so far to swell the number of E. and S.E. winds included in the Table.

Gales were recorded on 139 occasions at 9 a.m. or 9 p.m., of which 59, or considerably more than one-third, happened within the first quarter, only 10 in the second, 19 in the third, and 51 in the fourth. January (with 30 gales) was the stormiest month. There was only one gale in June.

Thunderstorms occurred on 102 days, of which 9 were in the first quarter, 37 in the second, 42 in the third, and 14 in the fourth. June (with 22 storms) and July (with 31) were the months in which electrical disturbances most frequently took place. Not a single thunderstorm happened in December.

The *amount of cloud* varied from 64 per cent. at 9 a.m. to 57 per cent. at 9 p.m., the average being 60 per cent. February is the most cloudy month (66 per cent.), May is the least so (55 per cent.). On 382 occasions the sky was clear, or the amount of cloud was under 20 per cent.; on 1,093 occasions it was overcast, that is, the amount of cloud exceeded 80 per cent. The first quarter was the cloudiest (325 overcast skies), the second quarter was the clearest (109 clear skies).

The *mean relative humidity* was 82·6 per cent. (81·3 per cent. at

9 a.m., but 84·0 per cent. at 9 p.m.); it was highest in December (87·2 per cent.), and lowest in May (75·8 per cent.)—this latter being the month when temperature is rising most quickly, and when, therefore, the capacity of the air for vapour is at a maximum.

In Tables II., III., and IV. are embodied the results of twenty years' observations on three of the more important meteorological factors—mean temperature, rainfall, and number of days of rainfall. As the dry and wet bulb temperatures were not observed until March, 1869, I am unable to add a twenty years' table of the relative humidity, and so I must be content with the ten years' results already given in Table I.

Temperature.—Table II. gives the arithmetical means of the daily maximal and minimal temperatures for each month and year from 1865 to 1884 inclusive. The average annual temperature is 49·9°; it varied from 51·6° in 1868 (a very dry, warm year) to 47·3° in 1879 (a phenomenally cold year, which almost produced a famine in Ireland). The coldest months are December (M. T.=41·3°) and January (M. T.=41·4°). The warmest month is July (M. T.=60·7°). The stationary temperature in March (43·5° compared with 43·2° in February) is interesting. The warmest month in the twenty years was July, 1868 (M. T.=63·5°), but the same month was nearly as warm in the two following years (M. T.=63·2° and 63·3° respectively). The coldest month was December, 1878, when the M. T. was within one degree of freezing point (32·8°); but January, 1881, was nearly as cold (M. T.=33·2°). March was unusually cold in 1867 (M. T.=39·0°), and in 1883 (M. T.=39·0°); so were April in 1879 (M. T.=44·5°), October in 1880 (M. T.=45·4°), and November in 1878 (M. T.=38·2°). The last-named month was singularly warm in 1881, when there was actually an increase of temperature from 48·1° in October to 50·3°. Something like this occurred again last year (1885), when the M. T. in October was very low (45·5°), while that in November was 45·9°, or 0·4° higher.

In an Appendix to the Table, I have grouped together four quinquennial periods. This brings out the curious result that the latter half of the twenty years was decidedly colder than the former half.

TABLE II.—*Monthly and Yearly Mean Temperature of the Air in Dublin, and the Average Mean Temperature for the Twenty Years*, 1865–84, *inclusive.*

Year	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Year
1865	37·8	41·1	41·2	50·4	53·6	61·0	62·0	59·6	61·4	50·7	45·1	46·2	50·8
1866	43·8	40·3	42·0	47·9	50·1	57·8	61·0	58·3	53·0	51·3	46·1	45·1	49·7
1867	35·7	45·7	39·0	49·9	52·0	58·8	58·6	61·3	56·4	50·3	43·2	42·0	49·4
1868	41·9	44·8	47·3	50·1	55·8	60·5	63·5	60·8	57·9	48·0	43·3	44·7	51·6
1869	44·1	46·7	41·2	50·0	48·2	56·0	63·2	59·4	57·1	51·8	45·0	39·8	50·2
1870	41·3	40·5	43·5	49·8	53·9	59·6	63·3	60·5	57·2	50·3	42·2	37·3	50·0
1871	37·9	46·1	46·6	49·6	53·9	57·1	60·4	62·0	54·6	51·6	43·2	42·1	50·4
1872	42·3	45·9	45·9	48·4	50·4	56·2	62·4	60·0	55·7	47·6	44·4	42·1	50·1
1873	43·0	37·9	42·9	47·3	51·9	59·1	61·5	60·2	54·1	48·2	45·7	45·5	49·8
1874	43·6	42·5	46·7	50·4	50·8	57·9	61·8	59·1	55·8	50·4	46·5	36·8	50·2
1875	46·3	41·0	43·9	48·0	54·9	56·5	58·2	61·1	58·2	50·1	44·6	41·2	50·3
1876	43·1	42·4	41·1	47·1	50·5	57·1	62·1	60·1	54·9	53·1	44·4	44·6	50·0
1877	43·7	44·7	42·5	46·1	49·7	58·5	58·9	58·6	53·4	51·2	45·9	42·3	49·6
1878	43·2	44·6	44·5	48·9	53·5	58·2	62·1	60·7	56·7	51·6	38·2	32·8	49·6
1879	35·3	40·1	42·5	44·5	48·8	55·9	57·2	57·7	54·3	49·7	43·9	37·9	47·3
1880	39·7	45·0	45·4	47·8	52·1	57·1	58·9	61·5	58·6	45·4	44·3	42·4	49·9
1881	33·2	40·6	43·2	45·6	53·5	56·4	61·0	57·0	54·6	48·1	50·3	40·7	48·7
1882	44·7	46·2	46·9	47·1	53·2	55·8	59·5	59·2	53·0	50·1	43·6	38·2	49·8
1883	43·2	43·6	39·0	46·7	51·7	56·4	57·9	59·4	55·3	50·0	44·4	42·6	49·2
1884	45·2	43·4	45·4	46·4	52·6	57·9	60·8	61·5	58·0	50·3	43·9	41·6	50·6
Twenty years	41·4	43·2	43·5	48·1	52·1	57·7	60·7	59·9	56·0	50·0	44·4	41·3	49·9
1885	41·5	43·6	41·8	46·7	48·7	56·2	60·8	57·1	54·4	45·5	45·9	42·0	48·7

Table II. (Appendix).—*The same arranged in Four Quinquennial Periods.*

Year	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov	Dec.	Year
1865–69	40·7°	43·7°	42·1°	49·7°	51·9°	58·8°	61·7°	59·9°	57·2°	50·4°	44·6°	43·6°	50·3°
1870–74	41·6	42·6	45·1	49·1	52·2	58·0	61·9	60·4	55·5	49·6	44·4	40·8	50·1
1875–79	42·3	42·6	42·9	46·9	51·5	57·2	59·7	59·6	55·5	51·1	43·4	39·8	49·4
1880–84	41·2	43·8	44·0	46·7	52·6	56·7	59·6	59·7	55·9	48·8	45·3	41·1	49·6
Twenty years	41·4°	43·2°	43·5°	48·1°	52·1°	57·7°	60·7°	59·9°	56·0°	50·0°	44·4°	41·3°	49·9°

N.B.—The temperature values given above are the arithmetical means of the daily readings of the maximal and minimal thermometers.

I may mention that in the original draft of this Table the mean temperatures were deduced from the maximal and minimal readings of the self-registering thermometer by Kaemtz's Formula[a]—viz.:—Min. + [Max.—Min. × ·41]=M. T. This formula reduces the mean temperature values in winter by half a degree, and those in summer by somewhat more than one degree Fahrenheit.

The *extreme* temperatures in the shade recorded in the twenty years were 87·2° on July 15, 1876, and 13·3° on December 14, 1882—a range of 73·9° Fahr. But these values are very exceptional. As the Table shows, the average annual range of temperature is not quite 20°—namely, 19·4°.

The year 1868 may be cited as an example of an unusually *warm year*. There was an almost complete absence of frost, and during ten out of the twelve months the mean temperature was above the average—the excess varying from 0·5° in January to 3·8° in March—the warmest March within the twenty years now under discussion. October and November were cold—the deficit of temperature amounting to 2·0° and 1·1° respectively. Notwithstanding this, the M. T. of the whole year was 51·6°, compared with an average of 49·8° (excess=1·8°). A remarkable drought prevailed from the last week in April to the 10th of August, when a tropical rainfall occurred. During this period of nearly three

[a] Trans. R. I. A. Vol. XXII., page 422. 1854.

and a half months, only 2·797 inches of rain fell in the city. On six occasions during the summer of this year the thermometer rose to 80° in the shade in Dublin—the highest readings of all being 86° on July 15th and 85° on July 21st. On August 1st the maximum was 82°, and even as late as September 6th the high reading of 77° was noted.

In marked contrast to 1868, and as an instance of a *cold year*, 1879 stands out in bold relief. The annual mean temperature was only 47·3°—that is, 2·5° below the average (49·8°). *Every* month was colder than usual—the deficit of mean temperature ranging from 6·1° in January, 3·6° in April, 3·5° in July, and 3·4° in December, to 0·3° in October and 0·5° in November. Curiously enough, these last-named months were relatively the coldest in the warm year, 1868. There was a singular absence of summer heat in July and August; in each of these months the shade temperature exceeded 70° on one day only in Dublin, and on nine days in July it did not reach 60°. The low temperature was accompanied with—to some extent depended upon—a continuous rather than a heavy rainfall. During the six months ending September 30, rain fell on 125 out of 183 days—that is to say, on two out of every three days. The amount of cloud during this cold, damp, sunless year was 7·5 per cent. over the average. The cold weather, which persisted almost throughout 1879, set in first on October 21, 1878. This period of low temperature had probably not been paralleled for intensity and duration within the present century. The deficiency of mean temperature of the last 11 days of October, 1878, amounted to some 6°, being the coldest weather experienced in that month since 1873. The succeeding November appears to have been the coldest observed in Dublin since 1807—the M. T. was 38·2°, or 6·2° below the average. December, 1878, was also a most inclement month. Snow lay on the ground in the city from the 8th to the 27th, and the M. T. was only 32·8°, or 8·5° below the average, and this notwithstanding a spell of really warm weather on the 30th and 31st.

Rainfall, and Rainy Days.—The average annual rainfall during the twenty years, 1865–84, was 28·015 inches, and the average annual number of rainy days—or days on which at least one-hundredth (·01) of an inch of rain was measured—was 194·6.

The year of the *least* rainfall was the last of the series—1884, in which only 20·467 inches were registered on 187 days. In 1870 only 20·859 inches fell, and that was really a drier year than 1884, for in it there were but 145 rainy days. In 1884 there were as many as 187 rainy days, compared with an average number of 194·6 in the twenty years, 1865–84 inclusive. Contrary to what usually occurs, more than half the rainfall fell within the first six months—11·872 inches having been registered up to June 30 on 92 days. In fact, the most striking feature in the distribution of the rainfall was the scanty downpour observed in August (·777 inch) and October (·834 inch), usually two of the wettest months in the year, the averages being 2·877 inches and 3·025 inches respectively. On no one day did one inch of rain fall—the heaviest daily rainfalls were ·863 inch on April 4, and ·700 inch on February 20. It will be observed that on April 4 more rain fell than during the whole month of October, and that on February 20 the downpour nearly equalled the rainfall of August.

The next driest years were 1873 (23·820 inches on 189 days) and 1868—the "warm year"—(24·935 inches on 160 days).

The year of the *greatest* rainfall was 1872, in which no less than 35·566 inches of rain were measured on 238 days. In 1880 the precipitation measured 34·512 inches, but was distributed over only 188 days—exceptional downpours, amounting to 39 per cent. of the entire precipitation for the year, took place in July (6·087 inches on 24 days) and October (7·358 inches on 15 days), and caused the excess.

The years 1876, 1877, and 1882, were also very wet—the rainfalls being 32·663 on 195 days in 1876; 32·146 inches on 229 days in 1877, and 31·184 inches on 227 days in 1882. Had it not been for a dry autumn and winter in 1879—the "cold year"—that year would probably have been the wettest of all, as it was the coldest, for 25·275 inches of rain fell on 174 days in the nine months ending September 30.

TABLE III.—*Monthly and Yearly Rainfall at Dublin during the Twenty Years* 1865 *to* 1884, *inclusive ; and in* 1885.

Year	Jan.	Feb.	March	April	May	June	July	August	Sept.	Oct.	Nov.	Dec.	Yearly Rainfall
	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches	Inches
1865	1·893	2·321	1·476	1·301	3·412	·727	3·633	3·395	*·056	3·056	3·329	2·863	27·462
1866	1·889	2·063	3·629	2·119	1·810	3·568	1·013	2·172	2·734	1·733	1·489	1·660	25·879
1867	3·086	1·809	4·972	2·181	2·804	1·118	3·443	1·775	1·176	2·848	1·258	·771	27·241
1868	2·638	1·232	1·646	1·930	·891	·677	·741	4·745	2·684	·856	2·146	4·749	24·935
1869	4·258	1·272	2·422	1·490	5·414	·791	·739	1·559	3·957	1·074	1·989	2·594	27·559
1870	2·347	1·440	1·789	·838	1·157	·796	·539	1·514	1·634	5·194	1·218	2·393	20·859
1871	2·624	1·648	·815	3·162	·378	2·265	‡4·391	1·065	4·048	2·917	1·258	·797	25·368
1872	2·864	2·557	2·419	2·655	2·164	3·276	1·098	4·302	2·464	3·421	3·414	4·932	35·566
1873	2·650	·925	2·391	·498	·907	·939	3·408	3·944	2·368	3·089	2·009	·692	23·820
1874	2·019	2·683	·953	1·315	1·747	·405	2·515	4·946	1·709	2·508	3·179	3·207	27·186
1875	2·141	2·477	1·040	1·008	1·071	2·989	2·751	1·883	3·180	7·049	3·051	1·310	29·950
1876	·406	3·012	2·158	2·601	·798	1·260	1·337	2·260	3·146	4·505	3·614	†7·566	32·663
1877	4·322	1·560	2·741	4·707	2·343	·921	3·300	3·536	1·795	2·153	2·438	2·330	32·146
1878	1·557	1·576	1·157	2·350	4·540	5·058	·650	4·641	1·684	2·095	1·338	1·616	28·262
1879	1·714	3·706	1·827	1·997	2·048	4·046	4·187	3·704	2·046	1·320	1·251	1·012	28·858
1880	·563	2·581	3·129	1·832	·847	2·166	6·087	1·401	2·061	§7·358	3·235	3·252	34·512
1881	1·369	2·879	1·885	1·329	1·532	2·666	1·863	4·739	1·599	3·470	2·173	1·529	27·033
1882	1·476	1·862	2·260	3·526	1·533	2·334	3·722	1·872	2·619	2·804	3·344	3·782	31·184
1883	2·679	3·752	1·056	2·207	2·023	1·932	2·222	3·307	3·637	2·205	3·074	1·257	29·351
1884	2·358	3·518	1·858	1·532	1·358	1·248	2·350	·777	1·214	·834	1·412	2·008	20·467
Means	**2·243**	**2·244**	**2·081**	**2·029**	**1·938**	**1·962**	**2·499**	**2·877**	**2·289**	**3·025**	**2·312**	**2·516**	**28·015**
1885	1·617	2·812	1·530	2·911	2·532	1·506	1·154	3·050	2·862	3·500	2·398	·742	26·614

* September, 1865, was the *driest* month of the twenty years.
† December, 1876, was the month of the heaviest rainfall.
‡ July, 1871, was the *wettest* month of the twenty years.
§ Heaviest rainfall in 24 hours—2·736 inches, on October 27th, 1880.

TABLE IV.—*Monthly and Yearly Number of Rainy Days* at Dublin during the Twenty Years* 1865 *to* 1884, *inclusive ; and in* 1885.

Year	Jan.	Feb	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total Rainy Days
1865	13	20	14	9	19	5	17	19	‡ 3	17	18	15	169
1866	22	22	21	18	13	17	13	20	22	13	15	19	215
1867	17	18	22	25	12	6	17	16	13	20	8	13	187
1868	17	14	11	12	10	6	5	13	11	15	19	27	160
1869	18	18	17	14	19	11	9	10	21	11	17	20	185
1870	14	18	11	8	14	9	8	7	11	18	11	16	145
1871	20	16	12	20	9	16	† 28	12	13	16	14	15	191
1872	23	20	21	12	22	19	12	17	22	22	24	24	238
1873	21	8	22	8	17	13	25	23	13	18	14	7	189
1874	14	12	12	16	14	9	19	18	13	22	19	18	186
1875	23	17	14	12	15	20	18	14	14	26	19	13	205
1876	9	23	23	17	6	14	10	14	17	20	20	§ 22	195
1877	25	19	20	21	18	12	25	24	10	16	22	17	229
1878	20	14	17	16	23	19	9	22	16	16	11	19	202
1879	10	23	16	17	23	24	24	19	18	14	10	10	208
1880	8	17	16	20	9	18	24	10	15	15	20	16	188
1881	14	18	17	13	15	21	15	21	12	18	18	16	198
1882	17	16	17	20	16	25	25	11	15	20	24	21	227
1883	20	17	12	10	13	18	22	14	14	16	19	13	188
1884	18	20	17	11	16	10	25	8	14	14	14	20	187
Means	**17·1**	**17·6**	**16·5**	**15·0**	**15·1**	**14·7**	**17·6**	**15·5**	**14·5**	**17·2**	**16·8**	**17·0**	**194·6**
1885	23	19	13	16	23	8	10	14	23	22	17	10	198

* *I.e.*, days on which ·01 inch, or upwards, of rain fell within the 24 hours.
† *Wettest* month of the twenty years. Rainfall = 4·391 inches.
‡ *Driest* month of the 20 years. Rainfall = ·056 inches.
§ Month of the heaviest rainfall = 7·566 inches.

Tables III. and IV. show that May and June are the months in which least precipitation takes place in Dublin, the averages being 1·938 inches on 15·1 days in May, and 1·962 inches on 14·7 days in June. Summer showers and thunderstorms raise the averages for July and August to 2·499 inches on 17·6 days, and 2·877 inches on 15·5 days, respectively. September is a dry month (2·289 inches on only 14·5 days, thus having a fewer number of rainy days than any other month). It is followed by October—the month of the heaviest rainfall (3·025 inches on 17·2 days). The monthly precipitation then remains about $2\frac{1}{4}$ inches on some 17 days each month until March, when it sinks to 2 inches, remaining low until July.

Borrowing the language of the agriculturist, we may roughly regard the first quarter of the year (January—March) as "Seed-time;" the second quarter (April—June) as "Growing-time;" the third quarter (July—September) as "Ripening and Harvest-time;" and the fourth quarter (October—December) as "Fallow-time." We find then that the average precipitation in "Seed-time" amounts to 6·568 inches, and is distributed over 51·2 days; that in "Growing-time" is 5·929 inches on 44·8 days; that in "Ripening and Harvest-time" is 7·665 inches on 47·6 days; and that in "Fallow-time" is 7·853 inches on 51·0 days.

September, 1865—the mean temperature of which month was 61·4°, or 5·4° above the average (56·0°)—was the month of the *least* rainfall in the twenty years, rain having been measured in appreciable quantity (·01 inch or upwards) on only 3 out of 30 days to the remarkably small amount of ·056 inches. This month was, therefore, not only a phenomenally warm one, but also the *driest* experienced in the twenty years.

On the other hand, December, 1876, was the month of the *largest* rainfall—7·566 inches having been registered on 22 days. In October, 1880, also, 7·358 inches of rain fell, but on only 15 days—the precipitation on the 27th alone was nearly *two and three-quarter* inches (2·736). In July of the same year, as already stated, 6·087 inches fell on 24 days. The *wettest* month—that is, the month in which there were most rainy days—was July, 1871, when 4·391 inches fell on no less than 28 days. In December, 1868—the otherwise "dry and warm" year—rain fell on 27 days to the amount of 4·749 inches.

An inch of rain—equivalent to a downpour of 105 tons of water on every statute acre—seldom falls within 24 hours in Dublin. On October 27, 1880, however, as many as 2·736 inches were measured; and on August 13, 1874, also, 2·482 inches of rain were registered. I may add that a tropical downpour, amounting to 1·719 inches, occurred in Dublin and its immediate vicinity on the 4th of August, 1885. The local character of this torrential rain was remarkable. Even at Greystones, County Wicklow, distant 17 miles, the fall amounted to only ·170 inch, or less than one-tenth of the measurement in Dublin.

In a series of exhaustive papers on the "Climate of the British Islands,"[a] Mr. Alexander Buchan, F.R.S.E., Secretary to the Scottish Meteorological Society, observes—"The only part of Ireland where the rainfall falls short of 30 inches, and even there it does not fall below 29 (? 28) inches, is a small district around Dublin." He offers no explanation of this, but it clearly depends on the geographical surroundings of Dublin—its situation in the east of the island, and the grouping of high lands to the S.E., S., and S.W. of the city, whereby the rainbearing winds are drained of their superabundant moisture before they reach the valley of the Liffey and the plains lying north of that river.

The climate of Dublin is, in the fullest sense, an *insular* one, free from extremes of heat and cold—except on very rare occasions—and characterised by a moderate rainfall (about 28 inches) annually, which is distributed, however, over a large number of days (about 195 in each year). Clouded skies, a high degree of humidity, and a prevalence of brisk winds—chiefly from westerly points of the compass—make up the climatology of the Irish capital.

In common with the rest of the British Islands, Dublin owes its mild equable climate to the proximity of the North Atlantic Ocean and its surface current of warm water—usually called "The Gulf Stream," because its head-springs arise in the Gulf of Mexico.

[a] Journal of the Scottish Meteorological Society. Third Series. William Blackwood and Sons, Edinburgh and London.

This sets in a north-easterly direction, laving in its course the western shores of Europe, and carries even into the Arctic Regions north of Scandinavia temperatures from 20° to 30° above those due to the latitude alone. But local natural advantages as regards situation exercise a further beneficial effect on the climate of Dublin. A few miles S. of the city lies a range of mountains, with summits varying in height from 1,000 to more than 2,500 feet. This mountain chain intercepts the vapour-laden winds at all points between S.S.E. and S.W., and so the rainfall is diminished, and the sky is comparatively cleared during the continuance of the southerly and south-westerly winds which so frequently prevail. The absence of any very high ground to the northward of the city—with the exception of the Hill of Howth, which rises, however, only to 563 feet—also prevents excessive precipitation with S.W. winds. It is true that with easterly (S.E. to N.E. or N.) winds the precipitation (often in the form of hail and in winter of sleet or snow) in and about Dublin exceeds that which occurs at such a time inland or on the Atlantic coasts. Were it not for this "lee-shore" condensation the Dublin rainfall would be considerably smaller even than it is.

The second local feature which ameliorates the climate of the capital is the proximity of the sea to the eastward of the city. The keen, dry, searching easterly winds of winter and spring are much softened in their passage across the Irish Sea, so that during their prevalence the thermometer occasionally stands some 5° or upwards higher in Dublin than it does at Holyhead, although this latter place is actually on the sea. It is true that the converse holds good during westerly and north-westerly winds, when severe frost sometimes occurs in winter in Dublin, while the thermometer remains decidedly above the freezing point at Holyhead. Yet these latter winds are never so piercingly cold and parching as those from easterly points. Nor is it in winter merely that the Irish Sea confers a benefit upon Dublin. In calm, clear weather in summertime, no sooner has the sun mounted high in the heavens than a cool, refreshing sea-breeze—a typical "inbat,"[a] as the

[a] Evidently a derivative from ἐμβαίνω.

modern Greeks call it—sets in towards the land, so that consequently extreme or oppressive heat is rarely experienced. Indeed, an oppressive atmosphere happens only when a damp, warm S.W. wind is blowing, with a more or less clouded sky. Temperatures above 80° in the screen in Dublin nearly always coincide with winds off the land, from some point between S. and W., and a clear or only slightly clouded sky.

Among climatic epiphenomena the infrequency of thunderstorms and the relative frequency of hail-showers in Dublin are worthy of note. In winter fog and frost often prevail in the city, when a northerly breeze is blowing along the coast, accompanied by a higher temperature and perhaps showers of rain. Lastly, in summer, with a westerly wind, heavy planetary showers fall at times in the valley of the Liffey, while the neighbouring higher lands enjoy dry weather.

SUB-SECTION OF ANATOMY AND PHYSIOLOGY.

COMPLICATED MUSCULAR ANOMALY IN HUMAN ARM.

By FRANCIS T. HEUSTON, M.D., M.Ch., F.R.C.S.;
Lecturer on Anatomy to the Carmichael College of Medicine;
Surgeon to the Adelaide Hospital.

[Read in the Sub-Section of Anatomy and Physiology, February 11, 1886.]

The following muscular anomaly was found by me in the Carmichael College dissecting-room, during the past Summer Session, in the right upper extremity of a male subject. The muscles of the left extremity presented the normal arrangement:—

From the latissimus dorsi muscle, opposite about the junction of the middle with the lower third of the axillary border of the scapula, a band of muscular fibres, somewhat over an inch in breadth, took its origin, and having twisted on itself in such a manner as to cause the anterior aspect at the origin to look posteriorly, then crossed the upper third of the brachial artery to become attached, opposite the internal border of the coraco-brachialis, into a strong triangular tendon, the external prolongation of which tendon crossed the biceps and pectoralis major, to be eventually attached into the internal and deep surface of the deltoid, within half an inch of its insertion; this tendinous prolongation was about half an inch in breadth.

From the inferior angle or apex of the triangular tendon a rounded tendon passed down the arm, lying at first on the internal border of the biceps, and external to the brachial artery, which vessel it crossed superficially at the middle third of the arm (the basilic vein being superficial to the tendon), and was ultimately

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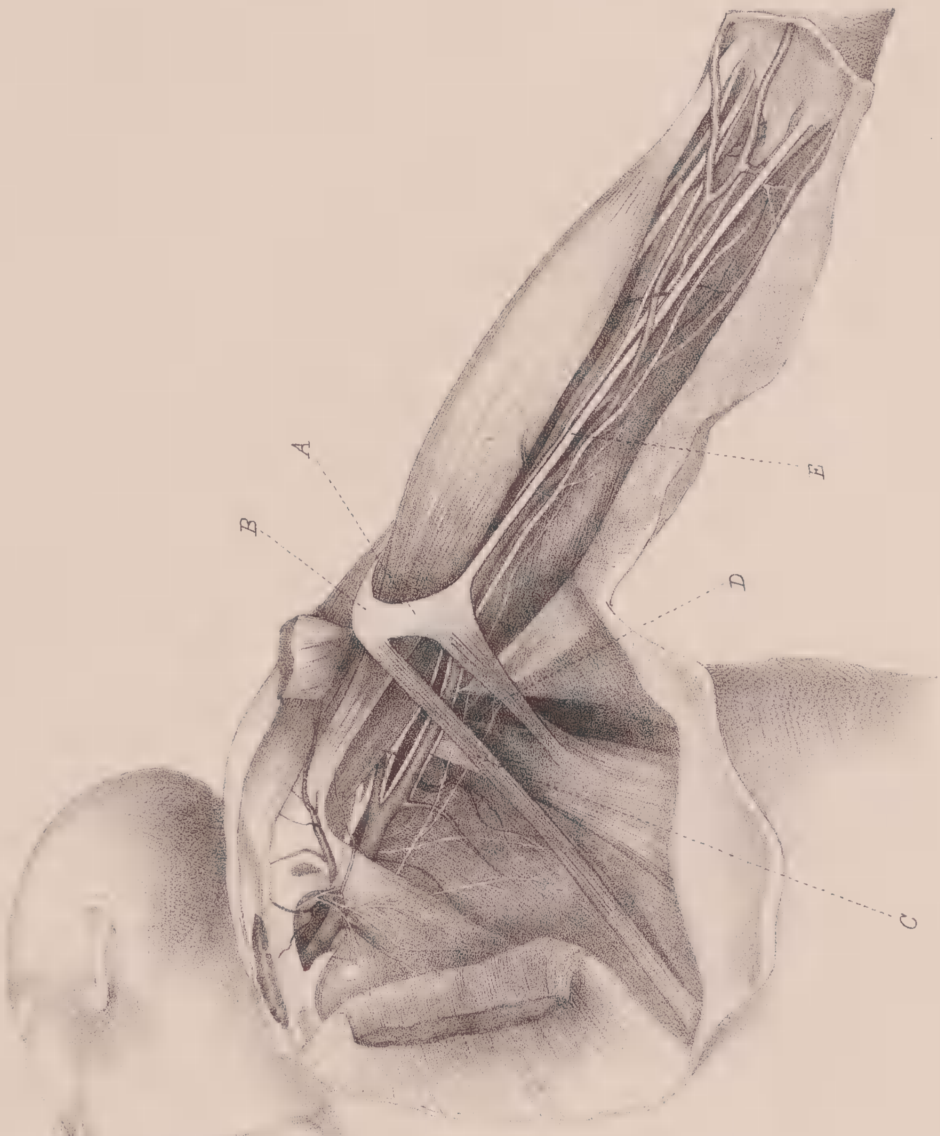

Dr F. T. HEUSTON ON COMPLICATED MUSCULAR ANOMALY OF ARM.

F. Huth, Lith.r Edin.r

attached into the internal condyle of the humerus, being continuous with the internal brachial ligament for about two inches above its insertion. A second set of muscular fibres took their origin from the cartilages of the sixth and seventh ribs, and lying close to the inferior border of the pectoralis major, then crossed the floor of the axilla and third stage of the axillary artery, to be inserted into the superior border of the triangular tendon before mentioned; this band of fibres was about half an inch in breadth. The foregoing arrangement is admirably shown in the accompanying drawing by my prosector, Mr. A. J. Cary.

Remarks.—The first set of fibres described present an example of the achselbogen of Ramsay, but I am not aware that an attachment of this muscle into the deltoid has been noted before. I may here mention that I have this Session seen another example of the achselbogen which was partially inserted into the pectoralis major, and partially into the greater tuberosity of the humerus and capsular ligament of the scapulo-humeral articulation by a tendon which lay in the bicipital groove with the long head of the biceps. The second set of fibres described I look on as an example of the chondro-epitrochlearis of Wood, the round tendon which passed from the inferior angle of the triangular tendon to the internal condyle being the proper tendon of this set of fibres. Unfortunately, I was not able to satisfy myself as to the proper nervous supply of this muscle.

EXPLANATION OF PLATE.

(*A*) Common triangular tendon.
(*B*) Expansion of tendon passing to the deltoid.
(*C*) Muscular fibres from cartilages of sixth and seventh ribs.
(*D*) Muscular fibres from the latissimus dorsi.
(*E*) Tendon to the internal condyle of the humerus.

VARIATIONS IN THE NERVE-SUPPLY OF THE FLEXOR BREVIS POLLICIS MUSCLE.

By H. St. JOHN BROOKS, B.A. and M.B., Dubl.;
Demonstrator of Anatomy in the University of Dublin.

[Read in the Sub-Section of Anatomy and Physiology, February 11, 1886.]

One of the earliest facts impressed upon the student, when he takes up the study of Practical Anatomy, is the relatively slight tendency shown by nerves to vary; in particular, the nervous supply of muscles is supposed to be immutable. This dogma has been extended to Comparative Anatomy by the Heidelberg School, and Professor Ruge "asserts, with Gegenbaur, that a muscle is to be regarded as the end-organ of a nerve, and therefore when a muscle alters in position and connections, its original and typical relations can always be identified by its nerve of supply." [a]

In the foot, where in man the muscles are arranged upon a more primitive plan than in the hand, we find that the abductor and flexor brevis pollicis are supplied by the internal plantar nerve, while all the other intrinsic muscles receive their supply from the external plantar. It is interesting to note the deviation from this fundamental arrangement among the Mammalia as worked out by Professor Cunningham. He says:—

"In the Elephant the internal plantar nerve supplies the flexor brevis indicis; in the Hyrax the internal plantar nerve supplies the flexor brevis indicis, the adductor indicis, and the second dorsal interosseous muscle; in the Beaver a still more remarkable deviation is found. From the internal plantar nerve proceed the twigs of supply for the abductor hallucis, flexor brevis indicis, flexor brevis medii, and the first and third dorsal interossei. Lastly, in the Fox-bat there is an example of the external plantar nerve

[a] Morphologisches Jahrbuch., 1878—quoted by Professor Cunningham.—Challenger Reports, Part XVI., p. 49.

encroaching on the domain of the internal plantar, by supplying a twig to the outer head of the flexor brevis hallucis."[a]

This is a most interesting case of two nerves struggling, as it were, for a group of muscles, sometimes the internal plantar gaining ground, at other times the external plantar extending its domain.

In this paper I have to record a similar struggle (if we may call it so) between the two homologous nerves of the human hand, the median and the ulnar; and we shall find that the nerve-supply to muscles is not immutable, even when our observations are confined to one animal—Man.

Table of Variations in the Nerve-Supply of Flexor Brevis Pollicis in Man.

Outer head supplied by deep branch of ulnar alone, .	5 cases.
Outer head supplied by ulnar and median, . . .	19 „
Outer head by median, inner head by ulnar, . . .	5 „
Median giving twigs to both heads; inner head also with an ulnar nerve-supply, ...	2 „

In some of these cases the twig from the deep branch of the ulnar nerve to the outer head was large; in others very small. The size of the twigs from the median was in inverse proportion to those from the ulnar.

We are told in the text-books, Continental as well as English, that the outer head of flexor brevis pollicis is supplied by the median nerve, and the inner head by the deep branch of the ulnar. This seems at first sight a deviation from the fundamental plan as observed in the foot, until we remember that Bischoff[b] has shown that the muscle usually described as inner head of flexor brevis is in reality a part of the adductor, the true inner head lying concealed beneath it on the metacarpal bone of the thumb, and described by Henle as the interosseous primus volaris. I have not as yet been able to trace the nervous supply of this true deep head of the flexor brevis, but in the two cases in my table, in which the median gave twigs to the inner head, I believe some filaments passed through it to reach the interosseous primus volaris.

[a] Op. cit., p. 135.

[b] Beiträge zur Anatomie des Hylobates leuciscus, 1870, p. 20.

It may be well to note that the outer head of flexor brevis usually consists of two parts; a large mass of fibres from the annular ligament and os trapezium (this is the true outer head) and a fasciculus from the inner head, which crosses the deep surface of the long flexor tendon to join the more superficial part of its insertion; it is the latter portion that one would naturally expect to receive an ulnar nerve-supply, but in every case both parts of the outer head received twigs. In the seven cases in which the outer head was supplied by the median alone, I found that in one case the contribution from the deep head was absent, in another it was very small; in the remaining five this point was not noted. I found it absent in both hands of a young female Chimpanzee, which was kindly placed at my disposal by Professor Cunningham; the outer head was here supplied by median alone. I am inclined to think that the fasciculus of fibres from the deep to the superficial head has acted as a bridge, and, as it were, dragged the branch of the ulnar nerve across. In one of the cases given in the above table the ulnar had encroached in a very unusual manner on the domain of the median; after giving twigs to the outer head of flexor brevis, it pierced the latter and supplied the opponens and abductor pollicis. The branches to the two latter muscles were as large as those which they normally receive from the median, and appear to have replaced the latter entirely.[a]

At first sight it appeared as if this trespassing of the ulnar nerve on median territory might be explained by a communication between the median and ulnar in the arm, but in both arms of the Chimpanzee there was a very large communicating branch from the median to the ulnar in the fore-arm, and yet the outer head of flexor brevis was supplied by the median only.

As the distribution of the ulnar nerve described above has apparently escaped the notice of anatomists, a few words on the method of exposing it may not be out of place. The nerve in question usually comes through the adductor pollicis near its radial margin, and, passing on the deep surface of the long flexor tendon, pierces

[a] I had the pleasure of showing this rare case to Professor Cunningham, and also to the University Anatomist, Dr. T. E. Little.

the outer head of flexor brevis. Sometimes it is entirely concealed by the adductor, and may be brought into view by separating the heads of the flexor brevis. It is surrounded by parallel strands of connective tissue, which give it an intensely deceptive appearance. In looking for this nerve I begin by securing the branches of the median to the outer head; these I find sometimes close to the annular ligament in company with the nerves to abductor and opponens, often lower down from the first or second digital branches, sometimes in both these situations. I then rip up the sheath of the long flexor tendon, pull the latter aside, and separating the two heads of flexor brevis, by pushing without cutting, I see something stretching across which looks like a strand of connective tissue. I follow this into the muscle, and its nervous character becomes evident. I then take a note of the name of the student who is dissecting the part, and when he is doing the deep dissection of the palm he invites me to trace the nerve, and I verify its connection with the ulnar.

The first example of this nerve I found last winter session (1884–85), while dissecting a hand in the Dissecting Rooms of Trinity College, Dublin, and, supposing it to be a very rare anomaly, I showed it to Professor Cunningham. I found that he had an entry of a similar case in his note-book, and also the record of the dissection of the hand of a negro from Sierra Leone, in which the median gave twigs to both heads of flexor brevis. I must take this opportunity to express my thanks to Professor Cunningham for placing these two cases at my disposal.

In the five cases given in my Table, in which the radial head is said to be supplied by ulnar *alone*, the branch from the ulnar to the outer head was large; in two out of the five the whole dissection was made by myself, and after a very careful search I could not find any twigs from the median to the outer head. In the other three cases the integument had been removed before I examined the subject.

As in 19 out of 31 cases the outer head of flexor brevis had a double nerve-supply,[a] I think we should be justified in regarding this as the normal arrangement.

[a] From ulnar and median.

ON THE MORPHOLOGY OF THE INTRINSIC MUSCLES OF THE LITTLE FINGER, WITH SOME OBSERVATIONS ON THE ULNAR HEAD OF THE SHORT FLEXOR OF THE THUMB.

By H. ST. JOHN BROOKS, B.A. & M.B., Dubl.;
Demonstrator of Anatomy in the University of Dublin.

[Read in the Sub-Section of Anatomy and Physiology, May 13, 1886.]

The intrinsic muscles of the hand, more especially the marginal groups known in Human Anatomy as the "thenar and hypothenar eminences," have been frequently made the subject of investigation, but more from a descriptive point of view than with the object of elucidating their morphological value. In works on Human Anatomy great differences of opinion have prevailed as to the division and even the number of the short thumb muscles. Henle describes a double-headed flexor brevis distinct from the interosseus primus volaris, and also separate from the "outer head of flexor brevis" as ordinarily described in English text-books. The latter he terms a deep part of the abductor. Gegenbaur, on the other hand, ignores the presence of an ulnar or deep head altogether. Bischoff [a] first threw light on the true ulnar head of flexor brevis pollicis, which he shows to be identical with the interosseus primus volaris of Henle, but he does not appear to think that the muscles of the little finger call for any special attention. Professor Young,[b] of the Owens College, Manchester, has contributed some valuable papers on the muscles of the hand, and adopts (provisionally) the classification first proposed by Professor D. J. Cunningham,[c] of dividing the muscles of the manus and pes into three layers—

[a] Beiträge zur Anatomie des Hylobates leuciscus, 1870.
[b] Jour. of Anat. and Phys. Vols. XIV. and XVI.
[c] Zoology of the Challenger. Part XVI. pp. 19 and 48.

Fig. 1.

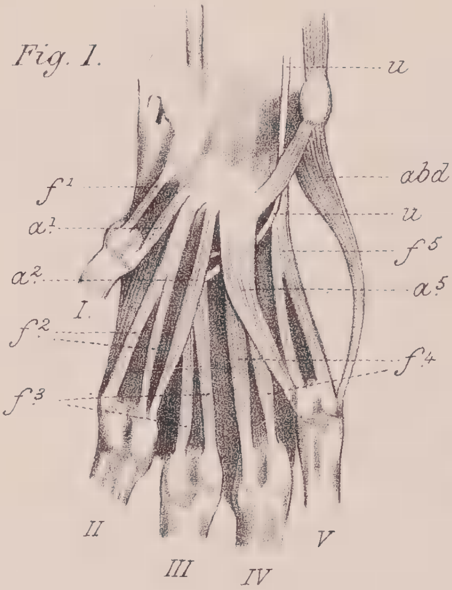

Fig. 2.

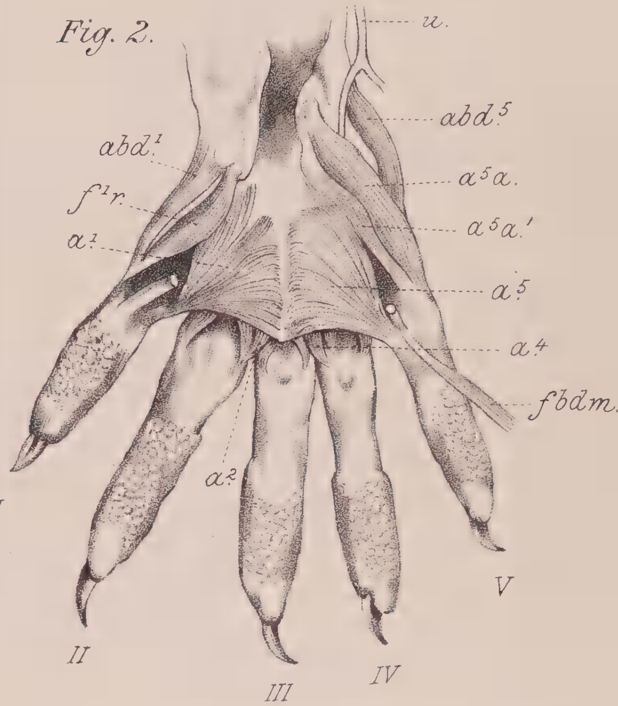

Fig. 5.

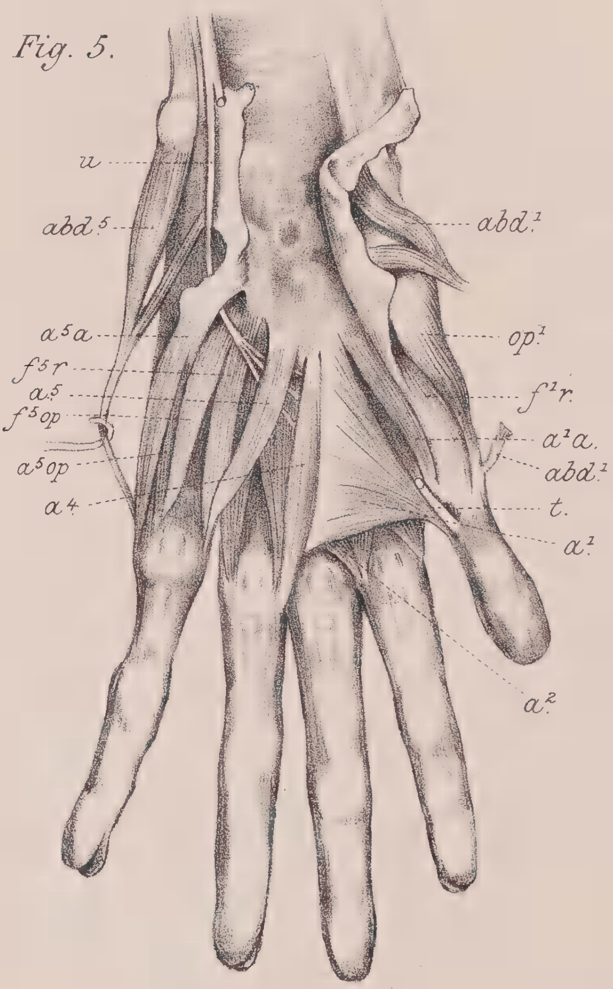

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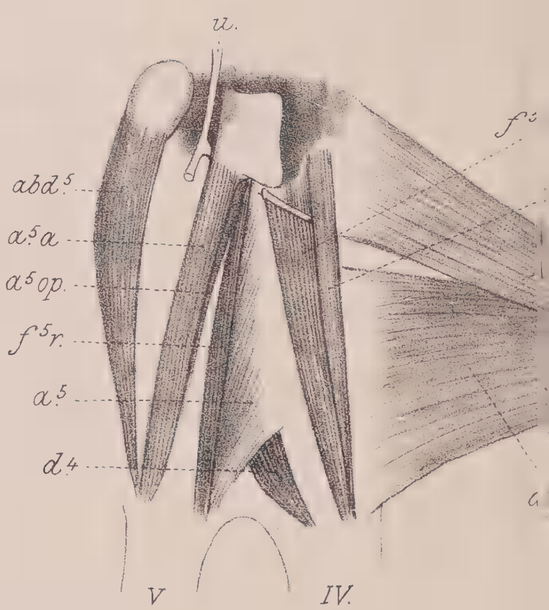
Fig. 7.



Dr H. ST. JOHN BROOK

ig. 3.

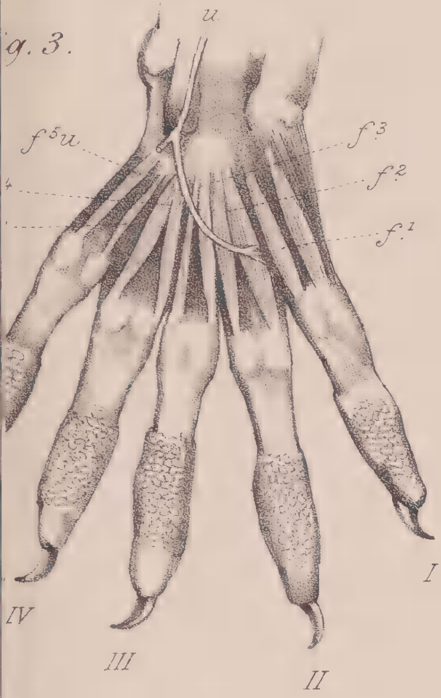
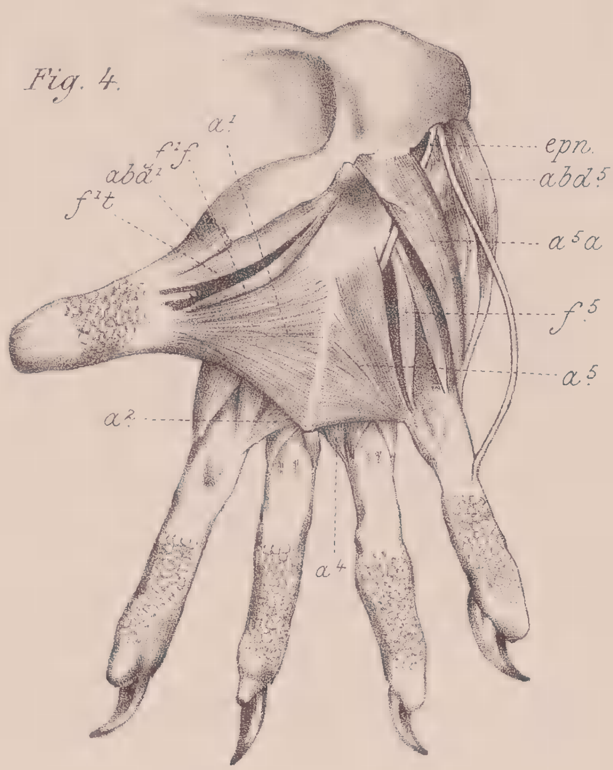


Fig. 4.



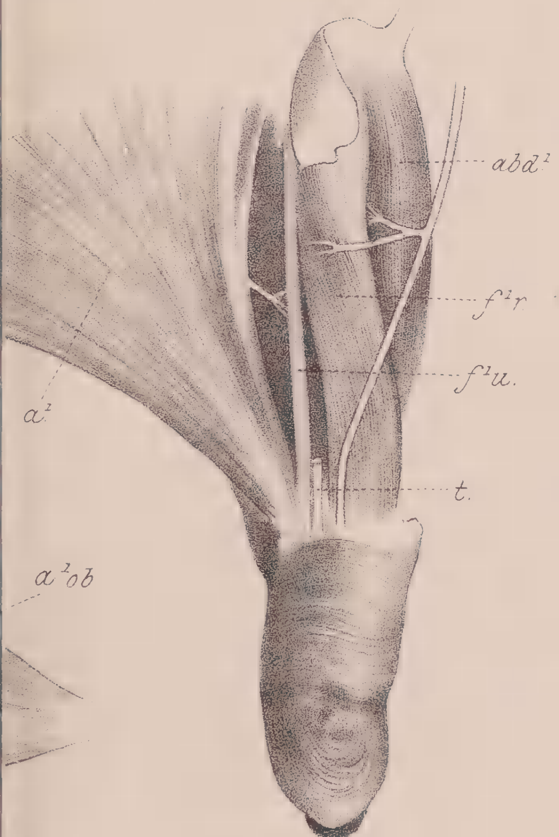
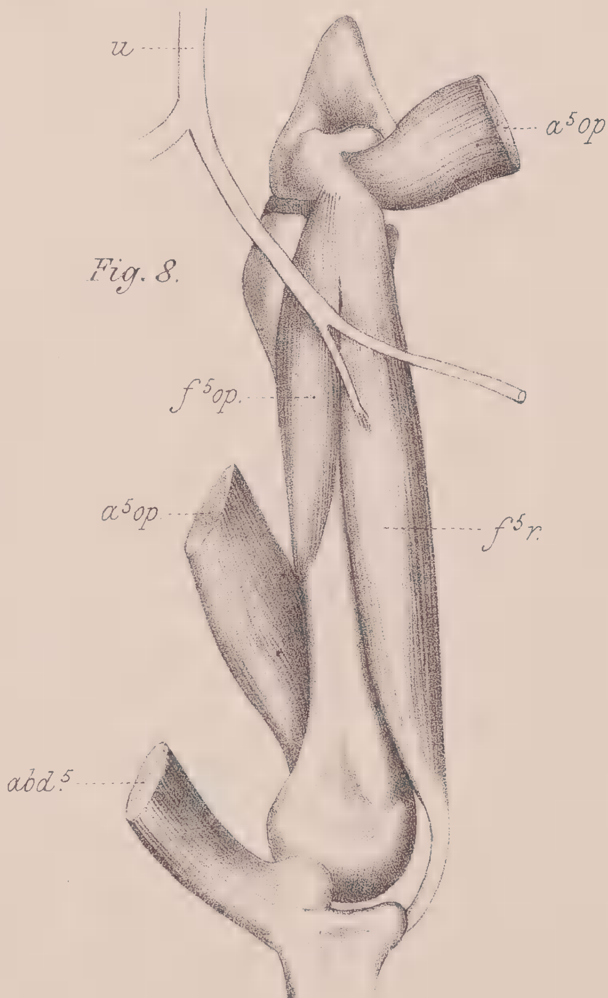


Fig. 8.



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N MUSCLES OF HAND.

1. A plantar layer of "*adductores.*"
2. An intermediate layer of "*flexores breves.*"
3. A dorsal layer "*abductores.*"

Professor Cunningham has established this classification for the foot, and has shown that the origins of the marginal members have a tendency to wander towards the heel. The abductores hallucis and minimi digiti are thus the marginal and enlarged "dorsal interossei." One of the most potent arguments by which Cunningham has established this theory is the fact, that in tetradactylous animals the origin of the (now marginal) abductor indicis is found farther back than the other interossei, unless confined by a rudimentary metatarsal. He adopts, with some reservation, the statement of Ruge,[a] that all the muscles which lie superficial to the deep division of the external plantar nerve are "contrahentes," or "adductores." But, he remarks, "the great objection to this method of classifying the adductors, however, is that it is incapable of being extended to the hand. In the manus the deep division of the ulnar nerve passes inwards, under cover of the flexor brevis minimi digiti, and also under (or through) the opponens minimi digiti, both of which, therefore, according to this generalisation, would be looked upon as contrahentes or adductores." [b]

The author believes that the facts detailed in this paper will show that this "flexor brevis minimi digiti" and the part of the opponens superficial to the deep ulnar nerve, are really adductores. The true ulnar head of flexor brevis quinti in man is represented by that part of the opponens which lies beneath the deep ulnar nerve, and the radial head by the "third palmar interosseous." Further, the dissections described will tend to show that the true deep head of flexor brevis pollicis is very frequently indistinguishable; it is difficult to say whether it is suppressed, or fused with the adductor obliquus, the valuable guide of nerve relation being absent on the radial side of the hand.

With a view to establishing these points, I have examined the manus in Man, Chimpanzee (*Troglodytes*), Orang (*Pithecus*), *Cyno-*

[a] Morph. Jahrbuch. Bd. IV. p. 645.
[b] Op. cit., p. 136.

cephalus anubis, *Macacus nemestrinus*, *Colobus*, Marmoset (*Hapale*), Cat (*Felis domestica*), Capybara (*Hydrochœrus*), Virginian Opossum (*Didelphys Virginiana*), and Australian Opossum (*Phalangista vulpina*). These animals were all most kindly placed at my disposal by Professor Cunningham from the stores of the Anatomical Department of Trinity College, Dublin.

As I have to trace the wandering, both in origin and insertion, of the adductor or contrahentic[a] group of muscles, it will be well to mention what appears to be their typical condition. They are a group of flattened muscles, springing by a common pointed tendinous origin from the ligaments on the palmar aspect of the carpo-metacarpal articulation of the middle digit, and separated from the subjacent interossei by the deep branch of the ulnar nerve.[b] They diverge to be inserted into the proximal phalanges in such a manner as to adduct the fingers to a line which usually passes through the middle digit. The origins of the two marginal members (a^1,a^5, fig. 2) show a great tendency to wander distally and thus overlie and dwarf the others; not only this, but they also spread towards the sides of the carpus, *creeping along the concavity of a curved plane*, formed partly by the unciform and os trapezium and partly by the anterior annular ligament. The insertion may also vary. Cunningham figures and describes a slip to the *radial* side of index finger in the hand of Cuscus, also a slip to the *fibular* side of the fourth digit in the foot of the Koala.[c] Ruge[d] describes and figures an adductor or contrahens passing to the *fibular* side of the fifth digit in the foot of *Dasyurus hallucatus*, while Cunningham figures a similar muscle in *D. vivervinus*,[e] but describes it as

[a] First described in the foot of Macacus, by Halford of Melbourne, and named by him "contrahentes digitorum." He states that they are absent in the hand of the same animal (p. 12). In both of the specimens I dissected the contrahentes were well represented. "Not like man, bimanous and biped, nor yet quadrumanous, but cheiropodous." Melbourne, 1863.

[b] Ruge (op. cit.) established this nerve-relation for the contrahentes of the foot, and it is evidently applicable to the hand also.

[c] Zoology of the Challenger. Part XVI., Plate II. fig. 3, *a*, and Plate VII. fig. 2.

[d] Op cit., p. 647.

[e] Op. cit., pp. 55 and 136; and Plate XI. fig. 5, $d^{6\prime}$. As far as I can judge from the figure the origin is close to the other adductors.

"one of abductors of the minimus," regarding the nerve-relation as exceptional.

In estimating what is a typical condition of adductores or contrahentes, it appears to me that we should regard symmetry in size and direction of fibres rather than the presence of the full complement of muscles; the same remark applies to the flexores breves. The Cat affords a favourable example. It possesses two adductors for the index and little fingers, arising from the ligamentous structures of the central part of the carpus, and lying superficial to the deep ulnar nerve (fig. 1, a^2, a^5). There are four paired flexores breves for the four ulnar digits. These are equal in size, and each arises near the base of the corresponding metacarpal by a pointed origin, which soon bifurcates into two symmetrical fleshy bellies, to be inserted into the ulnar and radial sesamoids. These all lie beneath the deep ulnar nerve. There are two short thumb muscles, one (f^1) representing the radial head or flexor brevis, and the other (a^1) probably the adductor pollicis or first contrahens, as its origin is associated with the other adductores. If this view be correct, there is no trace of the ulnar head of flexor brevis. There is an abductor minimi digiti, but the muscle known in Human Anatomy as "flexor brevis minimi digiti"[a] is entirely absent.

In the Marmoset a very similar condition of the flexores breves is found, but we have here a different arrangement of the true flexor brevis minimi digiti (f^5); while the radial head (f^5r) is inserted into the sesamoid as before, the ulnar head (f^5u) has no insertion into the phalanx either direct or indirect, but is inserted into the whole length of the metacarpal bone, forming an opponens. There is a "flexor brevis" minimi digiti (a^5a) arising from the hook of the unciform and from the anterior annular ligament and inserted with the abductor. It is relatively larger than in Man, and is separated by a small interval from the adductor quinti digiti;

[a] As this name is confusing on account of the existence of a true flexor brevis of the little finger, I shall distinguish the "flexor brevis" of Human Anatomy by inverted commas, and by the letters a^5a (adductor quinti aberrans). The true flexor brevis minimi digiti is indicated by the letters f^5r=radial head, and f^5u=ulnar head.

the latter and the adductor pollicis have spread in both directions in the manner indicated above. They thus unite in a median raphe as far as the metacarpo-phalangeal joint, and conceal the other two adductors (a^4 and a^2); they also travel along the curved plane towards the little finger and thumb respectively. An abductor pollicis and a radial head of flexor brevis pollicis are also present, but the ulnar head of the latter is either suppressed or fused with the adductor.

In Capybara we have chiefly to notice a remarkable case of suppression, while there are three powerful double-bellied flexors breves for the index, middle, and ring fingers, and the ulnar head of flexor brevis minimi (f^5u), is well represented, the radial head (f^5r) is not merely entirely wanting, but its place is not taken by any other muscle. The two adductors (to index and minimus) have the typical arrangement, and the adductor minimi remains comparatively remote from the vacant space where the radial belly of flexor brevis ought to be. We shall compare this with the Opossum later on. There is an abductor, but no sign of "flexor brevis" minimi digiti (a^5a). The abductor indicis shows, in a marked degree, the tendency of marginal muscles to take their origin from a point more proximal than the rest of the group (the thumb being absent).

In the Virginian Opossum there exists a very remarkable condition of the adductor quinti digiti. A large triangular sheet of muscle, like that in the Marmoset, is present, but this extends further ulnarwards and arises from the median raphe and the ligamentous structures at the base of the metacarpus (fig. 2, a^5). This origin is continuous with another muscular mass arising from the base of the hook of the unciform (a^5a'), and diverging to be inserted into the ulnar sesamoid of the fifth finger. No sign of separation can be seen between these two muscles for at least a quarter of their length. Another muscle (a^5a) lies quite parallel to the one last described, and arises from the tip of the unciform hook and from the annular ligament; it is separated by a slight areolar interval from a^5a', but the origins and insertions of the two muscles are absolutely adjacent and their fibres are perfectly parallel. The muscle (a^5a) is evidently the "flexor brevis" of Human Anatomy, as the origin, insertion, nerve-supply, and nerve-relation are the same. There is a well-formed and typical abductor minimi having the same connections as in Man (fig. 2, abd^5), and there is also a muscle arising from the annular ligament near the origin of a^5a and running parallel to the radial side of the latter to give the perforated tendon to the fifth digit—this is a flexor brevis digitorum manus. The broad adductor pollicis extends its origin radialwards along the curved plane, and has concealed and coalesced with, or suppressed, the true ulnar head of flexor brevis pollicis. On removing the muscles which *lie superficial to the deep branch of the ulnar nerve*, a complete set of flexores breves come into view (fig. 3). On two of these, however, doubt may be thrown—(1) the ulnar head of flexor brevis pollicis is probably only an artificially separated part of the adductor; (2) the minute radial head of flexor brevis minimi is partially blended with the fourth dorsal interosseous, and the existence of an arched tendon from the latter, "attached by one extremity to the distal end of the fifth metacarpal bone and by the other to the adjoining side of the first phalanx of the ring finger," as described by Professor Young,[a] renders the discrimination more difficult. In both hands, however, dissecting from both palmar and dorsal aspect, I was able to trace a small fasciculus to the radial sesamoid of the fifth finger.[b] The ulnar belly (f^5u), a very small but perfectly distinct muscle, arises chiefly from the unciform but partly from the base of the fifth metacarpal, and is inserted into the ulnar sesamoid of the fifth finger. Both heads of the flexor brevis minimi are easily and naturally separable from a^5 and a^5a', even without the aid of the deep ulnar nerve, which passes between them and makes "assurance doubly sure."

Professor Young has arrived at results somewhat different from the above.[c] He describes the abductor minimi (fig. 2, abd^5) as an

[a] Journal of Anat. and Phys. Vol. XIV. p. 153.

[b] It will be remembered that in Capybara this muscle f^5r was entirely absent, though there could not be any fusion with another muscle in that case.

[c] Op. cit.

opponens. (It arises from the pisiform bone, and is inserted into the ulnar side of the base of the proximal phalanx of the little finger. I could not find any fibres inserted into the metacarpal bone.) The muscle a^5a he calls the abductor, and a^5a' the ulnar head of flevor brevis; he has also a separation,[a] of which I could find no trace, in the adductor minimi, marking off the ulnar margin of it as the radial belly of the flexor brevis.

It will be instructive at this stage to compare the foot of the same animal (fig. 4). Here we have an "annular ligament" presenting a superficial resemblance to the anterior annular ligament of the hand, though its connections are very different; but it has this property in common, that it affords a *curved plane* along which the origin of a muscle can creep; and we have here a muscle (a^5a) whose insertion and nerve relation is identical[b] with the muscle similarly lettered in the hand, and whose general position appears very similar. Between this and the adductor quinti (a^5) we have an interval in which the deep branch of the external plantar nerve appears lying superficial to a well-developed double-bellied flexor brevis quinti. We note the complete absence of anything corresponding to a^5a' (fig. 2), also the slighter lateral extension of the origin of the adductor quinti, therefore the lesser degree of functional replacement of, and therefore the better development of flexor brevis minimi digiti. In the foot, unlike the hand, the fibular belly of flexor brevis hallucis (f^1f) is quite distinct, and arises in common with the tibial head considerably proximal to the adductor. I found an indication (not shown in the figure) of the separation into adductor obliquus and a. transversus described by Ruge[c] in *Didelphys cancrivora.* Ruge, however, gives no fibular head of flexor brevis hallucis for this species, and also omits all mention of the muscle a^5a which is present in *D. virginiana;* the latter muscle is

[a] Op. cit. Plate VII. fig. 1; a line between *adm* and f^5.

[b] Besides the phalangeal insertion a strong fasciculus passes to the base, and a weaker set of fibres to the head, of the metatarsal bone. This reminds us of the development of an opponens from the corresponding muscle in the hand, so frequently found in higher forms, and is also an argument against regarding this muscle as a flexor brevis digitorum.

[c] Op. cit. P. 648, and Taf. XXXV. fig. 49.

figured and described (after Young) by Cunningham,[a] but he calls it an additional head of the abductor, and the nerve-relation is not shown or described.

The Australian Opossum or Vulpine Phalanger, while corresponding in a general way with the Virginian Opossum, shows some instructive differences. The adductor indicis is on the same plane as the adductor pollicis, so that adductores pollicis and indicis arise from the radial side of the central raphe and adductor minimi from its ulnar side. The muscles a^5a and a^5a', which are separate in the Virginian Opossum (fig. 2), are here united to form one muscle, the deeper fibres of which (corresponding to a^5a') are inserted into the metacarpal bone, and are therefore homologous to a^5op (fig. 5 and fig. 8). The muscles corresponding to a^5a' and a^5 in the Virginian Opossum, although still united at their origin, are more distinct than in the latter animal, as there is a trace of an aponeurotic septum between them. The flexor brevis digitorum manus is absent, as the flexor sublimis supplies a perforated tendon to the fifth finger (while in Virginian Opossum the flexor sublimis has only three tendons, to the index, middle, and ring digits). On reflecting the layer of adductores, the deep branch of ulnar nerve is seen lying on the flexores breves. The fourth dorsal interosseous muscle shows the arched tendon to head of metacarpal and proximal phalanx of ring finger, as described by Cunningham in Cuscus, &c., and by Young in Virginian Opossum, but on the palmar surface of the latter muscle there is a beautifully distinct, fusiform, fleshy slip, inserted by a minute tendon into the radial sesamoid of the little finger; this is the radial belly of the true flexor brevis (f^5r). The ulnar belly of the same muscle appears to be absent, while in the Virginian Opossum it is well represented (fig. 3, f^5u). The radial border of the adductor pollicis functionally replaces the ulnar head of flexor brevis pollicis; while arising in common with the well-developed outer head of flexor brevis, and inserted into the ulnar sesamoid of the thumb, is a distinct fibrous band, which is evidently the rudiment of the true ulnar head. The presence of this band increases the probability that the real

[a] Op. cit., p. 68, and Plate VII. fig. 4, d^5

ulnar head is suppressed, not fused, in the Virginian Opossum, and therefore the muscle shown in fig. 3 is the result of an artificial division of the adductor. Professor Cunningham,[a] in his description of the hand of the Vulpine Phalanger, expresses the opinion which I have given above—viz., that the real ulnar belly of flexor brevis pollicis is suppressed; he regards, however, the muscle corresponding to a^5a' and a^5a (fig. 2) as the ulnar head of flexor brevis minimi digiti.

In *Macacus nemestrinus* there are four adductors, those to the index and ring digits being small. There is a broad triangular adductor pollicis inserted directly into the radial side of base of the first and *second*[b] phalanges of the thumb. From the part arising from the carpus a fasciculus diverges to be inserted into the *radial* sesamoid (compare a^1a in Cynocephalus). These two parts are intimately connected for at least the proximal half of their length. On separating these two the deeper fibres of the large triangular portion are seen to be inserted into the ulnar sesamoid; as the origin of these latter fibres is from the ligaments at the base of the second and third metacarpal bone they evidently form a part of the real adductor, and cannot be regarded as a flexor brevis. Flexor brevis radial head and opponens pollicis are similar to the same in Man, but there appears to be no trace of an ulnar head of flexor brevis. The muscles of the little finger differ in only one point from those of the Marmoset; the radial head has become rather the larger, and more distinctly resembles the homologous muscle in Man (third palmar interosseous). The condition of paired flexores breves for the three middle digits, however, has vanished, and in its place we have an arrangement of "interossei" similar to that in Man.

In *Colobus* the thumb is rudimentary, appearing only as a minute

[a] Op. cit., p. 25.

[b] If this condition is constant in Macacus it shows a curious reappearance of a reptilian character in a high form of mammal. A similar condition (insertion into second phalanx) of the adductor minimi digiti pedis is described by both Ruge and Cunningham in the Ornithorhynchus. The latter also describes the insertion of the adductores into the ungual phalanges in the pes of Echidna, also a slip to the ungual phalanx of the hallux in Cuscus (op. cit., p. 57).

projection before the skin is reflected; it possesses, however, a metacarpal bone and phalanges, the distal phalanx being the size of a pin's head. There is no long flexor tendon, but the short muscles are remarkably distinct and well-defined. Abductor pollicis is rather weak, but otherwise typical. The flexor brevis pollicis arises by a pointed tendinous process from the annular ligament near the os trapezium, and divides into two bellies, of which the radial is rather the larger; these are inserted with the abductor and adductor respectively. The sesamoid bones appear to be absent. A very few fibres of the radial head are inserted into the metacarpal bone, but there is a slip from the *ulnar* head which forms an opponens; it is inserted on the ulnar side of the middle line of the shaft. The broad, flat triangular adductor arises from the fascial structures over the middle metacarpal bone, and is separated by a comparatively wide interval from the flexor brevis.

There is a "flexor brevis" minimi digiti (a^5a) from the deeper part of the origin of which an opponens is formed. The latter is inserted into the whole length of the metacarpal bone of the little finger. There is a true flexor brevis separated from the two preceding muscles by the deep branch of the ulnar nerve; its radial belly forms the "third palmar interosseous;" its ulnar belly is very small and forms an opponens, a few fibres reaching as far as the head of the metacarpal bone; it is overlaid and dwarfed by the greatly developed opponens derived from the palmar layer of adductores.

In *Cynocephalus anubis* (fig. 5) there is a large adductor pollicis arising from a strong fibrous band over the middle metacarpal, and from the ligaments over the bases of index and middle metacarpals, a slip (a^1a) passes from it to be strongly inserted into the radial sesamoid. There is a powerful radial head of flexor brevis (f^1r) arising from the anterior annular ligament and os trapezium; from the deeper part of its origin the ulnar head springs. It is about one-fifth the size of the radial head, and is inserted along the middle line of shaft of metacarpal, its most distal fibres inclining to the ulnar side, to be inserted close to the ulnar sesamoid but not reaching it. It will be remembered that a similar muscle forms part of the ulnar head in Colobus; this muscle is concealed by a^1a in the figure; it is separated from the latter by a strong fibrous sheath. Turning to the little finger, we find a "flexor brevis" (a^5a) arising from the annular ligament close to the tip of the very short unciform hook; intimately blended with its deeper fibres of origin we find an opponens (a^5op) inserted into the distal two-fifths of ulnar border of shaft of fifth metacarpal. The deep branch of ulnar nerve separates the foregoing from the true flexor brevis, which arises chiefly from unciform and diverges to form "third palmar interosseous" (f^5r), and an opponens inserted into the whole length of the shaft of the metacarpal (f^5op). The adductor (a^5) is in this form rather widely separated from (a^5a.)

In the Chimpanzee the "flexor brevis" minimi digiti (fig. 7 a^5a) has the same origin and insertion as in Man; from its deeper fibres of origin an opponens (a^5op) is developed, which is inserted into the distal four-fifths of the fifth metacarpal on its ulnar border. The deepest part of the origin of this opponens, from the base of the unciform hook, *is united with the pointed proximal extremity of the origin of the adductor minimi digiti;* the latter has also a thin scattered extensive origin from the fascia over the ring metacarpal. The radial head of flexor brevis minimi digiti (f^5r) resembles the "third palmar interosseous" in Man; a few fibres spring from the unciform. Closely associated with this origin, the ulnar head (f^5u) arises from base of unciform hook, and is inserted into the proximal fifth of the ulnar border of shaft of fifth metacarpal; it is slightly blended at its insertion with the other opponens (a^5op) but quite separable at origin; the deep branch of the ulnar nerve passes between them. The adductor pollicis arises from a fibrous band, which passes from the head of the ring metacarpal to the base of the middle metacarpal, and from the ligaments over the bases of the middle and index metacarpals; it shows a slight division into adductor obliquus and a. transversus. There is no trace of any slip like a^1a in *Cynocephalus.* There is a well-developed radial head of flexor brevis pollicis arising from the annular ligament close to the os trapezium, and inserted with the abductor. The inner head is represented by a sharply-defined glistening fibrous band, which arises from the extreme base of the thumb metacarpal, and is inserted with the adductor into the ulnar side of the base of the proximal phalanx of the thumb. In the specimen dissected (a young female) there were no sesamoid bones.

In the Orang there is a condition of the muscles of the little finger more resembling Man than any other animal I have examined. The "flexor brevis" minimi digiti (a^5a) is relatively larger than in Man, and the opponens which arises in connection with it is inserted into the entire length of the shaft of the fifth metacarpal bone. The adductor of the thumb is the only adductor present, unless the others are represented by a tough fibrous tissue which covers the interossei and the deep branch of ulnar nerve. Flexor brevis minimi digiti radial head is as in Chimpanzee, but the ulnar head appears to be entirely wanting. The adductor pollicis has the same connections as in man, but the slip which passes to the radial sesamoid (a^1a) is wanting. The flexor brevis pollicis arises from the os trapezium and annular ligament, and divides into two fleshy bellies, a small ulnar, and a much larger radial, which are inserted into corresponding sides of the proximal phalanx, and separated by the slender long-flexor tendon (t, fig. 6).

In Man the "flexor brevis" minimi digiti (a^5a) is relatively smaller than in any of the other animals, and is frequently wanting; intimately connected with its deeper fibres of origin from the unciform hook we find an opponens, which is usually inserted into the distal four-fifths of the shaft of the metacarpal, as in the Chimpanzee, but may be limited to the lower half or even less (fig. 8, a^5op). Arising from the shaft of the fifth metacarpal, and in most cases from the unciform[a] is the radial head of the true flexor brevis ("third palmar interosseous," f^5r) and closely associated with the unciform origin and inserted into the proximal fifth of the metacarpal bone there is a small muscle (f^5op) separated by the deep branch of the ulnar nerve from the first-mentioned opponens, and quite distinct from the latter at its origin. In the specimen from which fig. 8 was drawn this muscle was unusually large, and at its origin *formed one muscle with the third palmar interosseous.* In the

[a] Henle, Muskellehre, p. 246.

thumb we find a very powerful adductor, whose origin is pierced by the deep palmar arch, the division into adductor transversus and a. obliquus being thus indicated;[a] the a. obliquus is described in English text-books as the deep or inner head of flexor brevis. From the adductor obliquus a strong bundle of fibres[b] passes to the radial sesamoid of the thumb; this is identical with a^1a in Cynocephalus. The radial head of flexor brevis is strong, and arises from the lower border of the annular ligament close to its attachment to the os trapezium. The true ulnar head (interosseous primus volaris) arises from the extreme base of the metacarpal bone of the thumb, and is inserted with the adductor, some fibres being often prolonged on to the dorsum of the phalanx to join the long extensor tendon.

It is evident from the above descriptions that the so-called "flexor brevis" minimi digiti of the human hand is not homologous to the muscle of the same name in the foot; the flexor brevis minimi digiti of the foot lies close to the metatarsal bone and beneath the deep division of the external plantar nerve, while in the hand "flexor brevis" is superficial to the deep branch of ulnar nerve. Professor Cunningham[c] has shown that the flexor brevis minimi digiti of the foot and the "third plantar interosseous muscle" are the fibular and tibial bellies of the same muscle. In Opossum (fig. 4 f^5), and in many of the mammalian feet figured by Professor Cunningham,[d] this condition is very evident, the two bellies being symmetrical and the insertion on each side phalangeal. In the Chimpanzee I found in both feet the flexor brevis minimi digiti and "third plantar interosseous" arising together by a pointed tendinous origin from the sheath of the peroneus longus tendon, as far back as the ridge on the cuboid bone; the "interosseous" had an additional origin from the shaft of the fifth metatarsal. The fibular belly was inserted into the shaft of the metatarsal, its only connection with the phalanx being through the

[a] Bischoff, op. cit., p. 20.
[b] See Quain's Anatomy, 9th edition. Vol. I., p. 226.
[c] Op. cit., p. 116.
[d] Op. cit. Plate VIII. fig. 1, f^5; Plate XI., figs. 1, 6, and 10.

abductor tendon, into which a few fibres were inserted. A very similar condition to this is described in Professor Cunningham's notes on the dissection of a negro foot from Sierra Leone;[a] the insertion into the phalanx of the flexor brevis, however, was present. In European feet the following is a frequent condition of the flexor brevis minimi digiti:—The greater part of the muscle arises (more or less associated with the third plantar interosseous) from the sheath of peroneus longus, the remainder arising from the base of the fifth metatarsal; the insertion is partly into the metatarsal, partly into abductor tendon, and the remainder directly into the phalanx.

While in the foot the fibular belly of flexor brevis minimi shows a tendency to become reduced to an opponens, this tendency is more pronounced in the hand. In the following animals we find the insertion becoming more and more proximal, and the size of the muscle diminishing *pari passu*:—Cat, Marmoset, *Macacus*, *Cynocephalus*, *Colobus*, Man, Chimpanzee, Orang. In the latter it appears to be absent altogether. In the first three we find the opponens (a^5op) (which lies superficial to the deep ulnar nerve) absent. In the others it shows a gradual increase in size, functionally replacing the deeper muscle. Again in the Cat the "flexor brevis" (of human anatomy) is absent, but present in all the rest, and in the Cat alone (in this series) we find the true flexor brevis (ulnar head) with a phalangeal insertion.

Having now demonstrated the true flexor brevis, let us examine the so-called "flexor brevis" of man. It corresponds in every way with (a^5a) in Cynocephalus, Opossum, and Chimpanzee (figs. 5, 2, and 7). In the Opossum its origin is continuous with the adductor minimi digiti, the gap usually found between their origins being occupied by a muscle (a^5a'), which is evidently only a deeper fasciculus of a^5a. This deeper fasciculus, which is phalangeal in insertion, shifts its insertion to the metacarpal bone in higher forms, and appears as a^5op in Cynocephalus, Chimpanzee, and Man. We have thus in the Opossum what is practically one sheet of muscle of a^5, a^5a', and a^5a separated by the deep branch of ulnar

[a] Unpublished as yet.

nerve from the layer of flexores breves. It appears probable that the adductor minimi digiti extends both its origin and insertion towards the ulnar side of the hand, and that the pressure of the long flexor tendon (or tendons) afterwards divides the insertion into two slips,[a] the deeper fibres of the ulnar part afterwards contracting an insertion into the metacarpal bone. The pressure of the tendon and the greater development of the unciform process then separates the origin[b] also, so that the ulnar border of the adductor minimi digiti becomes divorced from the rest, and we call it "flexor brevis" and opponens minimi digiti in Human Anatomy. In Man and in Orang the typical adductor minimi vanishes, and the aberrant part remains as its only representative.

In Man, *Macacus*, and *Cynocephalus*, we find an indubitable part of the adductor pollicis (a^1a) passing to be inserted into the radial sesamoid; in some human hands, on raising the long flexor tendon, the adductor fibres are seen to pass in an almost continuous sheet across the base of the phalanx from the ulnar to the radial sesamoid, some small twigs from the arteria princeps pollicis being the only separation. The pressure of the single flexor tendon of the thumb has been insufficient to separate the insertions in some cases, or in any case to separate the origins.

The travelling in a radial direction of the adductor pollicis causes it to overlie, to functionally replace, and therefore to dwarf, the true ulnar head of flexor brevis. As the valuable guide of nerve-relation is absent on the radial side of the hand, it is difficult to say in most cases whether the ulnar head becomes fused with adductor pollicis or aborts entirely. I believe that fusion rarely if ever occurs, as we find either a ligamentous rudiment of the true ulnar head of flexor brevis (as in Chimpanzee and Vulpine Phalanger), or a complete absence of any fibres arising in common with the radial head or from the base of the metacarpal bone of thumb (as in *Macacus*). It is curious that the true inner head

[a] In the more primitive types the muscles in the hand are powerful, while in higher forms there is a tendency for the muscular structures to retreat to the fore-arm, the hand being occupied by tendons.

[b] In Cynocephalus, in which the flexor tendons are enormously strong, we find a wide separation between a^5 and a^5op (fig. 5).

should be better represented in some of the higher primates (Man, Orang), than in many of the lower monkeys.

Bischoff[a] describes "two heads of the flexor brevis pollicis, the inner somewhat weaker and deeper than the outer," in *Cynocephalus maimon*, *Cercopithecus sabäus*, *Macacus cynomolgus*, *Pithecia hirsuta*, and *Hapale penicillata.* I could not find any true inner head in *Macacus nemestrinus* or in *Hapale*, and from a careful inspection of his figures of *Cynocephalus maimon* and comparison with *Cynocephalus anubis*, I believe that what he calls the inner head of flexor brevis is identical with a^1a fig. 5, being part of the adductor inserted into the *radial* sesamoid.

While the dissections described above have shown that the so-called "flexor brevis" minimi digiti of the hand of Man is a different muscle to the true flexor brevis, there are two possible views of its morphology besides the one advocated above:—

(*a*) That it is a part of the abductor.

(*b*) That it is the division of the flexor brevis digitorum manus to the little finger.

(*a*) In favour of the first view, it may be urged that the origin of the corresponding muscle on the radial side (abductor pollicis) has a great tendency to travel ulnarwards along the annular ligament, even passing the middle line in some cases. On the other hand, the nerve-relation is a strong argument against this view. In *Cynocephalus*, where a second slip of the abductor takes origin from the annular ligament (fig. 5), the normal nerve-relation is still preserved. The absolute continuity of origin of the "flexor brevis (a^5a) with the normal adductor, which has been shown in Virginian Opossum, Vulpine Phalanger, and even in so high a form as the Chimpanzee, is another reason against regarding "flexor brevis" as part of the abductor.

(*b*) In favour of the second view, the fact may be noticed that in the foot the tendons of the short flexor are not always perforated. In the Chimpanzee I found the tendon to the little finger inserted into the floor of the flexor sheath, on the fibular side of the base of the *proximal* phalanx of the little toe, and I

[a] Op. cit., pp. 90, 91.

have seen the same condition in Man. In the toes of Ornithorhynchus, Cunningham has shown that none of the tendons of the short flexor are perforated. A strong argument against this view, however, is the presence of a flexor brevis digitorum manus to the little finger in the Opossum (*fbdm*, fig. 2) co-existing with a^5a. In the foot of the Opossum there is a perforated tendon for the fifth toe coexisting with the muscle (a^5a, fig. 4), and it appears probable that the latter is the homologue of the "flexor brevis" minimi digiti of the hand.

EXPLANATION OF PLATE.

Fig. 1. Manus of Cat.—f^1 to f^5, flexores breves,—f^1 has only the radial head; *u*, deep branch of ulnar nerve passing under pisi-uncinate ligament, and then under two of the adductores, a^5 and a^2; a^1, adductor pollicis; abd^5, abductor minimi digiti.

Fig. 2. Manus of Virginian Opossum.—a^1, a^2, a^4, a^5, adductores; a^5a and a^5a', parts of adductor minimi digiti whose insertion has wandered to ulnar side of fifth digit; *u*, ulnar nerve, its deep branch passing *under* the adductores; *fbdm*, flexor brevis digitorum manus reflected; f^1r, radial head of flexor brevis pollicis; abd^1, abductor pollicis.

Fig. 3. Deeper dissection of Manus of Opossum.—f^1 to f^5, flexores breves. The deep branch of ulnar nerve is shown passing superficial to all of these.

Fig. 4. Pes of Virginian Opossum.—*epn*, deep branch of external plantar nerve; f^1t and f^1f, tibial and fibular heads of flexor brevis pollicis.

Fig. 5. Manus of *Cynocephalus anubis.*—f^5r, radial head of flexor brevis minimi ("third palmar interosseous"); f^5op, opponens minimi derived from true flexor brevis; a^5op, opponens from layer of adductores; a^5a, "flexor brevis" minimi digiti; op^1, opponens pollicis; a^1a, part of adductor pollicis inserted into radial sesamoid; *t*, tendon of flexor longus pollicis.

Fig. 6. Thumb of Orang.—f^1r and f^1u, radial and ulnar heads of flexor brevis pollicis, having a common origin, but separated at their insertion by *t*, the slender tendon of flexor longus pollicis.

Fig. 7. Diagram of the adductores of the manus o a Chimpanzee.—d^4, fourth dorsal interosseous; a^1ob, adductor obliquus; a^1tr, adductor transversus; other letters as before. Observe that the origins of a^5 and a^5op are absolutely continuous for a short distance, and that a^5op and a^5a are united by half their length.

Fig. 8. Short muscles of the little finger in Man, showing an unusually large part of the opponens minimi digiti derived from the true flexor brevis; f^5r, "third palmar interosseous." The "flexor brevis" minimi digiti (of human anatomy) was absent in this case.

IRREGULARITIES OF THORACIC DUCT.

By F. A. G. DAVIS, M.B.

[Read in the Sub-section of Anatomy and Physiology, May 13, 1886.]

The irregularities of the thoracic duct which have been described are exceedingly various. It may bifurcate at a variable height and send a terminal branch to each side of the neck, or, having bifurcated, may unite again before its termination.[a] It has also been found entering the vena azygos major.[b]

Instances of a right thoracic duct terminating on the right side of the neck have been recorded by Allen Thomson,[b] Fleischman,[b] M. Watson,[c] Todd,[d] and Brown.[e] In each of these cases, with the exception of Watson's, some irregularity of the great vessels existed. In Allen Thomson's case there was a right aortic arch, while Todd, Fleischman, and Brown found a right subclavian artery arising from the arch of the aorta. The following is a short account of a similar arrangement which was observed at the School of the College of Surgeons during the past Winter Session :—The innominate artery was absent, the great vessels having arisen separately from the arch of the aorta, the order from right to left being right common carotid, left common carotid, left subclavian, right subclavian. The last vessel came off by a well-marked dilatation from the right aspect of the arch, about a thumb's breadth below the origin of the left subclavian artery; it then passed behind the trachea and œsophagus to its usual position on the right side. This artery gave off the ordinary branches, with the exception of the inferior thyroid, which arose from the right

[a] Todd. Ency. Anat., Vol. III., p. 225.
[b] Wutzer. Arnold. Quain, Vol. I., Ed. IX., p. 529.
[c] Journal of Anatomy and Physiology. Vol. VI., p. 427.
[d] Ency. of Anatomy. Vol. III., p. 232.
[e] Journal of Anatomy and Physiology. Vol. XVI., p. 301.

common carotid. The thoracic duct commenced by the union of a large number of branches in the usual position between the aorta and right crus of the diaphragm and beneath the right renal artery. No dilatation such as that usually described could be distinguished. Having passed through the aortic opening of the diaphragm the duct lay in the posterior mediastinum between the aorta and vena azygos major, and at the sixth dorsal vertebra received a large tributary from the left side. At the fourth dorsal vertebra it inclined slightly to the right, and passing upwards became related on its left side to the right pneumogastric nerve and to the irregular right subclavian artery, which vessel lay also on a plane posterior to it. Approaching the sterno-clavicular articulation it crossed in front of the right subclavian artery, and having passed beneath the junction of the internal jugular and subclavian veins, terminated in the usual manner by joining the upper margin of the angle of union of the two veins. The opposite side of the subject was carefully examined, but no left lymphatic duct was discovered.

The descriptions given by Todd, Watson, and Brown, are essentially identical with the above, excepting that Brown found a lymphatic duct in addition on the left side. The explanation of this vascular irregularity can be deduced from Rathke's arterial arches, and is already described in Henle's Anatomy—that is, that the fourth and fifth right embryonic arterial arches become obliterated, and that the right posterior aortic root persists and forms the right subclavian artery. It follows from this that there will be a considerable alteration in the position of the right recurrent pneumogastric nerve. In the normal condition this nerve passes round the right fourth arterial arch, which is ordinarily converted into the subclavian artery, but should the latter have its origin from a persistent right aortic root, the inferior laryngeal nerve passes directly from the pneumogastric trunk to the larynx, as has been recorded in many instances.

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[*The Names of Contributors are printed in* SMALL CAPITALS.]

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