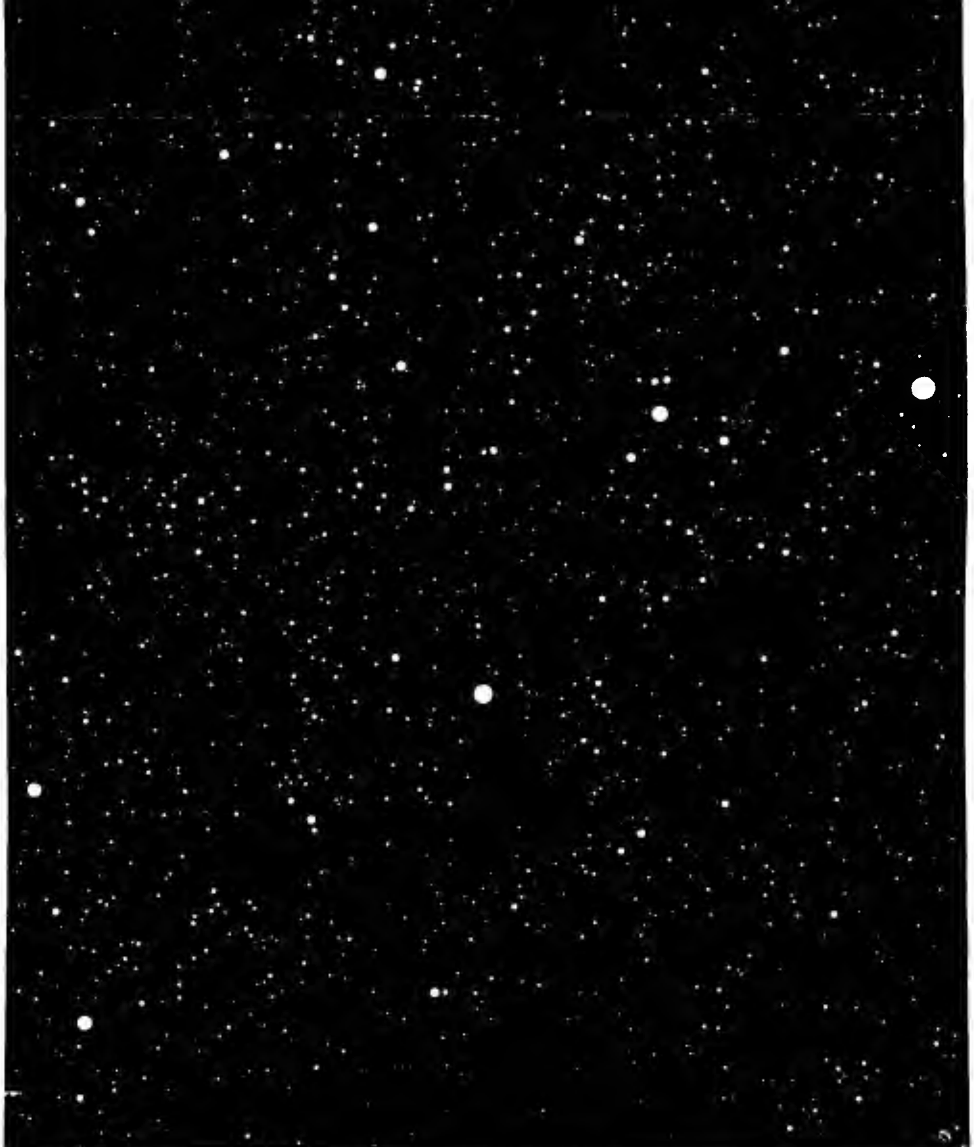
OTHER SUNS THAN OURS



PHOTOGRAPH OF A "RICH REGION" IN THE CONSTELLATION CYGNUS.

(Three Exposures of One Hour.)

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FRONTISPIECE. [*See page* 71

OTHER SUNS THAN OURS

A SERIES OF ESSAYS ON

SUNS—OLD, YOUNG, AND DEAD

WITH OTHER SCIENCE GLEANINGS, TWO ESSAYS
ON WHIST, AND CORRESPONDENCE WITH
SIR JOHN HERSCHEL

BY

RICHARD A. PROCTOR

AUTHOR OF "OTHER WORLDS THAN OURS," ETC.

"Other suns
With their attendant worlds thou may'st descry."
MILTON.

NEW IMPRESSION

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PREFACE.

TWO-THIRDS of this volume may be regarded as completing the preliminary study of astronomical facts and theories, which I have deemed necessary in the preparation of a treatise on general astronomy, announced long ago, when I had not as yet learned fully to appreciate the amount of investigation, discussion, and thoughtful study which the production of such a treatise would require. I have sometimes felt inclined to wonder at the ready rapidity with which such treatises have been produced in recent times until I have recognized that they have followed each other along well-worn tracks, each repeating (in company with the small quota of original matter) statements, ideas, and illustrations which have been common property for generations. I hope to show, in the course now of a few months, that what Tennyson has called the "New Astronomy" really is an advance on the astronomy of a quarter of a century since.

The correspondence with Sir John Herschel will, I believe, be found full of interest. His latest views on the subject of our galaxy are therein presented.

The essays on whist need no comment. They find a place here much as whist finds a place in my scientific magazine *Knowledge:* whist is not only a scientific game, it is the scientific student's best recreation. The article on English misuse of the letter H is also semi-scientific in character.

RICHARD A. PROCTOR.

ST. JOSEPH'S, MO.,
May 1887.

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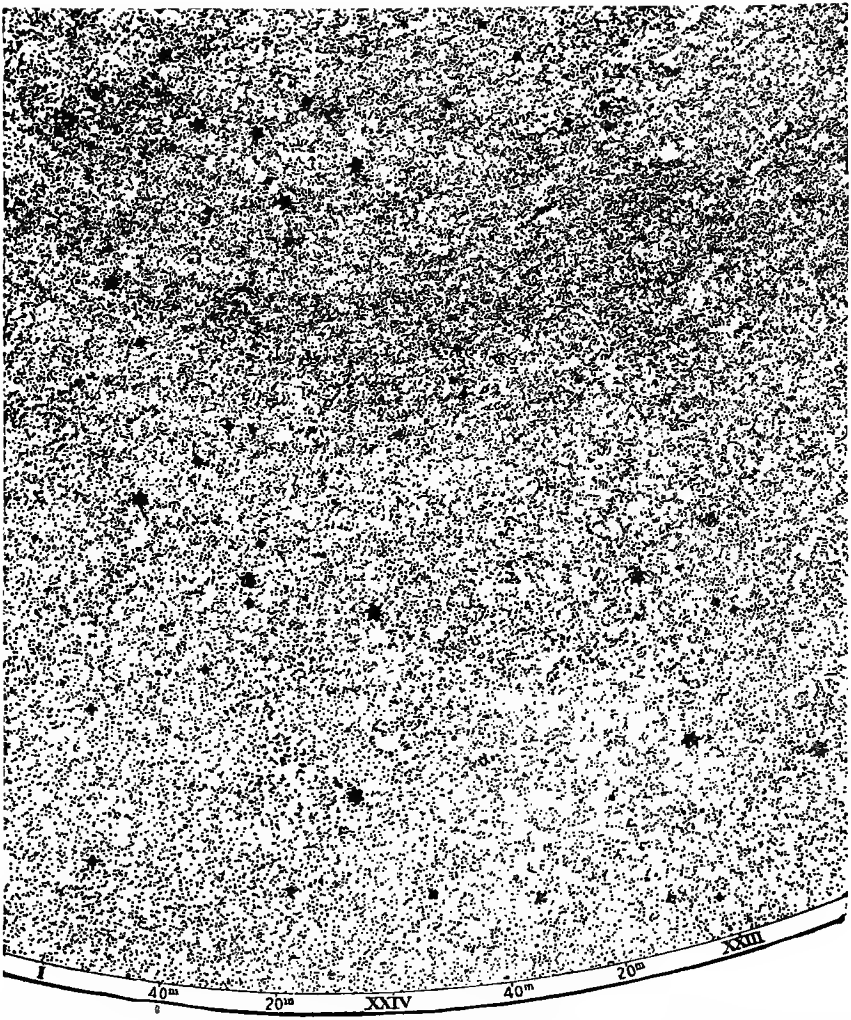

FIG. 1.—SECTION FROM MY CHART OF 324,198 STARS, SHOWING WHERE THE NEW STAR APPEARED.

(*For Plan, see opposite page.*)

FIG. 2.—KEY TO THE MAP OF MANY STARS GIVEN ON OPPOSITE SIDE.

[*To face page 1.*

OTHER SUNS THAN OURS.

CHAPTER I.

THE NEW STAR IN THE ANDROMEDA NEBULA.

ALTHOUGH the new star which has appeared in the Constellation Andromeda, in the very heart of the great Andromeda Nebula, is not in itself so remarkable an object as many of the new stars recorded in past years, it throws a stronger and clearer light than any of those orbs on the architecture of the heavens. It answers questions which have been asked for more than two centuries, nay, almost from the very year when the telescope was invented. It discloses much of the real nature of the galaxy—our "island universe," as Humboldt poetically called the stellar system; and though it is silent respecting the system of galaxies in which many astronomers have believed, it is more eloquent in silence than all the stars in the heavens have been in direct testimony.

I propose to consider the aspect in which we must hereafter view the galaxy, noting what had before been imagined, suggested, or proved, and how the new star illuminates the whole system of celestial architecture. But first it is essential that the true position of the new star should be recognized. I cannot hope for attention to the subject on which a new star in the Andromeda Nebula would assuredly throw light, if it remains doubtful whether the orb which appears to be in the heart of that nebula is really there or not. If the new star be not physically associated with the Andromeda Nebula, then it is only interesting as a new star of a somewhat novel order (as will presently be seen), not at all as throwing new light on the structure of the stellar universe. It might, for aught that its position actually proves, be many millions of miles nearer than the nebula, or many millions of miles farther away. In either case it would merit only such attention as astronomers gave to the new star in the Northern Crown in 1866, and to the less brilliant star which appeared ten years later in the Swan.

It will, of course, be admitted that the apparent agreement of the new star in position with the rich region of nebulous light within the large Andromeda Nebula, affords in itself a strong argument from probability that the star and the nebula are physically associated. It may be said that, so far as we know, a new star may appear *anywhere*, and that there can be no reason why the particular point on the star sphere where a new star appears should not be a point already occupied by nebulous light. So it might be said that since, as among all possible deals at whist, any particular arrangement is as likely to appear as any other, we ought not to be surprised if a particular deal gives all the trumps to the dealer, and to each of the other players all the thirteen cards of a single suit. Yet, as a matter of fact, we know that such an arrangement would surprise every one present; and would incline each to believe that before the deal the cards had been specially arranged. Nothing but the clearest evidence that there had been no special "manipulation" either before or during the deal would convince the observers that what they had seen had resulted from mere chance-coincidence. Their thought would be sound and just. As a problem in what is called inverse probability, an explanation which would account for the observed strange fact without requiring that one chance among many thousands of millions had come off would antecedently be far more likely than one which involved so strange a coincidence. So, assuredly, it is in the case of the new star in the Andromeda Nebula. We cannot but be surprised—regarding the matter at first as depending on chance-coincidence—that so unusual an object as a new star should make its appearance in the midst of the richest part of the light from a nebula so remarkable that it has been called by astronomers in former times "the transcendently beautiful queen of the nebulæ." In any nebula a new star would be strange, insomuch that few astronomers would feel any doubt about the existence of some connection between nebula and star. But a new star in the most wonderful and beautiful of all star cloudlets!—certainly that would be a strange chance, if it could be shown really to be casual. Nothing but the clearest evidence could convince the astronomer that this was the true explanation—though it must be admitted also that nothing but clear evidence of physical association, outside the evidence derived from position, would justify any astronomer in saying that such a physical association was a demonstrated fact.

We naturally turn to other new stars to see if they have given any evidence bearing on the peculiar position of the new star in Andromeda. If, for example, we find that new stars, or variable stars (for most new stars are only stars which vary greatly in lustre) affect nebulæ, we shall be prepared to admit that the evidence of physical association between the new star and its nebular surroundings is greatly strengthened.

The only new stars we need consider are those which have appeared in recent times, since it is only within recent times that the greater number of the nebulæ have been discovered.

The star Eta Argûs must be regarded as one of those varying sufficiently in splendour to be within our category. This star ranges in brightness from bare visibility to a lustre exceeding that of every star in the heavens except Sirius alone. In 1840 it was shining with this maximum splendour. It had risen to its greatest glory by a succession of leaps, with occasional retrogressions. After 1840, Eta rapidly diminished in brightness, then faded more gradually, until it reached its present lustre, that of a sixth magnitude star. It is manifest that if we were at a greater distance from this strange variable, it would be now invisible, and therefore its return to the splendour of 1840 would correspond with the appearance of a new star in the heavens. This star, then, may throw light on the question whether nebulæ and new stars are apt to be associated.

The apparent association between Eta Argûs and a nebula is of the most striking kind. Eta is in the very brightest part of the most remarkable nebula in the southern hemisphere—the far-reaching mass of star-mist known as the Keyhole Nebula (because of the peculiar shape of a dark space in its midst). It had been held to be possible, but barely possible, that the association between Eta Argûs and the surrounding nebula was only apparent, not real. It is hardly necessary to point out that this extreme improbability—that the most remarkable variable star in the southern heavens should be apparently in the very heart of the most remarkable southern nebula, yet not really connected with it, is immensely increased by the appearance of a new star in the midst of the most remarkable northern nebula. The corresponding improbability in the case of the new nebula in Andromeda does not double the resulting improbability, but introduces an improbability many times greater.

The chance is one in about a million that, with fair tossing, "heads" will be tossed twenty times running, and, of course, it is extremely improbable that this will happen at a first trial. If it happened, we might reasonably suspect that the tossing was not fair. If it happened at a second trial, the strength of this suspicion would not be doubled, for the chance of twenty heads coming in each of two trials is not one in about two millions (or twice the number in the case of a single trial), but one in about a million millions)—not a million and a million added together, but a million and a million multiplied together. Similarly in the case of new stars and nebulæ, the occurrence of two cases where a new or highly variable star is associated with a nebula, increases many thousandfold the probability that such association is not accidental, but real.

Let us, however, consider other new stars.

The new star in the Northern Crown was not associated with any nebula. In its case, therefore, we find no evidence confirming, though also (I need hardly say) we find none disproving, or even tending towards disproving, our belief (now almost certainty) that the new star and the nebula in which it has appeared are physically associated.

But the third case—and there are but three—is decisive.

In 1876 a new star of the third magnitude appeared in the constellation Cygnus. Like all new stars it gradually lost its newly acquired lustre. But unlike Eta Argûs and the new star in the Northern Crown, the new star in the Swan eventually disappeared—as a star—altogether. Where it had been there was no star, *but*—there was a nebula! The nebula was, or rather is, a very faint one—a circular bluish disc, so faint and wan that it would escape notice, had not the new star pointed to it. Doubtless it had escaped notice before the star appeared. For no one supposes that a new body had really come into existence when that so-called new star shone out. Even the regular sky-surveyors, who as a rule are very hard to convince, did not imagine anything quite so unlikely as that. The nebula had been there—a faint planetary nebula as now—but it had escaped notice, situated as it is in a singularly crowded region of the heavens.

Here, then, was a case where not only had a new star appeared in the midst of a nebula, but where it had made a nebula known to us by so appearing, and had afterwards subsided so completely into the bosom of its parent nebula that no sign of it could any longer be seen.

Two, then, out of the only three new stars available for our inquiry have been in the most remarkable manner associated with nebulæ. Scarcely one was really needed to convince us that the new star is associated with the remarkable nebula in the midst of which it has appeared; considering that there are but three new stars to deal with we might very fairly have expected that not one would give us any satisfactory evidence; but as a matter of fact two out of the three give evidence, and in each case (unlike though the two cases are) it is evidence of the most remarkable kind.

To believe in the face of such testimony as this that the new star is not physically associated with the nebula in which it seems to lie, would not be scientific caution but dull and obstinate incredulity. This, however, is by no means all the evidence. Nay, overwhelming though this evidence is, it is by no means the strongest part of the evidence.

The new star in the Swan owed the chief part of its lustre—and towards the close *all* its lustre—to matter of the same nature as the gaseous planetary* nebula into which it faded away. It shone at

* It may be necessary to explain that this word "planetary"

the last with only three tints, those with which the great Fish-mouth Nebula in Orion, and the great Keyhole Nebula in Argo, are observed to shine. I make no doubt whatever, that if a telescope, armed with a stellar-spectroscope, could have been turned on Eta Argûs in 1840, that resplendent orb would have been found to shine—during its exceptional lustre—with the same kind of light which comes from the nebula around it (besides its own customary stellar light). But evidence of this kind was not then available.

If the new star in Andromeda, like the new stars in the Northern Crown and the Swan, showed the signs of gaseous matter, glowing with intense lustre, we should be no wiser as to its physical association with the Andromeda Nebula, than without spectroscopic evidence: for the Andromeda Nebula is not one of the gaseous kind. The chances were that, formed suddenly as it had been, the new star would give a spectrum of bright lines indicative of gaseity. This would have decided nothing; for the star in the Northern Crown, as surely a star as our sun, and now for nearly a score of years shining steadfastly with its original stellar lustre, yet shone for several weeks with light giving a spectrum quite unlike the normal spectrum of a star; and, which is still more to the purpose, quite unlike the spectrum of that star itself in its normal condition.

It was antecedently very unlikely that the new star would show the same spectrum as the nebula in Andromeda; for the simple reason that not only is this spectrum very unlike the spectrum of any new star yet observed, but it is a very strange, one may

relates to the aspect of the nebulæ, thus termed. They are by no means planetary in the sense of being wandering bodies; but they present round discs like the planets. Indeed, though very faint and dim, these bodies look much like some very distant planet, only faintly illuminated by the sun's rays.

say a unique spectrum. It is not like the spectrum of a star, which is a rainbow-tinted streak crossed by dark lines. It is quite unlike the spectrum of a gaseous nebula, which consists entirely of three or four bright lines. It is not like the spectrum of the new star in the Northern Crown, which was a rainbow-tinted streak, crossed by dark absorption lines, but also by four intensely bright lines, belonging to the intensely hot gases from which the greater part of that remote sun's light came during its outblazing. Nor is it like the spectrum of the new star in the Swan, at any stage of that star's existence.

The spectrum of the Andromeda Nebula is a rainbow-tinted streak, without any bright or dark lines, and only giving evidence of the absorptive action of vapours by a sudden degradation of light near the red end. An unlikely spectrum, one would say, for a new star, even though that new star had its birthplace in the very heart of the Andromeda Nebula.

Yet, quite unexpectedly, this peculiar spectrum is just what the new star gives when its light is sifted by the most marvellous of all the instruments of our time—the spectroscope. Carefully observed by spectroscopists of proved skill, and under atmospheric conditions so favourable that the delicate light of the gaseous nebulæ showed its characteristic spectrum of bright lines, the new star was found to give no bright lines at all, but the same dull continuous rainbow-tinted streak, suddenly degrading at the red end, which is given by the great Andromeda Nebula itself.

Even this is not quite all, though this is much more than enough to prove that the new star is physically associated with the nebula. The condensed part of the Andromeda Nebula had been more than suspected of change during several weeks before the new star appeared, and has been observed to change considerably since that event. The new star itself varies from time to time in aspect. On the 3rd of September it was not so sharply defined as it had been, but had a somewhat hazy aspect; in technical terms, it was impossible to bring it to a disc. The fault was not in the air, for other stars came sharply into focus. On the following night the new star presented the usual appearance of an eighth magnitude star, neatly and sharply defined. Moreover, the new star faded gradually in lustre from the time of its discovery.

The new star, then, is in reality, as well as in appearance, in the heart of the great nebula, and we have to consider what, so situated, the strange orb may mean.

The new star's teaching, though not new to one or two who had reasoned out from other evidence the same results, is altogether new for those who have retained their belief in the doctrines impressively taught by Humboldt, Arago, Nichol, and others, but never definitely adopted (as they supposed and as is commonly supposed) by Sir William Herschel—the greatest astronomical observer the world has known.

In his surveys of the celestial depths, Sir William Herschel recognized about three thousand star-cloudlets of various orders. Sir John Herschel, second only to his father among astronomical observers, completed the survey of the celestial sphere from a station near Cape Town, and added so many more star-cloudlets as to bring up the number to close on five thousand. At present, with all the work of other observers, that is still about the number of known nebulæ.

Sir W. Herschel suggested many ideas about these objects, or rather many ideas occurred to him in the course of his labours. It may surprise many who know of Herschel's views only through Arago, Nichol, Guillemin, Flammarion, and others, or through Humboldt, whose knowledge of Herschel's work was derived from imperfect abstracts, to learn that Herschel regarded the rich nebular region in the Constellation Virgo, as consisting of the fragments of a vast galaxy able to extend itself nearer towards us there, because on that side our galaxy has its least extension. It is so commonly believed that Herschel regarded all the stellar nebulæ as external galaxies, that this suggestion, according to which hundreds of such nebulæ are only fragments of a part of what was once a great galaxy, is worth studying, if it were only to show how little is popularly known either of the great master's wonderful work or of his daring conceptions.

In 1817 and 1818, when Herschel was very old, at the actual close, indeed, of his observing career, he threw out ideas as to the possible distance of stars and star-cloudlets, if estimated by their light, and by the power of the telescope necessary to resolve star groupings into discrete stars. Then it was that he showed how—*if we so judge*—some of the nebulæ must lie at distances so great that light cannot come to us from them in less than thousands—nay, in some cases, hundreds of thousands—of years. Many of the stellar nebulæ, estimated in this way, must be considered outlying galaxies, as grand perhaps as our own, or even grander. But Sir W. Herschel did not live to test, as he had proposed to test, this particular method of gauging the celestial depths. He made numbers of observations preparatory to the work of testing it; he obtained multitudes of results which, judged by the suggested criterion, possessed a most marvellous interest; but he was not able to test the criterion, as he had before tested his first gauging method.

That first method, it will be remembered, was based on the assumption that the stars which form our stellar system are strewn with general uniformity alike as to size and number throughout all parts of the system; and it was by this criterion that he proposed to measure the extension of the galaxy in different directions. Had he died after collecting his gauges, without using them to test his criterion, it might have remained an accepted truth that he had been content with it. Fortunately, besides collecting those valuable results, he examined and interpreted them, announcing in so many words: "I am now convinced, after my long and careful examination of it, that the stars which form the Milky Way are very differently arranged from those in our neighbourhood."

Had Sir W. Herschel lived to test his second method* of gauging the star-cloudlets, he would have seen that the criterion on which it depends is unsound, the results to which it leads being incongruous, as will presently be seen.

But unquestionably the results which Sir W. Herschel published in 1817 and 1818 justify the belief that, as interpreted by his criterion, large numbers of the nebulæ must be regarded as external galaxies. This grand conception fascinated the minds of astronomers, and of some who, like Humboldt, though not themselves astronomers, yet had under-

* So carelessly have Herschel's papers been read—or skipped through—by those who most confidently quote them, that the very existence of two methods of gauging in his descriptions of his plans has not been noticed. When the point is mentioned, answer comes readily that the two methods are practically the same. How far this is the case will be seen when I note that in the first the same telescope was used throughout, the gauging test depending on the number of stars seen in different directions; while in the second telescopes of many degrees of power were used, the gauging test depending on the resolution of one and the same star grouping. One method was employed because the other had failed. Yet in some books the result obtained by both methods are hopelessly mixed together.

stood and appreciated the work of the great observer. The idea of "island universes" strewn throughout the ocean of space impressed the world by its overwhelming suggestions of vastness alike in space and time and energy.

It was Mr. Herbert Spencer who first showed the incongruity of the results which Sir W. Herschel had collected in those papers. On the one hand, we learned that our galaxy is in parts absolutely unfathomable, insomuch that even with Herschel's most powerful telescopes there still remained regions of irresolvable milky light, though with each increase of power more and more stars had been revealed. On the other hand, we were told of outlying galaxies, similar to our own in structure, but lying at distances many times exceeding its whole span, and these galaxies were found in many cases to be resolvable into stars. According to the very principle on which the second method of gauging distances was based, these galaxies being resolvable are much nearer than the outer parts of our own galaxy. So that if the principle be accepted these resolvable star-clouds are not external: if they are external, the principle of the second method of star-gauging must be rejected, and all the results based on it must be re-examined.

Another objection, almost as fatal, was noted by Mr. Spencer in the same remarkable essay (on the "Nebular Hypothesis"). Sir W. Herschel noticed that when in surveying the star-depths he came to regions where stars were few, nebulæ were almost sure to be found. On such occasions he would call to his assistant (his elder sister, Miss Caroline Herschel) "Prepare to write, nebulæ are about to appear." Strangely enough this peculiarity has actually been adduced as affording evidence that the nebulæ are outside our system, as though insects in a tree who should notice that as they pass from branches to twigs, and from the larger twigs to smaller ones, they came upon leaves, should regard this as affording proof that the leaves do not belong to the tree. Of course, as Mr. Spencer points out, this peculiar relation proves, if it be shown to be a real and not an accidental relation, that the nebulæ belong to the same great system as the various orders of stars.

In the years 1867–70, unaware of Mr. Spencer's prior observations, I noted the same objections and several others. I made equal-surface charts, indicating the distribution of stars and nebulæ. These showed in the clearest possible way that not only do the nebulæ appear in small regions clear of stars, such as Sir W. Herschel was dealing with, but that on the larger scale also the same peculiarity is presented. In my maps the streams of the Milky Way appear like the branches of a tree, the nebulæ occupying the region around like the twig-work and the leaves of a tree around the branches. Classifying the nebulæ, the peculiarity of arrangement was still more significant, and still better corresponded with the illustration I have just used. The coarser clusters were found near the Milky Way, as the larger twigs lie near the branches of a tree; the closer clusters were farther away, while the irresolvable nebulæ were found in greatest profusion far from the rich galactic regions. But the irregular gaseous nebulæ were all found on the Milky Way, so that they might be compared to ivy clinging close around the great branches of our illustrative tree.

But there is one region of the heavens where this relation is exactly reversed. In the two Magellanic Clouds, or Nubeculæ, which look as if they were great masses that had floated off from the Milky Way, there are all orders of stars, from those just beyond the reach of the naked eye, down to star-clouds of milky aspect, which no telescope can resolve. In this respect the Magellanic Clouds resemble those parts of the Milky Way which Sir W. Herschel found unfathomable. But in both clouds are found all orders of star-cloudlets. There is even an example of the great irregular gaseous type—the celebrated Lover's Knot Nebula in the Sword Fish. In my maps of nebulæ the Magellanic clouds are actually *pictured* by the great gathering of nebulæ over their areas. In this respect the Nubeculæ are entirely unlike the Milky Way.

Dr. Whewell (in his "Plurality of Worlds") was the first to show that this peculiarity requires us to re-examine the theory that the nebulæ are external systems. Their gathering on the Magellanic Clouds is certainly not accidental or only apparent, their real positions being far beyond the stellar parts of the Nubeculæ. These great clouds are certainly rounded in real shape, and therefore their remotest parts not farther than their nearest in greater degree (determined from their apparent size) than as ten exceeds nine. Hence, within these narrow limits of distance, we have all orders of stars, from the seventh to the twentieth magnitude and beyond, and all the various orders of star-cloudlets. These star-cloudlets then, at any rate, are not external galaxies.

Is this peculiarity inconsistent with the other, so that it tells a different story? On the contrary, it tells precisely the same story. In the segregation of nebulæ from star-streams we have what corresponds to the view of a tree from within, as by insects, seeing leaves plentiful at some distance from the branches, but not close to them. In the aggregation of nebulæ within the Magellanic Clouds we have what corresponds to the view of a tree from without, as by men who see branches, twigs, and leaves as all parts of one system.*

* That we are outside the Magellanic Clouds is clear from their appearance—but proved in another way by Sir John Herschel's observation that "the access to the Nubeculæ is on all sides through a desert."

To consider no further evidence, it had in reality been demonstrated, as I pointed out in my lecture on "Star Drift, Star Clouds, and Star Mist," at the Royal Institution in May, 1870, that the nebulæ of the stellar sort are not external galaxies. But reasoning such as had hitherto been employed has very little influence even on students of science. The sun's corona had long been proved by reasoning, based on evidence already obtained, to be a solar appendage, before the success of the photographers during the total eclipses of 1870 and 1871 convinced every one that it is so. And in like manner Mr. Spencer's arguments and my own, demonstrative though they were, convinced few, being "caviare to the general."

The new discovery just made, however, or rather the event which has just taken place, can be misunderstood by none. A new star in the midst of an external galaxy would require to be many millions of times larger than our sun to be visible as the new star in Andromeda has been. The changes of lustre shown by the star would signify changes of energy, in the way of increase and diminution, the least of which would correspond to all the energy shown by our sun during hundreds of thousands of years. Even admitting for a moment that the new sun might be such an orb as this implies, yet the existence of such a sun within the great nebula in Andromeda would of itself show that that star-cloud cannot be a galaxy in the slightest degree resembling our own.

The new star tells us, then, that the Andromeda Nebula, as its position had long since shown the more observant, lies within the limits of our galaxy. This strange mass of matter, not gaseous yet not stellar, a vast cloud perhaps of cosmical dust maintained at intense heat in some way as yet unknown, surrounded by other cloud-like matter capable of intercepting the red rays, much as dust in our own air intercepted and reflected the red rays of the sun when we had those marvellous coloured sunsets, tells us of one of the varieties of constitution and aggregation existing within our stellar system. We have every reason for inferring that the new star appeared in a part of the galaxy which is probably the nearest to our solar system. In this part, from Orion to Cepheus, the five nebulæ visible to the naked eye are all found. Two are shown in the illustrative maps (figs. 1 and 2), and these objects are probably as near to us as, on the average, are the lucid stars (*i.e.*, those visible to the eye), if not nearer.

The picture of the Andromeda Nebula (fig. 3) is from the view made with the Harvard telescope by Trouvelot. The new star is not quite central, but it is in the very heart of the nuclear region of the nebula. It is no external galaxy but a part—a strange and highly interesting but in reality a very small part—of our own galaxy. So it will now be admitted are its fellow nebulæ of all orders.

Our stellar system, or galaxy, presents itself then to us in a new aspect. Like a mighty tree it spread broad arms, the stellar branches and streams and closely gathering aggregations which form the complicated wealth of the Milky Way. But the main stem is found in the four orders of isolated suns, the giant suns like Sirius and his fellows, the suns like our own, the suns which show signs of darkening vaporous envelopes, and doubtless multitudes of dead suns. These are the chief though not the most numerous bodies in the galaxy. Even among these we find other varieties, in single, double, triple, and multiple suns, separate suns of all colours, sun groups presenting the most singular and beautiful combinations of colour. But as we pass to the borders of the Milky Way we find other varieties of structure. Here we have diffuse clusterings of stars,

FIG. 3.—THE GREAT NEBULA IN ANDROMEDA.

To fac

farther off we find definite clusters of many orders, the least compact and most readily resolvable being on the whole nearer to the Milky Way than those in which the texture is finer or closer, while the irresolvable nebulæ tend to the regions remotest from the galaxy. Other forms of star-cloudlet there are also, which cannot be described as mere clusters. Then there are all the varied orders of real nebulæ—ring-shaped, spiral, planetary, and irregular. Doubtless, also, as our survey is continued, fresh forms of structure will be recognized, till men are disposed to smile at the cloven flat disc of uniformly strewn suns which has so long done duty for our amazingly complex galaxy. We may still believe in external galaxies, though none may be within the ken of our telescopes, incompetent as yet to reveal the full extent and the full glory of our own star-system. But those external galaxies do not repeat the uniform and scarcely interesting galaxy we formerly judged ours to be, but our galaxy as it really is, infinitely complex in structure, immeasurable in extent, and to our conceptions full of infinite and everlasting vitality and energy.

CHAPTER II.

THE BIRTH OF WORLDS.

THE new star in Andromeda has been popularly regarded as probably a new world. This, whatever else it may be, it assuredly is not. In like manner the new star in the Northern Crown was popularly regarded (by persons unversed in science) as a world in flames. Stars are of course not worlds, whether they be new or temporary or simply variable. The idea gains ground steadily that all so-called new stars—even the glorious orbs seen in remote times, which outshone Sirius in splendour—were but variable stars, with a somewhat exceptional range of variation, and probably of very long period. If the star Mira or Wonderful, in the constellation Cetus, were so situated that when at its faintest it was visible as a third-magnitude star, it would outshine all the stars in the heavens when at its *maximum* of splendour. So would Eta Argûs, and so also would the so-called New Star in the Northern Crown. Indeed, if we regard the nebula in Andromeda as lying farther away than the faintest star visible to the naked eye, then, were we brought so much nearer that its distance was only that of a first-magnitude star, the *nova stella* (probably but a *stella mutabilis*) which shone out recently in its midst would have been resplendently visible instead of needing a telescope for its detection.

Neither this star, nor any other new variable, or temporary star ever observed, can be said to have thrown the least light on the birth of worlds. Certainly, if the nebular hypothesis of Laplace represents the real way in which solar systems are formed, no new star has thrown light upon that process, or possibly can ; for the process imagined by Laplace involved no catastrophes. It was a steadily acting process, rather leaving nebulous rings behind than throwing them off as commonly supposed ; the rings separated into parts as they shrank longitudinally by a gentle movement, and the various fragments coalesced rather than collided, for they were all travelling the same way round ; in fine, Laplace imagined no fierce conflict of matter with matter such as the sudden outburst of splendour in what we call a new star necessarily implies.

It might be well, however, if the interest excited by the new star, though it may throw no new light on Laplace's hypothesis, should direct some degree of attention to the very remarkable defects which any astronomer who knows aught of physics, or any physicist who knows much of astronomy, cannot fail to recognize in that remarkable speculation Attracted by the effective way in which some features of our solar system for which the theory of gravitation cannot account, appear to be explained by Laplace's hypothesis, many astronomers overlook the startling difficulty which Laplace overleapt at the outset. On the other hand, many physicists are unaware that the hypothesis started from what, with the knowledge of physics obtained since Laplace's time, is seen at once to be an absolute impossibility ; they know only that a number of astronomical facts appear to require some such theory ; of the details which are also required (but which a physicist at once sees to be quite impossible) they know little.

Let us consider how the theory of Laplace was suggested and what the theory required, premising that if the basis of the theory shall appear to be more than unstable that involves no discredit to Laplace, seeing that in his days certain physical laws which are now among the axioms of science were not even suspected. We may take, as an example of what Laplace could and could not do, that masterpiece of mathematical analysis, his inquiry into the stability of Saturn's ring-system. Here the mathematical work was almost perfect, and the conclusion, that the rings must be narrow and eccentrically weighted, was demonstrably right, on the assumed premisses; but these premisses were erroneous. A knowledge of physical laws such as Laplace could not have, but such as many boys in our times have acquired, would have shown Laplace that the rings of Saturn could not be what he assumed them (quite unquestioningly) to be, at the very outset of his inquiry. Solid rings on the scale of the Saturnian system could no more remain unbroken under the forces to which they are subjected than a model of the Menai Bridge, perfect in all other respects, but on such a scale as to span 100 miles, could bear its own weight. In this case, where not a theory, but a magnificent calculation of his, was in question, science has not hesitated to set Laplace's conclusions aside, because of the falsity of his assumptions, adopting, instead, the results which Clerk Maxwell, Pierce, the Bonds, and others have established—viz., that the Saturnian rings consist of myriads of tiny satellites, like sands on the sea-shore for multitude. But, strangely enough, in the case of his far-famed (chiefly because so imposing) hypothesis of the birth of worlds, which starts from a similar, or rather from a much more monstrous, mistake (very natural, though, in Laplace's time), science has scarcely even questioned his results, far less examined his initial assumptions.

The facts which the nebular hypothesis of Laplace was intended to explain are simply these:—The planets travel the same way round, and in nearly the same plane (all but the zone of minor planets, whose entire mass is less than the tenth part of our earth's). The central sun turns the same way on his axis, so do all the planets whose rotation has been observed ; all the moons travel round their ruling planets the same way—except the moons of Uranus (known, it must be remembered, to Laplace) and the moon of Neptune ; and these bodies, travelling as they do at the very outskirts of our system, may be regarded as having, perhaps, been exposed to disturbing influences affecting, in their case, the action of the laws, whatever they were, which gave these features of uniformity to our solar system. Laplace suggested, as a hypothesis which seemed to him to result (*une hypothèse qui me paraît résulter*) from these features, that the whole mass of matter out of which the solar system was formed was once an immense disc, extending beyond the path of the remotest planet now known, and rotating as one gigantic whole. Granting only this assumption, and starting from it, all the features of the solar system mentioned above would follow. The ring would gradually shrink as its heat was radiated into space, until the outer parts, retaining their original velocity, could no longer cohere, but would be left outside in the form of a gigantic ring. This ring, as it further shrank (along its length now), would dissolve into fragments, and these would eventually coalesce into a single planet, the outermost. Then another would be formed in the same way, and another, and yet another, until at last there would be left, in the middle, the great mass that was afterwards to govern that family of worlds. Each planet, at its beginning, being like the original gaseous disc, would go through a similar process of contraction, and form as many bodies subsidiary to itself as its quantity of matter and the conditions under which it had itself been formed would allow. The process would fail in some cases, and so several small planets would be formed instead of one large one, as we see in the case of the asteroids ; or, as in the case of Saturn's system, a ring or set of rings (rings of small satellites, as we now know) would form instead of a single large satellite.

Laplace's theory, if we grant its initial assumption, accounts fairly for all the features of the solar system, except the singular distribution of the planets into families :—the giant planets outside, as if guarding the rest ; the terrestrial planets near the sun, as if under his protecting wing ; and the asteroids or minor planets in the mid space between these families, as if keeping them apart. But unfortunately the initial assumption, on which the whole theory depends, is as utterly inadmissible as the theory that Saturn's rings might conceivably be solid. It is almost inconceivable how amazingly impossible that initial assumption is. Few probably know that a solid disc of steel, extending only to the earth's orbit, could not move as a single mass. If the central part of such a disc—say a region as large as the sun's globe—were set rotating as by some mighty hand, the outer parts would not feel the impulse until more than ten months had elapsed. But imagine a disc extending to the orbit of the planet Neptune, thirty times farther from the centre than is the earth's path. Imagine, further, such a disc-shaped region of space, not occupied by a mighty mass of the stoutest steel, but by a vaporous mass many thousands of times more tenuous than the air we breathe. It is such a disc that we have to imagine, according to Laplace's theory, rotating as a single mass. No argument is really needed to show that this is absolutely impossible. But it is a truly remarkable circumstance that, while a mathematician like Clerk Maxwell did not hesitate to point out (with perfect justice, be it remarked) that the solid flat rings which Laplace recognized in the Saturnian system, because they seemed to be plainly visible there, would be absolutely plastic under the forces to which they were exposed, astronomers and physicists have been apparently afraid to acknowledge that a vaporous disc such as he only imagined, a disc rarer than the rarest known gas, so vast that the whole Saturnian system would be but as a speck by comparison, and moved by far mightier forces than act on that system, could have no coherence whatever, and could not possibly even begin to behave as Laplace's theory required. If the mere mathematician had been thus weak, we might not have wondered, for mathematicians often rejoice over problems depending on impossible conditions—perfect rigidity, absolute uniformity, entire absence of friction, and so forth. But physicists and astronomers have usually required conditions more in accordance with the actual workings of nature.

CHAPTER III.

WILLIAM HERSCHEL'S STAR SURVEYS.

"Cœlorum perrupit claustra."—*Herschel's Epitaph*

AMONG the researches that I should wish to live to see undertaken by astronomers, and especially by astronomers capable of applying photographic methods to the work, I regard with particular interest the survey of the stellar depths, in accordance with the original ideas of Sir William Herschel, but on principles such as he by no means supposed to be correct when he began his labours. It has been unfortunate for the work of research in this direction that Herschel's ideas and results during forty years of observation have been dealt with, by astronomers who came after him, as though they had been presented in a single treatise and indicated his views at some one given time. In a sense, there is something singularly appropriate to the grand subject with which he dealt in the particular quality of the picture that we have received from his hands. The starlit heavens present a similar diversity in regard to time. We find it difficult, nay, impossible, to conceive that the stars as we see them are not as they actually are, nor even as they were at any given time. We do not see any star in its true place, even after correction has been made for such effects as are produced by atmospheric refraction and aberration of light. For each star is rushing swiftly through space, changing its apparent position in the celestial sphere, and although, owing to the enormous distance of each star, the apparent movement is not perceptible by ordinary eye-sight in less than hundreds of years, yet as light takes many years in reaching us from any star, it remains strictly true that the position apparently occupied by a star is not its real position, but one that it occupied long ago. Again, we do not see any star with its real light at the moment, but with that (by no means necessarily the same, even in amount) which it emitted many years ago. Even this is difficult to conceive; but this is little. Each star tells us of its history at a particular time, corresponding to its distance. Yonder bright star shows us its position and lustre a score of years since; the less brilliant orb apparently close by, lies so much farther off that we must assign the news it brings us to at least a century ago. But many even of the brighter stars lie at much greater distances; while, when we pass to the fainter stars, we must often have to consider light-journeys of many hundreds or even many thousands of years. If we regard the telescopic view of the heavens as the real view presented to the eye,—at least, to the mind's eye of science,—we must recognize, in the case of the faintest stars seen by the most powerful telescopes, such vast distances that light cannot have come to us from those stars in less than hundreds of thousands, perhaps millions of years. So that the scientific view of the universe of stars has as wide a range in time as in space. We have no picture of the galaxy as it actually is, or even as it was, but of different parts inextricably intermingled, and at different, and very widely different, periods of time.

But science enables us to correct the mistaken idea that in the stellar heavens we see the universe of stars as it is at this very time. Though the mind may never be enabled to conceive the reality, and is, indeed, hopelessly unable even to approach the conception, yet the reason has been convinced long since that the stellar heavens tell the amazing story of vast realms of space and enormous durations of time, which modern astronomy has in part been able to read.

It is not very wonderful, but it is interesting and significant, that the labours of the man that has done most to bring the great problem of the star-depths before us should have been misinterpreted somewhat as we are so apt to misinterpret the heavens themselves. Writers even so able as Humboldt and Arago take statements from this and that part of Sir William Herschel's long series of papers, and set them side by side in the same page, or even in the same paragraph; nay, I have seen such statements wrought into a single sentence, when in reality they belong to entirely different parts of Herschel's process of inquiry, or even present entirely distinct views on the particular matter to which they relate. Although my chief work has long been to try to put myself in the position of those who are apt to make mistakes, in order that I may be the more successful in correcting such errors, I still marvel how so gross a mistake can have been made.

Sir William Herschel suggested, in the course of his career as an observer of the stars, two entirely distinct methods of gauging the star-depths. They were so different in character, that—to take but one point of difference—one depended on the use of one and the same telescope throughout, while the other required that a series of telescopes of gradually increasing power should be employed. Yet, not only have superficial readers overlooked the characteristic difference between the two methods, and the reason why one method gave place to the other, but even those that have professedly undertaken the work of analyzing and abstracting the labours of the great astronomer of Slough, have fallen into the same preposterous mistake. I know of only one, Wilhelm Struve of Pulkova, who has clearly recognized and insisted upon the difference between the two systems of space-gauging that were employed by Sir William Herschel at the beginning and toward the close of his marvellous series of observations. Even Struve failed to recognize clearly that Herschel never did more than sketch in outline the results that would have followed from his second method of gauging, interpreted in a way that seemed to him likely to be sound and just. Herschel was too old to do more; and, apart from this, it may be said that he left those who came after him not only to apply the method fully, but even to interpret satisfactorily the few results that he had himself been able to collect.

Every one knows the nature of the system of star-gauging that Herschel at first adopted; in fact, it is the only one about which the great majority of students of astronomy know anything. It was the method suggested originally by Wright of Durham. Supposing all the stars visible in the telescope to belong to a certain system of stars tolerably uniform in size and distribution throughout (our sun being one of them), it is easily seen that if the telescope we use brings into view all parts, even the remotest, of this star-system, we can determine the shape of the system with considerable accuracy. For, in whatever direction we turn the telescope, we shall see a number of stars, greater or less according as the boundary of the stellar system in that direction is farther or nearer. Wright of Durham applied this method of gauging, with a telescope of moderate power, with results closely resembling those that are presented to this day as among the chief triumphs of Sir William Herschel's entire series of labours. Wright found so many stars in the direction of the Milky Way, compared with the numbers seen in those parts of the sky that are free from milky light, that he was forced to assign a much greater extension to the stellar system in the direction of the Milky Way than elsewhere. Forced, at least, when we consider the assumption on which his inquiry had been based; for of course there were several other available explanations of the observed facts. Thus Wright was led to enunciate the theory, commonly attributed to Sir William Herschel, that the stellar system has the shape of a gigantic flat disc of stars, tolerably uniform in distribution. The Milky Way being divided into two streams along a part of its course as known to Wright, it was necessary to assume that the disc was cloven throughout half of its extent.

Sir William Herschel, making a more careful survey on the same plan, but with a much more powerful telescope, found that while in a sense this cloven flat disc theory was supported by the results he obtained, it was yet necessary to assign a much more complex figure to the stellar system, so long as the results of his gauges were interpreted in accordance with the assumptions suggested by Wright. It became clear that on these assumptions the bounding surfaces of the flat star-system were by no means smooth. Instead of a section of the stellar system through its centre (near our sun) and at right angles to its median plane being bounded by straight lines, the outline must be of the most irregular form. Herschel drew one of these sections, which presented a shape somewhat like that of a long, dentate leaf. He appears not to have been at all struck by the peculiarities of outline thus presented, when he was considering only a section of the stellar system. It is obvious that a system of stars forming a sort of island universe might be expected to present many irregularities of shape, and a section athwart the middle of such a system might as probably be shaped like a toothed leaf as in any other way.

But as the work of survey went on, Herschel began to find that not only particular cross-sections, but the system itself, presented peculiarities of form, and that these were related in too special a way to the position of the observer on the earth to be easily explicable as really belonging to the system of stars. Consider, for instance, such a case as the following: Over a certain region of the heavens, nearly circular, Herschel found that his star-gaugings invariably gave high numbers, while over the region all around this nearly circular space they as systematically gave very low numbers. Thus, if we suppose A, B, D, E to represent such a circular region, having

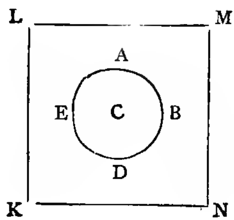

FIG. 4.

its centre at C, Herschel found that within the boundary A, B, D, E he always had fields of view rich in stars; while so soon as he directed the telescope to points outside of A, B, D, E, he found not more than perhaps four or five stars, instead of hundreds, in each field of view. The meaning of this result—if the assumptions adopted by Wright and Herschel are accepted—is obvious. Herschel himself never hesitated in recognizing this meaning; yet those who quote Herschel constantly, and regard with intense disfavour the idea that he could, under any circumstances, have made a mistake about the stellar universe, overlook the direct result of his observations, the result pointed out by himself and frankly accepted.

If within a small circular or roughly rounded space, such as A, B, D, E, many stars can be counted in every field of view, while over the whole space L, M, N, K, outside of A, B, D, E, few stars are seen, and if a great number of stars seen in any direction indicate a correspondingly great extension of the stellar system in that direction, then, of course, it follows inevitably that the stellar system extends toward the region A, B, D, E very much farther than toward any of the region around A, B, D, E. If the stars over the space A, B, D, E were uniformly distributed, the conclusion would be that a cylindrical projection or rod-shaped extension of the stellar system existed in the direction toward C, the centre of this rounded, rich region of stars. If, on the other hand, as Herschel found to be almost invariably the case, the stars, though rich over the whole region A, B, D, E, were much more closely aggregated near the centre, C, than toward the edge, the conclusion would be that there was a conical projection of enormous length compared with its breadth, having its axis directed toward C. In neither case could the conclusion be regarded as reasonably likely, scarcely even within the bounds of probability. It would be strange enough to imagine a star-system of vast extent, with long cylindrical or conical projections extending from portions of the central group, the extensions being many times longer than the diameter of the parts of the central mass (the cloven flat disc of stars) from which they sprang. Nay, this would not only be strange, but altogether inadmissible when dynamical laws are taken into account. But if we overlook the strangeness and the unscientific nature of such a supposition, we find another and overwhelming difficulty in the peculiarity that every one of these strangely projecting cylinders and cones of stars must be conceived as having its axis directed exactly toward the solar system, from a member of which we make our observations. Our sun is, by the very assumption on which the system of numerical star-gauging depends, but one among millions of suns forming a system of stars. There is no reason whatever for supposing that he lies at the centre of the system, or, indeed, that the system is of such form as to have a "centre of figure," which, of course, can only exist in the case of a symmetrical system. On the contrary, there are abundant reasons in the complex form and various degrees of brightness of the Milky Way, and in the general superiority of lustre found within its southern portions, for believing that the system of stars (if, indeed, the Milky Way represents its richer parts) is exceedingly complex in shape, and the sun eccentrically placed within its limits. Yet, as seen from this casual star—for in looking from the earth we get, to all intents and purposes, the same view of the stellar depths as if we looked from the sun—all the strange projecting spikes of stars (I can think of no more suitable name) are foreshortened into the appearance of round star-clusters! This is absolutely incredible.

There can be no doubt or question as to the significance of the observed facts, if the assumption on which the star-counting method depended is accepted, and it is scarcely possible to entertain any doubt or question as to the absolute inadmissibility of the result thus obtained. If the greater the number of stars seen in any field of view, the greater is the extension of the star-system in the direction of those stars, there must be enormous spike-shaped projections of stars wherever clustering aggregations are seen, along the Milky Way or elsewhere; while the existence of such projections, always directed exactly toward the sun, cannot be admitted as possible by any reasoning mind.

Sir William Herschel, at any rate, felt no doubt on the subject. He saw at once, that since the principle he had assumed in the beginning of his star-gauging by the counting method led to a result that was manifestly preposterous, the principle that had seemed so reasonable must be rejected as unsound. Repeatedly we find him saying that a long continued examination of the star-system has convinced him that the idea of uniformity of distribution, which he had imagined at the beginning, must be given up as inadmissible. He remarks that he has satisfied himself that the stars in the Milky Way are distributed very differently from those in our neighbourhood. He understood the real meaning of the clustering aggregations of the stars along the Milky Way, regarding these as manifestly real clusters of stars, not stellar projections.

It would indeed matter little if Herschel had failed to recognize the meaning of what he had himself observed. Had he so failed, we should have found but another instance among hundreds known to us of the inaptitude of even the keenest observers to analyze their observations, and educe the full meaning of what they have discovered. Herschel differed from the rank and file of mere observers—the working army of science—in the power he possessed in this respect, until approaching the end of his wonderful observing career. But had he in this case failed to reason right—as in later years we find he actually failed—this should in no sense influence our judgment respecting facts that are as clearly before us as they were before him. We know that the assumption he first adopted would compel us to assign to the star-system a shape that is antecedently unlikely even as a shape, and is rendered utterly inconceivable when we take into account the peculiar relation of all its most marked features to the sun. If we saw a number of grains scattered over a surface at random, and found that as they fell they arranged themselves in the form of a star, all the radiations of the star-form being directed exactly toward a certain mark on the surface, we should be absolutely certain that there were peculiarities in the surface, differences of level, or the like, which brought about this result. It could not possibly be accidental. We ought to feel as certain that there cannot be multitudinous radiating streams of stars, all extending straight from our sun, unless there is some special peculiarity in our sun to cause this singular conformation of the star-system. And since we know certainly that no such peculiarity exists, we cannot but reject decisively the belief that the star-system is so shaped. It could make no difference whatever in our conclusion that Sir William Herschel had failed to notice the inference directly deducible from his observations. But, as a matter of fact, the elder Herschel accepted the rich clustering regions along the Milky Way as in reality what they appeared to be—that is, as clusters, not as projecting streams of uniformly strewn stars.

Of course, the principle that he has assumed as the basis of this system of star-gauging—the principle of generally uniform distribution—had to be abandoned in at least these special cases. Probably Herschel was not prepared to admit that it must be given up altogether. This seems much clearer in our time, with our vastly increased knowledge about the stars, than it could have been to Herschel, keen though his insight into such matters unquestionably was.

But Herschel went on at this time with a series of sidereal observations of the widest scope and the most diverse character. He had practically the whole field of stellar and nebular research; the universe was all before him where to choose, a noble but truly a bewildering scene. So far as observational work was concerned, he could hardly go wrong, let him undertake what portion of the survey he might. Again and again he sent to the Royal Society the results of fresh series of observations—now a thousand or so of new nebulæ discovered by him in his "sweeps" of the mighty dome of the heavens; anon the survey of regions containing hundreds of thousands of stars; then an inquiry into the distribution of nebulæ and stars: and all this work went on in company with the observation of sun, moon, planets, and comets, the construction of new telescopes by hundreds, the study of many complex physical problems, and other scientific inquiries of minor importance.

As these labours went on, and clearer ideas of the constitution of the heavens presented themselves, Herschel must have begun to see that the system of gauging the galaxy by counting stars was utterly inadequate. With all the various orders of star-clusters and nebulous masses, how could he longer imagine that mere numerical wealth of stars, or of points of light looking like stars, indicated enormous extension in the direction of the line of sight toward such regions? Distance, indeed, he felt to be indicated by the close aggregation of multitudinous points of light. But the vast distance that he recognized in some of these clusters of stars was something entirely different from the long array of stars in particular directions that he had originally assumed as the explanation of great wealth of stars in such directions. His original idea of the structure of the stellar universe had not included the conception of star-clusters, either of the larger sort, such as he had found in parts of the Milky Way, or of the smaller kind, rounded, elliptical, irregular, ring-shaped, or of other forms of small clusters, which sometimes he was disposed to regard as external stellar universes, at others as fragmentary portions of our own galaxy.

It was a natural outcome of such observations as these, and of the doubts they inevitably cast on Herschel's original method of star-gauging (or rather of the conviction forced upon him that the principle of that method was untrustworthy), that Herschel should be led to devise another method. I wish specially to show that the method he now adopted was entirely different from the other, insomuch that it is among the marvels of misapprehension which the study of science brings before us that this method should be confounded with the earlier system. But I wish also to show how naturally the new method of star-gauging arose out of the observations on which Sir William Herschel had been engaged since his earlier star-gauging had shown him that the universe of stars is not constituted as at first he supposed it to be.

Let us take the latter point first. Among the nebulæ Herschel had found all orders of what he called "resolvability." Some of them are clusters so coarse in texture that it was not easy to draw a line of distinction between them and the more clustering portions of the galaxy itself. I may notice in passing a feature that was not known to him, viz., that the nebulæ of this coarsely clustering type are more numerous upon and in the neighbourhood of the Milky Way than over the rest of the heavens. Others, again, are compact clusters, still easily resolved into stars with a telescope of moderate power. Then there are others so difficult to be resolved into stars, that until powerful telescopes were applied they presented the appearance of round or elliptical cloud-like spots. Yet others are still finer in their starry texture, so that only a few of the most powerful telescopes in the world will resolve them into discrete points of light. And lastly, so far as the nebulæ of regular shape are concerned, there are some that have not yet been resolved into stars by any telescope. It is noteworthy that, arranging the nebulæ into classes in the order of their resolvability, those most easily separated into stars show the most marked tendency to aggregation along the Milky Way, and are irregular in shape. Those that come next in order are nearly circular, and though still showing a certain increase of wealth toward the Milky Way, are found in tolerable frequency elsewhere over the star-sphere. The nebulæ that are resolvable with difficulty, on the other hand, are elliptical, and are absent altogether from the Milky Way. These points are manifestly associated with the great problem of the constitution of our galaxy, though not directly related to Sir William Herschel's observations. In fact, though he noticed the remarkable circumstance that the nebulæ cluster near the northern pole of the Milky Way (that is, near the point farthest on the northern heavens from the central line of the Milky Way), he did not recognize the manner in which this peculiarity is associated with the character of the nebulæ, and he supposed that the nebulæ are rich along a ring-shaped region akin to the Milky Way, but at right angles to it, and formed of star-clouds instead of stars.

Recognizing these diversities in the structure of nebulæ, Herschel was naturally led to regard them as due to differences of distance. He supposed the coarser clusters to be the nearer, and the finer in stellar texture to be the more remote. All nebulæ might fairly be regarded, at that stage of the inquiry, as farther away than the stars forming our own sidereal system, even to the farthermost parts of the galaxy. Herschel does indeed speak of the possibility that toward the side of our flat sidereal system, as he viewed it, there might be room for the nearer approach of the parts of a former great single nebula, as though the nebulæ seen clustering in great numbers over the wings, shoulders, and head of Virgo might be but the parts of a former nebula of gigantic proportions. But this notion seems not to have been more than a passing idea with him, or to have much influenced the development of his views. The idea gradually gained force, on the contrary, that in the greater or less telescopic power needed to resolve a nebula, or a group of stars, we may find evidence of the greater or less distance of the object so scrutinized. So soon as this idea had taken firm root in his mind, which was not till toward the end of his observing career, he proceeded to put to use this means (as he supposed) of determining distance. I refrain from saying that he put it to the test, for I have no evidence that he consciously did so. He seems to have taken it for granted that the visibility of a star as a separate point of light, by a telescope of given power, was in itself a test of distance. He stated the principle, and showed how it might be applied to stars, star-groups, star-clusters, and nebulæ of various orders; then he proceeded to employ it as a means, first, of measuring the scale on which the stellar system is constructed, then of determining its shape, and lastly, of ascertaining the distances of the nebulæ.

And now to show how entirely distinct was this method of gauging the star-depths from that which Sir William Herschel had before employed. We may call the first method star-gauging by enumeration; the second, star-gauging by resolution. In the first method, the same telescope (a powerful one) was to be applied to different parts of the star-depths, the number of stars counted, and, as the number was greater or less, the limits of the stellar system in the given direction was assumed to be farther away or nearer. What was taken for granted in this method was, first, that the stellar system is formed of stars generally uniform in distribution throughout the system ; secondly, that the telescope employed was powerful enough (it was eighteen inches in diameter) to reach to the limits of the system ; thirdly, that there are no vacant spaces in the system.

In the second method, different telescopes, ranging in power from the weakest in use to the most powerful he could make, were directed to each region examined, until the whole region had, if possible, been resolved into stars well defined on a black background, without any trace of milky nebulosity. What was assumed in this method was, first, that the sidereal system is formed of stars not differing greatly from one another in size ; secondly, that in the various clustering regions throughout the sidereal system the average distances between stars are tolerably uniform, or, in other words, that what may be called the stellar texture of each part of the system is the same throughout, though there may be vacant spaces in some parts, and clustering aggregations of various forms in others ; thirdly, that any part of the system that the most powerful telescope he employed failed to resolve, lay at a distance beyond the gauging or fathoming range of that telescope. To bring the two methods more clearly into contrast, note that

In Herschel's first method of gauging, it was essential that one and the same telescope should be used throughout the work.	In Herschel's second method of gauging, it was essential that a series of telescopes differing in power should be employed.
The comparisons made related to different fields of view, seen with the same light-gathering powers.	The comparisons made related to the same field of view, seen with different light-gathering powers.
The inference deduced related to the extension along the line of sight of the objects counted with the one telescope employed.	The inference deduced related to the distance of objects seen with the different telescopes employed.

Had Herschel been a younger man when he thought of the second method of gauging the star-depths, it is probable he would have felt from the beginning that the method was one to be tested before it could be trusted. He would have been prepared to find that while, if his assumptions were sound, his results would have such and such a meaning, it was at least possible that his results might show that his assumptions were altogether inadmissible, and therefore that his new method of star-gauging was altogether unsound. But Herschel was nearly seventy-nine years old when he began to employ his second system of star-gauging, and though he still possessed much of his skill as an observer, he had lost much of that versatility of mind which had enabled him not only to observe skilfully, but so to analyze his results as to see whether they were consistent with the assumptions by which they were to be interpreted. Can we wonder if at that advanced age Herschel was content to work resolutely at the task on which he had entered, without considering very closely or thoughtfully the question whether the principle by which he proposed to interpret his results was sound or otherwise? It had seemed to him so reasonable as to appear almost unquestionable; we do not find a line or a word tending to show that he ever questioned it. The principles on which the first method of star-gauging had been based had seemed to him equally unquestionable at first; but he had found them to be unsound by noting that his observations interpreted by means of them led to absurdities. The observations made in accordance with the second method of gauging led in like manner, if interpreted by means of the principles on which that system was based, to absurdities. But this, his attention being directed too exclusively to the results themselves, he failed to recognize.

Herschel began this new work of star-gauging by examining individual stars. It is clear that the principle of the method is applicable to a star as readily as to a star-cluster. If we can determine the average distance of those stars that we can just see with the naked eye on a dark and clear night, and stars generally throughout the stellar system have the same mean size (by which I mean that the average for a thousand stars in any one part of the system is the same as for a thousand stars in any other part of the system), then, of course, a telescope increasing the light-gathering power of the eye fourfold will just show a star twice as far away; one increasing that power ninefold will just show a star three times as far away, and so forth. It was by observations made in this way that Herschel was led to the belief that among the stars shown by his most powerful telescopes are some that are thousands of years' light-journey from the earth. Singularly enough, the very evidence that shows in this case that the principle of the new method of star-gauging failed, has shown that the same result can be inferred that Herschel based on that principle. We know now, for example, that many of the brightest stars —as Sirius, Capella, Vega, Arcturus, and Aldebaran —are much farther away than some—as 61 Cygni— that are barely visible to the naked eye on the darkest and clearest night, instead of these being (as they should, if the principle of the new method were sound) fully a hundred times farther off. We cannot, then, any longer assume, as Herschel did, that the faintest stars seen with his largest telescopes are thousands of times farther away than those forming our constellations. They may be relatively near, and look small because they really are much smaller than their fellows. But while, on the one hand, we cannot now suppose faint stars necessarily far away, we are precluded, on the other hand, from inferring that bright stars are necessarily near. Since it is certain that many of the brightest among the stars visible to the naked eye are really farther away than many of those that are barely discerned, we may infer, with considerable confidence, that the same holds in the case of the field of view of the mightiest telescope yet made. Now, the faintest stars seen in such a field are those that would be the brightest in fields of view obtained by penetrating still more deeply into space. Among them, therefore, must be some farther away than those yet fainter stars; among them, in fact, are probably stars like Sirius, Canopus, and Alpha Centauri, which owe their brightness to real vastness, and lie at depths remoter than the daring conception even of the elder Herschel had suggested.

But it is when we turn to the study of star-clusters that we recognize at once how thoroughly the principle of the new method of star-gauging was disproved, and how important, nevertheless, are the results that Herschel's observations on the new plan established. If he had found that each cluster, whether in the Milky Way or of the nature of a star-cloud, had been resolved by the application of a certain telescopic power, or of powers ranging between tolerably close limits, he might logically have been content to believe that his principle was sound. An easily resolved cluster would be set relatively near, and one resolved with difficulty would be set far away. But, as a matter of fact, he met with a very different result in many cases; and a single case of the kind would have sufficed to dispose of the principle he had adopted. He found clustering regions (rounded in form) that were partly resolved by even his weakest telescopes, and more and more resolved on each increase of telescopic power, until he brought into action his very largest telescope; but even with this instrument, milky nebulosity still remained. This peculiarity would be limited to a certain rounded space, in some cases not so large as the disc of the full moon.* Nichol says of these regions, "What wonder if even Herschel shrank back appalled in the presence of these unfathomable abysms?" Herschel himself spoke less turgidly. He simply says, "When I have been unable to resolve the Milky Way with my most powerful telescopes, it has been because the Milky Way is unfathomable."†

Now, this observation, interpreted by the principle of the second method of star-gauging, leads to precisely the same absurdity to which Herschel had been led by his first method—and still more definitely, though not quite so obviously. All round one of these regions that he found unfathomable, the star-depths were easily fathomed, and therefore in those directions the stellar system had no great extension. But in the direction of these unfathomable regions the star-system had an enormous extension, if the principle of the new method could be trusted. The case is precisely the same as though a surveyor of the depths of ocean found that all over a large area of the sea bottom, save one spot, a few yards perhaps in length and breadth, he reached bottom with a hundred fathoms or so, while at that spot he could

* Herschel himself does not dwell on this particular point, though it could not possibly have escaped his attention; but any telescopist can ascertain for himself that all round the "unfathomable" regions noted by Herschel are regions that even moderate telescopic power will completely resolve.

† Struve was led into a singular mistake by this sentence (which I quote from memory, but correctly in essentials). He wrote it out probably in German, "Wenn Ich," etc.; or if not, he simply understood it as if the English word "when" were equivalent to the German "wenn"; for in his "Études d'Astronomie Stellaire" he writes the sentence with the word "Si" for "when," making the statement, which Herschel applied to those parts only of the Milky Way that he could not fathom, relate apparently to the whole of the Milky Way, and suggesting consequently an infinitely extending flat, galactic disc for Herschel's finite one.

not reach bottom with a line of two or three thousand fathoms ; except that, marvellous as such a deep and narrow hole reaching straight down two or three miles, but only a few yards across, would seem to the observer taking such soundings, it would be easy to explain, compared with the sidereal phenomenon that Herschel had before him. We can imagine causes for a deep vertical hole in the earth's crust, but we can neither imagine any cause for a straight star-strewn projection of the galaxy in a direction exactly from the sun, nor admit the possibility that such a projection could continue if it had ever existed. That there should be several such projections would be simply impossible, even if we admitted the possibility of the existence of one.

But this result, which thus conclusively proved that the principle of the new method of star-gauging was unsound, established nevertheless a most interesting fact. Since the clustering regions that yielded in part to Herschel's weakest telescopes, but not wholly even to his most powerful instruments, could not possibly be long, straight projections similarly constituted throughout their length, it follows that they must be clustering aggregations presenting a wide variety of stellar texture. There must be larger stars separated by wide intervals, stars not so large and separated by intervals not so wide, and stars smaller and smaller in real size and set more and more closely, till even with Herschel's most powerful telescope they could not be separately discerned. In other words, instead of penetrating more and more deeply into space, as he supposed, he was in reality scrutinizing more and more closely the stellar structure of one and the same region of space.

This variety of feature within clustering regions of the Milky Way would have appeared strange to Herschel (in fact, the idea scarcely presented itself to him), but in our time it appears the most natural thing in the world. The analogy of the solar system, as known to us, suggests precisely such variety of structure in the greater system that Herschel was studying. Analyzed by optical powers varying in range from unaided vision to the keenest telescopic scrutiny yet available, the solar system presents a constant increase of complexity. The eyes see sun, moon, and a few planets; the telescope reveals more planets, some really as large as Uranus and Neptune, but faint through vastness of distance; others nearer than Saturn and Jupiter, but looking faint because small; and yet others associated with the larger planets as dependent orbs; more and more bodies come into view with closer and closer scrutiny of the solar domain; yet portions still remain unresolved, such as the Zodiacal region, where astronomers more than suspect that millions of millions of nerolites and meteorites are travelling around the central orb. With this knowledge for our guidance, it seems as strange to the thoughtful student of the heavens in our time to regard the stellar system as generally uniform throughout in texture, as the diversity of texture that we recognize in the solar system would have appeared to Herschel.

Observations of star-clouds regarded by Herschel as external galaxies, should have led him (and doubtless would in earlier years) to a similar conclusion. It is true that in many of these systems there is an apparent uniformity of stellar texture consistent with the idea that they are formed of stars of about the same size, and strewn with general uniformity through the whole region occupied by the star-cloud. Most probably, indeed, the consideration of these features encouraged Herschel in the belief that our own galaxy is similarly uniform in texture. Moreover, in comparing one star-cloud with another, Herschel was not necessarily led to recognize the possibility that, even as one star differs from another in glory, so the nebulæ may differ much from one another in structure, regarding them for a moment as he did, that is, as external galaxies.

But there was a simpler yet absolutely fatal objection, in the results that he obtained, to the theory that ran through all his work at this time, viz., that not only is the texture of our own galaxy uniform throughout the extent of the stellar system, but the same sort of star-texture exists, with considerable general uniformity, among all the island universes within our ken. Herbert Spencer was the first to note this objection; but it occurred independently to me (it is, indeed, obvious) in 1867, when I had not as yet read a line of his works. That it did not occur to Herschel himself, shows clearly how unready, in his extreme old age, he had become to analyze his results as he had in earlier years.

Herschel had found parts of our galaxy unfathomable, which showed that, in accordance with his assumptions, the outermost extensions of the galaxy are beyond the resolving power of his mightiest telescope. But the nebulæ, if they are external galaxies, must lie hundreds of times farther away than the outermost parts of our own galaxy. For each one of them, from its observed size, is known to lie at a distance exceeding hundreds of times its own diameter—that is, the diameter of our galaxy, on the assumption that galaxies are all of about the same size. Thus, then, we have this absurd result, that, whereas parts of our uniformly textured galaxy, at a distance of half its diameter, are irresolvable by the most powerful of Herschel's telescopes, many similar galaxies, hundreds of times farther away—corresponding to the diminution of their light tens of thousands of times—are resolvable with telescopes of much smaller power! Manifestly the principle of the second gauging method fails here again for the third time, and most hopelessly. Whether the star-clouds are external galaxies or not, the principle that Herschel had adopted for their interpretation, and in order to bring them into comparison with our own stellar system, must be given up.

But we know now—I venture to speak of it as certain, though many suppose it to be but a theory of my own—that the nebulæ are part and parcel of our own galaxy. Herschel's results went far to prove this, and had he but analyzed them he would have seen as much. Not only does our galaxy differ greatly in texture in its various parts, but it is as varied even in constitution as our solar system, or, rather, it is doubtless infinitely more varied in reality, though presenting to us the evidence of only about the same degree of variety. As in the solar system there are large planets and small ones, so in the stellar system there are stars of many orders of real size; as in the former we have streams of tiny bodies, like the asteroids, so in the galaxy we find streams of small stars, as in the Milky Way; as in the solar domain there are meteor-clouds and comets partly or wholly gaseous in structure, so in the great galaxy to which our sun belongs there are clouds of star-dust and mighty masses of nebulous matter (chiefly gaseous), like the Orion nebula.

I may hereafter give a brief sketch here of the evidence respecting the architecture of the stellar heavens already obtained by astronomers. In such a sketch the work of the Herschels would hold a prominent place. I may also show the methods of survey that commend themselves for future employment. My present object has been, first, to show how entirely distinct were the two methods of star-gauging that many who suppose they know something of Herschel's work have hopelessly confounded together; secondly, to point out how thoroughly the application of each disproved the assumption on which either had been based; and lastly, to show how, nevertheless, the results obtained by each method threw useful light on the great problem that Sir William Herschel, first of all men, successfully attacked by observational methods.

CHAPTER IV.

NEEDED STAR SURVEYS.

IN the preceding chapter on Sir W. Herschel's two methods of gauging the star-depths, I showed that, in a sense, both methods failed, one obviously to himself, the other as tested by his own method of reasoning. But let us consider what we mean when we say that either method failed, and then note what each method showed, what other methods are suggested by the results of applying those, and lastly, what further plans are available for the survey of the star-depths.

Herschel's first method of gauging the heavens was based on the assumption that the greater the number of stars seen with a given telescope in one and the same direction, the greater the extent of the sidereal universe in that direction. It can only be said to have failed in *this* respect, that it showed the incorrectness of the assumption on which it was based. Herschel found that a great increase in the number of stars seen in particular directions may arise—and in many cases certainly does arise—from the clustering of great numbers of stars in their particular regions of space—a condition of things of which his preliminary assumption had taken no account.

But while this involved the utter failure of the process of measurement which he had proposed to apply to the stellar universe, it by no means implied the failure of his observations to reveal any new truth. On the contrary, the very circumstance that he had to give up his preconceived idea of stellar distribution shows that a quite unexpected discovery had rewarded his star-gauging labours. He had been able to demonstrate the clustering of stars in particular regions of space, and therein lay a discovery of extreme interest.

Herschel's second method of gauging the heavens was based on the assumption that the greater the telescopic power required for the resolution of the milky light of the galaxy into discrete stars, the greater the extent of the sidereal universe in the direction thus explored. This method also failed; but it only failed in *this* sense, that it showed the assumption Herschel had thus made to be incorrect. In some regions of small extent he found the resolution of the milky light to begin with his lowest powers and continue until his highest were used, milky light even then still remaining unresolved. And although he did not himself note the point, it is manifest that this, if his original assumption had been sound, would have signified the existence of long spike-shaped projections of stars from the sidereal system, all these projections, by an incredible chance, being directed exactly from the solar system. As such a supposition cannot be accepted for an instant, it is manifest (though Herschel, then in extreme old age, did not notice this), that there must be a clustering of stars of many orders of real magnitude within particular regions of space—a condition of things of which Herschel's second preliminary assumption had taken no account.

But here, also, while the complete failure of this second process of measurement was involved, this failure by no means implied the failure of Herschel's observations to reveal new truth. Here, as in the other case, the very circumstance that a certain idea of stellar distribution had to be given up, showed that a discovery of importance had rewarded Herschel's labours. He had been able to show that the clusterings of stars already demonstrated was not a clustering of stars nearly equal in magnitude, but of stars differing enormously in real size. Some of the rounded clusters thus examined by Herschel are so limited in extent that, assigning to them a roughly rounded real form (inferable from their obviously rounded apparent form), we see that the farthest parts of these clusters are not farther away than the nearer parts in greater degree than as 100 is greater than 99. But within these narrow limits of real distance Herschel found differences of stellar size and resolvability, through some eighteen star magnitudes, which would have corresponded (had his assumption been true) to distances differing much more than a hundred differs from unity. The discovery that within rounded regions of the stellar universe there may exist so many orders of suns, the largest exceeding the smallest thousands of times in volume, was of extreme interest, and threw an entirely new light on the architecture of the sidereal system.

In like manner it is to be noticed that Herschel's observations of star-clouds or nebulæ, although by no means to be interpreted as he had at first supposed, are most important in their bearing on our ideas respecting the structure of the sidereal system. He regarded the nebulæ as outlying universes resembling our own galaxy,—a grand idea justifying what is said on his tombstone, that he had broken through the bounds of our heavens—*Cœlorum perrupit claustra.* It is, however, certain in reality that all these star-clouds are within the limits of our sidereal universe. Herschel's own principle of interpreting his observations, though inadequate and inexact, suffices to prove so much as this. It is certain that with his most powerful telescope he was unable to reach the limits of our galaxy; it is manifest, therefore, that he could not see with them the individual stars, or even the milky light, of galaxies lying far beyond those limits. Therefore all the nebulæ observed by him were within these limits. There is no possibility of escaping this conclusion, unless we admit the possibility that there exist outside our galaxy others consisting of enormously larger and more brilliant stars—stars thousands of times larger than Sirius and Vega, which are themselves at least a thousand times larger than our great and glorious sun. Of course, all these results may be said to have been proved at one stroke by Sir John Herschel's observations of the *Nebeculæ* or Magellanic Clouds. He found in *those* rounded regions (i.) immense *numbers* of stars, indicating enormous range in distance if his father's first gauging principle is accepted; (ii.) immense varieties in the size of stars (from the seventh to stars so faint that in his powerful gauging telescope they could not be individually seen), and this, according to his father's second gauging principle, indicated also enormous range in distance; and (iii.) great numbers of nebulæ of all orders, indicating, if his father's views about nebulæ were sound, that beyond each of the Magellanic Clouds, but separated from them by enormous distances if unoccupied, lie immense numbers of galaxies of suns forming two systems of such galaxies so situate, by an incredible chance, that they seem to correspond exactly in shape and position with the Nubeculæ. All this is certain, if the older methods of interpretation are insisted upon. "The access to the Nubeculæ on all sides," says Sir John Herschel, "is through a desert." "Intensely poor and barren regions," he says of those spaces which surround these Magellanic Clouds. In directions, then, all round the Nubeculæ the limits of the galaxy are very near —according to both the gauging principles of Sir W.

Herschel. Again, the Nubeculæ are absolutely richer than any other parts of the heavens in nebulæ, which here form apparently two clouds of star-clouds, coinciding in extent with the Magellanic Clouds. If these nebulæ are outlying stellar universes, they must lie at enormous distances beyond the Nubeculæ, yet appear to be coincident in shape and position with them—a thing as incredible as that clouds in the sky should have precisely the same shapes and positions as dust marks on the pane of glass through which an observer sees those clouds.

Sir John Herschel naturally rejected the idea that the features of the Magellanic Clouds are to be thus explained. Even if we could suppose, he said, that one of these clouds had the shape of a long cylinder whose axis was directed exactly towards our solar system, it would be impossible to suppose that the other is to be similarly interpreted, that a similar strange chance had set a second long cylinder of stars in space with its axis bearing exactly on the sun's family. He does not seem to notice the yet more fatal objection that each Magellanic Cloud would have to be something more than a cylinder of stars so situate; at an immense distance beyond each cylinder, and exactly in the prolongation of its axis, there would have to be a cloud of star-clouds, to account for the multitudinous nebulæ within the Nubeculæ. But Sir John Herschel pointed out objections enough to convince every one that the Magellanic Clouds have in reality the rounded form which they appear to have. Then he went on to show that, this being so, the distance of the remotest object in either Nubecula does not exceed the distance of the nearest more than ten exceeds nine. Within these narrow limits of distance, he says, lie all orders of stars from the seventh down to the faintest visible in the great gauging telescope—nay, even to milky light completely irresolvable into stars, besides all orders of nebulæ, clustering, irregular, round, elliptical, resolvable and irresolvable, bright and faint.

All this shows that science had been quite mistaken in supposing that the stellar universe consists merely of stars, not differing greatly in size, or much more richly strewn in some parts than in others. Just as the old idea of the solar system formed by Copernicus, as a central body circled round by six planets, has long since had to give way to the diversified system recognized by the astronomy of to-day, with its sun and giant planets, terrestrial planets and asteroids, large moons themselves like worlds and small moons like those of Mars, the ring system of Saturn, and finally (at present at least) the Cometic and Meteoric systems, so has the old and simple idea of the stellar galaxy had to give place to the conception of a most complex system, with giant suns, suns like our own, and minor suns, double, triple, and multiple suns, clustering aggregations, streams, branches, clouds, and complex groupings of stars of all orders, with nebulæ of all kinds, stellar and gaseous, round, oval, ring-shaped, spiral, and irregular.

And here I would pause for a moment to correct an idea which has been very frequently suggested in terms implying that it is the obvious explanation of what we see instead of being absolutely inadmissible. In almost all works of astronomy, when the varying degree of resolvability within cloudlike regions of star space has been mentioned, we find the minute points of light, recognized when resolution is effected, treated as if of necessity they were suns like our own, each girt round by its family of worlds. But this is altogether incorrect. It is absolutely certain that stars strewn through space like our sun and his fellow-suns (the individual stars of our constellations), could never appear as a milky, unresolved nebulosity; for the simple reason that with increase of distance the individual stars, even were they as large as Sirius, would disappear long before they drew close enough together to present the appearance of irresolvable cloud. This is easily shown:—

Suppose the stars visible to the naked eye to be all suns like our own, the faintest being therefore about a hundred times farther away than the brightest. Then for that spherical region of space to be removed to so great a distance that the whole set of some 6,000 stars formed a cloud as large as the moon, the centre (our sun suppose) would have to be removed to a distance exceeding more than a hundredfold the entire diameter of the sphere, and therefore exceeding more than two hundredfold the distance of the faintest visible star from our solar system. All those 6,000 stars then would lie not only enormously beyond our unaided vision, but beyond the range of telescopes of considerable light-gathering power. For, removing the faintest to a distance two hundred times greater, would correspond to reducing its light to one-eight millionth part of its present amount. But six thousand stars strewn over such a portion of the heavens as the moon covers would be easily separated; the average distance between them would be twenty seconds of arc, and double stars separated by such a distance as that are considered quite "coarse." Thus, long before the individual stars were merged into each other by the effect of distance, each would be separately undiscernible. Now, it should hardly be necessary to point out that to speak of stars separately invisible, but lying at distances easily discernible (as such), forming a milky light, however faint, is utterly absurd. It is essential for the production of such milky light as we see in the galaxy, that the apparent distances of the separate stars should be lost through effect of distance before the stars cease to be visible.

As the point considered in the last paragraph is of great importance, and very little understood (or noticed, if understood), I give the following illustrative tests:—

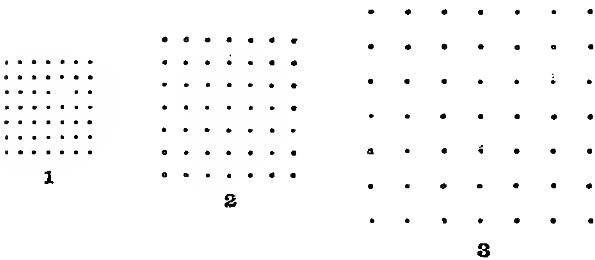

FIG. 5.

Here we have three groups of dots all of the same size, but less closely set in 2 than in 1, and still less closely set in 3. Now if the page be set up where the light falls well upon it, and then the observer retreats gradually from it, he will find that at a certain distance 1 assumes the appearance of a darkish grey square, while the separate spots in 2 and 3 remain still visible. Farther away 1 remains as a darkish grey square, of just the same tint as before, but smaller; 2 appears as a light grey square, and the separate spots of 3 are scarcely discernible. But passing farther away the separate spots in 3 disappear altogether from view without having coalesced, as those of 1 and 2 successively did, so as to form a tint. When last seen they are still at recognizable distance from each other. Moreover, the tints of 1 and 2 remain unchanged as you pass farther away. All that happens with these squares is that they appear to become smaller with increase of distance.

These illustrative tests show that the mere visibility of milky nebulosity in the star-depths tells something about the distribution and nature of the stars within the region observed. Stars separated by considerable distances can never appear like a diffused cloud. Stars of the same size, but some what more closely set, will appear as a very faint nebulosity if far enough away; if still more closely set, such stars will appear as a brighter nebulosity, even when at a more moderate distance; and a number of such stars very closely set indeed will appear as a very bright nebulosity even at a small (relative) distance. But wheresoever set beyond the distance at which nebulosity results, a star-cluster will appear neither brighter nor fainter, only larger when nearer and smaller when farther away.

Thus when we see a bright milky nebulosity in a rounded region (the shape showing that we have not to do with enormously long ranges of stars in the direction of the line of sight), we know that we have before us closely-set stars, not stars strewn like those which form our constellations. We know that the stars in Cassiopeia, for instance, could never form a nebulous group such as the Pleiades appears to weak eyesight, or the beautiful cluster in the sword-hand of Perseus to the keenest vision. For we know that long before the stars in Cassiopeia had approached near enough to each other—through the recession of the group—to coalesce, they would have disappeared wholly from view. Nor would any further increase of distance and the use of the telescope make any difference: the telescope would increase the brightness of the stars themselves as it increased the apparent distance between them; and at whatever distance the stars disappeared to telescopic vision they would still be as far from coalescing as when similarly disappearing to ordinary vision.* So that the group of stars forming the Pleiades is altogether differently arranged from the group of as many stars forming Cassiopeia ; the group forming the Beehive (Præsepe in Cancer) is again differently arranged ; the group in Perseus differently arranged still : and in fine each star-grouping is unlike its fellows, just as the family of giant planets is unlike the family of terrestrial planets, and that family again unlike the zone of asteroids. The arrangements of stars are as varied as are the stars themselves unlike in size and glory: the architecture of the stellar universe is as diverse as its materials.

Suppose we could pay a visit to the midst of the Pleiades. What should we find? According to ordinary ideas we should find simply a number of suns, each, like our own, the centre of a system of worlds. Yet it is demonstrable—and easily—that we should see around us something entirely unlike the star-strewn heavens that we now see. Probably most of the stars now visible to us would still be in sight, and scattered with much the same relation between the lustre and average apparent distance as in our present skies. But how about those stars which belong to the group we are visiting? With what lustre would the six stars shine which ordinary eyesight recognizes in the Pleiades, or the fourteen stars which some keen eyes can discern in the group? It is certain, from the apparent size of the group, that the entire space of the Pleiades cannot be more than the fiftieth part of the distance separating the Pleiades from us. Therefore set in the middle of that

* Perhaps I should rather say to ordinary vision corrected by a glass just making the stars neat and well defined ; for there is not one man in a thousand whose view of a star group is not to some degree improved by the use of an eye-glass just adapted to correct the defects of his vision—defects scarcely noticeable otherwise.

group we should be within less than one-hundredth of our present distance from all the stars of the group. Alcyone now shines as a third-magnitude star, five others of the group as stars of the fourth magnitude. How would they appear if we diminished our distance to one-hundredth part of its present amount? Their lustre would be increased, not a hundredfold, but one hundred times a hundredfold, or ten thousand times *at least.* Many of them would be far more greatly increased in brightness. They would no longer be stars, but suns, just as Sirius in the great reflector of Lord Rosse, though still but a mere point in apparent size, shines like a young sun. The scene presented by the hundred stars of the Pleiades would be indescribably beautiful. In the background would lie a star scene as beautiful as the heavens we now see; but it would be scarce noticeable amid the splendour of a hundred suns, the least outshining Sirius a hundredfold in splendour. And among these, the greater glories of the night skies within the Pleiades, there would be varieties in glory as great as among the stars of our own skies; for the stars which *seem* so unequal in the Pleiades are *really* as unequal as they seem, since the whole group must be regarded as practically at the same distance from our earth.

But this is nothing compared with what we should find if we could visit some of those glorious clusters which have been poetically described by Tennyson as "bee-like swarms of suns." The idea that the stars of those clusters are distributed like the suns of our firmament, that we have merely to count their number and say *there* are so many suns each girt round by its family of worlds, and each repeating not only the glory of our own sun but all the wonders of the solar system, is demonstrably incorrect. From the observed size of those clusters we know that the entire span is less than the thousandth part of the distance separating us from them. Yet at the distance at which they lie we can discern *separately* thousands of stars. Thus the average distance from star to star within some of these groups cannot be one-millionth part of the distances separating those groups from us. The splendour of some among those stars, as seen from the midst of those groups, cannot therefore but exceed a billion* times their lustre as seen by us. Since, then, these stars can in the case of the more magnificent clusters be seen with very small telescopes, it follows that from the middle of those clusters they must shine with a glory comparable with that of our own sun, whose lustre at the distance of the nearest of our stars would not be reduced to more than one 50,000 millionth part. If, then, there are worlds circling round those suns, there can then be no night for their inhabitants, but probably a constant daylight exceeding many times in glory the most resplendent of our summer days. I should be disposed for my own part to imagine rather, though I must confess I *know* nothing on the subject, that if there be inhabited worlds at all in connection with a glorious cluster of this sort, they must be worlds circling round the whole cluster, not round the individual stars composing it. But more probably, I should say, there are no such worlds, but those clusters will hereafter aggregate into suns which in due course will become the centres of solar systems, more or less (probably very little) like the family of planets to which our earth belongs.

These considerations may serve to show what interest surrounds the inquiry into the architecture of the sidereal heavens. If two varieties of stellar

* By a billion I mean a million millions, by a trillion a million million millions, and so forth—the English way of reckoning, by which a billion, a trillion, a quadrillion, and so forth, means a million raised to the second power, the third power, the fourth power, and so forth. The other system seems unreasonable.

arrangement alone suggest such diversity of condition, what might we not expect to follow from the consideration of all the peculiarities of stellar distribution which may be recognized when the heavens are carefully surveyed ? It is to such work as is thus suggested that I referred in the opening remarks of the present chapter. The failure of the two methods of gauging, devised by Sir W. Herschel, should by no means discourage astronomers from prosecuting diligent researches into the noble problems dealt with by him. Not only, as I have shown, did each failure involve an important and quite unexpected discovery, but both these failures helped to show along what lines the inquiry may best be prosecuted.

I propose now briefly to consider what these lines are, touching somewhat on the work I have myself done in pursuance of this special inquiry, and dealing somewhat more fully with the work which has to be done (even in the earliest stages of the inquiry), wherein also I hope to bear a part.

Wilhelm Struve was led by his study of the papers of Sir W. Herschel to recognize—but only indistinctly—the importance of *combining* the principles which underlie the two methods of star-gauging :—

In any true survey of the star-depths it is manifestly essential that the system of counting stars with the same telescope, and as nearly as possible under the same conditions, should be carefully applied. And we shall not be led astray by this system if we do not interpret our results on an incorrect principle, as Sir William Herschel did in the beginning of his work. Moreover, we can apply this system in ways which at first he would have rejected as useless. He supposed that the great gauging telescope which he applied reached in all directions to the very limits of the sidereal universe, and it is clear that nothing short of such a power as he thus supposed himself to be applying could have served his purpose if our galaxy were such a system as he imagined. But that particular space-penetrating power, though it did not do what Herschel had expected (because the stellar universe is not what he supposed it), and though it was unequal to the task of resolving all parts of the stellar heavens, disclosed, as we have seen, important truths. It is manifest that less telescopic power would have also given important results—seeing that the condition Herschel had supposed essential to the validity of his survey had no real existence. Nor can one see any reason to limit the diminution of telescopic power by which useful results might be obtained. Without telescopic aid at all, the distribution of stars numerically might be well worth studying. Nay, it might be worth while to examine the distribution of stars visible with less than ordinary powers of vision.

It was the recognition of this (possibly a half-unconscious recognition) which seems to have suggested Herschel's second method of star-gauging. In this he took only a very small region for survey, and examined that with constantly increasing telescopic power, under the idea that he was thus penetrating more and more deeply into space. Now this kind of research, too, is manifestly essential in any true survey of the star-depths. Nor shall we be led astray by this system unless we misunderstand what we are doing. We *may* be penetrating more deeply into space as we increase our telescopic power, or we *may* simply be analyzing more and more scrutinizingly a particular region of stellar space: more probably we are doing both. But if we keep our minds free from any bias one way or the other, our results will always be available for the increase of our knowledge so soon as we can co-ordinate them properly together, and combine them duly with results otherwise obtained. But manifestly we must for this purpose extend this method of survey to larger regions than Sir William Herschel dealt with. If his principle of interpretation had been sound, his plan of applying the method would have been all that was needed. But so soon as we recognize the unsoundness of the principle, and note how that unsoundness was shown by the study of small regions of the heavens, and how important in itself was the discovery thus made, we see that results of great importance may be obtained from extending the survey by this method. Nor can we see any reason to limit the extent of the survey thus made. It may be applied to the whole heavens, if only a large enough array of labourers can be persuaded to take part in the work.

The study of the proper way of applying each method points, then, to one and the same result—viz., that the whole star sphere requires to be surveyed with every order of visual power (separately) from the unaided vision, or even from visual powers lower than ours ordinarily are, to the highest telescopic power that can be obtained.

A colossal work truly: but then, fortunately, it is not necessary that the whole work should be undertaken at once. Any part of it—the survey of any portion of the heavens with such and such telescopic powers, or the survey of the whole heavens or any part of them with any definite telescopic power—means so much added to our knowledge of the architecture of the complex system of stars of many orders, star-clusterings, star-clouds, and other forms of matter, which we call the galaxy.

The elder Struve, recognizing the importance of combining both systems of survey, began the task by a piece of work which cannot but be regarded as very rough indeed, though it has been enthusiastically admired by the late Prof. Nichol of Glasgow, and some others, who seem to have no idea of the real sort of work which is required in dealing with the architecture of the heavens.

Struve saw that some importance attaches to the inquiry whether stars of the brighter orders—such, for instance, as can be seen with a two-inch telescope —are more richly strewn on the Milky Way than elsewhere. Clearly, according to Sir W. Herschel's earlier ideas they ought *not* to be. The range of distance around our sun within which such stars are included, on the assumption of generally equal distribution, falls well within the breadth of Herschel's flat-disc galaxy, and (on that assumption) it is only when we pass far beyond such distances, that we come either on the vacant space bordering the flat sides of our galaxy, or on the mighty vistas of stars along its regions of greatest extension which produce the soft light of the Milky Way. Here, then, was a general test for the validity of the method which Herschel had found to fail him only in specific instances.

The proper way of applying the test by those statistical methods which Struve loved, but which I reject as utterly inadequate for any but the simpler problems of stellar distribution, would clearly have been to have counted the stars in the Milky Way (or on a galactic zone of such and such breadth), noting the area of the region thus dealt with, then to have counted the stars in the regions of the heavenly sphere outside the Milky Way (or that galactic zone), noting the area *thus* dealt with, and then to have compared the wealth of stars in these two areas—the galactic and the non-galactic. Supposing the charts or catalogues used for this work to have resulted from a fairly uniform survey of the heavens, or were it even only of the northern hemisphere, with a telescope of small power, the result Struve would have thus obtained would have been satisfactory enough.

This was not what he did. Probably he had not time. The process he actually applied indicated certainly that he was somewhat pressed for time, since it could not have taken him much more than ten minutes. He took a certain catalogue of stars down to the eighth magnitude. In these the stars to a certain distance on either side of the equator were arranged in the order of the twenty-four hours round the celestial sphere (technically, in order of their Right Ascension) and numbered from first to last. The number of stars in the first hour was thus indicated in the catalogue as the number of the last star within that hour, the number in the second hour was obtained by subtracting the number of the last star in the first hour from the number of the last star in the second hour ; and so on for the numbers of stars in the third, fourth, fifth hours, and so on, up to the twenty-fourth. Twenty-three subtractions gave Struve all the statistics he employed. He found that the fifth, sixth, and seventh hours on one side of his zone, and the eleventh, twelfth and thirteenth on the other, were richer in stars than the rest, in such degree as to show that the Milky Way, which crosses the equatorial zone aslant at those hours is richer than the non-galactic parts of the heavens. But it is hardly necessary to say that the real relative star-wealths of the Milky Way and of parts outside it could not be properly indicated by so rough an inquiry as this. It afforded an independent proof of the general law which Sir W. Herschel had already recognized at the beginning of the present century ; but it scarcely added more to our knowledge.

My own inquiry into this particular point involved rather more labour. I proposed at first to use the catalogues and charts of Argelander, in which 324,198 stars (down to magnitude 9–10) are included, taking the numerical distribution over the Milky Way, and then over regions outside of it. But a few tests showed me that while this method would involve almost as much work as a process of actual charting, it would be much less satisfactory. I determined, therefore, after consulting the venerable Sir John Herschel on the subject (this was but a year before his death), to chart every single star of the 324,198 in its proper place, on an equal-surface projection of the northern hemisphere,—that is, a projection in which equal surfaces on the heavens were represented by equal areas in the map. I laid down in pencil a series of radial lines a degree apart (360 in all), and ninety-two concentric circles at one-degree distances (gradually diminishing outwards), corresponding to the particular projection which I was employing. Then in the 33,000 spaces thus formed I marked in the stars shown in the corresponding 33,000 spaces of Argelander's forty charts. Thus I had, charted on a uniform scale, all the stars observed by Argelander and his assistants, during seven years, in their survey of the heavens from the north pole to a distance of ninety-two degrees all round, or to two degrees south of the equator.

The work occupied me in all almost exactly four hundred hours.

But the result was, I think, well worth the trouble.

In the first place, I note a peculiarity in the large chart of 324,198 stars, which attracts attention at once, yet is manifestly accidental, or due, rather, to the method in which the original series of forty charts, and the single chart itself, were formed. The peculiarity is a defect, though of little importance,—yet interesting as illustrating the points which have to be attended to in such work. The circular chart seems to show in places multitudes of concentric streaks produced by the aggregation of stars along certain very narrow zones, concentric with the boundary of the map—that is to say, having the north pole of the heavens as their centre. As my friend

Professor Young pointed out, there cannot conceivably be any real tendency in the stars to form circular zones around the pole as centre, or along declination parallels: yet such a tendency seems manifestly suggested by the appearance of the great chart when closely studied. So far as the broad results sought and obtained are concerned, this peculiarity is of no more weight than the direction of the linear streaks by which in an engraving effects of light and shade are produced. Still until or unless the peculiarity is explained, it detracts somewhat from the confidence with which those broader results are accepted.

Not really existing in the heavens, how does this peculiarity of star-distribution come to appear in the chart? The answer, though not at first view obvious, is simple enough. The wonder would be if the peculiarity had not shown itself. Argelander and his assistants, in their survey of the northern heavens, swept the skies in circles round the north pole, after the manner of survey with the equatorial telescope, which works in that sort (its main axis being directed polewards). Now herein is at once a possible cause of circular striation in the resulting charts, from the circumstance that one sweep might be made when the air was exceptionally clear, when moonlight was wanting, and other conditions for showing faint stars favourable (amongst other causes, difference of observing power among the six who took part in the work must be taken into account), while the next sweep might be made under conditions unfavourable for the work. This, however, is only a *possible* cause of circular striation, though in so long a series of observations it must inevitably have occurred at times, and so had *certainly* a share in producing the peculiarity in question. But there was also a sure and certain cause of striation. The field of view of a telescope is a circle, and in "sweeping" the centre of one field runs along a certain arc, while the next field is taken a certain distance south of that arc (or north of it, according to the way the observer works). Say the field is half a degree in diameter, and the change north or south for successive sweeps nearly as great of a degree, so that one field only overlaps by a little the field next north or south of it. Then it is clear that the chance of discerning a faint star near the course along which the centre of the field sweeps, is much greater than the chance of discerning a star where the fields overlap ; for in one case a whole diameter of the field is available for search, in the other only a short arc. In sweeping, the star will not escape in one case if it be seen at any part of the comparatively long time during which that diameter is passing ; but in the other case, if it be not caught while the short arc is passing it will not be caught at all. Thus, it is absolutely certain that fewer stars will escape along or near the tracks of the centres of the sweeping fields than midway, or nearly midway, between the tracks of the centres. A concentric circular striation must necessarily result. To this must be added the probability that, however carefully I marked in my ninety-two circles, there may have been slight departures from their true positions, whereby some of the zones were made slightly wider or slightly narrower than they should have been. This would make the striation more marked in some places, less marked in others, than it would otherwise have been, but, on the whole, would help to make it coarser, and therefore more obvious.*

* It is interesting to notice how inevitably any peculiarity in the method of distribution in such cases is bound to show itself. I remember being very much struck by an example of this which arose when I was endeavouring to secure perfectly equable chance distribution for comparison with the unequal distribution of stars of various orders, which I regard as so important a feature of the stellar heavens. After trying various methods, I thought of the following :—I drew a square 10 inches

But, thus explained, the circular striation is of no more moment than the linear striation in engravings.

The broad general results deducible from my equal-surface chart of 324,198 stars were very striking. According to the older theory of William Herschel's we do not come *near* the boundaries of the sidereal universe with such a telescope as Argelander used. Except for some few exceptionally large suns at distances where ordinary suns would not be reached by such a telescope (only 2½ inches in diameter) there should be no greater number of stars in the Milky Way zone observed with so small a space-penetrating power, than elsewhere. Even

in the side, and divided it into 10,000 squares by equidistant parallel lines in pencil, 100 each way. Opening then a book of logarithms at random, I brought down a pencil point at random on the tables of figures, taking out the digit nearest the point. When I had obtained four digits in this way—as say 4725, I regarded the two first as showing the number of divisions I was to take along one side of the square, in this case 47, and the other two as showing the number of divisions I was to take along an adjacent side, in this case 25,—where these divisions crossed, that is where the 47th row from the left crossed the 25th from the right, was the square in the middle of which I was to set a point. So I went on until I had marked in many thousands, and, indeed, tens of thousands of points. For I got others to help me in the work. Here surely was a method of pure chance, bound to result in equable distribution. But no. When the work had gone far enough, I rubbed out the pencil lines, and found a marked tendency to parallelism in certain bands where there were more dots than elsewhere, this parallelism showing itself on the vertical zones corresponding to the numbers 2, 3, . . . 5, 6, ... 8, 9, 10, . . . 12, 13, . . . 15, 16, ... 18, 19, 20, . . . 22, 23, ... 25, 26, ... 28, 29, 30, and so on, and on the horizontal bands corresponding to the same numbers. The light bands corresponded to the vertical and horizontal bands numbered 1, . . . 4, ... 7, ... 11, . . . 14, ... 17, . . &c. The explanation was simple enough. The figures representing 1, 4, 7, cover less space than the others, and by my method of taking out digits there was more chance of taking one of the *fat* figures, 0, 2, 5, 6, 8, and 9, than one of these *lean* figures, 1, 4, and 7.

for such exceptionally large suns Herschel's theory made no allowance. But what is actually the case? These stars, which ought to be no richer on the Milky Way, are actually so much richer that, merely by their increase of wealth, they positively show the Milky Way on the chart almost as clearly as if it were mapped there!

It is manifest that the circumstances of the survey by no means favour such a result, but the reverse. Every one who has ever studied the star-depths knows that a star which is quite clearly seen when alone or with few others in the field of view, may be undiscernible when the whole field of view is crowded with stars as in the richer regions of the Milky Way. Many stars then were lost in Argelander's survey of the richer fields which would have been well seen had they been in poorer regions. Thus great as are the numbers of stars seen along the Milky Way regions in my chart, there would have been many more had the chance of catching faint stars here been as great as elsewhere. This I carefully tested. Reducing the power of my much larger telescope until it was about equal to that of Argelander's instrument, I examined certain rich galactic regions surveyed by him, and others far from the galaxy, satisfying myself by using various ways of reducing the field so as to exclude the blaze from many stars in the former case, that whereas Argelander and his assistants could have seen but few more stars in the non-galactic parts of the heavens, even though they had limited their observations to the darkest and clearest nights, they could have seen half as many stars again in the Milky Way had they reduced their field of view in such sort as to avoid the blaze of multitudinous stars.

Thus, marked though the increase of star wealth is in the Milky Way regions as observed by Argelander, the increase could have been very much greater if all the stars within the range of his telescope had been recorded.

The inference from this increase of wealth on the Milky Way is obvious and important. We learn that just as the rounded rich regions along the Milky Way are really round (*i.e.*, roughly globular) regions of space, in which multitudes of stars of many orders of real size are strewn, so the streams of the Milky Way are real stream-shaped or branch-shaped regions in space in which not only the very small stars, as observed by the Herschels with their great gauging telescopes, are much more richly strewn than elsewhere, but also stars very much larger, and well within the range of very small telescopes indeed.

Extending the principle yet further, I have made (and published) investigations into the distribution of stars visible to the naked eye, finding them much more numerous on the Milky Way than elsewhere, and otherwise less uniformly arranged than had been supposed. (In this work I used scissors and a delicate alance to give the areas of irregular regions of the heavens.) I even examined the distribution of stars down to the fourth magnitude only, finding it by no means uniform, and well worthy of attention.

I have further applied the same method of charting to the star-clouds (using Professor Cleveland Abbe's excellent statistical results* as the basis of my work), finding the clearest and most convincing evidence that the nebulæ form part of our sidereal system.

I have further charted the stellar proper motions, —the only possible way of recognizing any law in these movements. Such charts show that many large groups of stars have a common drift, so as manifestly to form separate systems. In the only

* I had already published charts of the nebulæ on a less complete plan before Prof. Abbe had obtained those results.

case where one of these sets of stars has been dealt with by the spectroscopic method for determining motions of recession and approach, it has been found that (as I specially predicted in that case*) the stars all had a common motion in the direction of sight, as well as athwart that direction. This showed that the evidence given by charts of proper motions is trustworthy.

But this is the merest beginning. We want surveys of the heavens made with many other powers, —as with a $1\frac{1}{2}$-inch telescope; a 4-inch telescope (this I have partly tried, and I know the results of a full survey would be most valuable); with a 6-inch telescope; and with a 12-inch telescope: the more interesting regions disclosed by such surveys as these being then examined with the highest telescopic powers that can be brought to bear upon them. It is essential that the southern hemisphere should be at least as carefully surveyed as the northern; for no one who has ever looked at the southern skies can fail to recognize that they are much more variegated, and therefore much more likely to be instructive in regard to celestial architecture, than the skies north of the equator.

Lastly, these surveys should be accompanied by widely extended study of proper motions; by the application of the spectroscope to determine the constitution of stars in different parts of the heavens, and their movements of recession and of approach.

I venture to predict that as this work proceeds (for I am sure it will in due time be undertaken), science will be compelled to give up more and more the idea of uniformity of structure within the stellar

* The group of stars β, γ, δ, ϵ, and ζ of the Great Bear, with ζ' a small companion, Alcor, called in country parts of England "Jack by the Middle Horse."

universe, recognizing a grandeur and complexity in its architecture, a variety yet harmony in its movements, and a significance in its amazing vitality, akin to but of a far higher order than the corresponding qualities within the planetary system.

CHAPTER V.

PHOTOGRAPHING FIFTEEN MILLION STARS.

A MAGNIFICENT suggestion has been made by French astronomers. I have already dealt somewhat fully, elsewhere, with the work done by the photographic eyes of science, directed towards the heavenly bodies. By the power of instantaneous vision which the photographic eye, unlike the human eye, possesses, the sun's cloud-laden surface has been delineated, despite the constant fluctuations of the air through which the sun has to be viewed. By the power of selecting special colours wherewith to work, the photographic eye has drawn the corona when no trace of that solar appendage has been visible to ordinary eyesight. The delicate features of the star-clouds have been depicted, through the power which the photographic eye possesses, of seeing more and more by long-continued gazing upon faintly luminous objects. And now it is proposed to do what assuredly no astronomer, nor any band of astronomers, could hope to effect, even if working for the whole duration of the longest life. It is proposed to chart in their true positions all the twenty millions or so of stars which are included in the first fifteen magnitudes, so that the astronomers of future generations may know for certain the aspect of the stellar heavens—to that vast depth, at least—towards the close of the nineteenth century. Let us see what are the conditions of the task.

Using a telescope provided with a specially prepared object-glass of about 13 inches in diameter, MM. Paul and Prosper Henry have been able to take in a single hour photographic charts of spaces in the heavens extending 3 degrees in length and $2\frac{1}{4}$ degrees in breadth—say six moon-breadths by four and a half. Their actual plan has been to give in each case three exposures, with such slight displacements that each star is tripled, and so there can be no possibility of mistaking accidental dots on the plate for stars in the heavens. (It might be well, however, if in the photographs finally prepared only single images of each star were given, a preparatory plate with triple images serving for the correction of the one finally prepared, which might have three hours of exposure without displacement.) Now, a space of 3 degrees by $2\frac{1}{4}$ degrees on the heavens, or $6\frac{3}{4}$ square degrees, is about 1-6, 112th part of the whole star-sphere. So that if twelve observatories, in different parts of the northern and southern hemisphere, were employed to photograph the star-sphere on one and the same plan, then at each observatory only about 510 plates would have to be made. Counting about fifty-one moonless nights of clear sky and still air, one night being given to each plate, the whole work would be completed in ten years. If the charts thus obtained could be combined in sets of four, in the manner already employed by MM. Henry, there would be 1,528 sheets, each representing a portion of the heavens, extending 6 degrees by $4\frac{1}{2}$; but although Admiral Mouchez suggests this plan as desirable, it appears open to exception on account of change of scale near the edges of the plates.

The number of stars which would probably be shown in this splendid contribution to the astronomy of the future would be about twenty millions. In a single plate (*see* frontispiece), obtained by MM.

ꟾGRAPH OF THE PLEIADES, BY THE BROTHERS HENRY.

(Three Exposures each of One Hour.)

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Henry recently, nearly 5,000 stars can be counted; and if 6,112 gave each such a number—say 6,000 times 5,000—that would be thirty millions of stars. But the region shown in this particular plate belongs to a rich part of the Milky Way, and it has been shown by my chart of 324,000 northern stars down to the tenth magnitude, that there is a much greater density of stellar aggregation on the Milky Way long before the space-penetrating powers have been employed which the Herschels thought probably necessary to reach the regions whence the nebulous light of the Milky Way was supposed to proceed. If the photographic method were applied uniformly over the whole heavens, with a space-penetrating power reaching stars of the fifteenth magnitude in all directions, it is probable that about 20,000,000 stars would be shown. The great gauging telescopes used by the Herschels would show at the very least 100,000,000—or rather would have shown that number if it had been possible to bring every portion of the star-sphere under their survey.

Fig. 6 is from the photograph of the Pleiades.

A new era of stellar astronomy will open with his photographic work. The problems connected with the architecture of the heavens, hitherto dealt with by very imperfect methods, will now be discussed with all the advantage of at least a perfect system of survey. It is impossible, indeed, to overestimate the advantage of a system of charting over all the methods of statistical research which astronomers formerly employed. William Herschel in his first method counted all the stars which one and the same telescope—a very powerful one, 18 in. in diameter—would show in different directions. He could only take a field of view here and a field of view there, not many hundreds in all, his son and worthy successor in the work making similar observations in the southern hemisphere. No peculiarities of arrangement, nothing, in fact, but the roughest features of stellar distribution, could be recognized by such a method as this. It showed, however, how vast the number of stars forming our galaxy is, and it satisfied Sir W. Herschel that the assumption by which he had proposed to interpret his numerical gauges was inadmissible, the stars not being strewn throughout our galaxy with any approach to uniformity. Herschel's second method, commonly confounded with his first (insomuch that one may often find even men like Arago and Humboldt mixing up in the same paragraphs the results of both methods of observation) was entirely different. He now no longer trusted to the use of the same telescope, turned in different directions, to tell him (after mere counting) the depth of our galaxy of stars in those directions ; he turned different telescopes, gradually increasing the space-penetrating power, in the same direction, to tell him, by the power required to resolve the whole field of view into stars, the probable extension of the system in that direction. Herschel made many observations by this method, but in his advanced old age, when these observations had been gathered together, he did not recognize the absurdity of the result to which they tended, on the assumption he had employed. He found regions of the star-sphere, for instance, wherein stars of all orders were richly strewn, from those visible to the naked eye down to the faintest which his most powerful telescopes could show, and fainter orbs yet, whose lustre could only be recognized as milky nebulosity (resulting from the combination of the light of many stars separately undiscernible). Around these rich regions were regions comparatively poverty-stricken, regions deserving the description applied by the younger Herschel to the spaces around the Magellanic Clouds, of which he wrote that "the access to the Nubeculæ is on all sides through a desert." If his assumption had been correct, these seeming clouds of many varied orders of stars, brought into view successively with increase of telescopic power, would be long cylindrical star-clusters, or rather spike-shaped projections of star-strewn space, hundreds of times longer than their thwart breadth, and chancing by some strange accident to have their axes directed exactly towards our place in the star system. Unlikely, one may almost say incredible, in a single case, this peculiarity would be utterly impossible in several; and the clouds so to be interpreted (if Herschel's assumption were retained) are many.

Obviously we must reject this porcupine theory of the stellar system, with the solar system for the "pole" of all the stellar spines. We see that the rich cloud-like regions are real clouds of stars of many varied orders, and that in each case where Herschel had assumed (though only tentatively) that he was penetrating farther and farther into space, he was in reality only analyzing more and more scrutinizingly a complex cloud of stars. His position might be compared to that of an observer trying to gauge our solar system from a distance, who might naturally assume at first that the giant planets were much farther away than the sun, the terrestrial planets much farther away than the giant planets, the asteroids than the terrestrial planets, the meteorites than the asteroids, the small meteors than the meteorites, and the still smaller particles in comets' tails than meteors: such an observer, as soon as he recognized the association of all these objects into a system, would see that, instead of attributing the variety of aspect within the system to the variety of distance, he must regard it as due to real variety of size. The meteors which he had interpreted as millions of times more remote than the giant planets, he would now find to be in many cases close alongside of those large bodies, and, on the average, no farther away than the chief orb in the system, the great controlling sun. In like manner the faintest stars in the great clustering regions of the Milky Way are, on the average, no farther away than the leading orbs in the same star-clouds (which, be it noticed in passing, is by no means the same as saying that the fainter stars of the stellar-depths are, on the average, no farther away than the more conspicuous). The assumptions made by the elder Herschel, though shown as his work proceeded to be mistaken, did not prevent his accumulated results from being most valuable. But the validity of statistical methods was shown to be doubtful. The assumptions of Wilhelm Struve were still more improbable antecedently, and still more thoroughly discredited by the evidence. He took a zone of the heavens thirty degrees wide, assumed that the stars (down to the eighth magnitude) might be supposed to be first compressed along the mid-line of that zone, and then strewn out uniformly in twenty-four sectors, into which he divided the circular area enclosed by that mid-line. This naturally led to a result having no validity whatsoever.

The fault of all such statistical methods is that in effect they depend on a process of averaging by which, even if the initial assumptions were trustworthy, the significance of all the actual peculiarities of stellar architecture would be concealed. We want to have these peculiarities emphasized rather than hidden. Charting alone can do this effectually. But who can pretend to chart the whole heavens to any great depth around our solar system? Struve used in his statistical inquiries about 70,000 stars, and I have shown in a single equal-surface chart 324,000; but what are they among tens of millions of stars within the range of Herschel's gauging telescopes? That single chart required first seven years of observatory labour by Argelander and his assistants, then 400 hours of charting by myself; yet it shows only stars down to between the ninth and tenth magnitudes, and even in regard to these is affected by all the variations arising from the "personality" of the different observers. The proposed photographic survey would extend very much farther into surrounding space, would be far more trustworthy, and would be entirely independent of "personal equation." The idea is a magnificent one, and it may be hoped that the astronomers of all nations will help in carrying it out.

CHAPTER VI.

FIGURE OF THE MILKY WAY IN SPACE.

NINETEEN years ago* I wrote a paper called "Notes on Star-Streams," in which I discussed the relations presented by the Milky Way, looked upon as in reality a star-stream, and not the mere projection on the celestial sphere of a widely extended disc of stars. I endeavoured to show that although Sir W. Herschel's view respecting our galaxy was perhaps the only one which he was justified in forming when prosecuting his celebrated star-gaugings, it is yet one which is far from being in accordance with the information which he himself gathered for us, and is still further opposed by the facts which Sir John Herschel observed during his survey of the southern heavens. And I dwelt in particular on the evidence which the strange convolutions of the Milky Way, its narrow necks or isthmuses, the knots or clustering aggregations upon it, and still more the circular gaps which pierce it, afford respecting its structure. These point to the conclusion that whatever the Milky Way may be, it is certainly not what Sir W. Herschel had supposed. But I was forced at that time to admit that the problem of suggesting the real configuration

* "Intellectual Observer" for August, 1867. This paper was written, however, seventeen years ago. I found it along with the letters of Sir John Herschel I have elsewhere quoted. The paper and the illustrations appeared to me worth preserving in connection with Sir J. Herschel's letters.

of our galaxy was more than I could manage. Its complexities seemed unintelligible; though I did not wholly dismiss the hope of discovering a tolerably simple solution of the difficulties which presented themselves. "I may, perhaps," I remarked, "return on some future occasion to the consideration of the subject." When I thus wrote I was in hopes that the apparently intractable windings of the galaxy, as exhibited to us in the drawings of Sir John Herschel, would have been long ere this reduced into something like order.

For I must admit that it seemed to me as though our astronomers had been wilfully increasing the difficulty of the problem by the perverse way in which they had chosen to regard it. It was well fitted to the noble genius of Sir William Herschel to take a wide view of the sidereal scheme. Indeed, standing where he did when he first attacked the problem, he had no choice but to select the more obvious and general features of the stellar scheme for his consideration. But as he progressed with his work he gradually began to modify many of the views which he had formed when the work was commencing. Or rather, I should perhaps say that he began to test the general principles on which he had been compelled to base his inquiries. And there are few more interesting subjects of study than the gradual progress by which our great astronomer made his way from one point to another, until towards the end of his life he seemed preparing to lay before the world views which, while the direct fruits of his earlier hypotheses, were yet altogether opposed to them.

The work which the elder Herschel has thus carried so nearly to its completion fell into no unworthy hands. Sir John Herschel, inheriting his father's grand powers of generalization almost undiminished, possessing also a capacity for laborious and farsighted observation altogether equal to his father's, and a more thorough acquaintance with mathematical modes of reasoning, seemed capable of pushing the theories of the universe to that point which I believe they would most certainly have attained had Sir William Herschel lived a few years longer.

But there was, I think, a difficulty in the way. The feeling I have when I rise from the perusal of any of those noble passages in which the younger Herschel presents or discusses the views formed by his father is, that he has been at times prevented from prosecuting inquiries which seem opposed to the general direction of his father's researches, by a feeling—very natural and amiable—of respect for his father's work and fame. I could point to many passages which seem to me to force this view upon us, but I will content myself with noticing two singular illustrations.

Sir John Herschel is describing the configuration of the Milky Way in the southern heavens. He has occasion to speak of the striking brightness of the galaxy in the southern skies. Now it need hardly be remarked that on Sir W. Herschel's theory of the galaxy this great brightness is very difficult of explanation. That this is so, in fact, is proved by this, that, whereas Sir John Herschel felt that we could only explain the phenomenon naturally by supposing our sun to be nearer this part of the Milky Way, Professor Grant points out (very justly) that on Sir William Herschel's theory the phenomenon requires that the sun should be nearer to the opposite part of the Milky Way, for on this supposition alone would the number of stars towards the south be greatest. Sir John Herschel gives the obvious explanation, however; and he seems to feel how strongly it is opposed to his father's theory, for he adds that the galaxy "*on this view of the subject would come to be considered* as a flat ring of immense and irregular breadth and thickness, within which we are eccentrically situated nearer to the southern than to the northern part of its circuit." Yet he nowhere adopts this view. I feel certain that had the disc theory of the galaxy been due to any but Sir W. Herschel, the observation would have led the younger Herschel to adopt at once and finally the ring theory, though I believe he would soon have seen reason to modify his opinion of the ring's shape and figure.

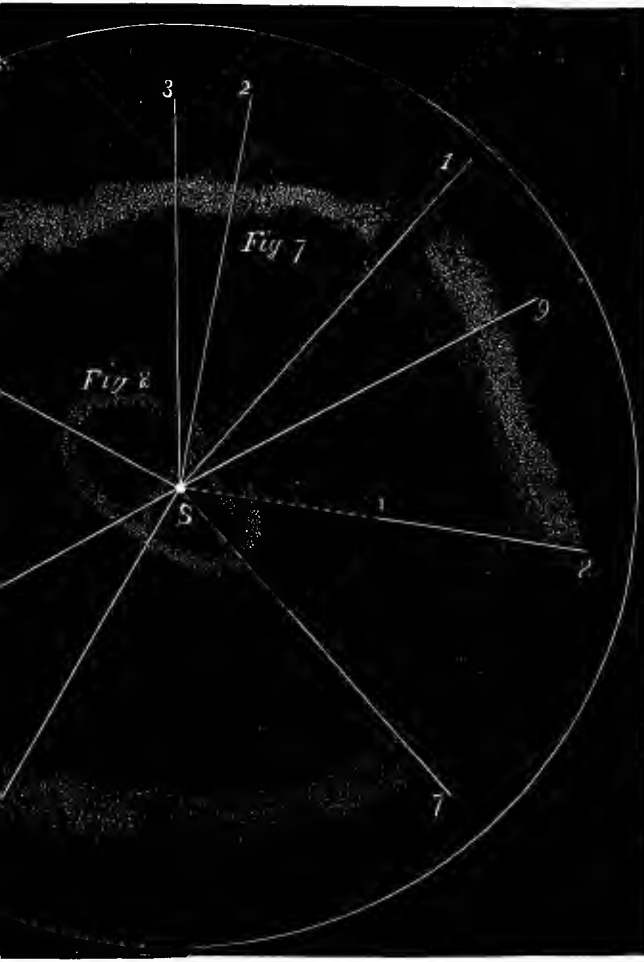
Again, Sir John Herschel is discussing the Magellanic clouds. He is impressed with the evidence they seem to afford of the fact that, within very moderate limits of distance, the faintest telescopic stars and nebulæ of all degrees of irresolvability may be mixed up with stars of the eighth and ninth magnitude. Nay, he points out in his own lucid manner that, according to all the laws of probability, we must look on this fact as established beyond dispute. He sees also, as in the preceding instance, that this view is altogether opposed to his father's views respecting the universe. Yet he closes the discussion of the overwhelming evidence thus afforded against one of the most striking of his father's views with the simple remark that, "It might lead us to look with some doubt on conclusions which in former pages of this work have been somewhat positively insisted upon." A certain fact is proved beyond all question, yet in the remaining pages of the "Outlines of Astronomy" that fact is completely ignored.

Even as it is, Sir John Herschel's views respecting the galaxy are marked by a certain advance upon his father's. Although not definitely adopted, we must look on the ring theory of the Milky Way as that which the younger Herschel held in preference to the disc theory.

Now, it will be noticed that wherever Sir John Herschel has occasion to refer either to the narrower portions of the galaxy or to the branches which appear to extend from it, he always exhibits a preference for the view that these narrow star-beds are in reality the side views of widely extended star-strata. He says, indeed, in one place, speaking of a region where several branches of this sort are visible, "it is obviously more reasonable to suppose that these are sheets of stars viewed edgewise, than to imagine they are real columnar excrescences, bristling up from the general level."

I think we must recognize in this peculiarity the influence of the preconceived opinion that not merely our sidereal system but all the parts of it exhibit a certain tendency to lateral extension, so that the existence of a columnar star-group, or of what I should prefer to call a star-stream, is improbable *à priori*. Otherwise, I confess I am unable to conceive how his intimate acquaintance with the principles of probabilities could have failed to enforce upon Sir John Herschel the feeling that the many long and narrow streams which he saw extending from various parts of the galaxy must in most instances, if not in all, be stream-shaped. Nay, even with preconceived views rendering the estimated chance of the existence of galactic star-streams only $\frac{1}{100}$, yet the existence of two such streams would have balanced that *à priori* improbability, since we can hardly estimate at more than $\frac{1}{10}$ the chance of a sheet of stars being seen edgewise, and therefore the chance of two being so seen would be only $\frac{1}{100}$. Now, Sir John Herschel saw *many* such excrescent streams.

It will be seen at once that the existence of small streams extending from the galaxy goes far to prove the stream-formation of the galaxy itself. When this evidence is added to that which I adduced in my former paper, the conclusion seems to me to be altogether obvious that the apparent stream of milky



FIGS. 7 AND 8.

[*To face page 85.*

light which we term the galaxy is in reality a stream of small stars, surrounding us on all sides.

But I would go farther, and assert that the naked-eye appearance of the Milky Way is sufficient evidence on which to ground the belief that there is a distinct ring of matter out yonder in space, and that this ring is not flattened, as Sir John Herschel thought, but is (roughly speaking) of nearly circular section throughout its length. I conceive that nothing save the perverse way in which astronomers have chosen to deal with the phenomenon would ever have led them to forget the evidence of their senses in this matter. Of course, if we insist on taking the average number of stars visible on a certain space of the heavens as indicating the density of the stars over that space, although it is perfectly obvious to the eye that there is a distinct and systematic arrangement of the stars there, wholly negativing our initial supposition, we must expect to be misled. With all respect for the elder Struve's labours, I must admit that this seems to me to be what he has done in his famous distribution of the stars according to zones of given galactic polar limits.

Consider, however, fig. 7, which represents the galaxy as actually seen in the heavens,* and it becomes wholly impossible to believe that we have to deal with the projection upon the celestial sphere of a widely-extended cloven disc of stars. The view does not account for *one* of the peculiarities of the galaxy proper, however justly it may seem applicable to the sidereal system. The great break in Argo (opposite line I in our figure) is of itself sufficient

* The mode of projection must be conceived to be as follows :—Suppose that on a celestial globe a band is taken, including the whole of the Milky Way, and that this band is spread as a long, straight slip on a plane surface. If, then, we conceive the band turned into a circular strip, from the uniform contraction of one edge, we shall have such a map as fig. 7.

to negative the disc theory ; so is the coal-sack near Crux, and so are the somewhat similar vacancies in Cygnus and Argo. The fact, too, that the second stream (which has led to the assumption that the sidereal disc is cloven) is not continuous, is one which cannot possibly be explained on the disc theory.

But, although one may feel convinced that the galaxy is really a stream of relatively small stars surrounding our heavens, it has always seemed to me a very difficult matter to account for the various phenomena presented by the Milky Way on any reasonable hypothesis. It was easy to see that, whatever hypothesis we adopt, we must be prepared to admit of the existence of great irregularities. In fact, as such a stream as I conceive the Milky Way to be would be subject to a number of attractions, swaying its length here in one direction, there in another, those irregularities were to be looked for independently of any considerations founded upon the observed appearance of the Milky Way. But there were certain features which I felt that any hypothesis for which support could reasonably be claimed ought to explain.

The difficulty I found was in conceiving how, first, the *interruptions*, secondly, the *variations of brilliancy*, and thirdly, the *lacunæ* in the Milky Way, could be accounted for by any single stream however shaped. An explanation, which accounted for the interruption opposite line 1, left the interruption opposite line 2 unaccounted for. Again, I did not find it easy to account for the sudden access of brilliancy at 8, the extreme faintness at 7, or the fact that of the two branches starting from 5 the fainter becomes presently the brighter, and *vice versâ*. The three coal-sacks also were a great mystery to me.

I have again and again attacked the problem (which now seems perfectly simple and easy) without being able to imagine a stream of reasonable figure which would account for these peculiarities.

At length (more than two years after I had come to the conclusion that the galaxy is really a spirally formed ring of generally circular section), by looking on the break opposite as due to increase of distance, not to a real interruption of continuity, I was able to construct a single spiral curve which seems completely to meet all the requirements of the problem. This curve is exhibited in fig. 8, which is supposed to exhibit the actual figure of the galactic spiral in space. It is so situated that the various lines drawn from our sun, supposed to be at S, intersect the various portions of the figure representing the real galactic stream, opposite the regions in which these lines meet the figure of the galaxy on our heavens.

We see that line 1 passes through a gap between the two loops of the galactic spiral. This seems (to begin with) a simple explanation of what has hitherto been admitted to be one of the most perplexing features of the Milky Way. Passing to position 2 the line crosses two branches of the curve, and the coal-sack is accounted for by the deviation of one branch (or both branches) slightly from the mean galactic plane. From position 3 the line crosses one branch at a very small distance, the other being much farther off. This corresponds closely with the appearance of the two branches, the continuous one being very much the brighter, and some portions along this part of its length being described by Sir John Herschel as singularly bright. It is also well worthy of notice that the two stars which are nearest to the sun (so far at least as observation has yet shown) lie along this branch of the galaxy—α Centauri very nearly where the branch approaches closest to the sun, 61 Cygni in direction

S5, where the branch is some three times farther off.* The farther branch attains, along S4, so great a distance from the sun as to become invisible. This corresponds with the *mode* of the discontinuity of this part of the Milky Way, for each end of the broken division *loses itself*, not terminating abruptly like the two fan-shaped terminals opposite the line S1.

Near this portion of the circuit we are provided with an explanation of what had always been looked upon as a great difficulty. Where the two branches start from the coal-sack in Cygnus (on S5), the northern branch is much the brighter, but presently the northern branch grows fainter and ultimately vanishes, while the southern grows brighter and brighter. This is fairly accounted for by the figure I have assigned to the spiral.

The projection at 6 may be accounted for by assuming the end of the spiral to be curved backwards as I have shown it.

Lastly, the faintness at 7, the projection at 8, and the vacuity at 9 are obviously accordant with the figure given to the end of the spiral which falls opposite the lines to these parts.

Without asserting that the actual figure of the galaxy in space is that shown in fig. 8, I yet think it probable that the order of its windings resembles that shown in the figure. I believe, however, that there are many irregularities not merely in the direction in which the spiral extends through space along its general plane, but in directions inclined to that plane. The appearance presented by the Milky Way in Aquila and Scorpio is strongly suggestive of such peculiarities in the real figure of the spiral.

* According to the annual parallaxes assigned to these stars, 61 Cygni is between two and three times as far from us as α Centauri.

I feel convinced, further, that the study of the Milky Way as presented in fig. 7 will at once dispose of the notion that the galaxy can be either a cloven disc or a flat ring, or that the section athwart any branch of it can be otherwise in general than roughly circular.

CHAPTER VII.

THE SIDEREAL SYSTEM FATHOMLESS.*

It is commonly supposed that Sir W. Herschel's plan of star-gauging demonstrated that the sidereal system has limits to which his gauging telescopes penetrated (save in a few directions), and that even where the system has its widest extent its limits are certainly attainable by such telescopes as men may well hope to construct. I would invite attention to certain evidence pointing to a very different conclusion.

It is perfectly clear that if the sidereal system have the figure hypothetically assigned to it by Sir W. Herschel, that of a lens-shaped stratum throughout which stars are distributed with tolerable uniformity, then we must accept the evidence adduced by him as sufficient to prove that we can attain to the limits of this stratum. To use the words of Professor Nichol: "When an eye is directed towards a prolonged bed of stars, there is no reason to fancy that it has reached the termination of that stratum, so long as there appears behind the luminaries, which are individually seen, any milky or nebulous light, such light probably arising always from the blended rays of remoter masses. But if, after struggling

* This paper, like that on the Figure of the Milky Way, was written when I was in correspondence with Sir John Herschel about the architecture of the heavens.

long with a nebulous ground, we obtain a telescope that gives us additional light with *a perfectly black sky*, we then have every reason the circumstances can furnish on behalf of the supposition that at length we have pierced through the stratum—a probability, indeed, which can be converted into certainty in only one way, viz., when no increase of orbs follows the application of a still larger instrument." Sir John Herschel also says that in those regions where the zone is clearly resolved into stars well separated and seen projected on a black ground, it is *certain* if the ordinarily accepted theory be correct, that we look out beyond into space.

But this conclusion would no longer follow as a necessary consequence of such observations if, instead of regarding the sidereal system as of the figure and structure suggested by Sir W. Herschel, we supposed it to consist of clustering aggregations (including *streams* under that expression) of stars of every variety of magnitude. Then, in struggling with a nebulous ground, we should not be penetrating farther and farther into the celestial depths, but should be simply analyzing more and more searchingly a definite aggregation of stars.

Let us consider a noteworthy instance, interesting not only because it illustrates the mistakes which might arise from falsely assuming a certain uniformity in stellar aggregation, but because it shows how a thoughtful astronomer like Sir W. Herschel would instinctively recognize, under such circumstances, the fact that he was going astray, and would be capable of quietly relinquishing views on which he had before laid considerable stress.

In the constellation Perseus there is a magnificent double cluster, visible to the naked eye on tolerably clear nights, and presenting, even in small telescopes, a scene which forces sensations of awe and reverence upon the least thoughtful mind. With a large telescope the spot "appears lighted up," says Nichol, "with unnumbered orbs, and these pass on and on, through the depths of the infinite, until even to that penetrating glance they escape all scrutiny, withdrawing into regions unvisited by its power. But shall we adventure into these deeper retirements? Then assume an instrument of higher efficacy, and lo! the change is only repeated; the nearer stars now shine more brilliantly; those scarce observed before appear as large orbs; and behind a new series begins, again shading gradually away, leading towards further mysteries! The illustrious Herschel penetrated, on one occasion, into this spot, until he found himself among depths whose light could not have reached him in much less than four thousand years. No marvel that he withdrew from the pursuit, conceiving that such abysses must be endless!"

But this conclusion, that the light from the farthermost parts of the cluster occupy some forty centuries in reaching us, while the light from the larger stars in the cluster, according to the usual estimate of star magnitudes, would occupy but one or two centuries, brought with it perplexities which Sir W. Herschel was too clear-sighted not to recognize. It required that the real shape of the cluster should be somewhat as is shown in fig. 9, in which S is the sun, and S B is some twenty times as great as S A. On no other supposition could the peculiarities of the cluster be explained, so long as it was understood that a general uniformity of magnitude and distribution prevails among the component stars.

Sir W. Herschel was thus led to recognize the cluster as including within its bounds stars varying greatly in real magnitude. Nay, he pronounced the opinion that we have in this cluster a sort of nodule of the Milky Way—a distinct clustering aggregation of stars within the limits of the galaxy.

Now let us consider what such a change of view

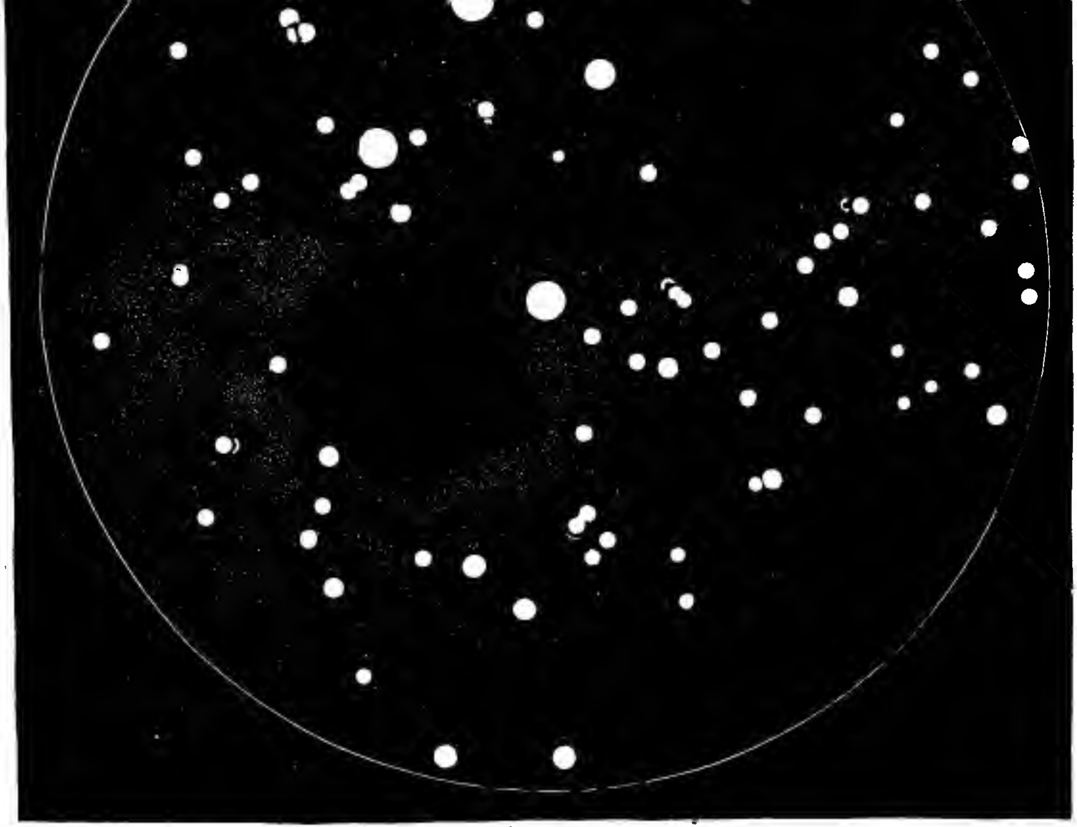


FIG. 10.—THE COALSACK IN CRUX.

[*To face page 93*

meant. Instead of supposing that each increase of telescopic power had enabled the observer to pierce farther and farther into the sidereal depths in this direction, Sir W. Herschel now saw that all the successive investigations had dealt with the same region of space. The difference was as great as though an astronomer, discovering new asteroids and conceiving their minuteness to be due to distance, so that they all lay hundreds of times farther off than Jupiter, suddenly learned their true nature, and that all his researches had dealt with a zone far within the orbit of the giant planet.

Are we quite sure that a similar error does not affect the whole system of star-gauging, or rather the fundamental principle on which that system is established? What real evidence have we that, when we are poring more and more searchingly into the ecesses of the great star-girdle, we are passing ever to distances farther and farther from the sphere of the lucid stars?

It seems to me that, before accepting the results which have been supposed to flow from the star-gaugings, we are bound to inquire somewhat more closely than has yet been done into the question whether the probabilities are in favour of that general uniformity of distribution and magnitude on which the plan was based.

And here an important point presents itself to our consideration. *Admitting* this fundamental hypothesis, it is very obvious that we must pay no attention to the signs of special laws of association among the lucid stars. These stars are altogether dissociated from each other in reality, however they may seem associated—if only that hypothesis is correct. But when we are inquiring *whether* that hypothesis is correct, these signs of association are all-important for our guidance. We are bound to inquire whether they *can* be accidental. And so we are no longer free to smooth the star-groupings away by taking averages.

This bears in a very important manner on the problem presented by the Milky Way. Sir John Herschel, following very accurately the law of star-gauging, compares the total number of lucid stars on the galactic zone with the total number on the rest of the sky, and finds no trace of any aggregation in the former region. Hence he concludes (very justly, when once the fundamental law is accepted) that there is no real association between the lucid and the telescopic stars on the galactic zone.*

But suppose that instead of considering the galactic zone, instead of spreading the galaxy over a belt which it does not really cover, we look at the galaxy itself. And suppose, further, that as a first process of examination we compare the number of lucid stars falling on the galaxy with the number falling on the dark rifts and coal-sacks in the Milky Way, and on the space which separates the two branches where the galaxy is double. Doing this, we find at once the most striking evidence that the lucid stars are closely associated with the telescopic galactic stars ; for we find a marked disproportion between the number of stars on the dark regions and the area covered by these regions. In many places, especially in the southern heavens, we find the very shape of the Milky Way indicated by the stars which lie round the border of the dark regions, but

* In his "Outlines of Astronomy" he uses expressions which would seem to indicate that he had forgotten the facts very clearly established and described on pages 381 and 383 of his observations made at the Cape of Good Hope. Nor can one wonder at this when one considers the wonderful range and extent of the observations recorded in that most valuable treatise, second only (if second) in value to the series of papers by his father in the Philosophical Transactions of the Royal Society.

withdraw themselves, so to speak, from those vast openings into space.

Take as an illustration the coal-sack in Crux. Is it an accident that over this large dark space, covering about 50 square degrees, there is not a single lucid star, while all round its borders lucid stars are strewn in plenty? The whole surface of the heavens exceeds the coal-sack some 800 times in extent; and as there are about 6,000 lucid stars, one might expect seven or eight such stars to be found in the coal-sack. But this is far from being all. The neighbourhood of the coal-sack is much richer in lucid stars than other regions in the heavens; so that it is just where stars should be most richly distributed that this vast black spot makes its appearance. The question whether the absence of stars from the coal-sack and their presence in great abundance in the Milky Way around that vicinity are to be regarded as a mere coincidence can scarcely be doubtful, I think, to any one who studies thoughtfully the portion of the galaxy depicted in fig. 10. Nor, perhaps, is the way in which the sharply defined semicircular cavity on the right is associated with a semicircular stream of stars less significant. No one who examines this region thoughtfully can doubt, I should imagine, that the lucid stars seen in it are mixed up with the telescopic stars forming the Milky Way here.

But if we admit that such evidence as this (and much more of the same kind might be adduced did space permit) should lead us to regard the Milky Way as forming a stream of really small stars, swayed into its present figure by the large ones in its neighbourhood, it might seem that, so far from showing that our sidereal system has no limits, we should have gone far to prove that its dimensions are much smaller than had been imagined.

It is true that, according to these views, the small stars in the Milky Way would be far nearer to us than has been commonly supposed. But, on the other hand, it would follow with equal certainty that we could no longer imagine we had even in any one direction pierced to the limits of the sidereal system. If in searching into the depths of any part of the Milky Way we are, in truth, merely searching more and more closely within a definite group of stars, beyond that group there may lie at enormous distances other groups which no telescope we can construct may even render visible. It was only, indeed, whilst it was thought that the sidereal system is continuous throughout its limits that astronomers could hope to say where those limits lie. If, on the contrary, I am right in believing that the sidereal system consists of aggregations of every conceivable form, those aggregations may extend into space, millions on millions of times beyond the limits of the most powerful instruments man may ever be able to construct.

CHAPTER VIII.

SUNS AND METEORS.

IT may seem strange to associate suns and meteors, fixed stars and shooting stars. One can scarcely imagine bodies more unlike—suns, the mightiest, because the most massive, of all the subjects of astronomical research, and meteors, many of which are so small that in their brief rush through our air they are entirely dissipated, and in a sense destroyed. For millions, nay, for hundreds of millions of years, a sun endures, pouring forth moment by moment supplies of light and heat—the life of worlds circling around him — in quantities so enormous that the human mind is utterly unable to conceive them. The falling star glows but for a few seconds, and then its brief career comes to an end. Weighed in the scales of science, the suns which people space are found to outweigh, severally, such globes as our earth, hundreds of thousands of times : the falling star has also been weighed, and its average weight is found to be but a few grains!

Yet, as shooting stars have been unmistakably associated with comets, which seem so utterly unlike them, so have they now been connected, by evidence which seems too strong to be resisted, with suns. Quite recently, indeed, meteors of a certain kind have been discovered which tell us *that* respecting the noblest order of suns which no instruments made by man could have revealed.

Let us briefly consider the line of reasoning by which it has been shown that large numbers of the meteoric bodies which reach our earth from outer space have been ejected from the interior of suns, or of bodies in a sunlike state. We may then examine the new discovery, and consider its bearing on the theory of the origin of meteors.

In former times it was the received theory respecting meteors that they had their origin in the upper regions of the air. But it was at length proved that, instead of that long-received theory, a theory which had been rejected as too absurd for credence must be accepted. It was found that meteors reach our earth from interplanetary space. As Humboldt well expressed it, "They bring to the earth extra-terrestrial matter; they are the only messengers which reach us from regions outside the world on which we live."

But the nature of their paths was long unknown. All that had been proved was that they travel in flights around the sun as their ruling centre. The proof was twofold. Because shooting stars are seen in showers on special days of the year—not of each year, but still so often as to show that the coincidence of date is no mere accident—it is certain that they travel on paths crossing the track of the earth at particular points. Each star-shower having a special date forms a distinct system. The second proof was equally decisive. The meteor-paths during any great display always seem to radiate from the same fixed point on the star sphere, no matter how many hours the display may last, or how much, therefore, that point may change in position with regard to the horizon. It follows that their paths are parallel before they reach the earth.

The last point is to be specially noticed. It not only affords a subsidiary proof of what was already established by the agreement of dates. It tells us something new about the meteors and their movements. The observer on earth is carried round the earth's axis during the display by the earth's motion of rotation. This motion, though slow compared with the movement of the earth in her revolution around the sun, is nevertheless considerable in itself. At the equator, a point on the earth's surface moves rather more than 1,000 miles an hour; in latitude 45° north or south, the rate of motion is about 750 miles an hour. (London is carried round the earth's axis at the rate of more than ten miles per minute.) Now the earth travels round the sun at the rate of 18½ miles in a second, and meteors would usually cross the earth's track with velocities greater than these, since a body travelling (as most meteors travel) around the sun, on an orbit extending far beyond the earth's, would have at the earth's distance from the sun a velocity of about 26 miles per second. The effects then of the earth's rotational movement, as hour by hour an observer's direction of motion (due to this cause) is altered, can but slightly modify the apparent direction of meteoric motion. Still it might be expected that in many cases these effects (which may be compared to the apparent change in the direction of rainfall, as our motion through the rain, in walking or riding, is modified) would be recognized. The circumstance that no observer of meteors has ever detected such effects shows that in all cases hitherto dealt with the velocities with which meteors encounter, or overtake, or pass athwart the earth are enormously greater than the velocities with which points on the earth's surface are carried round her axis, and greater also than the velocities which the earth can communicate to bodies approaching her from outside.*

* The velocity which the earth could communicate to a body drawn to her surface from an indefinitely great distance, by her own attraction only, would be nearly seven miles per second;

From Olmsted's demonstrated theory of meteors (the credit of which has been very calmly bestowed of late on persons who had done no more than note a few circumstances consistent with it) has been followed, since 1866, by a series of interesting discoveries. It has been shown that the meteors of November 13-14 travel in a period of 33¼ years round the sun in a path extending beyond the orbit of the planet Uranus, and passing very close to this orbit at one point. It has been shown further that the meteors of August 10-11 and of November 13-14 travel on the tracks of known comets. It has been rendered highly probable that every meteor system tells us of the course of a comet, though not necessarily of a comet now in existence, while every comet is followed by a train of meteoric attendants. This train, by the way, must by no means be confounded with the comet's tail—a very different formation and occupying an entirely different position. In the only case where the earth has ever been known to be approaching the track of a known comet, prediction was made (by myself and Professor Alexander Herschel) that a display of shooting stars would be seen, radiating from a particular point of the heavens, at the time when the earth was plunging through that comet's train of meteoric attendants; and this prediction was fulfilled to the letter. We may fairly infer that what has been shown of all the comets whose paths have crossed the earth's track is true of comets generally.

When so much as this was known about shooting stars it was natural that astronomers should begin

but the bodies which come near the earth, or actually encounter her, are already travelling, for the most part, with much greater velocities, communicated by solar attraction; and she has not time, during their swift rush towards or past her, to impart more than a tithe of the velocity which she could communicate were she alone at work upon them, and they had no sun-imparted velocities.

to form ideas as to the origin of these bodies. Accordingly a theory was advanced in 1866 by Signor Schiaparelli of Milan, which, because no one at the time dwelt on any of its shortcomings, or advanced any other theory, has come to be regarded by many as an accepted theory, and was so spoken of recently by Professor Young, of Princeton, N.J., in his farewell address before the American Association for the Advancement of Science.

Schiaparelli's theory was this:—He assumed that flights of meteors are travelling about through the realms of interstellar space in the form of nebulous clouds. Under the attraction of some sun towards which their course has already in some degree directed them, they travel towards the region wherein his family are travelling. If by chance a flight of meteors should come near enough to one of the members of such a family, it is deflected from the course it had been following, and *may* (under particular conditions) be retarded. If so the future course of that flight of meteors will be a closed though eccentric orbit around the sun attended on by that disturbing planet. Such closed orbit will necessarily pass through the point where the disturbance was produced by which the meteor flight was, in a sense, captured. The theory requires further that an immense number of such captures should be made. For our earth passes through great numbers of meteor flights, and it is certain that for each meteor system (among those captured) through which our earth passes, there must be millions to which she does not draw near.

That this speculation—for it is obviously nothing more—should be described by so careful a student of astronomy as Professor Young, in terms implying that it is a theory based on the thorough investigation of an adequate amount of evidence, is strange, to say the least of it. One speaks of the Copernican system as the received theory of planetary motion, but even Laplace's widely-known hypothesis of the origin of the planetary system is not called the "received theory." Newton's theory of universal gravitation is received, but Le Sage's speculation respecting the origin of the force of attraction is regarded as a speculation only. In like manner the theory that meteor systems travel around the sun, or rather that all meteoric bodies reach our earth from outside with planetary velocities, is established by evidence which cannot be shaken; but the suggestion that meteors are drawn from interstellar space by our sun's attraction, and then by the casual intervention of one or other of the giant planets forced to travel on a closed path around the sun, is but a speculation, as little based on any real evidence as the old-fashioned idea that rain comes down upon the earth from some great reservoir of water above the crystalline.

Of the general idea that meteors, and therefore comets, come to us from interstellar space, it may be said that in one sense it is manifestly probable, if not certain, in regard at least to many systems of meteors. Of many comets and meteors we have to admit that unquestionably the region whence they came on their last visit to the earth was that vast realm outside the solar domain which we call interstellar space. It is one thing, however, to admit this, another and a very different matter to regard interstellar space as a sort of breeding-place for meteors and comets. To explain them thus is to interpret a marvel by a miracle. It may be difficult to say whence meteors came to occupy in such inconceivable numbers the interstellar spaces; but it would be hopeless to attempt to show how they might be understood to have been there from the beginning.

But while there is this overwhelming negative objection to Schiaparelli's speculation, that in effect it explains nothing, there is a positive objection of the most decisive nature. It is one which I pointed out long since, one whose validity has been admitted, and one which has never yet been in any way answered, though Professor Young has suggested that possibly some way of answering it *may* yet be suggested.

I will not here enter on the considerations, chiefly mathematical, on which the objection I am about to indicate is based. I will note only what is the certain result of applying mathematical tests. The giant planets *cannot* do what Schiaparelli's theory requires that they should do. The individual members of a flight of meteors travelling from interstellar space towards the solar system *may* chance to pass near enough to one of the giant planets to be caused thenceforth to travel on closed paths around the sun; nay, the flight itself might be captured (in this sense) bodily. But there is no possible way in which a flight of meteors, consisting, like the November meteors (the Leonids*) and the August meteors (the Perseids*), of many billions of billions of discrete bodies, could be so captured by a member of the sun's family, even by the giant Jupiter himself, as to travel on the paths which these systems actually pursue. As a matter of fact, if the Leonids have been captured at all, as Schiaparelli imagined, it must have been by Uranus, whose capturing power is utterly insignificant compared with that possessed by Jupiter and Saturn; while the Perseids, if captured by any member of the solar system, must have been captured either by some planet exterior to Neptune or by the earth herself; for the Leonids only approach the orbit of Uranus and the earth in their

* The reader is not to suppose that the Leonids are the only November meteors, or the Perseids the only August meteors; I add these names to show which particular set of November meteors and which particular set of August meteors I am referring to.

course around the sun, while the Perseids approach the orbit of no known planet except the earth. Now, taking the Leonids (for, be it observed, a single instance will suffice, and the Leonids have long been regarded as strikingly illustrating Schiaparelli's theory), we find that for a single member of this family to have had its path changed from one passing out into interstellar space to one having a period of $33\frac{1}{4}$ years—the actual period in which the Leonids complete their circuit—that meteoric body must have passed very close indeed to the globe of Uranus. A certain amount of the meteor's motion would have had to be withdrawn by the attractive power of Uranus, and as the velocity eventually abstracted is only the excess of the quantity abstracted during one part of the time when the body was near Uranus over the quantity added during the rest of that time, it is clear that Uranus must work very hard to produce the desired effect on a body which rushes past the planet with a sun-imparted velocity of several miles per second. When details are considered, it is found that the approach of a meteor to Uranus, as the meteor came in from outer space, would have to be so very close as to preclude the possibility that a flight of many billions of billions of meteors could *all* pass near enough to have that path assigned to them along which *all* the Leonids actually travel. And so with other cases—with every other case where the actual periods, and therefore velocities, of meteors are known.

Despite the opinion of Prof. Young, that in some way or other this objection may be explained away, I venture to say with the utmost confidence (and I think undue confidence about such matters is not a fault with which I can be charged) that the giant planets cannot have captured one of the flights of meteors whose true period of revolution has been determined. It may be that some among the four hundred or so of meteor systems which the earth encounters in the course of each yearly circuit around the sun have been captured in this particular way; but, so far as known facts are concerned, and especially those known facts which led Schiaparelli to formulate his so-called theory, it is certain not only that we have no evidence in its favour, but that all the real evidence is opposed to it.

It was this which led me to believe that meteors have had their origin, or that at any rate multitudes among known meteor systems have had their origin, in another way.

Note, first, that the researches of Stanislas Meunier and others have led many—as Tschermak and Ball, for example—to the opinion that some at any rate among the meteors annually encountered by the earth are her own children. In other words, there are reasons for thinking that during some remote past period the earth had the power, being then full of the fiery energies of planetary youth, of ejecting from her interior flights of missiles—clouds of world-dust, so to speak—with such velocity that the matter thus ejected was free thenceforth to travel around the sun, with no other subservience to its parent orb than is involved in the circumstance that, for ever thereafter, the paths of such ejected missiles would cross, or pass very near to, the track of the earth.

With regard to this idea, which at first seems fanciful in the extreme, I may remark that there seems reason to believe that every orb in space passes through stages of orb-life which may be divided roughly into three: the sunlike, the earthlike, and the moonlike;* and therefore we must re-

* Another classification may be suggested—the glowing vaporous state (like the sun's), the fiery state (like that in which the giant planets seem to be), the life-bearing state (like the earth's), the state of old age (of which Mars seems to afford an example), and the death-like state (which the moon seems to have reached).

cognize in the past history of our earth a time when her energies were far more active than those she now has. We cannot infer her power of ejecting matter from her interior, when she was in the sunlike state, from that which she possesses now when she is in the middle of the life-bearing portion of her career. When she was a sun she was a very small sun, a mere dwarf compared with the giant Jupiter when he was a sun, and a mere speck of light compared with the mighty sun which rules our system. Yet she probably possessed then eruptive powers compared with which those she now possesses are as nought. Yet Krakatoa taught us recently, as at other times the earth-throes of Peru and Chili, of Sicily, Naples, Spain, and Iceland have taught us, that the earth's eruptive energies are even now in no sense contemptible. The probabilities are at least highly favourable to the theory enunciated by Tschermak. For the immense numbers of sporadic meteors encountered by our earth almost compel the belief that her track must be regarded as in a sense infested by meteors—crossed, that is, by greater numbers of these bodies than traverse similar parts of the solar system outside, or within, or above, or below (north or south*) of the earth's path. This would mean, of course, that the earth has had something to do with the strewing of this track with meteors; and as the earth most assuredly has never had the power of drawing meteors from paths on which they had entered under solar influence (as Schiaparelli imagined that the giant planets might have done) it seems to follow inevitably that the earth has given birth to this surplus stock of earth-crossing meteors.

Let it next be noticed that there are certain

* It is as correct to speak of north and south with reference to the plane in which the earth travels, as with reference to the plane of the earth's equator.

families of comets which have been for many years associated with the giant planets. Many years ago, and long before I recognized the real meaning of the phenomenon, I wrote an essay, which appeared in a weekly magazine of wide circulation, in which I treated of the "comet-families of the giant planets." I gave this name to certain families of comets which, though circling around the sun as their real attracting centre, yet have paths approaching so near to the orbits of the giant planets that we may fairly regard these comets as in some way or other dependent on the giant planets — each on the particular giant planet with which it thus seems associated.

Now, as comets are known to be followed by trains of meteoric attendants, we may say that we have here a phenomenon closely akin to, if not practically identical with, the peculiarity in relation to the earth's orbit which Tschermak and others have endeavoured to explain (and, as I think, have successfully explained) by assuming that millions of years ago the earth herself ejected those particular meteors which form as it were the extra population of the earth's orbit region. So that we seem justified in adopting here, also, a similar explanation. Of course, if Schiaparelli's theory were anything more than a speculation, and still more if it deserved to be regarded as a received theory, we might hesitate before we rejected what would be, in fact, an explanation of the very peculiarity we are considering. But we have seen that not only has Schiaparelli's theory no claim to be regarded as a received theory, or as a theory at all in any proper sense of the word, but the objections to it are, in fact, absolutely insurmountable. We therefore turn to the other explanation as one which here naturally suggests itself —we inquire, at any rate, whether the cometic and meteoric families of the giant planets may not be regarded as originally ejected, in the form of meteoric streams, from the giant planets, when these were in the sunlike state.

It is manifest that we are justified in assuming that if the earth ejected meteoric bodies when she was in the sunlike state the giant planets would have done so likewise. Therefore there are *à priori* reasons for regarding as probable the theory to which we have thus been led by *à posteriori* considerations. Moreover, as the giant planets are still in a semi-sunlike state, we see that in all probability the meteor streams expelled from these planets would retain something of their original coherence; that is, they would appear in company with comets (each comet representing a cloud of meteors originally expelled as a coherent group). Thus we could understand the existence of the comet-families of the giant planets, though, of course, we can also understand that many comets formerly belonging to these families have disappeared as comets; indeed, we have been able to watch, in the case of Biela's comet, the process of disintegration, by which one of the members of Jupiter's comet-family has ceased to exist as a comet, and remains only as a stream of meteors.

But now two problems of interest present themselves to our consideration. In the first place, we have in the sun an example of an orb in that particular stage of orb-life during which, we have been led to suppose, meteoric ejection takes place, and we are naturally led to inquire whether the sun ever ejects flights of discrete bodies from his interior; and this inquiry will naturally be extended to his fellow-suns the stars. In the second place, we are led to ask how those comets and meteor streams are to be explained which assuredly have not been ejected from the earth or any of the planets; and *this* inquiry will have to be extended to those comets and meteoric streams which not only cannot have come from any member of the solar system, but cannot possibly have been derived from the central ruler of that system.

Now, among the remarkable discoveries made by means of the spectroscope, one of the most striking has been the recognition of tremendous solar disturbances of an eruptive, or rather of an explosive nature. In 1872, Prof. Young, of Princeton,* N.J., observed a solar eruption, in which what looked like filaments of glowing hydrogen (each many thousands of miles in length !) seemed to travel upwards from the sun's surface at the rate of about 145 miles per second, till they had reached a height of not less than 210,000 miles. Even then they did not cease to ascend; but, losing their lustre, faded out of view. If shreds of hydrogen were really shot out on that occasion we should scarcely find in the event anything bearing on the matter before us—the possible ejection of meteoric matter. But no one who considers the phenomenon with attention, or studies the evidence obtained in regard to it, can for a moment imagine that what look like ejections of glowing hydrogen can be really of that nature. It is obvious alike from *à priori* considerations and *à posteriori* evidence that the jet-like streams of hydrogen are in reality the *tracks* of ejected matter, solid or liquid. For, not only is it impossible that streams of such a substance as hydrogen should be ejected to heights of many thousands of miles through an atmosphere of probably greater and certainly equal density, but the shapes assumed by the hydrogen streaks are inconsistent with the idea that they can have been themselves ejected. For instance, the shreds of hydrogen observed by Prof. Young (some of which

* Of Dartmouth, N.H., when the discovery was made. It has been hoped that Prof. Young, with the much more powerful telescope at Princeton, will make discoveries even exceeding in interest those which he made at Dartmouth.

were thousands of miles long) were irregular in shape; had they really been travelling through a resisting atmosphere, at the enormous rate of 145 miles per second, they would certainly have been pear-shaped, rounded in front and tailed behind, like fire-balls in our own air. But they resembled, rather, the irregular streaks showing where our air has been rendered luminous by the passage of meteoric masses through it.

Prof. Young's observation proved, in fact, that on that particular occasion the sun had shot out from his interior a flight of many thousands of bodies. The bodies themselves would not be visible, because the phenomenon was observed through a telespectroscope, admitting only red light of the same tint as the red of glowing hydrogen. But the light from the heated hydrogen along the tracks of these ejected missiles would be clearly visible. The streaks would, of course, seem to ascend. For they would always be close up to the missiles producing them, so that their forward ends would advance, while their rear ends would seem also to advance as the light gradually faded out along those parts of the track which were farthest from the missile.

What Prof. Young saw has been seen since, at various observatories. The sun, then, *has* the power of ejecting matter from his interior—presumably in volcanic explosions. Moreover, a calculation which I made respecting Prof. Young's explosion shows that the matter ejected on that occasion passed away from the sun with such velocity that it would never return to him. Those missiles were thenceforth akin to meteoric bodies travelling freely through space.

We may fairly extend the evidence thus given respecting the one sun we are able to study to other suns—and the extension may be made to other suns in *time* as well as to other suns in space. If the one sun we are able to study, because he is comparatively near to us, and because he is a sun *now*, is able to eject flights of bodies from his interior to vast distances, and even to cast such bodies for ever away from him, the other suns which people space possess in all probability a similar power, and orbs which *were* suns in the remote past, possibly also orbs which *will be* suns hereafter, were or will be similarly active.

Taking first the extension of the evidence given by the sun to bodies no longer suns, we see that what has already been suggested in other ways is confirmed by the evidence of the actual eruptive power possessed by the sun. We see that millions of years ago, when Jupiter and Saturn were active suns, they probably possessed the power of ejecting flights of bodies from their interior as the sun does now, and many millions of years ago, when our earth and her fellow terrestrial planets were sunlike bodies, they were similarly active (each in its degree). For it is, of course, obvious that though a body like Jupiter would have nothing like the sun's eruptive energy (in amount), such an orb would need nothing like that energy to eject matter from its interior never to return. So with a globe like our earth. The sun must eject a body with a velocity of 380 miles per second, that it may never return to him; and Jupiter would have to impart a velocity of about forty miles per second to reject for ever a mass erupted from his interior; but in the case of our earth a velocity of seven miles per second would suffice to carry ejected matter for ever away from her (apart, of course, from the chance of subsequent capture by accidental encounter with the parent orb, whose course the track of the ejected mass would always thereafter approach or intersect). Now, though no volcanic explosions which at present take place eject bodies from the earth with anything like this velocity, yet remembering the intense activity of an orb in the sunlike stage, as compared with the energies of the life-bearing stage, we see that even apart from the evidence given by solar explosions, and from the subsidiary evidence given by the meteoric paths, we might safely infer that the volcanic outbursts taking place during our earth's sunlike stage were probably quite sufficiently intense to eject matter for ever from her interior. If such an explosion as that of Krakatoa can take place now, outbursts of the mightier sort necessary for meteor-ejection may well have occurred when the earth was a small sun. We have similar actual evidence even in the case of the giant planets, for whatever theory may be formed of the great red spot on Jupiter, there can be no doubt that a disturbance affecting an area nearly as large as the whole surface of the earth, and lasting seven years in full activity, implies most tremendous energies when Jupiter was in the sunlike stage of his career.

As to the future, we cannot speak so confidently. We know not what the bodies are, if bodies there be, which will hereafter become suns. Possibly the great gaseous nebulæ are forming into stars. It seems unreasonable, at any rate, to suppose that, as there are suns much younger than our own (Secchi's first order), as well as suns much older (Secchi's third and fourth orders), and suns long since dead (being dark), there are not also suns as yet unformed.

Ranging through space we recognize in every star a sun, not only like our sun pouring out light and heat, but doing doubtless such other work as our sun is doing. If he pours out in a single explosion thousands of meteoric bodies, in the millions of years of his life, he must have poured out many millions of millions of such bodies. The millions of millions of other suns which people space must have done likewise. So that inconceivable numbers of bodies expelled from existent suns must now be traversing space.

In such meteor streams—or comets—we find the explanation of those comets which reach our solar system from outside the planetary system. Some of the comets of long period may be regarded as having had their origin from our own sun, but only those whose paths approach very near to his globe. For although planetary perturbations might prevent a body ejected by the sun from actually returning to him, as, if undisturbed, it must inevitably do, such perturbations could not possibly give to a sun-expelled body a path passing far from the sun's globe.

The comets, therefore, or meteor systems which travel around the sun on orbits passing far outside the planetary system, and those whose orbits carry them away from our sun never to return, are explained as flights of bodies ejected either from our sun himself (in the case of a very small proportion only) or from other suns.

But among the suns there are some so much mightier than the rest that we might expect the meteor systems sprung from them to differ in marked degree from all others. I refer to the giant suns like Sirius, Vega, Altair, and others of Secchi's first order.* Sirius, judged by the quantity of light he emits, is probably at least a thousand times larger than the sun; and we may infer that the other suns of the same order are in like degree superior to our sun both in size and in energy.

Surely the meteor flights ejected from these giant suns would be as markedly distinct from those ejected by our sun and his fellows as these meteor flights are distinct from those ejected by the giant planets, and these in turn from those ejected by the earth and her fellow planets of the terrestrial order. In particular, the velocities of comets or meteor flights ejected from Sirius, Vega, and their fellows,

* As classified by their spectra.

would be apt to exceed enormously the velocities belonging to meteors ejected from suns of the same order as our own.

When I was first led to adopt the theory which I have here indicated I thought it likely some evidence might be obtained of meteor systems ejected from the giant suns. But no such evidence actually existed at that time—about twelve years since. Now, however, evidence of absolutely decisive nature, evidence not only confirming my theory, but explicable—so far as I can see—in no other way, has been obtained.

Five years ago Mr. Denning, of Bristol, announced that he had recognized some meteor systems which radiate for several months in succession from the same point in the star sphere, a result which seemed so surprising that at that time many rejected it. I rejected it myself for awhile. It seemed to me, indeed, too good to be true. But it has since been shown to be undoubtedly true. Now, the same reason which forces us to regard the radiation of meteors during several hours from the same point, as proving that our earth's velocity of rotation is insignificant compared with the velocities of these meteors, compels us to regard the velocity of the earth's revolution as insignificant compared with the velocities of meteors which radiate during several months from the same region among the stars. In six hours the rotational motion of a point on the earth changes through a right angle; in three months the motion of revolution of the earth herself changes in direction in the same degree. But one motion has a rate of only a third of a mile per second even at the equator, the other has a rate more than fifty times greater. Mr. Denning's observation shows that there are meteor systems travelling many times faster than the earth in this swift rush round the sun. These meteor systems can be no other than those which have been expelled from the giant suns.

Hence finally we recognize, by direct evidence,* four orders of suns and four orders of meteors.

First, earth-suns, long since dark, which expelled such meteor systems as those which have been recognized as earth-born.

Secondly, giant planets, long since deprived of sun-like brilliancy, but not yet dark, which expelled such meteor systems as now travel on orbits passing near the paths of Jupiter, Saturn, Uranus and Neptune.

Thirdly, bodies like our sun, which expelled and still expel such meteor systems as travel on orbits extending far beyond the solar system.

Fourthly, bodies like the giant suns, which expelled meteor systems travelling with much greater velocities than could be imparted by our own sun or his fellows of the same order.

* I have said nothing here of the evidence given by the microscopic, chemical, and physical examination of meteors. Such evidence has, in reality, proved that those bodies were once in the interior of orbs in a sunlike state.

CHAPTER IX.

COMETS AND METEORS.

WE find among men of science a singular mixture of caution and daring, degenerating sometimes into timidity on the one hand, and into rashness on the other. The scientific caution of a Newton, testing the theory of gravitation by line and measure, and calmly resigning it for awhile, because, as it chanced, line and measure were both inexact, may be compared with the noble daring of a Halley, boldly announcing that the comet of 1682 would return in 1758* on the strength of observations which, in our day, would certainly be thought insufficient to determine a comet's period. The timidity with which the profound reasoning of Olmsted respecting meteors was rejected, till simple observations made that obvious which he had made certain, may be contrasted with the rashness shown by those who have accepted the speculations of Laplace about the universe as though these were demonstrated theories.

Comets, the most mysterious of all the bodies known to astronomers, have been subjects of most marked timidity and of most daring rashness of scientific reasoning. That men should have been

* I am quite aware of the fact that the comet really returned in 1759, that is to say, that it was in 1759 that the comet passed its point of nearest approach to the sun. Halley's prediction, however, named 1758, and made as it was when the theory of gravitation was in its babyhood, it was a very fair guess.

unwilling to formulate definite theories about these wild wanderers is, perhaps, natural enough. But the calm, uninquiring confidence with which ideas have been advanced and suggested respecting comets is not so easily explained. One of these ideas, regarded by many as if it were an established truth, I propose now to inquire into,—the idea, namely, that comets have been drawn from those paths on which they chanced originally to approach our solar system, by the perturbing influences of the giant planets, and have thus been, in certain instances, compelled to travel around the sun in elliptical paths, instead of the parabolic or hyperbolic orbits on which they had been travelling before they were thus captured. I think I shall be able, first, to show that this theory is antecedently most unlikely; then to prove that even if it had been the most natural and probable theory conceivable, it is entirely inconsistent with observed facts, and, therefore, untenable. I shall then suggest a theory in its place which, were I to mention it just here, would probably be rejected at once as the wildest speculation imaginable. Possibly, introduced as it will be by a series of observed facts not otherwise explicable, it may not seem so repellent a little further on. But I shall ask the reader interested in matters cometic, not to turn to the end of this essay until he has read the beginning.

We start from the conception that all comets originally entered our solar system from without. They come, say Heis, Schiaparelli, and others, who have advanced the Capture Theory, from out of interstellar space. Now, it is no valid objection to this view that it gives us no idea how cometary matter came to exist in interstellar space, for in all inquiries into the past condition of the celestial bodies we must always come short of their actual origin. Thus, in considering the past of our solar system we may start from a chaotic vaporous state, or from a past condition in the form of cosmical dust, or from a condition in which the vaporous and the dust-like forms are combined; but if we are asked whence came the vapour or the cosmic dust we are obliged to admit that we cannot tell. If, hereafter, we should be able to say that it came from such and such changes in a quantity of various forms of matter, which we may represent by X, Y, and Z, we should still be unable to say how X, Y, and Z came into existence. So that I make no serious exception against the supposed origin of comets on the ground that it really leaves very much to be explained. Interstellar space is a convenient place to which to assign the origin of bodies so mysterious as comets. *Cela explique beaucoup de choses.* Almost anything might happen in regions of which we know so little, or rather, of which we know absolutely nothing.

Yet it may be worth while to remark that, on the whole, the interstellar regions are less likely to be the regions whence comets originally came to visit suns and sun systems, than to be regions whither comets strayed after leaving originally the neighbourhood of solar systems. The most probable idea about the interstellar spaces is that they are the most vacuous regions within the range of the sidereal system. The mere circumstance that comets came from out of them affords no better reason for regarding them as the original home of comets, than the circumstance that comets pass from the solar system into these interstellar spaces affords for rejecting that assumption. There is, in fact, simply no reason whatever for imagining that the place where comets came into existence is the vast unknown region around the solar system which we call interstellar space. Most comets come to us from thence; as many comets are travelling into that unknown region as are coming out of it. To form an opinion about the origin of comets from no better evidence than their last journey (out of millions, very likely) can afford, would be as absurd as for a day-fly to reason that the river flowing past the home of his race came out of the sky because a few drops of rain came thence.

Suppose, however, we admit that in interplanetary space there have been in the past, and still exist, such flights of meteoric matter as the theory we are considering assumes. Let us grant them, also, such motion as may save them from what otherwise would inevitably be their fate, viz., a process of direct in-drawing toward the nearest sun, and consequent destruction (with mischief probably to his orb), after a period of time which must be regarded as utterly insignificant compared with the time-intervals measuring the duration of a solar system.

It follows, then, that each flight of meteors would, in the long run, draw near some sun, without however rushing directly upon him; and, sweeping round his globe upon such path as chanced to result from the combination of its original movement and his attractive influence, would pass out again into interstellar space. This might happen tens, hundreds, thousands, or even millions of times, a comet either sweeping in a long elliptical orbit, with enormous periods of revolution, around one sun, or, if its velocity were slightly greater than that supposition implies, rushing first round one sun, then out into the depths of space to visit another sun, then to yet another, and so on, flitting from sun to sun for ever, or until the kind of disturbance in which the holders of the theory we are considering believe, had changed this sort of motion into actual orbital circuit.*

* I have here considered only two kinds of cometic orbit—the elliptic and the hyperbolic; for a true parabolic orbit would

In either case the minimum velocity with which a comet would be moving, when at any given distance from our sun, would be determinable within a few yards per second. It is well known that the velocity with which a body travelling to the sun from an infinite distance (though one cannot, of course, conceive such a movement) would reach the sun, would not exceed by a foot per second the velocity with which a body would reach him after travelling from the distance of the nearest fixed star. So, also, the velocities of bodies moving in orbits reaching half as far from the sun as the distance of the nearest star, would be the same within a foot or so per second as the velocities with which bodies coming to the sun from infinity would reach the same distance from him. If such bodies had originally a great inherent velocity, of course they would reach any given distance from the sun with much greater velocity. But this would not affect our estimate of the least velocity at that distance. Thus we know what the giant planets to which has been attributed the final capture of those comets which now form a part of the solar system, had to do. We can tell the precise velocity in miles per second, or, at least, the minimum velocity, with which our imagined meteoric flight would cross the orbit of Neptune, or Uranus, or Saturn, or Jupiter, as the case might be, before its capture. We know, in the case of each comet supposed to have been captured, the precise velocity of the comet at the distance of the planet which captured it—its special planet-master. The difference is the amount of velocity which the capturing planet had to take away in order to effect the supposed capture.

Observe that we are here on sure ground, if the theory is sound. It is certain that a comet in coming

be as unlikely, or rather as impossible, as a truly circular orbit among the planets.

from remote interstellar space to the solar system would have at the distance, say, of Jupiter, a certain velocity. It is certain that a comet now travelling in a particular orbit, approaching at one point very near to the orbit of Jupiter, has at Jupiter's distance a certain velocity, very much smaller. Hence, it is certain that, if Jupiter captured that comet by disturbing it, as it approached him on the last of its many free visits to the sun, the giant planet must have deprived the comet of so many miles per second of its former velocity. All we have to do is to find out how the planet could do this ; in other words, how near the comet must have approached the planet to be thus effectively disturbed.

These pages are not suited for the close and exact discussion of the case of any particular comet. I have elsewhere (in a paper which appeared in the "Proceedings of the Astronomical Society") given the details for certain cases which have been regarded as among the most satisfactory illustrations of the comet-capturing ways of the giant planets, and have shown that the theory is in those cases, and therefore in all, absolutely untenable, though so resolutely held. Still it may be well here to consider an illustrative general case—the simplest that can be taken, and also the most effective, because the conditions are, in reality, much more favourable than they are in any known case.

Imagine a flight of meteors to travel from interstellar space toward the sun until it reaches the distance of Jupiter, and that when at that distance it chances to pass very close to the orbit of Jupiter, and at a time when Jupiter himself is very near the place where the meteor flight crosses his track. Observe that the chances against each one of these contingencies are enormous. If we conceive a sphere around the sun, girdled by Jupiter's orbit, the meteor flight in its course sunwards might traverse the surface of that sphere (or, which is the same thing, might traverse the part of its course where it is at the same distance as Jupiter from the sun) anywhere, and we are supposing that it traverses that surface close to a particular girdling circle (technically, a "great circle" of the sphere). Suppose that by "close" we mean within a million miles; then the imaginary girdle of the sphere through which the meteor flight must pass to fulfil the required conditions is two millions of miles broad. The sphere itself has a diameter of some 960 millions of miles, and by a well-known property of the sphere,* its surface is 480 times greater than that of the girdling strip. The chance is but one in 480 that any meteor flight coming from interstellar space toward the sun will be within a million miles of Jupiter's orbit when at Jupiter's distance from the sun. Then Jupiter's path has a circuit of more than 3,000 millions of miles. Thus the chance that at the moment of the meteor flight's passing the orbit, Jupiter will be within a million miles on either side of the place of passage, is as two in 3,000, or one in 1,500. But the chances that both these relations hold is only as one in 1,500 multiplied by 480, or as one in more than 700,000. Thus, assuming—though the case is otherwise—that a million miles would be an approach near enough for capture, still only one meteor flight out of 700,000 which came from outer space could be captured by Jupiter.

This, however, is but the mere beginning. We may admit that millions of times as many comets or meteor flights approach our system as the planets have captured; and if so, we need recognize no special force in any such considerations as have just

* The property is this: that the surface of a sphere exceeds the surface of a girdling strip, such as we are considering, in the same degree (if the strip is relatively narrow) that the diameter of the sphere exceeds the breadth of the strip.

been presented. I only advanced them to suggest the conditions which are, as it were, essential for the process of comet-capturing by a giant planet.

Arrived at Jupiter's distance from the sun, the meteor flight from interstellar space will have a velocity of about eleven miles per second. Now let us inquire what its velocity must be reduced to in order that it may thenceforth be compelled to travel in a circle around the sun. As a matter of fact, all the members of Jupiter's comet-family travel in orbits whose remotest parts are near Jupiter's orbit, and to give a comet such an orbit as one of these, much more must be done in the way of reducing velocity than is necessary merely to make the meteor flight from outer space travel thenceforth in a circle at Jupiter's mean distance. We are taking, in fact, a very unfavourable case for our argument. Still, the velocity must be reduced, even in this case, by nearly three-tenths, or by more than three miles per second.

Now Jupiter's power to withdraw velocity from a body in his neighbourhood is measured by his power to impart velocity. In fact, both processes are but different forms of the same kind of work. Precisely as we say that the sun can communicate a velocity of 382 miles per second to a body approaching him from interstellar distances, and that therefore the sun can withdraw such velocity from a body leaving his surface at that rate and eventually bring such a body to rest out yonder in interstellar space, so can we make a corresponding statement for any planet,—Jupiter or Saturn, the earth, our moon, and even for the least of all the asteroidal family (supposing only the mass and size known). In the case of Jupiter, for instance, we find that the utmost velocity he can impart to a body reaching him from external space is about thirty-six miles per second. That, at least, is the velocity with which such a body would reach the visible surface of the planet. What the velocity might be with which the real surface, far down below the visible envelope of clouds, would be reached, we do not know,—not knowing where that surface lies. In the case of our own earth, the velocity with which a body would reach the surface, if brought thither solely by the earth's action from interstellar space, would be a little over seven miles per second, or more than twenty-seven times greater than the velocity of the swiftest cannon-ball.

But while Jupiter—to keep for the moment to our giant planet—has thus, theoretically, the power of giving or taking away a velocity of thirty-six miles per second, he is not practically able to do anything of the sort. He is not left to draw matter to himself, or to act on the recession of matter from himself alone. The bodies which come near to him from outer space have been drawn by solar might within that distance from the sun, and almost the whole velocity they there possess is sun-imparted. We have seen that it is some eleven miles per second. Now, manifestly, this greatly affects Jupiter's power of imparting or withdrawing velocity. Both processes require time, and it is clearly impossible for Jupiter to produce anything like the same effect on a body rushing past him with a sun-imparted velocity of eleven miles per second as he would produce on a body left undisturbed to his own attraction. Jupiter's action at any moment is the same, whether the body is moving or at rest; but the number of moments is very much reduced owing to the swift rush of the body past the planet. To use the old-fashioned expression of the first students of gravitation (an expression which has always seemed to me amusingly quaint), the solicitations of Jupiter's attractive force are as urgent on a swiftly rushing body as on one at rest; but if a body will not stay to hearken to them much less effect must be produced.

In all this part of my reasoning, I may remark, I am not pleading a cause, but indicating what every student of celestial dynamics knows.

We may fairly regard twenty-five miles per second as the utmost velocity that Jupiter can impart or take from any body coming out of interplanetary space past him, as close as such a body can pass without being actually captured. Moreover, in every possible case, Jupiter can only abstract or add a small portion of this amount ; for this reason, simply, that in every possible case there will be first an action of one kind (abstraction or addition of velocity), and afterward an action of the opposite kind (addition or abstraction respectively). It will be but the difference between these effects, in most cases very nearly equal, which will actually tell on the body's future period of revolution around the sun.* This makes an enormous reduction on Jupiter's potency to modify cometic revolution. Certainly ten miles per second is a very full estimate of the velocity he can abstract or add in the case of a body passing quite close to his apparent surface.

But even this may seem ample. Seeing that a loss of three miles or so per second would cause a body which had reached Jupiter's distance from the sun, after a journey from out of interplanetary space, to travel in the same period around the sun as Jupiter himself, and since we seem to recognize a power in Jupiter to abstract ten miles per second, it would seem as though Jupiter's capturing power were in fact demonstrated.

But while, to begin with, the close approach required for this capturing power to exist is something very different from that approach within a million

* As distinguished from the orbit. The orbit might be largely affected even in a case where the velocity at Jupiter's distance remained absolutely unchanged ; but in this case the period of revolution would remain the same.

miles which I before considered, there is a much more important difficulty to be considered, in the circumstance that we have thus far dealt with Jupiter's capturing power on one body, not on a flight of bodies, such as a comet approaching from interstellar space is held to be, according to the theory I am discussing. Let us take the former point, though the least important, first.

At Jupiter's apparent surface the actual maximum velocity which the planet could give to a body approaching from a practically infinite distance would be about thirty-six miles per second, and we reduced the actual maximum effect on a body passing Jupiter very closely, under such conditions as actually prevail in the solar system, to ten miles per second. Let us see what would be the corresponding numbers in the case of a body passing within a million miles of him, remembering that even that would carry such a body right through Jupiter's system of satellites, the span of that system being about four and a half millions of miles. Since a distance of one million miles exceeds the distance of Jupiter's surface from his centre nearly twenty-five times, it follows (I need not explain why: mathematicians will know, and for non-mathematicians the explanations would be tedious and difficult) that the velocities which Jupiter can give or abstract at the greater distance would all be reduced to little more than one-fifth those determined for Jupiter's surface. So, instead of ten miles per second, we should get but two miles per second, as the greatest Jupiter could abstract from a body approaching him within a million miles. And this would not be sufficient reduction to make such a body travel thenceforth in Jupiter's period, still less in one of the much shorter periods observed throughout what has been called Jupiter's comet-family.

But the other difficulty is altogether more serious.

A comet approaches Jupiter, on the theory we are dealing with—and, indeed, the same may be assumed on any theory—as a flight of scattered bodies. Either this flight is so close as to be in effect, because of mutual attractions, a single body, or it is not. If it is, the flight will not be broken up by Jupiter's action; and, if not so broken up, will remain for ever after a united family. But if, as is more in accordance with observed facts, the cometic flight is so large that the attraction of the flight, as a whole, on the separate members, can be overcome by Jupiter's action, then not only will the flight be broken up, but the orbits given to different members of it by Jupiter's disturbing action will be widely different. Suppose, for example, the extent of the flight to be such that the parts coming nearest to Jupiter approach his centre within fifty thousand miles (a very close approach indeed to his surface), while those parts which are remotest from him at the time when the flight, as a whole, is nearest, came only within sixty thousand miles from his centre. Then, in round figures, the reduction of velocity of the nearer members of the flight will be greater than the reduction for the farther members, as six exceeds five. Supposing, for argument's sake, the former reduction to be three miles per second, as it must be to make those members of the flight travel thenceforth in Jupiter's period round the sun, then the reduction for the outermost members would be but three and a half miles per second; or thenceforth one set of meteors formerly belonging to the comet would have at Jupiter's distance a velocity of eight miles per second (eleven less three), while another set would have a velocity of eight and a half miles per second (eleven less two and a half) at that distance. This means that thenceforth the mean distance of the latter set from the sun would exceed the mean distance of the former set about as nine exceeds eight.* Since the former set would thenceforth be travelling at Jupiter's distance, or about 5·2 times the earth's, the latter set would be travelling at a mean distance greater by one-eighth of this, or ·65 of the earth's distance, say some sixty millions of miles. The latter set would be at their nearest to the sun when at Jupiter's distance, would pass sixty millions of miles farther away to their mean distance, and as much farther away still at their greatest distance. Practically, then, even in this case, as favourable for capture as can be well imagined, the capture, though effected, would result in spreading out the comet, which had arrived as a compact flight of meteors ten thousand miles only in span, over a region one hundred and twenty millions of miles broad. It is hardly necessary to say that nothing like this is observed in the case of any member of Jupiter's comet-family. We know that along their track meteors are strewn to distances which, in some cases, may well exceed even the enormous distance just named ; but they lie along the track, not ranging more than a few hundred thousand miles on either side from the path of the comet's head. This means that the orbit of every single meteor of such a system has practically the same mean distance from the sun.

The difficulty last considered is simply fatal to the theory that the comets forming what have been called the comet-families of the giant planets were captured by those orbs in the way imagined by Heis, Schiaparelli, and others. We must seek for a different explanation, if we are to account for the peculiar relations of these comet-families at all. It

* The simple law is, that for two bodies having different velocities at the same distance from the sun, the mean distances from him differ as the square of those velocities. Now, the square of eight and a half is seventy-two and a quarter ; that of eight is sixty-four.

may be that the peculiarity, like many others presented by comets, may not admit of being explained. The considerations I am about to advance may to many appear not altogether convincing; nevertheless, as they involve the study and discussion of known facts, they are worth investigating, quite apart from all questions of the validity of the theory with which I associate them.

Observing that the giant planets have each their comet-family, we may safely infer that the sun also has his special family of comets; that is, a family the dominion of which he does not in any sense share with the giant families. The comets which we should thus regard as specially solar are those whose paths approach exceptionally near to his globe. Among numbers of comets which come from out of interstellar space toward the sun, and, sweeping around him, pass away again into the depths from which they came, many have paths passing so far from his globe that we cannot regard them as in any special way associated with him. Bodies coming casually, so to speak, from outside regions would have just such paths. So that of many comets, not belonging to the comet-families of the giant planets, we may say that neither do they belong to the comet-family of the sun. Yet even these teach something. Whatever theory we adopt as to the origin of comets, it must give an account of these comets, as well as of those which, passing very near to the globe of the sun, may be regarded as belonging specially to him, and those others which we assign as the special dependents of the giant planets.

Now, taking the two last-named classes, we recognize in the movements of the members of each class evidence of the introduction of these comets into the solar system, through the intervention, in some way, (1) of the giant planets in the case of one class, and (2) of the sun in the case of the other class. We have seen that the giant planets could not have introduced their comet-families from out of interstellar space by perturbing influences. We may infer with almost equal probability, or almost with certainty, that neither did the sun introduce his comet-family by drawing them from out of interstellar space.

Since, then, the sun and the giant planets did not introduce their special comet-families from interstellar space, yet did most manifestly introduce them in some way, where else can these comets have come from but from within the orbs of the sun and of the giant planets respectively?

At first sight this theory seems so strange and fanciful that we are almost deterred from examining it further by its apparent grotesqueness. We seek about for a way of escape from so wild a theory. We look back to a remote period when, in accordance with the ideas of Laplace, the sun's mass extended far beyond the present orb of the sun, and the giant planets also had orbs extending even as far as the orbits of their outermost satellites. Undoubtedly, if a flight of meteors in that far distant period rushed through the outer vaporous surroundings either of sun or of giant planets, the effects imagined by Schiaparelli and by Heis might have been produced. The diminution of the velocities of the meteors forming such a flight might well be far more effective than in the case we have hitherto considered of free space around a planet's globe.

But we may regard this theory respecting the introduction of comets into the solar system as one which may wait its turn until the other, of ejection, strange and fanciful though it may seem, has been examined. For there is nothing in the capture theory, considered in itself, to invite us specially to its adoption. It gives no account whatever of the actual origin of comets. It only suggests how, having somehow come into existence in interstellar space, comets would be drawn sunward, and might be captured by the sun or by planets. If to this inherent difficulty in Schiaparelli's theory we are to add all the difficulties involved in the supposition that the sun and the giant planets were once much larger than they now are, and that being thus large they were able to capture comets by actual interruption of their movements, we may at least consider that before discussing such views, before attempting to carry back our thoughts over the practically interminable time-intervals involved in such a process, it may be well to examine a theory which, though startling at a first view, promises to explain something more, if confirmed, than the scarcely less startling theory of comet-capture by expanded sun and by expanded planets.

Suppose that instead of looking into remote regions of space, and toward far-off periods of time, we examine meteoric masses, and inquire of them whence they came. We cannot expect each meteorite to have a story to tell; but after a goodly number have been examined, we may light upon one speaking with tolerable clearness respecting its origin. Our first studies shall be with the microscope.

Now, passing over a number of microscopic studies of meteorites which are suggestive enough, but not decisive, we come on the strange fact that certain meteorities show under the microscope the clearest evidence of having once been in the form of tiny globules of molten metal, numbers of which have become agglomerated together. The eminent microscopist and mineralogist Sorby, of Sheffield (England), asks respecting these particular meteors, where else could they possibly have existed in the form of metallic globules (liquid) except in the interior of a body like the sun? In the interstellar spaces intense cold prevails. In rushing close past the sun a meteoric mass might be molten, but would scarcely be vaporized, even though the orbit of the flight passed very near the sun's surface. But the meteorites which have visited our earth have not been associated with comets passing near to the sun. Manifestly the chances are very small that any meteorite following in the train of a comet like Newton's or the comet of 1843—that is, a comet travelling close past the sun—would ever reach the earth. But Sorby found microscopic evidence such as I have described in quite a large number of meteorites which he examined.

At any rate, the assumption for the moment, that such meteorites had their origin within the interior of a body like our sun, accords well with the theory we have had suggested to us, that comets and meteor flights (kindred bodies) came from within the orbs with which we still find them associated.

Turn now to the chemical analysis of meteorites. Here the evidence is perhaps even more suggestive. Masses of meteoric iron being placed under the air-pump, hydrogen which had been present in their substance—occluded in the iron, as it is technically expressed—has come out in such quantities that Professor Graham (of London) considers the amount fully six times as great as could be occluded in the substance of iron by any process known to chemists or physicists. This Lenarto meteor, he says, has brought to us across the interstellar spaces the hydrogen of the fixed stars. In other words, Professor Graham could see no other interpretation of the presence of so much hydrogen within the substance of this mass of meteoric iron than that the hydrogen had been forced into the iron while yet within the interior of a star. We know that beneath the visible surface of our sun there must be both the vapour of iron and hydrogen at enormous pressure. Under such conditions alone could masses such as the Lenarto meteorite be formed. Professor Graham, therefore, assumed confidently that the Lenarto meteorite and others of the same sort were formed in the interior of a body like our sun. He rejected, rightly, the idea that it was in our sun himself that the meteorites of that class were formed. For the chance of any meteorite ejected from the sun reaching our earth is but about as one in twenty-two hundred millions. The greater number of the sun-ejected meteorites must, he saw, have been ejected from the interior of the other suns which people space. There are hundreds of millions of such suns even within the range of telescopic vision; millions of millions doubtless exist; so that if we once admit the possibility of the ejection of meteoric masses from within a sun or star, we recognize the probability, or rather the certainty, that there must be billions of billions of such masses travelling amid the interstellar spaces.

All this was reasoned out thus before it had been shown that suns ever do eject masses with sufficient energy to carry them beyond the attractive influences of their parent orbs; nay, Sorby and Graham expressed their views respecting the origin of some meteorites when it seemed utterly unlikely that we ever should get the evidence of stellar eruptive powers which that theory requires.

But such evidence has now been obtained. Professor Young, of Princeton, N.J. (then of Dartmouth, N.H.), was the first, in 1872, to obtain evidence of the actual ejection of matter from the sun's interior with velocities sufficing to carry such matter for ever away from him; but the evidence was decisive, and since then kindred observations have been frequently made. What Young saw, indeed, was apparently the ascent of filaments of hydrogen, at an average rate of nearly two hundred miles per second; but it was easy to see that the irregular streaks of hydrogen were not themselves the ejected matter. If a thin gas like hydrogen could rush through the region immediately above the sun's visible surface at the rate of two hundred miles an hour,—which I reject as incredible,—the shape of such hydrogen missiles would be such as to indicate very clearly the resistance they were encountering. They would be pear-shaped, the rounded part of the pear in front, like fireballs in our air. But these were irregular streaks, like the luminous tracks of meteors, and such doubtless they were. A flight of masses of considerable density must have been shot out on that occasion, and on other occasions when similar phenomena have been observed, and rushing through the hydrogen in the sun's neighbourhood, caused the gas to glow along their track, just as fireballs in our air leave behind them long luminous trails. The rate at which these missiles advanced could be inferred from the rate at which the luminous trails followed them. Calculation, in which the sun's retarding action was taken duly into account, showed that the matter thus expelled from the sun left his surface at a rate of not less, probably, than five hundred miles per second. The ejected matter left the sun, then, never to return, and in the form of precisely such a flight of meteoric missiles as microscopic and chemical researches had shown to be travelling through the interstellar spaces.

When we consider the three lines of evidence, and note how independent they are of each other, we see that the theory of the ejection of masses akin to meteors from the suns which people space is rendered all but certain, independently of any line of *à priori* reasoning which had led us to look for evidence of such processes. Certain meteors have shown under microscopic study that they were certainly once in a condition such as could hardly exist except in the interior of a body like the sun ; others have shown under chemical analysis that they must have been ejected from the interior of a sun ; and now we have evidence showing that from our sun, and therefore presumably from his fellow-suns, the stars, flights of missiles akin to meteoric bodies are ejected from time to time with velocities sufficient to carry them into interstellar space. It seems reasonable to infer that here we have the solution of our difficulty ; we see that the sun, at any rate, has power to eject at times, from his interior, flights of meteoric masses, such as we recognize in the streams of meteors which exist within the solar system, and that the velocity of outrush is in some cases so enormous that the masses thus ejected can never return to the sun, but pass away through interstellar space. We find also that meteoric streams, which we are thus led to associate with the solar eruptions, are also associated with comets, every known meteoric stream travelling, probably (as many certainly do), in the track of a comet. Now, knowing the small masses of many comets, it is no very wild thought to suggest that those comets whose present orbits carry them close to the sun were originally expelled from his own interior. Assuredly the flights of missiles which we know to be at times driven from his interior are in all respects akin to what we know many comets actually to be, akin in structure, akin in mass, and akin probably in condition. For in whatever respects the coma and tail of a comet may seem unlike mere meteoric masses, we know that such peculiarities of condition are due to solar action, and that a flight of meteoric masses ejected from the sun himself would as certainly present these peculiarities under subsequent solar influences as any other flight of meteoric masses not ejected originally from the sun.

May not this reasoning be extended to the giant planets, either in their present demonstrably somewhat sunlike state, or in those past stages of their career when they were veritable suns, though small ones? In the great red spot of Jupiter, however, we have had evidence of even a present intensity of eruptive action by which meteoric and cometic matter might well have been ejected in such sort as to pass for ever beyond the control of the giant planet. At any rate, the great disturbance suggests, by parity of reasoning, that within comparatively recent times Jupiter and Saturn have possessed the necessary expulsive power. It must be remembered that thus to eject matter with velocities sufficient to carry it for ever away, Jupiter and Saturn would not need anything like the same ejecting power which the sun has to exert to expel matter for ever from within his globe. They are much weaker than the sun, but for that very reason they would need to exert much less eruptive force, seeing that it is their own attractive power they have to overcome, and that that is weaker in even a greater degree than probably is their eruptive power.

Now, there is a family of comets attending in a sense on Jupiter, and another family attending similarly on Saturn, precisely as we should expect them to do if originally expelled from the interior of these planets. After such expulsion, though free to pass away for ever from their parent planets, they would not be free to pass away for ever from the solar system. They would be thenceforth attendant on the sun, but with this peculiarity, that no matter what perturbations they underwent, their paths would always pass near to the path of their parent planet. Even if in some future circuit a comet of this sort came quite close — as it very well might—to the planet it originally started from, it would still, though very much disturbed, follow a path possessing this characteristic, however different from the path which it had before traversed. After many millions of years, indeed, it might happen, perchance, that resistance encountered in its movement around the sun, however ineffective to affect its orbit appreciably in a few thousands of years, would reduce the span of its circuit. But even then it would still be possible to classify a comet whose orbit had been so changed, with the family of comets to which it had originally belonged.

Now we find that among the periodic comets attending on the sun nearly all belong to families which have long since been relegated to the giant planets. There is a family of comets every member of which has an orbit passing very near to the orbit of Jupiter; another family every member of which can be similarly associated with Saturn; others depending in the same way on Uranus; others on Neptune; and, in fact, so fully has this sort of relation been recognized, that the idea has been thrown out that a planet travelling outside the orbit of Neptune, but as yet unknown, might be detected by the movements of a comet intersecting the great plane of planetary movement far beyond Neptune's orbit. It may be mentioned, indeed, in passing, that the comet of 1862, which has been associated with the meteors of August 10 and 11, intersects the plane of planetary movements at a place about as far beyond the orbit of Neptune as that orbit is beyond that of Uranus; and that it has been held probable that at that distance a giant planet as yet undiscovered may travel.

The existence of the comet-families of the giant planets can scarcely be explained without assuming that which we have thus been led on another line to recognize as probable,—the ejection from the giant planets of masses of matter, in eruptions akin to those taking place in the sun. Whether such eruptions take place now in the giant planets, or not, would be difficult to prove; for although we have evidence of tremendous disturbances, we have nothing to show conclusively that these would suffice to eject matter for ever from within these planets' globes. Whether a careful study of the region outside the discs of Jupiter and Saturn (the planets themselves being hidden by opaque discs) would decide the point, I am not prepared to say; but I am certain that the edges of the discs of the giant planets are worth much more careful study than they have yet received.

But undoubtedly most of the comets of Jupiter's family must have been added to the solar cometic system hundreds of thousands if not millions of years ago. Quite possibly both Jupiter and Saturn still eject matter from time to time with such velocities from their interiors that it passes away never to return to them. In this, as in many other features, Jupiter and Saturn are still somewhat sunlike. But they have passed their truly sunlike youth. They tell us what our own earth was like when she was young. We may trace back her history, however, even to the sunlike state. The same law which we applied to the giant planets may be applied also to her. Her eruptive energies must have been very much less active, even in her sunlike youth, than those of the sun now; but the force against which she had to work (her own attractive energy) was much less potent too: nay, it may probably have been less potent in even greater degree. Just as the moon in her volcanic youth upheaved her surface much more than the earth upheaved hers, because, though the moon was weaker, her subterranean energies had so much smaller downward-tending action of gravity to contend against, so it may well be that the smaller a planet when in its sunlike state, the more easily did eruptive forces eject matter beyond the range of the planet's attractive forces. In this case every planet at that stage of its career, as well as every sun, gave birth to cometic and meteoric systems, each after its own kind: solar comets being large ones like those which astronomers have not been able to associate with the planets' comet-families; the comets ejected by the giant planets coming next in order of size; and the comets ejected by smaller orbs, like the terrestrial planets, moons, asteroids, and so forth, being probably too small to be discerned even with telescopic aid.

CHAPTER X.

WHENCE CAME THE COMETS?

ALTHOUGH the astronomer has achieved many successes in studying comets, yet these objects still remain outside the surveyed fields of astronomy—now, as in the old days when men spoke of sun and moon, planet and stars, as including all the members of the heavenly host. The two comets now shining in our skies [1886] illustrate the present position of cometic astronomy. They have appeared without warning, we know not whence; they have not until now been known to astronomers as travelling on recognized orbits and in definite periods; and even hereafter, though the astronomer may determine their orbital motions and calculate the time when either should return, he cannot be sure that they will not be dissipated into unrecognizable portions before that time arrives.

I do not propose to remark here upon the probable nature of comets, or upon the possible interpretation of the various phenomena they present. The only circumstance in regard to them which I shall take into account in what follows is that close relationship between comets and meteor-streams which was established in 1866 by the combined labours of Schiaparelli, Adams, and Tempel. I shall treat this kinship between comets and meteors as rendering certain or highly probable the four following propositions:—

(1) Every meteoric stream follows in the train of some comet, large or small, which either exists now or has been dissipated, as Biela's comet was, leaving only its meteoric trail to show where it once travelled.

(2) Every comet is followed or preceded by a train of meteors (this train has nothing to do with the comet's tail), extending over a greater or less portion of the comet's orbit, according to the length of time during which the comet has existed.

(3) All meteoric bodies, from those which exist as the finest dust to the largest meteorites, hundreds of pounds in weight, may be regarded as bodies of the same kind, differing from each other, indeed, in constitution as they obviously do in mass, just as planets and asteroids do, but all to be interpreted—if they can be interpreted at all—in the same general way.

We may in some degree illustrate the nature of the assumptions here made in the three following assumptions which an insect, who had observed the phenomena of rain, cloud, mist, snow, &c., might be supposed to make: (1) Every shower of rain implies the existence of a cloud; (2) every cloud implies the descent, at some time or other, of rain, greater or less in quantity and heaviness; and (3) all drops of water, from the tiniest water vesicles in a cloud to the heaviest rain-drops, are of the same kind, differing only in shape or in size; snowflakes also, as formed of water particles in a changed form, must be put in the same class.

And as the insect, by studying the relations which exist between clouds and rain, might be led to form an opinion whence clouds come, which would tell him also (as we know) whence rain comes,* so per-

* To us, who know how clouds and rain are really produced, this imagined inquiry of the insect may seem trivial. But man had advanced far in scientific research before he had learned anything about the source and nature of rain, hail, snow, cloud,

haps may we, by studying the relations which exist between meteor-streams and comets, be led to form an opinion whence comets (which are meteor collections) originally came.

The very first suggestion ever made respecting the origin of comets came, indeed, from such considerations as I have mentioned above. Schiaparelli, to whom we owe the happy guess, and the beginning of its confirmation as a useful truth, that meteors are bodies following in the tracks of comets, threw out the idea that comets, regarded as flights of meteors, may be travelling in multitudes through the interstellar depths, and be from time to time drawn out thence by the attraction of our sun. He pictured our sun, in his swift rush onward with his train of planetary attendants, as coming into ever-fresh regions of comet-strewn space. A comet or meteor flight drawn towards him by the sun would approach the solar system on a path which may be described as casual. It might cross the general plane near which all the planets travel, at any point, the chance that that point would lie near a planetary orbit being very small indeed. Supposing the point where the meteor flight crossed that important plane—the life-plane of the solar system — to be on or near a planetary orbit, the chance would still be very small that the meteor flight would cross there at a time when the planet to which that orbit belonged was near that particular point. The chances would, in fact, be millions of millions, or rather of billions, to one that the meteor flight would visit our solar system without coming near any planetary body, in which

mist, and fog. The whole subject was as completely mysterious, for example, to all the writers whose works were included by the Jews among their sacred books (in probably *all* their ancient documents), as were the phenomena of comets, which with them were veritable angels or messengers from Yahveh.

case it would pass out from our solar system again, never to return to it.* But, if a meteor flight did chance to come very close indeed to a planet of adequate mass, the flight might—said Schiaparelli—be captured. The planet might abstract so much of the comet's velocity as to leave only a balance corresponding to motion in a closed or elliptic path; and on such a path would the meteor flight or comet necessarily travel thereafter—unless, perhaps, after many revolutions of each, the planet at some subsequent encounter undid the work which it had accomplished when first it approached the comet.

So far Schiaparelli reasoned soundly on the basis of his assumption. I say assumption of set purpose; for it is altogether a mistake to regard the idea thus thrown out by Schiaparelli as if it were a theory. His idea that meteors follow in the track of comets developed into a theory when it had been tested and confirmed by observation, and may now be regarded as a demonstrated and accepted theory. But the case is altogether different with the idea, or rather the mere fancy, that meteor flights are travelling hither and thither through the star-depths like fish in the depths of the ocean. However, his reasoning was thus far correct, assuming the meteor flights to exist and move within the interplanetary depths as he imagined.

But beyond this Schiaparelli did not reason quite correctly. A single meteoric mass, or even a small meteor flight, might be introduced into our solar system in the way suggested by Schiaparelli; for undoubtedly the giant planets possess the power he

* *Never;* because, by the nature of its supposed indrawing, it possessed relative motion of its own before it began to be drawn in, and the sun could not take from it that relative motion. He would impart motion, and take such imparted motion away again, leaving untouched the original motion. Another way of putting this is to say that the comet's path would be hyperbolical.

attributed to them, and if a body from without came near enough to any one of them, could so reduce its velocity as to change its path from the hyperbolic (or unclosed) form to an elliptic or closed orbit. And thenceforth such a body would travel around the sun systematically, on a more or less eccentric path passing very near the orbit of the planet by whose influence it had been originally introduced into the system.

But a giant planet could do no more. It could not generate a meteor-stream in the way suggested by Schiaparelli. So soon as we test the matter by mathematical analysis, we find that approach so close would have to be made to a planet that a single body might be forced into a closed path, and it is certain that a flight of bodies large enough to produce any of the known meteor-streams would have its components very widely scattered by the planet's perturbing action, simply because the different components of the flight would be exposed to very different degrees of disturbing action.

This I have shown mathematically, and my demonstration has not been questioned—though Professor Young, of Princeton, N.J., in admitting the validity of my reasoning, suggests the possibility that some way may hereafter be found for eluding the difficulty. But then Professor Young holds the strange idea that Schiaparelli's speculation as to the origin of comets and meteor-streams is an accepted theory; and, labouring under this delusion, imagines that there must be some way of meeting objections to it, even though they may be mathematically demonstrable.

But it is worthy of notice that Schiaparelli's fancy, even if accepted, would prove nothing about the origin of comets and meteors. To say that they came from out the interstellar depths on hyperbolic paths is to assert what can be disproved by mathematical demonstration. But, if it could be proved what would it amount to? Merely to this—that comets which now travel on closed paths once travelled on endless paths. We are no whit nearer the explanation of their origin. If the interstellar depths are crowded with meteor flights, we have to ask whence the meteor flights came. To say that fish which have been drawn from the sea were originally swimming about in the sea is surely not to add much to our knowledge about fish.

It may be urged, however, that comets and meteor-streams are simply the material left unused after the various solar systems in our galaxy had been formed by processes of meteoric aggregation.

Unfortunately for this explanation, the comets and meteor systems we have to explain are precisely those which, had they existed from the earlier ages when our solar system and its fellows were forming, would have been the first to be gathered up. For they are those which pass near the orbits of various planets, some near the orbit of Jupiter, some near that of Saturn, or of Uranus, or of Neptune, and about 400 which pass near the orbit of our earth. These comets, with their associated meteor systems, would have had less chance of escape than any others, during the millions of years belonging to the formative process of our solar system. Yet those are precisely the comets and meteor systems which we chiefly need to interpret.

Suppose that, instead of making mere guesses, we consider the actual facts, and open our eyes to the views which seem to be suggested by such facts.

I take first the millions of meteors encountered by the earth each year, and the hundreds of earth-crossing meteor systems already recognized. Taking for our guide proposition (1), we are led to the conclusion that in remote ages there were hundreds, if not thousands, of comets whose tracks crossed the track of the earth, or at any rate approached very near to it. That some of these comets thus crossed the earth's track casually, that is through mere chance coincidence, we may well believe. Nay, this is known, as will presently be seen. But if *all* did, then must there have been millions of millions of comets in remote times, to account for so many chancing to cross the earth's track—with this startling circumstance to be considered in addition; that ninety-nine out of a hundred of those whose paths did not cross the earth's track have entirely disappeared, while a considerable proportion of those which do cross that track (and which, therefore, have been exposed for millions of years to an extra risk of destruction) remain.

This idea we may safely reject. But, if we do, then we have to account for a special earth-crossing family of comets and meteor-streams, without going outside to look for the origin of such bodies—for the moment we go outside we encounter the difficulty which has just driven us from any merely casual interpretation.

In other words, we must look to the earth herself to explain the great majority of these earth-crossing systems.

In this way Meunier and Tschermak were driven to look to the earth herself for the origin of meteorites.

Proposition (3) above enables us to apply their reasoning, specially directed to particular classes of aerolites, to all classes of such bodies, to all meteors, down even to the tiniest falling stone, only visible perhaps in the field of a powerful telescope. Not all these bodies, but a goodly proportion, must have been generated in some specially terrene manner.

We have actually no possible way of explaining the terrestrial origin of any meteors but in volcanic outbursts. Moreover, we are obliged to set the time when such outbursts took place very far back in the past, seeing that at present the volcanic forces of the earth, even as manifested at Krakatoa recently, possess nothing like the power necessary for the ejection of matter beyond the range of the earth's back-drawing power. Looking, however, at the immense extrusive power of the volcanoes of the tertiary era, when basaltic lava covering hundreds of thousands of square miles to a depth of 1,000 to 14,000 feet were poured forth, we can conceive the still mightier energies of volcanoes in the secondary era, their still more tremendous power in the primary era, and so, passing backwards to millions of years beyond the first beginnings of life on the earth, we can even picture to ourselves volcanoes ejecting matter with velocities of ten or twelve miles per second. With such velocities flights of ejected particles would pass beyond the earth's attraction, and if she were the only body in the universe, such ejected matter would travel away from her never to return.

But, although such expelled bodies would never return to the earth, they would not escape from the solar system. To drive them for ever away from her, the earth would have to impart a much larger velocity—an average of about twenty-six miles per second. The greater number of the expelled bodies would travel thenceforth on an orbit round the sun, crossing the earth's track at or near the place where they were first sent forth from their parent planet.

One may almost say that this origin of many meteorites and meteor systems is forced upon us by the evidence. Still, it would be negatived if we found that volcanoes do not eject matter at all resembling meteorites in structure. The reverse, however, is the case. Ranging the products of volcanic ejection in order according to the amount of iron they contain, and ranging meteorites in like manner, we find the two series coinciding over the greater portion of the longer—the volcanic series. We might not, indeed, have known how closely the most ferruginous volcanic products resemble the iron meteorites * in structure but for the accident that Nordenskjold discovered a mass which he mistook for an iron meteorite, but which is found now to be really a volcanic ejection, akin in structure to the field of basaltic lava (at Ovifak on the shores of Greenland), in the midst of which it had fallen while the lava was still plastic to retain this missile as it fell after its flight through many miles of air.

We may, therefore, regard the terrestrial origin of many meteorites as highly probable, if not in effect demonstrated.

Here Tschermak and Meunier pause, as also does Ball, who thus far had followed them. The last-named does not even ask, in that singularly interrogative and unsatisfactory work, the "Story of the Heavens," whether we may not go further.

For my own part, I find in this result the first step in a most interesting and suggestive path of inquiry.

Regarding a large proportion of the material visitants of the earth as originally earthborn, we may conclude that in the remote time when our earth was a baby world, sunlike in condition, her path was traversed by hundreds of comets, her own progeny. These comets were followed severally by their trains of meteoric attendants. They were exposed to the action of those solar forces by which, within the last half century, a once promising member of another comet-family became dissipated until it finally lost altogether its cometic character. Millions of years ago, probably every one of them had been thus

* It is worthy of notice that the Greek name for iron was derived from the name for a star, and iron meteorites are still called siderites. Before iron had been found in the earth, it was known only from the iron meteorites which had fallen at various times. Of course these were regarded as special presents from the gods, and revered accordingly.

broken up until nothing remained but the streams of meteoric bodies, travelling round the orbit which had once been that of the earth-ejected comet.

But this being the case with the earth, was the case also no doubt with every planet. Even our little moon, whose scarred face still shows signs of the volcanic energies she once possessed, played her part in giving birth to such comets as she was equal to. If she possessed less volcanic power than the earth (at the same stage of the life of each), she required less power to eject matter for ever from her interior. On the other hand, the giant planets required greater power; but then they also possessed it. If Jupiter, for example, required power enough to eject bodies with a velocity of forty or fifty miles per second, yet it must be remembered that he is 310 times as massive, and therefore 310 times as strong as our earth. (For matter—inert matter as many choose to call it—measures in reality the strength of the orbs in space, and not only possesses power, but a power acting so swiftly across vast distances that the velocity of light is rest by comparison. Moreover, this power possessed by "inert" matter is the source of every form of energy of which we know, even of life itself.) So with the other giant planets.

Jupiter, then, and each one of his giant brethren, must during its sunlike stage have possessed the comet-ejecting power. Each giant planet must have had its comet-family at that remote time in the history of the solar system. And the comets thus formed by the giant planets, while no doubt very numerous, must, many of them, have been far more important than those to which our earth gave birth. Those comets would have lasted much longer, before dissipation due to solar disturbances set in. Then, also, the sunlike state of the giant planets must have lasted long after the earth and all the terrestrial planets had passed that stage. For being so much larger, the giant planets must have longer lives—the stages of planetary life being in effect stages of cooling. In fact, there are clear signs that neither Jupiter nor Saturn has cooled down to the earth's condition ; each is still too hot for the waters of its future seas to rest on its fiery surface. On this account also, then, we might expect to find that some comets, sprung from giant planets and forming their families, might have remained even to the present time.

Turning to the solar system, we find that this actually is the case. Nay, I myself, long before I had the least thought of attributing comets to planetary eruptive energies, had described the comets which hang about the orbits of the giant planets as "The comet-families of the giant planets." Some of the members of these families are among those from which the association between meteors and comets came first to be known. The meteor-stream from which the star-showers seen on August 10 and 11 (the *Perseides*) proceed is not indeed associated with a comet depending on the orbit of any known planet; but the meteors of November 13 and 14 (the *Leonides*) are associated with a comet depending on the orbit of Uranus; and the meteors of November 27 and 28 are associated with a comet depending on the orbit of Jupiter—Biela's famous comet.

Of course the members of these comet-families are exceedingly old. How old they are we cannot tell; but that they are very old indeed is shown by the way in which, while they are unmistakably associated with the paths of the several giant planets, their orbits yet diverge far enough from those of their respective planet parents to indicate hundreds of thousands of years of perturbing action, unless indeed in some cases we may suppose that not the slow perturbing action of bodies at a distance, but the very active influence of some orb coming very close to a comet may have shifted the comet's path. So many of their orbits pass through the widely-spread zone of asteroids, that we may very well imagine occasional very close approach to one or other of these bodies, and consequently a considerable change of orbit. It was thus that Sir John Herschel for a time tried to explain the disappearance of Biela's comet; "May it not," he said, "have got entangled in the zone of asteroids, and have had its course altered by the influence of one of these bodies?"

Encouraged by the confirmation of the expulsion theory of comets, which we have found at this our first step, may we not boldly proceed yet one step farther—a long step I admit, but yet one suggested by the theory itself with which we are dealing?

The stars, like the giant planets, should have their part to play—a grander part of course—in the world of comet expulsion. They differ only from the giant planets, nay from the earth herself, in being in a different part of their orb-life. It is probable, indeed, that among the stars there are orbs differing much less from Jupiter or Saturn than either of these still hot and fiery planets differs from the earth. Of course an orb like our sun, the one star we are able to examine, will require much greater energy to expel from his interior a flight of bodies, to become presently a flight of meteors or a comet, than would a planet even of the giant type. Our sun, for example, would have to impart a velocity of 382 miles per second to a body ejected from his interior, that that body should pass away from his control for ever. But the sun possesses the required power. His mass (and therefore his might) exceeds that of the earth more than 320,000 times, that even of Jupiter 1,048 times.

We have no means of recognizing by its orbital motion a star-expelled comet or meteor flight. But we need not seek for bodies to tell us of expulsion, ages and ages ago. The stars are *now* in their sun-like state. They must therefore be doing such work *now*, if there is any truth in the theory to which we have been led. Now there is one of the stars which is near enough to be asked whether it really possesses and uses such expulsive power—our own sun. His answer is unmistakable. In 1872 and at sundry times since, he has been caught in the act of ejecting bodies, probably liquid or solid, through the hydrogen atmosphere around his globe, with velocities so great that the matter thus expelled from his interior can never return to him—the velocities ranging to 450 miles per second at the least. What he is doing now he has doubtless done for millions, nay for tens of millions, of years in the past. What he has thus done, his fellow-suns the stars, thousands if not millions of millions in number, have doubtless done also. Uncounted billions, then, of ejected meteor flights or comets must be travelling through interstellar spaces, visiting system after system, flitting from sun to sun, in periods to be measured by millions of years. . .

The answer, then, to the question Whence came the comets? would appear to be:—

(1) Comets which visit our system from without were expelled millions of years ago from the interior of suns.

(2) Comets which belong to our system were mostly expelled from the interior of giant planets when in the sunlike state, but a small proportion may have been captured from without.

(3) The comets of whose past existence meteor-streams tell us were for the most part expelled from our earth herself when she was in the sunlike state, but some of the more important were expelled from the giant planets, and a few may have been expelled from suns.

CHAPTER XI.

A NEW THEORY OF SUN-SPOTS.

OF all the phenomena presented to the contemplation of astronomers, sun-spots are at once the most impressive and the most mysterious. On the face of that resplendent disc they seem, at a first view, mere dark marks, of little import or interest. To the astronomers who first observed them, Fabricius, Scheiner, and Galileo, they were mere stains on the surface of an orb which earlier astronomers, confident in half-knowledge, had regarded as absolutely without spot or blemish. But so soon as their real features are noted, and the real dimensions of the sun's orb considered, their amazing significance is revealed; while, when their movements are examined, and the strange laws noted according to which they wax and wane in frequency, they are found to present problems as mysterious as they are fascinating.

I am about to advance a theory about sun-spots, or rather about their more salient features, which at least serves, whether right or wrong, to associate together some of the most remarkable facts which have been discovered respecting the sun and his surroundings.

Let us first consider the nature of that surface in which sun-spots make their appearance, and the phenomena which they present.

We are apt to regard the visible surface of the sun as if it were either the actual surface of his globe, or, at least, very near to that surface. On a little consideration, however, of the facts known to us, it will appear that this view is not correct. Strangely enough, the earth under our feet tells us the nature of the interior constitution of the sun, while the face of the sun himself even veils from view what lies deep down below it. The crust of the earth, studied by geologists, has spoken in the clearest terms of many millions of years of sun work at the sun's present rate of emitting heat and light. We may shorten our estimate of the time by assigning to the sun a greater activity in past times than now, or lengthen it by assuming that of yore he worked less effectively; but the result remains the same, so far as our present inquiry is concerned; for it is the totality of sun work, not time, we have to consider. Dr. Croll, of Glasgow, has shown, if not conclusively, yet with such high degree of probability that it would be far less safe to reject than to accept his conclusions, that the earth's crust tells of at least 100,000,000 years of sun's work. Sir Charles Lyell accepted the evidence as to all intents and purposes decisive.

Yet if this is so, a great difficulty immediately presents itself. The sun's energy in emitting light and heat results, so far as can be seen, almost wholly from the action of gravity in drawing in towards the centre the matter which forms the great aggregation we call the sun. That mysterious power which resides in matter adds this other reason to the reasons, already strong, which make it the mystery of mysteries, that in it lies "the promise and potency" of light and heat throughout the universe itself. We owe to Helmholtz the first suggestion and study of the theory which shows how the contraction of the sun's mass provides, so to speak, for the constant expenditure of energy. We can ascertain precisely how much energy could have been derived from the contraction of the sun's globe to its present apparent size, supposing its mass strewn with tolerable uniformity through an orb of that size. Of course the larger the original volume of the sun, the greater the amount of energy which might thus have been produced. But let us assign to the original globe of the sun the greatest possible volume—infinity of space. Of course the idea is not admissible as a conception, but it can quite readily be dealt with mathematically, and will manifestly give us a superior limit to the length of time we wish to determine. We find, using this infinity of space, that the period deduced is but about 20,000,000 years. Taking, instead, an extension all round over half the distance separating the sun from the nearest star, we get very nearly the same result.

Here, then, there is manifestly something wrong. Our earth tells us one story, the sun seems to tell us another. I reject as absolutely inadmissible the suggestion for removing the difficulty by supposing that our sun's globe was formed by the collision of masses which had before been rushing with enormous velocities through space. All such ideas of collision appear simply preposterous to the astronomer who apprehends how enormously the distances separating star from star exceed the dimensions of individual stars. There is only one way of removing the difficulty, viz., by recognizing the fact that the sun's apparent globe differs very much in size from his real globe. If the process of contraction has gone on very much farther than it seems to have done, then we can readily explain the awful vistas of past time of which our earth's crust tells us. We may safely conclude from this one argument alone that the sun's real globe is very much smaller than the orb we see.

But there is other evidence to the same effect. Professor G. H. Darwin has shown clearly that unless the central part of the sun were very much more compressed and dense than the parts near (say within fifty or a hundred thousand miles of) the apparent surface, there ought to be measurable flattening of the sun's polar regions. Now it is absolutely certain that there is no such flattening. All the observations made at Greenwich, Paris, Vienna, Washington, and other great observatories, agree in proving this. Therefore the central part of the sun is much denser than the outer parts, and doubtless the real globe of the sun is very much less than the globe we see.

There is also another proof of the same important fact in the behaviour of the spots themselves. It will fall presently under our notice.

What, then, is that visible surface which lies as a luminous veil far above the real surface of the solar globe?

The telescope shows the general surface of the sun as formed of multitudinous small round objects, intensely bright, on a background which, though really bright, appears by contrast dark. These objects are only small in the sense that they look small as seen even with the most powerful telescopes. In reality, they average two or three hundred miles in length and breadth. Regarding those of nearly circular form as in reality spherical, the surface of one of these clouds (if so we are to regard them), 200 miles in diameter, would be about 125,000 square miles; so that in comparison with all such terrestrial objects as we can actually see and measure, they are of enormous size.

Now we can readily form an opinion as to the nature of these cloud-like masses—the so-called solar *rice-grains*—by considering what the spectroscope has told us about the vaporous atmosphere in which they float. This complex atmosphere indicates its presence alike in telescopic survey of the sun and in photographs of his disc, by the well-marked darkening towards the sun's edge. Anâlyzed by the spectroscope, it is found to contain the vapours of iron, copper, zinc, aluminium, titanium, sodium, magnesium, and many other terrestrial elements, chiefly metallic. In other words, in the atmosphere of the sun the metals have the same position which the vapours of water have in our own air; so intense is the heat of the sun that iron, copper, zinc, and so forth (doubtless, in reality, all the metals, though not all in sufficient quantity to indicate their presence), are turned to the form of vapour. The clouds, then, that float in the atmosphere of the sun are clouds in which drops of metal play the same part which drops of water play in our own clouds. We may describe the solar rice-grains, in fact, as mighty metallic clouds.

But here I would call attention to a consideration which seems to me of great importance in all inquiries into the sun's condition. The laws of gaseous pressure and density, as determined by experiments on the earth, are either modified under the conditions which exist in the sun, or else we cannot possibly regard the region of absorptive vapours certainly existing around the visible surface of the sun as of the nature of an atmosphere. From spectroscopic analysis we know that the pressure at which hydrogen exists just outside the sun's surface is much below the pressure of our atmosphere at the sea-level, yet certainly not so low as the thousandth part of that pressure. And whatever opinion we may form as to the effect of the intense heat prevailing close by the sun, we cannot overlook the influence of the enormous force of gravity at his surface. Under this force, more than twenty-eight times the force of gravity at the earth's surface, an atmosphere constituted like our own would double in pressure for every one-eighth of a mile of descent. Suppose that at the sun's surface a vaporous atmosphere such as he seems to have, an atmosphere constituted as the vaporous matter around him undoubtedly is constituted, doubled in pressure only once for every ten miles of descent. Then within the range of about 400 miles through which the sun's vaporous atmosphere has been observed (during total eclipse) to extend, there would be forty doublings, or the pressure, certainly not less than one-thousandth of our air's pressure, would be increased to more than *one thousand million times* the pressure of our air at the sea-level. Under such a pressure it would no longer be vaporous at all. Could it remain so, and obey the laws of gaseous matter, it would be many thousands of times denser than the densest metals known to us. Most assuredly no such pressure exists either at the sun's surface or thousands of miles below it. We can see to a depth of some 10,000 miles in the case of certain of the larger sun-spots.

We seem forced to the conclusion that the real atmosphere of the sun does not come anywhere near the surface we see, which, according to this view, would be regarded as formed of cloud-like masses, each with its surrounding of vapour, kept around it by the attractive energy which must necessarily reside in enormous aggregations of metallic globules such as these clouds must be. I am aware that this view will seem so strange, so unlike any that has heretofore been held, as to appear very daring. Yet it is infinitely more daring to overlook the enormous physical difficulties involved in the assumption that a continuous atmosphere surrounds the sun to a height of many hundreds of miles, while at the highest part of that self-luminous atmosphere the pressure is comparable with that of our own atmosphere at the sea-level.

Be this as it may (for the question has no direct bearing on the theory I am about to present), it is certain that under the action of various forces the solar rice-grains arrange themselves into groupings of varied form, in such sort that the general surface of the sun, when studied with a telescope not sufficiently powerful to show the separate rice-grains, presents a mottled aspect. Photography, which, as skilfully applied by Dr. Janssen, gives us the best views yet obtained of the details of the sun's surface, shows another reason for the mottled aspect, in the existence of a sort of network (varying even in form) of misty streaks where the rice-grains, though visible, are much less clearly defined than elsewhere. These blurred regions will doubtless find their explanation hereafter, as their changes of form come to be more closely studied.

But, yet again, the surface of the sun is disturbed by forces producing more marked movements of the solar clouds. These get driven together into closely-packed streaks which, even in telescopes of very moderate power, are visible as exceedingly bright objects. They are the so-called *faculæ* (named thus by Hevelius), from the Latin word for little torches, because of their brilliant aspect.

It is, however, when yet greater disturbances affect the cloud-laden region which forms the visible surface of the sun, that solar spots make their appearance. A region of disturbance, where many faculæ are seen making the sun's surface look like a froth-streaked sea, shows suddenly in the middle of a dark region, round which the faculæ appear at first as parts of nearly circular arcs. But they pass farther and farther away from the region of disturbance, the dark centre of which becomes better defined, and is presently seen to be bordered by a well-defined fringe of less darkness. Under close telescopic scrutiny this fringe (called the *penumbra*), which, though less dark than the central part (called the *umbra*), is darker than the general surface of the sun, is seen to be marked by streaks extending radially from the centre of the nearly circular spot. Larger and larger the spot grows, gradually losing its circular form, but still well rounded on all sides. The centre is found to be darker than the rest of the *umbra*, appearing, indeed, absolutely black, but not necessarily so, since the glowing lime-light appears absolutely black when on the sun's disc as on a background. This central darkest region is called the *nucleus*.

After remaining, sometimes for several days, sometimes for weeks or even months, a spot begins to show signs of breaking up, if one can speak of the breaking up of what really indicates the absence, not the presence, of matter. It loses its rounded form, becoming perceptibly pear-shaped. Large portions of the facular regions around break their way in upon the sun, chiefly on the edge, which remains more rounded, forming often bright bridges—usually curved—from side to side of the spot. On either side of the smaller part (the stalk end of the pear) larger but less brilliant masses seem to move in upon the spot, as though to cover it over with portions of the cloud-laden surface which had before been outside. These masses, as they move on, usually show widening dark streaks between them; and it is very noteworthy that on either side of these dark streaks there can be seen bright threadlike objects akin to the radial streaks around the umbra. But in the meantime these streaks, which had been originally radial and tolerably regular, have been tossed hither and thither as if irregular currents swept them in different directions. From the great masses thrown in on the dark background of the spot multitudinous filaments seem to stream in all directions, like fringe upon a storm-tossed banner.

More and more violently—"pell-mell," as Secchi used to say—the luminous masses rush in upon the spot region. At last it is completely covered over, though bright facular streaks show where the great opening had been, and where intense disturbance is still going on. Sometimes these streaks break apart and a fresh spot is formed; and it has happened that twice or thrice a spot has been, as it were, renewed in this way. But usually the facular streaks become less and less marked, until at length the region where the spot has been can be in no way distinguished from the surrounding parts of the sun's surface.

Such is the history of a spot of the larger sort. Occasionally there are peculiarities affecting the progress of some particular spot. For instance, there was the wonderful Cyclone Spot, seen by Secchi in 1857, the whole area of which was swept round as if by some mighty tornado. Again, there have been spots where a double tornado seems to have been in progress, the two whorls moving in opposite directions. In yet other cases there has been a whirling motion affecting the central part of the spot region in one direction, at one part of the spot's career, and in the contrary direction later. Other evidences also of exceedingly violent motion have from time to time been observed.

In smaller spots less marked signs of varying disturbance are noticed. The history of a small spot is comparatively uneventful. The chief interest in these lesser markings resides perhaps in the circumstance that to the unpractised observer they look very much like small planets in transit. For my own part, I may express my conviction that every recorded case of intra-mercurial planets seen in transit is to be thus explained, from the case of Lescarbault's Vulcan down to the case of Vulcan's supposed return as seen in China; though the last-named is the only case in which a photograph of the sun chanced fortunately to have been taken at the right time, proving unmistakably that what had been described as unquestionably a planet, moving like a planet and unlike a sun-spot, was nevertheless a small sun-spot after all.

But there are yet some other circumstances which must be noted before we proceed to consider a theory of sun-spots.

The spots are limited to two zones on the sun's surface, corresponding to the sub-tropical and temperate zones on the surface of the earth. The existence of such zones implies necessarily the occurrence of rotational motion, whereby the position of the sun's poles and equator has been determined. It has been, in fact, by observing the spots, that the axial position of the sun and his rate of rotation have been ascertained. But the movement of rotation, which seemed a comparatively simple matter when the first rough observations of Galileo and his contemporaries were in question, presents itself now as a complex phenomenon; for spots in high solar latitudes are found to indicate a rotation rate different from that determined by the observation of spots near the equator. The difference is so great as to become most perplexing when its real significance is considered. Judged by spots in the highest latitudes where spots have been seen on his face, the sun seems to rotate in about twenty-eight days. Judged by spots as near the equator as any have been seen, he seems to rotate in about twenty-four days. His real globe cannot well rotate save as a whole and in a single period; yet, judged by what looks like his surface, his equatorial regions seem to rotate seven times, while the mid-zones of his northern and southern hemispheres rotate only six times. Regarding the slower rate for a moment as the true rate of the sun's rotation, it would appear as though the visible equatorial regions gained one entire rotation on the surface beneath them in 168 days. Now, the sun's circumference is in round numbers about 2,660,000 miles, so that the gain of the whole equatorial zone takes place at the rate of nearly 16,000 miles per day, or about 650 miles per hour. Thus, viewing the varying rotation rate at the surface, we should have to recognize the existence of the most stupendous and far-ranging hurricane the mind can conceive.

We may fairly find in this amazing mobility another and simpler proof of what we have already seen to be demonstrated by subtler evidence, the vastness of the distance which separates the real surface of the sun from that visible surface which we call the photosphere.

One other point remains to be mentioned. The spots, besides being limited in space, are limited also in time. They cannot always be looked for with any probability that they will be seen. At this present time there are many spots on the sun's face. But if he is watched week after week during several coming years, it will be found that the spots grow fewer and fewer till none are seen. Then several weeks, or mayhap months, will pass during which no spots and few faculæ will be seen, when the mottling will be scarce discernible, and the darkness near the edge will be much less marked than usual. Then the spots will begin to return, will become more and more numerous till they attain their maximum frequency. Then they will diminish till they disappear, then return, then pass away again, and so on continually, waxing and waning with a sort of rhythmic flow. But the oscillation is not uniform. The average interval between two successive epochs of greatest spot frequency is a little greater than eleven years, but the interval has been as short as eight years, and it has been as long as sixteen years.

Such being the most striking peculiarities of the sun-spots, let us see whether they can be associated together, some or all of them, by any theory as to the way in which these great openings in the luminous cloud region are formed.

In the first place, it may be fairly assumed that the real seat of the disturbance seen when a spot appears, lies below the visible surface of the sun. There are, indeed, several circumstances which seem at a first view to suggest that the disturbance has its origin from outside. If the spot period were of constant length, one might be led to suppose that some as yet undiscovered comet, having a period of about eleven years, and followed by a train of meteoric attendants, travels in an oval orbit intersecting the outlying cloud envelopes of the sun, and periodically with its flight of meteoric followers breaks through the region of luminous clouds. There are also certain peculiarities of sun-spots, noted by the late Mr. Richard Carrington, which have been held to indicate an external origin. But as none doubt that the real energies of the sun reside in that concealed mass which lies within the photosphere, hidden by a veil through which man can never hope to penetrate, and as the spots by their size and movements tell of most energetic disturbing forces, we must, it would seem, look for their origin where alone such forces are at work.

Again, if the origin of the spots is below the photosphere, and at the real surface of the sun, since the distance between this surface and the photosphere is enormous, we can hardly imagine any way in which forces exerted at the surface can affect the photospheric cloud region, unless they are directed with great energy radially from the sun's surface. In other words, it would seem that the forces at work in producing sun-spots are eruptional.

Now, if we conceive the outburst of masses of strongly compressed and intensely heated gases from below the sun's real surface, and trace the result of their uprush, we are led to recognize certain phenomena, which certainly correspond well (be this explanation true or not) with what is seen on the sun. Even if the theory is incorrect, it has its value in thus associating together, as will be found, the various facts known about sun-spots, the coloured flames, and the solar corona.

Let us suppose that a great eruption begins deep down below the visible surface of the sun, imprisoned gases bursting their way forth, and in their outburst driving masses of solid or liquid matter like missiles through the distant photosphere. As the compressed vapours travel onwards to regions of diminishing pressure, they would expand, cooling in the process, and drive away from all round the region where they reached the visible surface the clouds which had covered that region. At the beginning there would be a central space, from around which the clouds were thus cleared over a continually widening area. Moreover, regarding the visible surface as part of a cloud stratum of great thickness (certainly not less than 10,000 miles in depth), it is clear that the constantly expanding masses of vapour, in their upward rush, would drive the higher parts of the cloud region farther apart than the lower portions. Thus looking squarely into the opening, from outside, as when we look at a spot near the centre of the sun's face from our terrestrial standpoint, we should obtain slant views of the cloud stratum.

Now, the clouds which had before been spread uniformly over the scene of disturbance, being driven away from it upon the surrounding region, would necessarily be packed closely together, and so would form luminous streaks all around the spot — the faculæ, which, as we have seen, surround the disturbed region. The penumbra would show what lies underneath the photosphere, but not in its normal condition; for the mighty uprushing and side-thrusting masses of vapour would displace all parts of the cloud stratum, even as the outer parts are displaced and made to form facular streaks. Still we can form an idea, from the aspect of the penumbral fringe, respecting the normal condition of the inner parts of the solar cloud region. The radiating streaks, which are manifestly slant streaks of luminous matter below the clouds, seem to tell us clearly of streaks which had been vertical before the disturbance. We may compare what we see round the spot to what one would see in looking down upon a field of wheat (from a balloon, suppose) over a part of which a small but violent whirlwind was passing. All round the centre of disturbance the stalks of wheat would be driven aslant, and we should see them sloping radially around that centre. The ears of wheat belonging to the storm-bent stalks would be driven closer together than the ears elsewhere over the field, and so would form circular streaks around the region of disturbance, and outside the slant radial streaks. These circular streaks of compressed wheat-ears would look brighter than the rest of the field if the ears were in their golden prime. So the glowing solar clouds, urged together by the expansive action of the vapours poured into the spot region, form streaks looking brighter than the surrounding surface; while extending from them inwards, towards the spot's centre, are seen the streaks of luminous matter which before had been vertical. What these vertical streaks may be is not very easily determined. They may be down-rushing streams of molten metal from the sun's metallic clouds, or they may be uprushing columns of glowing metallic vapours, capped by the clouds (as in our own air, uprising streams of aqueous vapour are capped by cumulus clouds), or they may include both forms; however they are to be interpreted, it is certain they exist.

After a while the eruptive forces cease; the ejected vapours for a while continue to extend themselves around the region of disturbance, but not long. All the forces now called into action are such as tend to fill in again, and cover over, the region which had been disturbed. As the surrounding cloud-covered regions strive to rush in, contests arise between the in-rushing masses and the vapours within the spot region. In these conflicts cyclonic action may arise, and usually does. Sometimes a single cyclonic whirl is generated; at other times two or more, which may be in the same or different directions; while at yet other times, changes in the conditions under which the conflicts take place, may cause a cyclone in one direction to be replaced by another in the contrary direction. Again, the enclosed vapours would maintain a better resistance and preserve the rounded form of the spot on that side towards which their motion urged them. On the other side, where the resistance would be less effective, cloud-laden masses from the solar photosphere would break in, or rather would be drawn in; and around this part of the disturbed region the photosphere would be more disturbed than elsewhere, and in many parts would be broken up.

The masses thus flung over or projected towards the region of the spot would be agglomerations of the luminous clouds with their vaporous surroundings and their filamentous appendages, which, in the more quiescent parts of the sun's surface are usually (it may be presumed) nearly vertical. A mass of clouds driven onwards, as by a mighty but irregular hurricane, would show its filaments as streamers from a wind-tossed pennon, as these luminous thread-like forms actually appear. Not parallel here, as around the edges of a yet youthful spot, the filaments would present an appearance more nearly resembling that of our cirrus clouds, with their wild mare's-tail streaks tossed seemingly hither and thither by the varying currents in our upper air. Indeed, Professor

Langley, to whom we owe decidedly the best views of the various features of the sun's surface yet drawn, finds every form of solar cloud illustrated in the clouds of our own air. But though we may thus find illustrations of solar features, we must not imagine that therefore we have necessarily their true analogues. The vast difference of scale must be carefully kept in recollection. The solar clouds, which seem simple rounded masses of luminous matter, are in reality vast cloud balls, two or three hundred miles in diameter; and doubtless, could we see them more clearly, would show amazing irregularities of structure where our present telescopes show uniformity. The filaments merely look like the thread-like forms which we see in our cirrus clouds; in reality they are forty or fifty miles in breadth, and some of them are fully 10,000 miles in length. Nothing that we know about our clouds enables us to form the merest guess as to the condition of such vast masses, such long streamers as these, or even to say that they are single masses or continuous streamers at all. And apart from all this, the intense heat which pervades the whole material of these seeming clouds and seeming streamers assures us that they are as unlike our clouds and cloud streamers in condition as they are in volume.

All that we can here say is that the sun-spots behave as though they were produced by the uprush of masses of vapour, caused by eruptive action far below the visible surface; for all the phenomena presented by a spot, from its first formation to its final disappearance, correspond with what might fairly be expected to result from such a process of formation. In passing, however, it may be noted as strong evidence in favour of the theory that sun-spots are due to the action of forces working below the visible surface, that they are regions of darkness and not of increased brightness. If sun-spots are produced in the way I have suggested, there would result great cooling from the expansive action of vapours which had been enormously compressed. On the other hand, if sun-spots had their origin from without, the bringing to rest of matter, meteoric or cometic, which had before been travelling with enormous velocity, would necessarily be accompanied by the generation of heat. Since the spots by their darkness and by the spectroscopic evidence of powerful absorptive action tell us that they are regions of cooling and not of greater heat, we may reasonably and safely infer that they are due to the action of forces working from within expansively, and not from outside with effects of compression.

But now let us see whether we may not find other evidence bearing on this theory of sun-spots, by looking outside the sun's surface for the effects, even as we have looked below for the cause of the disturbance to which they are due.

So soon as the coloured prominences had been shown by Lieutenant (now Colonel) Herschel, Janssen, Rayet, and others, to be great masses of glowing gas, it became possible to observe them without waiting for total solar eclipses. Shining with special tints only, their light could, by spectroscopic dispersion, be brought into rivalry with only such light from the surrounding sky, or even from the sun himself, as is of one of those tints. The totality of sunlight overwhelmingly surpasses the totality of prominence light; but red light from a prominence is not overwhelmingly surpassed by the red light of the same or very nearly the same tint, either from the sun or from the sunlit sky close by him. Thus, by keeping out all light save that of this special red tint of hydrogen, or if preferred the orange-yellow tint of helium, or either the indigo or the greenish-blue tint of hydrogen, the shapes and movements of the great coloured flames can be discerned and watched.

Now the most interesting of all the results which have followed from the application of this fertile method of observation has been the division of the coloured prominences into two definite classes. First there are the cloud-like prominences, which in form and movement closely resemble the clouds of a wind-swept sky, or sometimes of a sky comparatively calm. Secondly, there are the jet-like prominences, which by their form (their initial form at any rate) and by all their movements show that they are due to eruptive action.

The cloud-like prominences appear around all parts of the sun's edge, which is equivalent to saying that they occur at all parts of the sun's surface. In this respect they are like the solar clouds and the faculæ. They are apt to be somewhat larger and more numerous opposite the spot-zones, which amounts to saying that they occur in greater relative frequency, and attain a greater average size, over the spot-zones. In this respect they resemble the faculæ. It seems likely therefore that if (as is most probable) there is some connection between the coloured prominences and the phenomena of the sun's surface, the faculæ are the features to be specially associated with the coloured prominences of cloud-like form. These cloud flames often attain an enormous size and height, reaching sometimes eighty or even a hundred thousand miles above the sun's surface. They are less brilliant than the eruptive prominences, and though their movements (or rather their apparent changes of form) are sometimes amazingly rapid when compared with the movements of terrestrial clouds, yet they show nothing like the rapidity of motion observed in the prominences of jet-like form. The cloud flames may be looked for at all times, whether the sun shows many spots or few, or none; but they are apt to be rather more numerous when there are many spots.

The eruption prominences, on the other hand, are never seen except opposite the spot-zones, or, in other words, they never exist except over these zones of the sun's surface. Moreover, the jet prominences are only seen when there are spots on these zones; and though this has not yet been actually established by observation, there are strong reasons for believing that an eruption prominence is never to be seen except above a solar spot. Their occurrence only over the spot-zone, and at a time when there are visible spots, suffices of itself, however, to prove that they are intimately connected with the occurrence of that particular kind of disturbance which results in the breaking up of the photosphere and the formation of sun-spots.

This being so, it becomes probable, on *à priori* grounds, that by studying the jet-like prominences we may obtain information about sun-spots; and *vice versâ*, that any true theory we may be able to form respecting sun-spots will throw some light on the nature of the eruption prominences.

These jet-like protuberances are generally smaller, brighter, and better defined than their cloud-like brethren. They have usually been regarded as actual eruptions of glowing hydrogen; but this view seems as incorrect as would be the idea that the smoke and products of chemical action flung from the mouth of a cannon are the real missiles ejected. We may, indeed, by noting the behaviour of the glowing hydrogen in the eruption prominences, obtain clear and decisive evidence that it is to the smoke from a cannon they are to be compared rather than to the ejected missiles. We see lofty columns of the glowing hydrogen at first as though they had themselves been flung forth as mighty streams of gas from the sun's interior; but a few minutes later the upper parts of these columnar streams spread themselves out into cloud-like forms, much as the smoke which at first rushes straight enough from the mouth of a cannon begins presently to expand into cloud-formed masses. Such, for instance, was the behaviour of a mighty spiral column of glowing hydrogen seen by Zöllner as far back as 1870, and pictured in my treatise on the sun. Here was a column 32,000 miles in height, so that four globes like our earth, placed one upon the top of another, would not have reached to the summit of that long column. How unlikely, on the face of things, that a rare gas such as the hydrogen then seen (for, by the spectroscopic method of observation, its density could be determined and was found to be small) could be ejected through resisting vaporous matter to so enormous a height. But even could this have happened, it is certain that after rushing *thus far* the hydrogen would continue to ascend in jet-like form, not begin to spread into cloud form just where the jet-like motion would have become possible in consequence of the greatly diminished resistance.

If any doubt could remain after the consideration of such cases, it would be removed by the phenomena presented during the remarkable eruption witnessed by Professor Young in 1871.* On that occasion a long low-lying cloud of glowing hydrogen was torn into shreds by a tremendous outburst which occurred below. Long filaments of hydrogen were seen travelling upwards so swiftly that their motion was actually discernible, a circumstance very unusual, and meaning a great deal at the sun's distance. Higher and higher these filaments of hydrogen seemed to rush, until at last they had attained the enormous height of 210,000 miles (at least)† from

* Eruptions of a similar character have been witnessed since, but that was the first that had ever been seen.

† They may have passed much farther away than this, for

the sun's visible surface. Even at that enormous height they did not cease to ascend; they simply lost their lustre and became no longer discernible.

From a calculation based on the observed time in which this enormous distance seemed to be traversed, I determined the velocity with which the matter ejected on that occasion crossed the visible surface of the sun at certainly not less than 300, and probably not less than 500 miles per second. Now the filaments of glowing hydrogen by no means presented the appearance of bodies rushing with enormous velocity through a resisting atmosphere. On the contrary, they were long irregular streaks of luminous gas, pointed in front (with reference to the direction of their motion) as well as in the rear. I do not think they can possibly be regarded as the missiles then ejected. Their motion was probably apparent only, not real. I take it that when one of these filaments was seen apparently advancing with enormous velocity upwards, what was really happening was this: A solid or liquid mass was rushing upwards, tearing its way through whatever hydrogen lay along its track, and thus leaving behind it a trail of glowing hydrogen, growing at the upper end as the missile advanced, and losing length at the rear end as the imparted heat passed away, and so appearing to advance—even as the trail of a meteor seems to advance; though in reality the luminous matter forming that trail has not passed onwards, but the meteor passing onwards has caused atmospheric regions continually farther and farther forwards to become luminous.

It is tolerably obvious that on this occasion there was an ejection of matter solid or liquid (or if

the distance measured was the apparent distance; and if their course was aslant to the direction of the line of sight, the real distance was certainly greater, and may have been much greater.

vaporous, then of great density) at velocities so great that the ejected matter could never return to the sun. A velocity of about 380 miles per second is the greatest the sun can control in matter at his surface. In this case the ejected matter probably crossed the sun's surface with a velocity far exceeding this, and is now travelling, with velocity constantly diminishing but never to be entirely lost, into the remote depths of interstellar space. It is difficult to see how so enormous a velocity as this could have been acquired or imparted below that mobile surface which we call the photosphere. Professor Young has suggested that the sun is a gigantic bubble, and that beneath the skin (really the enclosing strata) of this bubble the forces of outburst may be restrained until they acquire the energy necessary to expel matter at the observed rate of ejection. But everything in the behaviour of the great eruption prominences speaks of an origin much more deep-seated than the inner layers of the photospheric cloud regions. Doubtless it is at and below the real surface of the sun that the eruptions occur by which missiles are ejected through the solar cloud envelopes, to pass in some cases but a few thousand miles higher, in others hundreds of thousands of miles away through the heart of the corona, and in yet others beyond the very limits of the solar system itself.*

Lastly, in the corona itself we find evidence of the action of eruptive or repulsive forces in the solar spot region, though indirectly rather than directly. There is, indeed, direct evidence of some such action

* It is noteworthy that in 1864 Mr. Sorby, of Sheffield, was led by the microscopic study of meteors to the belief, or rather to the conviction, that they had once been either in the interior of our sun, or of a body in the sunlike state ; while the late Professor Graham, of London, was led to a precisely similar conclusion respecting the Lenarto iron meteor, by the quantity of hydrogen which he found occluded within its mass.

in the greater extension of the corona opposite the spot-zones. But the indirect evidence is stronger. The light of the corona, under spectroscopic analysis, is found to be partly reflected sunlight, partly inherent light due apparently to two sources;—first, incandescent solid and liquid matter in the neighbourhood of the sun, and secondly glowing gas. The lines of glowing hydrogen show that this gas is present in the corona at times, if not always, though assuredly not as the component of a gaseous atmosphere extending from the sun to the distance of even the inner bright corona. But it is noteworthy that the lines of hydrogen have only been seen or have only been bright at a time when there have been many spots on the sun's face, and therefore at the season when eruption prominences appear. It seems reasonable to infer that at such times the eruptive or repulsive action of which the jet prominences give evidence, leads to the ejection or repulsion of meteoric and cometic matter through the hydrogen present in the corona, and consequently to the heating of the hydrogen in such degree that its bright lines show under spectroscopic scrutiny.

It seems certainly noteworthy that so many phenomena presented by the sun-spots themselves, the coloured flames, and the corona, accord so well with a theory originally advanced only as a suggested way of interpreting certain features of the solar spots. Whether the theory is sound or not, it serves conveniently to associate a number of highly interesting facts respecting these phenomena of the sun and of sun-surrounding space.

CHAPTER XII.

TWO SUNLIKE PLANETS.

In my last essay, in presenting "A New Theory of Sun-spots," I point out certain considerations which involve in reality changed ideas as to the sun's actual condition—but ideas not altogether new, since they were propounded by me several years since. According to these views, the surface of the sun, as we see it, is simply the outside of a region of clouds, having a visible depth of some ten thousand miles, and separated from the sun's actual surface by a region of vaporous matter probably many tens of thousands of miles in depth. I point to three kinds of evidence in favour of this view, in fact absolutely demonstrating its justice: first, the varying rate at which the sun-spots are carried round, those near the equator completing seven circuits while those farthest from the equator complete but six; secondly, the evidence of our earth's crust, which tells us that the sun has been at work far longer than we could infer if his real globe were as large as the globe we see; and thirdly, certain mathematical calculations by Professor G. H. Darwin, which show that unless the sun's central parts were much more compressed than the rest there would be measurable flattening at his poles. The consideration of the vast distance thus shown to separate the real surface of the sun from the surface we see, led to the inference that the cloud region which constitutes his apparent surface can hardly exist within an atmospheric envelope properly so called, simply because within such an envelope the pressures would increase so greatly with approach towards the sun's centre that within much less than the hundredth part of the distance separating that surface from the cloud region, pressures changing the gaseous into liquid matter must inevitably arise. Then we were led to consider the evidence given by the sun-spots, prominences, and corona, finding reason to believe that the sun-spots are phenomena of eruption from beneath the real surface of the sun, that in the mighty eruptions producing these phenomena matter is driven out through the region of the coloured flames, outwards even through the whole coronal region, farther yet to the very outskirts of the solar system ; nay, even in some cases beyond the limits of the solar system into the interstellar regions.

Now it is becoming generally recognized that in suns and planets, in all the orbs in fact which people space, there are stages of existence akin to the stages of life. There is a period of preparation, a period of youth, a period of mid-life, a period of decay, and finally there comes the end of life. The stages of an orb's life may be described as, in the main, stages of cooling. In suns we find evidence of such changes in the different condition (as shown by the spectroscope) of such orbs as Sirius, Vega, and Altair on the one hand, and orbs like our own sun, much smaller and therefore much more advanced in orb life, on the other. But also we find much older suns than ours in the orange-red and yellow stars, and older orbs still in the blood-red and garnet-tinted suns shown by the telescope in various parts of the star-depths. Nor do astronomers doubt that there are older suns yet, suns which have passed on to the last stage of all (mayhap), the stage of darkness. Moreover, as it is a known law of cooling bodies that the larger a cooling mass of given temperature is, the longer will be the stages of its cooling,* we may safely assume that, apart from differences (which may nevertheless be enormous) in the time of beginning orb life, the larger suns, having much longer periods of life, will have passed through less of their longer lives than the smaller suns.

If we extend such considerations to our own solar system, as indeed we may do with much more confidence, since, forming a single system, it has doubtless had a simple history, we find certain very interesting ideas suggesting themselves in relation to the various orders of bodies forming that system. We see that the giant planets, for example, being very much smaller than the sun, must have much shorter lives. The sun exceeds Jupiter 1,047 times in mass, and Saturn is less than a third even of Jupiter. It is clear that, even granting the sun a start of millions of years of orb life as compared with these giant members of his family, he would still be very much younger than they are in development. We understand, in fact, how it is that, whereas he is in the sunlike or glowing vaporous stage, they no longer have the sunlike aspect. But, on the other hand, they exceed our earth so enor-

* The law is experimentally verified in a great number of well-known cases, but the reason is not far to seek. The quantity of heat in a mass of matter, at a given temperature, is of course proportional to the mass (comparing bodies of the same constitution), which varies as the cube or third power of the linear dimensions, whereas the rate of parting with the heat is necessarily proportional to the surface whence alone the heat can pass, that is, to the square of the linear dimensions. Hence, in bodies of the same substance and similar in shape, the duration of a process of cooling is proportional to the linear dimensions.

mously both in size and in mass, that by parity of reasoning she ought to be very much older than they are (in development always, I mean, not in years). Jupiter exceeds her 310 times, Saturn exceeds her ninety-seven times in mass, and such differences as these imply not only difference in degree but kind. Jupiter and Saturn must not only be more youthful than the earth, but in a different stage of orb life altogether. We may say that they must be in a stage intermediate between the sunlike and the earthlike—they may be expected to show under careful study evidence of conditions alien to those found in the sun, as probably at least as of conditions resembling those existing in the case of our earth.

It seems to me that, this being so, we may reasonably look to the giant planets to give evidence respecting the sunlike stage of orb life, to show features such as we have recognized in the sun—however readily we admit, as of course we must, that they are not sunlike now.

At the very outset of the inquiry we find a resemblance which is at least striking, even if it be merely one of those which are rather accidental than actually significant. Jupiter's system is an almost perfect miniature of the central part of the solar system. If for a moment we regard Jupiter and the other outside giants of the system as not really forming part of the sun's family, then that family would consist of four worlds (one of them double): Mercury, Venus, the Earth-and-Moon, and Mars. Jupiter similarly has a family of four worlds, his four moons, and the paths of these lie at distances closely corresponding, but on a smaller scale to the distances separating the paths of the terrestrial planets. Moreover the moons of Jupiter are by no means such insignificant bodies as their telescopic aspect might seem to suggest. The least of

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them has a surface as large as that of North and South America together,—the largest is not much smaller than the planet Mercury. In fact the one feature which spoils the perfection of the miniature picture formed by the Jovian system is that his moons are relatively very much larger than the planets. Again, in the case of Saturn we have a system of eight worlds, the largest of which is nearly as large as Mars, the second in size nearly as large as Mercury, while the least has a surface large enough to be the abode of many millions of living creatures.

We thus see in Jupiter and Saturn, as in the sun, orbs ruling in one sense, orbs serving (but generously) in another sense. Each bears potent sway over a family of worlds, but each pours forth in plenty rays of light and heat by which life on those worlds may be nourished. This is true even if we have to regard Jupiter and Saturn as only reflecting rays of solar light and heat; simply because their power to so deflect towards their subordinate worlds the rays sent forth by the supreme centre is so enormous. I have calculated that in the skies of his nearest moon Jupiter must show a disc 1,300 times as large as that of the full moon. In the heavens as seen from the innermost Saturnian satellite, the ringed planet must appear so enormous that when the outer edge of his ring system touches the horizon the opposite part of that outer rim must reach to the point overhead.*

* It may be remarked in passing how carelessly writers on matters astronomical have accepted and repeated the suggestion of Brewster, Whewell, Chalmers and others, that the four moons of Jupiter make up to that planet for the small amount of sunlight he receives from the sun, while the eight moons and the ring system of Saturn play a like part, with even greater effect, for that still remoter planet. As a matter of fact, the total amount of sunlight reflected by all the moons of Jupiter is barely the sixteenth part of that reflected by our full moon; and the eight moons of Saturn reflect rather less. As for the rings of Saturn, I have shown in my treatise on *Saturn*

Here, then, at once is a solar attribute possessed by the giant planets—unlike the earth, they are like the sun in being the centres of systems of circling worlds. So far as analogy can be our guide at all in such matters, and I must admit it is not an altogether trustworthy guide, it would seem that we should rather regard Jupiter and Saturn as orbs nourishing life in a system of circling worlds, than as themselves worlds fit to be the abode of multitudinous forms of life.

But now let us turn to the more trustworthy evidence afforded by physical features—let us see what the telescopic aspect of the giant planets, and what the spectroscopic analysis of their light, may suggest as to their actual condition.

Here again, however, we are immediately struck by solar rather than by terrestrial features. We see a surface of cloud, not a surface of land and water. We see evidence of enormously rapid rotation. We recognize the existence of parallel banks or belts of clouds, akin in some degree to the parallel zones in which the sun-spots travel. When we watch the movements of the markings on the belts, we find that they are not only carried round by the rotational motion, but have different rates of motion, indicating (much as we found in the sun's case) widely different rates of rotation in the various zones of the planet.

Now the interpretation we were forced in the sun's case to place upon the existence of different rates of rotation in the visible surface, was that the real surface of the sun lies very far below the surface of

and its System, by mathematical demonstration which there is no disputing, that they serve, not to add to the planet's supply of sunlight, but enormously to reduce it, actually casting large parts of the planet into total eclipse, lasting for five or six of our years at a stretch, and causing scarcely less disastrous eclipses to every part of the planet from which the rings can be seen at all.

clouds which we actually see. We found other evidence which not merely supported but actually demonstrated this view. Let us see how the case stands with Jupiter. Is there any other evidence to confirm the belief that the visible cloud-surface lies at an enormous distance above the surface we see?

It appears to me that, although in this case we want the evidence which in the sun's case we derived from the crust of the earth, we have very strong evidence to show that the real globe of Jupiter is very much smaller than the globe we see and measure.

Compare first the quantity of matter contained in Jupiter with what we should infer from his apparent size. He is 1,250 times as large as the earth, but only 310 times as massive. Yet every part of that great mass of his possesses the power of attraction to compress the planet's substance towards the centre. Made, as in all probability* Jupiter is, of the same materials as the earth, we might fairly expect him to be a much denser rather than a much rarer planet. Even if his whole mass were molten through intensity of heat, still we might expect the slight expansion so arising to do little more than counterbalance the effect of the enormous self-contracting power residing in his mass or weight, even if it did as much. We are justified, then, seeing his mean density is but one-fourth of the earth's—or, as it chances, almost exactly the same as the sun's—in inferring that he is not so large as he looks. Doubtless his real globe is at the very least as dense as the earth's. In this case the volume of the true Jupiter is but one-fourth the volume of that globular

* Since the central sun is of the same material as the earth —one of his family—we may infer that he is of the same material as the other members of his family (for why should the earth differ from the rest?). If so, it follows that all the members of the solar system are formed of the same materials.

space, enclosed within vast cloud-layers, which we measure and regard as the real globe of the planet. This would assign to the true Jupiter a diameter of less than two-thirds his measured diameter, or, making his radius about 26,000 miles instead of about 40,000 miles, would leave a distance of at least 14,000 miles intervening between the surface of the real globe and that outside surface which we see and measure.

Next take the case of Jupiter's brother giant, Saturn. Here we have apparently an even younger orb than Jupiter. Saturn's ring system is in reality a part as yet unfinished of his system of dependent bodies. It consists of multitudes of tiny bodies travelling in the same general plane, and like sands on the seashore for number. Hereafter, under the mighty forces of the planet's energy of attraction, this system of rings will be broken up to form two or three other worlds akin to the eight satellites which already travel round the planet. While we thus find evidence of extreme youth in the ring system, we find confirmation in the singularly small density of Saturn. With a volume exceeding that of the earth seven hundredfold, he has less than 100 times her mass. We must explain this in the same way as in Jupiter's case. We must suppose Saturn's real orb to be more than a hundred times the earth's globe in volume, that is, one-seventh part of the volume of the cloud-enwrapped space we measure as if it were Saturn's veritable globe. This would make the diameter of Saturn fully 16,000 miles below the surface we see and measure; or, taking the mean radius of his cloud-surface at 36,000 miles, his actual radius would be about 20,000 miles.

Observe, now, the evidence of those parallel belts into which the cloud-surface both of Jupiter and of Saturn is nearly always arranged. If we could imagine anything akin—*constantly*—to the trade and counter-trade wind-zones on the earth in these belts, we might admit the same cause in explanation of them, the existence, namely, of atmospheric currents from the equatorial towards the polar regions and from the polar towards the equatorial regions. But no one who has ever seen these cloud-belts through a good telescope can admit such an explanation for an instant. Yet there is only one way in which cloud-belts in the direction of a planet's rotation can possibly be explained. They must be due to differences in the rates of rotation, causing a rush of cloud-masses forwards where regions of slower rotation are entered, and backwards where the regions reached are of more rapid rotation than those left. If these differences in rotational rate are not due to different distances from the axis in different *latitudes*—and manifestly they are not so caused—they must be due to difference of distance from the axis at different *levels*. Thus, then, in the multitudinous and ever-varying cloud-belts of the giant planets we have evidence of the vast range of distance, from and towards the centre, over which the cloud-masses around these planets can travel. When they rise they lag behind in long trailing masses; when they descend they rush forwards: in either case until frictional resistances cause them to attain the same rate of rotational progress as the surrounding masses.

But further, there is an argument in the case of the giant planets akin to that which was deduced from Professor G. H. Darwin's reasoning in the case of the sun. It has been shown that the perturbations of the movements of the inner satellites both of Jupiter and Saturn are absolutely inconsistent with the belief that the apparent globe of either planet is really occupied by the planet's mass. It is rendered certain by these researches that the real globe of either planet is very much smaller than the globe we see. How much smaller has not yet been ascertained by this method, but it is certain that the difference of size must be enormous and akin to that suggested by the other lines of reasoning considered above.

In passing I may consider a difficulty which, though it could not obviate the force of the evidence already adduced, deserves attention, even were it for no other reason than this, that nearly always the study of difficulties leads to the recognition of new truths. It follows necessarily, from the vastness of the distances intervening between the visible surface of the giant planets and the actual surface of their globes, that the rarity of the outer regions which we see must be enormous. Nay, we seem forced to recognize here something like what we recognized in the case of the sun—the absence of any continuous atmospheric pressure throughout the cloud-laden regions surrounding the giant planets: for, according to all the known laws of gaseous pressure, the densities attained even at depths of a hundred miles below a cloud region existing at such pressures as prevail within our own cloud strata would be enormous. But if we admit such exceeding tenuity in the cloud region forming the visible outer surface of the giant planets, it seems at a first view as though the edge of these planets' discs ought not to be sharply defined, but resemble rather a soft haze or mist. But this idea will be corrected if we consider the real state of things in a tenuous cloud region, such as we suppose to form the outer part of the visible surface of Jupiter or Saturn. At the distance of either planet a depth of fifty miles would appear in the most powerful telescope as the finest possible line. But a line of sight passing fifty miles below the outermost cloud-surface on Jupiter would traverse no less than 4,000 miles within that surface, that is, a range of 4,000 miles of cloud. A line of sight passing even but five miles below the outer surface would pass through 1,300 miles of cloud-strewn space. Is it likely that, however thinly the clouds might be strewn along a range of 1,000 miles, a line of sight could actually pierce through, so as to reach the region beyond? If a line of sight could not so pass through, then that is equivalent to saying that the planet up to that distance—less than four or five miles from its apparent edge—would appear as if absolutely opaque. Yet four miles would be far beyond the power of the largest telescope to appreciate at Jupiter's distance. Therefore, unless the clouds are so thinly strewn that the eye can pierce through a range of 1,000, nay of fully 4,000, miles of them, it is certain that the outline of the disc must appear as sharp and continuous as though the planet were a solid globe. In the case of Saturn the argument is even stronger, for he is very much farther away than Jupiter, so that 100 miles at Saturn's mean distance from us would look no larger than fifty miles at Jupiter's. Now a line of sight passing 100 miles below the visible surface of Saturn, would have to traverse a range of some 4,700 miles of cloud-strewn space.

But how if it shall appear that, though usually the cloud-layers around Jupiter suffice to give to the edge of his disc a well-defined appearance, not readily distinguishable from that of a solid globe (except, of course, for the parallel belts which are so strongly suggestive of a cloud-surface), yet at times the outer parts of Jupiter's disc are transparent to such a degree that a line of sight through 20,000 miles of the cloud-strewn region can yet pass onwards to detect faintly illuminated matter beyond? This has happened, in four cases at least,—and it need hardly perhaps be said that its occurrence even once would suffice to prove all that such observation, even though repeated a hundred times, could establish.

Let us consider the evidence.

The four moons of Jupiter, in their movements around the planet, pass athwart his face in one direction when on the hither side, and in the opposite direction when beyond him. They are all the time illuminated by the sun's light—except, of course, when they are in the planet's shadow. The degree of this illumination depends in part on the nature of their surfaces, but chiefly on their distance from the sun. Supposing them to have the same reflective capacity as our moon, which is probably very near the truth, the actual lustre of their surfaces under the sun's illuminating power is equal to one twenty-seventh of the lustre of the surface of the full moon; therefore it would naturally be much more difficult to see one of them through a cloud of given density than to see our moon. Now Mr. Todd, of Adelaide, Government Observer for South Australia, in response to a suggestion of Sir George Airy's, devoted special attention for many years to the movements of the satellites of Jupiter, timing them carefully as they entered on the planet's face, or passed off, or hid behind one side of Jupiter's disc, or reappeared on the other side. While engaged on such work, Mr. Todd has on four occasions seen a satellite of Jupiter when on the farther side of the planet, and so situated that, were the planet opaque to its very edge, the satellite would be just invisible, its unseen surface lying just inside (but touching, in the optical sense) the outer edge of the planet. On each occasion Mr. Todd's observation was confirmed by his assistant, nearly as skilful an observer as himself. The instrument employed was a fine eight-inch telescope by Cooke, of York. The circumstances were on each occasion most favourable for distinct vision—and in the singularly pure air of South Australia an 8-inch telescope, in good observing weather, would do better than a 12-inch or even a 15-inch telescope in our own hazy air.

Now it may be easily calculated that to see the whole disc of a globe 2,000 miles in diameter within the apparent outline of a globe 80,000 miles in diameter, the smaller being at the time on the farther side, the line of sight must pass (in order to reach the innermost edge of the smaller globe) through no less than 25,000 miles of the substance, whatever it may be, forming the outer part of the larger planet. A cloud-strewn region, then, we certainly have, since Mr. Todd and his assistant could not possibly look through 25,000 miles of solid matter. But, moreover, the clouds must be strewn with exceeding tenuity. What sort of continuous cloud, for instance, can we imagine, through ten miles of which our own moon, twenty-seven times better lit than the satellites of Jupiter, could possibly be seen? The air we breathe at the earth's surface suffices, even when at its clearest, to cut off an appreciable amount of sunlight when the sun's rays have to traverse but a few hundred miles of it. Probably 20,000 miles of such air would barely let the sun's light through, and certainly such a range of air would hide the sun from view if the air were not clearer than it is on an average summer's day with us in England.

The observation of a faint star through the outer parts of what looks like Jupiter's globe seems even more striking. Mr. Ellery, of Melbourne, Government Observer for Victoria, has witnessed this remarkable phenomenon. A star so faint as to be barely visible on the darkest and clearest night to ordinary eyesight, was occulted by Jupiter a few years ago, the passage of the planet over the star being visible only in southern latitudes. Mr. Ellery, armed with a telescope four feet in diameter (a reflector), watched the progress of Jupiter towards the star, expecting that the star would disappear the moment the planet's outline seemed to reach it.

But to his surprise the star continued visible for several minutes, not finally disappearing until the line of sight to the star passed 800 miles below the apparent surface of the planet. The range of the line of sight in that case carried it along a distance within the planet of some 16,000 miles. What chance would an astronomer have of seeing a sixth-magnitude star, no matter what the power of the telescope he employed, through a cloud-stratum eight miles in thickness? Yet here a star was seen through a range of more than two thousand times that distance through cloud-strewn space!

But there is clear evidence that, however sharply defined the outlines of Jupiter and Saturn appear they are of the thinest conceivable cloud-texture. The outline of Jupiter has been observed by Schroter and others to be at times irregular—portions looking flattened, as though parts of the outer surface were chipped off. The outline of Saturn is often so distorted that it has assumed what has been called the square-shouldered aspect—a peculiarity of appearance which, as it has been observed by the Herschels, Airy, the Bonds, Coolidge, and other well-known observers, we cannot reject as simply the result of carelessness in observation; nor can any form of illusion serve to explain it. Jupiter has been seen with a satellite just entered on his disc, and a few minutes later the same satellite—although, had there been no change in Jupiter's apparent outline, it would have been farther on the planet's face—has been seen off the planet's disc, as though it had changed its mind and gone back. This cannot possibly be explained except by assuming that the outline of the planet is really formed of thinly strewn clouds to a depth of many thousands of miles, and that at times, over wide areas—many millions of square miles at a time it must be—the clouds which had formed the apparent outline change from the form of visible cloud to that of the invisible vapour of water, so causing the apparent outline to shrink to layers lying far lower down. And lastly, the condition of the outer layers of Jupiter's apparent globe has been shown by the way in which the satellites, as they have advanced through the outskirts of the shadow of that globe, have been seen to wax and wane in lustre before finally disappearing.

We have other evidence, however, showing the partially sunlike condition of Jupiter. It is certain that a portion of the light which comes from the planet is inherent. We might fairly infer this from the vast superiority of the giant planet's light over that of Mars—for it is easy to calculate how much light we should get from Jupiter if his surface were of the same reflective quality—the same "whiteness," to use Zöllner's expression*—as that of Mars; and we find that, when due account is taken of his much greater distance, Jupiter ought to be rather less brilliant than Mars when the latter is nearest both to the earth and to the sun (as in the autumn of 1877). But Mars is never half as bright as Jupiter. Still, although this would indicate a difference of surface, and therefore probably of condition, it would not of itself suffice to show that the light of Jupiter is inherent; for if the surface of Jupiter were of driven snow, or even of pure white clouds, he would send us more light than we actually receive from him, yet without possessing any inherent light. It is when we notice that large portions of his surface are by no means white, that we infer from the total amount of light we receive from him that he is partly self-luminous. In the case of Saturn we have the same kind of evidence.

But Jupiter's satellites have put this matter beyond a peradventure. They cast shadows, which—at

* *Albedo* is the term he employs to indicate the reflective capacity of a surface.

least in the case of the nearer satellites—should appear as round black spots, if Jupiter has no inherent lustre. And so the shadows usually appear. Occasionally a tint of very dark purple has been suspected, but most observers, looking at the shadow of a satellite as seen in a good telescope, would regard the spot as absolutely black. The effect of contrast against the bright surface of the planet might make a really brown spot look black, but still, so far as one can judge from appearances, the shadow-spot looks ordinarily as black as a shadow thrown on an absolutely non-luminous body ought to appear. But we are able to correct this impression very effectually by observing the satellites themselves when in transit across the planet's face. Near the edge of the disc they seem scarcely different in lustre from the planet itself, and sometimes they are quite lost when in that position. But when they pass well on to the disc they nearly always look much darker than the planet. On some parts of Jupiter's face they look actually black—that is, they appear as dark as a shadow. When, as often happens, the shadow lies close beside the disc of the satellite itself, it becomes easy to make exact comparison between the apparent tints of shadow and satellite. Under these circumstances, it has occasionally happened that the satellite has been found to be at least as dark as its own shadow—in one or two cases it has appeared even darker. Now a single case of this kind suffices to prove all that could be shown by a hundred such cases. When we see side by side two round surfaces, one of which we know to be the surface of a satellite (a body similar in surface-contour, no doubt, to our own moon), the other a part of Jupiter's surface which no sunlight can reach, and find that these two portions of surface show precisely the same degree of darkness, it becomes certain that from the region cast into shadow there comes as much light as from the sun-illumined surface of the satellite. This may be but about the thirtieth part of the apparent luminosity of our moon's surface (which is allowing the satellite of Jupiter a darker surface), but still it is something. Off the disc of Jupiter the satellites, one and all, look bright enough. Hence, we have it proved that *in some cases* as much light comes from Jupiter's surface, not as reflected sunlight, but on account of the inherent luminosity of that surface, as we get from the surface of a satellite reflecting sunlight to about one-thirtieth the amount reflected from our moon's surface. Now, that any inherent light should be found in any part of Jupiter's surface, there must be intense heat. This would be the case even if the surface we see were the actual surface of the planet; for we know that no kind of rock-surface glows with inherent light unless it is very hot. But when we remember that the surface we see is a surface of cloud, that the cloud masses form layers probably thousands of miles in depth, it is evident that for any inherent light to show through these clouds (which cannot themselves be luminous) there must be an immense amount of light coming from the planet's real surface, which therefore must be intensely hot. It would seem as though, were the outer envelope of clouds removed, and the planet within seen from some point of view such that no reflected sunlight would come from him, he would be found to shine with considerable lustre of his own—probably akin to the kind of light which we get from the blood-red and garnet-tinted suns forming the fourth of Secchi's four orders of stars (as classified by their spectra).

We may pause for a moment to consider a point touched on, in passing, above. The satellites show much more nearly the same lustre as Jupiter's surface, when near the edge than when near the middle.

This, of course, shows that Jupiter's disc is darker near the edge. Yet it does not look so. On the contrary, it looks so much brighter to ordinary eyesight near the edge that the French astronomer Chacornac was actually led to devise an ingenious theory in explanation of the supposed superiority of lustre there. I remember well how astonished was an astronomical friend of mine, who had spoken of this theory with approval, when I asked him to test the amount of the supposed increase of lustre near the edge, assuring him that he would find decrease instead of increase. He observed Jupiter with carefully graduated darkening glasses, and found, as I had predicted, that the edges disappeared first. But the lesson derivable from Chacornac's mistake—a mistake into which many observers of Jupiter have fallen—is worth careful study. We learn how apt we are to be deceived by mere contrast. Just as the satellites of Jupiter in transit appear sometimes black by contrast with the planet's bright surface, and the shadows black when in reality only dark, so the parts near the edge appear brighter than the rest of the disc because contrasted with the dark sky around, though in reality darker. The general lesson is to beware always lest false theories should be suggested by illusions of the sort. The particular lesson is that the parts of Jupiter near the edge are darker than the rest, and the interpretation is, I take it, that these parts are to some degree transparent—not always or often, scarcely ever, perhaps, so transparent as they must have been on those occasions when Mr. Todd saw a satellite or when Mr. Ellery saw a faint star, through thousands of miles of this star-strewn outer covering of Jupiter, but still always transparent enough to allow much sunlight to pass through, and so to look darker than the rest of the planet's surface, because not reflecting so much sunlight. But the falling off of lustre may very probably be in part also due to the circumstance that none of the planet's inherent light can come from the parts near the edge.

The great red spot on Jupiter (see next Essay), of which during the last few years we have heard so much, is perhaps the most sunlike feature of all. It had a surface of about 150,000,000 square miles —three-fourths of the entire surface of our earth. From that surface came a ruddy light, which gave clear evidence, under keen spectroscopic cross-examination by Dr. Henry Draper, of being in part inherent. The singularly regular shape of the spot—a perfect ellipse—showed that it was due to expansive action of vaporous matter encountering, but overcoming, the resistance of the vaporous atmosphere around the region occupied by the spot. The only way, I believe, in which the form and colour and persistence of this great spot can be explained satisfactorily is by a theory akin to that suggested in my article on Sun-spots. It would seem that from deep down below the layers of cloud which, until 1876, had covered the region occupied afterwards by the spot, mighty masses of compressed vapour were flung upwards with intense energy, making their way through those layers of cloud, and rolling the clouds away on all sides till an enormous area was cleared. Through the region thus cleared of cloud we could see the ruddy light from the glowing surface within. So long as the forces at work below drove the clouds away from this region, the spot remained, retaining alike its colour and its singularly regular form. This lasted *for more than five years*, after which, though the spot did not disappear, yet it lost its lustre, while the irregularity of its shape showed that the vaporous masses flung up from below were no longer able to drive away, as before, the cloud-masses which were endeavouring (so to speak) to return to the region from which they had been driven.

The existence of disturbances such as this, so widespread, so long lasting, and giving evidence of such intense heat in the planet, must assuredly suffice to dispose of the belief that Jupiter is a world like our own, and to prove that, though not actually a sun, his condition is more nearly akin to the sun's than to that of our own earth. And what is thus proved of Jupiter is proved also of his brother giant Saturn, seeing that all the evidence shows Saturn and Jupiter to be in nearly the same stage of planetary life, Saturn, if anything, being the younger and more sunlike of the two.

CHAPTER XIII.

THE GREAT RED SPOT ON JUPITER.

PROFESSOR YOUNG in his farewell address to the Philadelphia meeting of the American Association for the Advancement of Science spoke of the great red spot which has for many years been the most remarkable feature of the planet Jupiter as a mystery "probably hiding within itself the master-key to the constitution of the great orb of whose inmost nature it was an outward and most characteristic expression." Without altogether accepting the view of the red spot thus in strangely mixed metaphor presented, I must most thoroughly express my agreement with the opinion underlying Professor Young's rhetoric. The great red spot on Jupiter is undoubtedly the most mysterious of all the phenomena which even the Prince of Planets has presented to the student of astronomy. A vast opening, about 150 millions of square miles in extent, lasting many years, undergoing changes of shape and of position most remarkable in character, this great red spot undoubtedly contradicts emphatically all the old-fashioned ideas respecting Jupiter: and it as certainly presents many perplexing questions for those to answer who have adopted the more modern ideas.

Yet it has always seemed to me that the more remarkable a phenomenon is, the better is it worth studying, and the more likely is it to reward careful study by truthful information. A perplexing problem of this kind may be compared to a complicated lock, which will not open to any ordinary key; but when a key which will open it has been found then may we feel well assured that that key is the right one; whereas, when a commonplace phenomenon has been accounted for, we can have no more certainty that our solution is right than we can feel respecting a key (one, perhaps, among a dozen) which will open a lock of commonplace construction. Without claiming that as yet the correct solution of the problem of the great red spot has been found, or even that it can be, let us proceed to examine the problem with a view to the determination of at least some of the points which the true solution must interpret.

It was in the year 1876 that the great red spot was first observed—by Professor Pritchett of Glasgow, Missouri. But I have before me a picture drawn by Professor Mayer, of the Stevens Institute, Hoboken, in 1871, wherein the place afterwards occupied by the spot is marked by an oval ring about the same size and shape. Whether this was actually the first beginning of the disturbance, or merely a coincidence, cannot now be very readily determined: but it is at least worth noting, even though it should be no more than a coincidence.

When first observed the great spot was symmetrical and well defined in shape, and of a somewhat strong ruddy tint. It was about 150 millions of square miles in extent (as was also the space enclosed within the oval ring seen by Professor Mayer). The greater axis of the oval was nearly three times as long as the shorter, but part of the difference was due to foreshortening. From a study of several hundred pictures I am led to conclude that the greater axis of the spot was not more than 2½ times longer than the shorter axis. Observations made by the late

Professor H. Draper with the spectroscope seem to suggest that the light of the ruddy spot was in part inherent; but others question whether the evidence accepted by Professor Draper was altogether valid. The spot continued visible, with little change of form or colour, for about six years, after which time, though it remained visible, it lost its symmetry of form and its characteristic ruddy tint. It was half-veiled for a time (at least in appearance) by the extension of a cloud-belt lying north of it, as though this cloud-belt lying at a higher level had spread farther and farther over the spot. At present the spot, or rather the traces of the spot, can still be seen; but it no longer presents any of the features, except enormous extension, which made it so remarkable a feature of the planet from 1876 to 1882.

It was noteworthy that compared with the equatorial markings on Jupiter the great spot seemed to lag, as if the equatorial cloud-belt were whirled round in a shorter time than the side zone on which the spot was seen.

The first point to be noticed, in this remarkable phenomenon, appears to me to be that which the eye first recognizes, the symmetrical shape which the spot presented. Of course the spot was less symmetrical when seen with high powers than when observed with a small telescope; but the symmetry of shape was none the less remarkable that it belonged to the spot as a whole rather than to the spot when minutely examined and largely magnified.

Of course symmetry of form implies, in such a case, uniformity in the action of the forces at work in determining form—in this case, uniformity in the action by which the spot was produced. The path along which a projectile travels is uniform, apart from atmospheric resistance, because a uniform force is at work on the missile from the beginning to the end of its career. The action on the projectile is along lines always parallel and vertical; consequently the symmetry of the path is related to the vertical: a vertical line divides the path into two portions perfectly resembling each other. Again, the course of a planet round the sun, or of a ball swung round the centre, is symmetrical, because of the uniformity of the forces directed towards the centre. In one case the path is elliptical, in the other the path is circular, but in each case the central nature of the forces at work on the moving body tend to make the path symmetrical with reference not to a line but to a centre. Again, observe a whirlpool, a tornado, the shapes of the clouds seen around a volcanic crater during eruption, and even the rounded forms of summer clouds, and we see in each case how tendencies towards or from a centre result in giving uniformity of shape to the aggregation of matter resulting from such tendencies. The existence of a shape centrally symmetrical, whether circular or elliptical, implies in every case the existence of forces tending either from or towards the centre. I know of no exception to this rule in nature, though of course artificial productions may show symmetrical forms without giving evidence of central forces.

We may assume, then, that whatever were the forces at work in forming and maintaining the great red spot on Jupiter, they were related in some way to the centre of the oval region affected by them. They may have produced motion from that centre, or motion towards it, or there may have been movements of both sorts: but assuredly central forces were at work in some way or ways, where the great red spot was formed.

While the symmetry of the spot's shape forces on us this general conclusion, the greater length of the spot in one direction than in another possesses also a special significance.

The spot was manifestly a surface feature, in this sense that the layer of clouds in which the spot appeared as a sort of opening was part of the visible surface of the planet. This was shown by the circumstance that as the spot drew near to and passed the edge of the planet its outline remained distinctly visible. Had the spot been due to some formation lying below the surface clouds of the planet the spot could not have been thus traced up to the planet's edge. Of course this need not prevent us from recognizing the true cause of the spot as existing far below the surface-level; but manifestly the cloud layer was laid open at its outer surface. Now this being so, it is clear that were the forces which formed the spot all at work at that same surface level, and all acting from or towards a centre, we should expect them all to act with about the same degree of force, and the spot to have therefore a circular shape, unless we can recognize some likelihood that in different latitudes on Jupiter different conditions would exist, or, in other words, unless we can recognize the existence of zones on the planet akin in some sense to the trade and counter-trade wind zones on the earth.

But although the most characteristic feature of Jupiter is the existence, almost always (if not always), of parallel bands or zones of clouds, diverse in their light-reflecting qualities, these zones have no permanent position like the trade zones on the earth. They vary almost capriciously in position. Sometimes there are but four or five of them, at others there are ten or twelve or even more. We cannot recognize any permanent difference, then, in the condition of the various latitudes on Jupiter, to account for the oval figure maintained so long (six years at least) by the great spot.

Yet we may still, or rather we must obviously, associate the lengthening of the great opening in a direction parallel to the cloud zones, with the forces to which the existence of those clouds—as such—is due. Now it has always seemed to me that as the trade wind theory, once complacently advanced to explain the parallel belts of Jupiter and Saturn, most manifestly fails, we are driven to another interpretation of the cloud-belts which is very significant in regard to Jupiter's condition. The trade winds and counter-trade winds, and the zones named after them, owe their existence to the difference between the rotational velocities in tropical regions which lie farther from the earth's axis and in temperate and arctic regions which lie nearer to that axis. The cloud-belts of Jupiter and Saturn must also be due to differences of rotational velocity,—not however between places in different latitudes on those planets, but between regions at different elevations in the cloud-envelopes of Jupiter and Saturn. We seem forced to admit, seeing that the belts are real, and no other way of accounting for their existence seems open to us, that there must be great movements of ascent and descent, in the cloud-laden envelopes surrounding the giant planets. Matter carried upwards, as columns of ascending vapours, or missiles ejected to enormous heights from Jovian volcanoes, passing as such matter does from regions near the centre, where the motion of rotation is slower, to regions higher up, where the motion of rotation is more rapid, lag behind and cause a trailing of cloud forms towards the west. On the contrary, matter descending, as torrents of falling rain, or matter falling back after ejection, would rush forwards and cause the cloud forms to be extended towards the east. Granted a sufficient range in height, whether in ascent or descent, and the parallelism thus arising would be as marked as we actually find it in the cloud-belts of the giant planets.

But here certain questions arise which we must dispose of before we consider in this light the lengthening of the great spot. *Can* we imagine that the cloud-laden envelopes surrounding the giant planets have the enormous depth which this explanation would assign to them ? The depth essential for this interpretation must bear a measurable proportion to the diameter of the planet. Less than at least a thousand miles (only a fortieth of the planet's diameter) would certainly not suffice ; for obviously the rotational velocities at the top and bottom of a cloud region one thousand miles high on Jupiter would not differ by more than about one-fortieth, or about $2\frac{1}{2}$ per cent., whereas the sharp parallelism of the belts indicates quite a considerable difference of velocities. Taking, however, even a depth of one thousand miles for an atmosphere which at its highest part bears clouds such as exist in our very highest cloud-bearing atmospheric strata, say ten miles above the sea-level, we find a very remarkable state of things at a depth of even but a hundred miles below the visible cloud-surface of Jupiter, unless we suppose laws connecting density and pressure to be very different on Jupiter from the laws recognized here,—a supposition which must not unnecessarily be introduced. For our air ten miles above the sea-level has a density equal to about one-eighth the density of the air we breathe, the density doubling for each $3\frac{1}{2}$ miles (or thereabouts) of descent. Taking this density as that existing at the outskirts of the visible cloud-envelope of Jupiter, we find that with his known (and well-measured) gravitating energy the density would double for each mile and a half of descent. But say that it doubles only once for two miles of descent. Then four miles below the visible surface of the planet the atmospheric density would be already half (instead of one-eighth) of our air's at the sea-level ; six miles below it would equal the density of our air ; eight miles below it would be double ; and only ten miles below the visible surface of Jupiter the density of his air would be four times the density of the air we breathe. After that, in the next ninety miles of descent, taking us only one hundred miles from the surface, there would be forty-five doublings of pressure and density, making the density millions of millions of times greater than that of air, thousands of millions of times greater than that of water, and hundreds of millions of times greater than the density of any terrestrial element. This of course is altogether preposterous; but it shows that in one way or another we have to admit the existence of conditions in Jupiter which are utterly different from any known on earth.

Less simple, but not less decisive, is the mathematical evidence adduced by Professor Geo. H. Darwin (son of Charles Darwin), who has shown that the movements of Jupiter's satellites would be other than they are if the mass of Jupiter were distributed uniformly, or with any approach to uniformity, throughout the globe we measure as Jupiter's. Either there must be great compression towards the centre of Jupiter's globe, or the outer parts of the region within the cloud-surface we measure must be of very small density, the real globe beginning thousands of miles inside that envelope.

I pass over for the moment the powerful argument derivable from the behaviour of Jupiter's satellites. But I must say that, in my opinion, when observers of great skill, like the late Admiral Smyth, Sir Thomas Maclear, Professor Pearson, Mr. Todd of Adelaide, Mr. Ellery of Melbourne, and the assistants of these last-named observers, record observations, such as the reappearance of a satellite after its transit across Jupiter's disc had already begun, and the visibility of a satellite when behind the planet and well within the disc, and the visibility of a faint star through the outer envelopes of Jupiter, it seems to me idle to advance optical-illusion interpretations such as would barely avail to explain such phenomena recorded by the merest beginners with the telescope. Thus Mr. Todd, Government Observer at Adelaide, who has had more experience than any man living in observing transits and occultations of Jupiter's satellites (having specially devoted himself to the work, in response to an appeal of Sir George Airy's), records that on four occasions he saw a satellite pass behind the well-defined edge of the planet, the form of the satellite continuing visible, without distortion, until at last the whole satellite was thus seen through the outer parts of the planet, and that on each occasion his assistant, a very cautious and well-practised observer, saw the same phenomenon. Reply is made that possibly Jupiter was a little out of focus, or his outline for some other reason indistinct, and the satellite not really seen within it, or possibly the observers (both of them!) mistook a false image of the satellite, the result of wearied eye, for the satellite itself. Surely we may say that such an explanation is inconsistent with all reasonable probabilities. A mere beginner in observation may have the edge of the planet out of focus, and suppose the blurred extension so produced to represent the real dimensions of the planet. But Mr. Todd is no mere beginner; he is an "old hand," and an old hand at this particular kind of work. His assistant, again, is no beginner, but a practised observer. In hazy weather, again, even a practised observer might form an unsatisfactory estimate of the position of Jupiter's edge (though he would by no means see a clearly-defined outline to the satellite); but the weather was not hazy; the sky was exceptionally clear and still (so Mr. Todd told me when I had the pleasure of meeting him at Adelaide in 1880). The wearied-eye theory would be quite out of the question in the case of a single observer of any skill; but when Mr.

Todd, seeing the outline of the satellite through the outskirts of the planet, called his assistant to take his place at the telescope, there was no wearied eye with a false image of the satellite on it, at work, but a fresh eye, which had not been looking at a satellite of Jupiter's for some time; and when Mr. Todd resumed his place at the telescope his eye, too, was practically a fresh one. So with other recorded cases, where skilful and well-practised eyes have observed phenomena which can only be explained by recognizing great tenuity in the outer cloud-laden regions of Jupiter, and a great extension of his gaseous surroundings in depth.

We seem to have travelled a long way from the great red spot, but in reality all that we have been inquiring into since we left the spot bears importantly on our interpretation of that remarkable phenomenon.

When we see so many independent lines of evidence all pointing to the conclusion that that state of things prevails to which the only valid explanation of the shape of the great red spot had already led us, all reasonable doubt seems removed. We may rest assured, I think, that the red spot really owed its symmetry of form to the central nature of the forces at work in forming it, and its elongated shape to the circumstance that regions at very different distances from the planet's centre took part in forming the spot, uprising matter being left lagging westwards and down-sinking matter being hurried forward eastwards, instead of travelling with uniform velocities from the centre of disturbance.

But now, as soon as we thus recognize a region below the visible surface of the planet as taking part in the disturbance indicated by the great spot, it is a natural thought that possibly the origin of the whole disturbance was not only below the visible surface of Jupiter but in the real globe of the planet. Let us see whether this idea leads us to any results which seem to correspond with the phenomena actually presented by the great spot.

If the origin of the disturbance were in the real globe of Jupiter, then it must be presumed that the original disturbance was due to the intense heat pervading the whole frame of the planet and was explosive in character. An outburst of compressed vapour from some gigantic volcano on Jupiter, carrying upwards vast vaporous masses to regions of much diminished pressure, would be followed by the rapid rush outwards of the expanding vapour, and by the sweeping away of the cloud masses which before had covered the region of disturbance, over an immense area. This area would be circular in shape in the case of a non-rotating planet, or in the case of comparatively shallow vaporous envelopes like those which surround our earth. But in the case of masses of vapour flung upwards from the real surface of a rapidly rotating planet like Jupiter, with sufficient energy to burst their way through cloud-layers thousands of miles above that surface, there would undoubtedly be a marked trailing off of the vaporous masses westwards. They would acquire a westerly motion sufficing to give the region of disturbance measurable superiority of length in an east-and-west direction. For the westerly motion would continue after the upflung vapours had reached their greatest height.

As a result of this process of westerly lagging the western end of the spot might be expected to be not quite so symmetrical in form as the easterly, a peculiarity which was actually noticed. Moreover, as the whole spot, or rather the whole of the cloud-region containing the spot, would drift steadily westwards, the planet turning all the time rapidly eastwards (one rotation in less than ten hours), it follows that the spot would have a slightly longer rotation period than the equatorial markings,—which also was actually observed.

According to this interpretation, the great red spot on Jupiter would indicate the occurrence of a tremendous outburst at the planet's real surface, an outburst compared with which the great earthquake at Krakatoa was as child's play compared with the labours of many giants. That the outbursts at its commencement was sudden may be well believed. Yet judging from the long continuance of the great spot and of the sequent disturbance, the eruptive action must have lasted a long time. Of course it does not necessarily follow that the disturbance which caused the great opening in the cloud-envelope lasted as long as the opening itself. It may well be that the movements by which a disturbed cloud-belt on Jupiter returns to its normal condition are sluggish compared with the fierce action by which the disturbance is brought about, at (or it may be below) the fiery surface of the planet itself. Still the gigantic elliptic ring seen as early as 1871, followed by a gigantic elliptic opening which remained for six years, and that again by a disturbed condition which has already lasted nearly three years and may last much longer,—all this seems quite inconsistent with the idea that the eruptive action giving birth (if my interpretation is correct) to this long-lasting disturbance was itself of short duration.

And after all, it would not be very surprising, when we consider the enormous scale on which Jupiter is constructed, the tremendous heat which must in all probability pervade his whole frame, and the correspondingly increased duration of all internal disturbances, if the analogues on Jupiter of volcanic outbursts which on the earth (so much smaller and now relatively aged) last often for many weeks, should on Jupiter last, occasionally, for several years. Jupiter, according to all reasonable probability, must be a very young planet. If his planetary career began at the same time as the earth's, he is certainly much younger than our earth; but even if he began his career as a planet millions of years before the earth, even then he would be younger than the earth in development. For those millions of years would be as nothing compared with the vast excess of the duration of Jupiter's life-stages over the duration of the corresponding life-stages of the earth. Regarding Jupiter as in a much more youthful stage of planetary life than the earth is now passing through, and remembering that even when Jupiter has reached the same stage as our earth his eruptive energies will be much greater than the earth's now are, we may well believe that the explosions now taking place on Jupiter must be on an incomparably grander scale than the mightiest volcanic disturbances on the earth. Applying to Jupiter the reasoning which was applied to the disturbance of Krakatoa in 1883, we might readily find that even a greater disturbance than the Great Red Spot indicated, tremendous and far-reaching though that disturbance was, could be explained as resulting from a Jovian volcanic outburst, vaster and fiercer than terrestrial outbursts, because Jupiter is at once a mightier and a much younger planet.

Let us look into this matter a little more closely: and first, let us ask if anything akin to the difficulty thus recognized in the case of Jupiter (and also in that of Saturn) exists elsewhere.

Now in the case of the sun we have an orb which is probably in large part gaseous. We certainly have, *visibly*, a gaseous region thousands of miles in depth, even estimating the depth only from the visible surface of luminous cloud which we call the photosphere. And in the sun's case the attraction of gravity on the atmospheric region thus recognized is ten or twelve times greater than the attraction on the atmosphere of Jupiter. Therefore we have in the sun's case a much greater difficulty than in the case of Jupiter or Saturn.

It is true that the intense heat pervading the whole frame of the sun suggests a way of meeting the difficulty which does not at first sight seem available in dealing with the giant planets. The laws which connect density and pressure at ordinary temperatures and at ordinary pressures may probably fail altogether where the temperatures are so high and the pressures so enormous as they must be throughout the whole frame of the sun. We may say, indeed, as I have elsewhere shown, respecting the outer parts of the sun we see, what Professor Young said of the usually unseen corona, that if the term atmosphere be understood as we understand it when speaking of our own air, the gaseous regions forming the parts of the sun next within the photosphere do not form an atmosphere at all. Here are his remarks in regard to the corona, each one of them being fully applicable to the gaseous envelopes within the visible surface of the sun:—"Granting for the moment that the corona is in part and largely composed of an envelope of exceedingly rare gaseous matter around the sun,—then we may call it an atmosphere, because being gaseous and attached to a cosmical body, it bears to that body a relation analogous to that borne by our atmosphere to the earth itself. So far the term is a proper one. But now further, and on the contrary, the term 'atmosphere' carries with it to most persons certain ideas as to the distribution of temperature, density, &c., in its different parts, which are based on the fact that our terrestrial atmosphere is nearly quiescent and in static equilibrium under the force of gravity, with a temperature not more than two or three hundred degrees above the absolute zero, while the density of the portion accessible to human observation is very considerable. On the sun the conditions are immensely, and almost inconceivably, different, so that the term 'atmosphere' becomes a very misleading one. There the equilibrium, so far as there is any, is dynamical, not statical, and the density, temperature, and condition of the gaseous substance is far more nearly that of the residual gas in a Crookes's vacuum tube through which an induction coil is sending electrical discharges ; so different from that of ordinary air that Crookes thought he had found a fourth state of matter, bearing some such relation to the gaseous state as the gaseous does to the liquid."

That this is so in regard to the sun is shown at once if we remember that the great openings we call spots disclose solar regions lying certainly not less than 10,000 miles below the sun's visible surface. Now the strength and breadth of the hydrogen lines seen in the spectrum of the sun's coloured flames show that the hydrogen present there is not indefinitely rarer than hydrogen at the pressure of the air we breathe. Putting the pressure at the sun's visible surface at the millionth part of the atmospheric pressure on earth at the sea-level, and noting that gravity at the sun's visible surface is twenty-seven times gravity at the earth's, we find that at a depth of two or three miles below the sun's apparent surface, atmospheric pressure would be the same as at our sea-level were the same gases present, and temperature the same there as here. For in about the eighth of a mile the pressure would double, so that in $2\frac{1}{2}$ miles there would be twenty doublings of pressure, raising the density from the millionth part of our air's to somewhat more than equality with the density of our air (two doubled, that double doubled, and so on, to twenty doublings, giving 1,048,576). In the next $2\frac{1}{2}$ miles the pressure would be increased more than a millionfold,—

always assuming the conditions to hold which we recognize in our own atmosphere. This would happen in five miles out of 10,000 miles of depth, known to be occupied by gaseous matter.

Even taking into account the tremendous heat prevailing in the sun, and the existence of much lighter gases in his surroundings than exist in our own air, we cannot escape conclusions scarcely less preposterous and assuredly quite as inadmissible as we have thus reached. If the pressure and density did not double in less than a mile, or than ten miles, or even a hundred,—which is altogether impossible,—we should still have, within a range of 10,000 miles, 10,000, or 1,000, or 100 doublings (in these cases respectively); and consequently even with the least of these numbers there would be a density at the base of the 10,000 miles exceeding a billion billion times* the density of our own air.

Undoubtedly it is not in the high temperature of gases near the sun, or not in this only, that the solution of the enigma lies. We have also to take into account the freedom of movement which exists throughout the gaseous envelopes of the sun, and the constant movements which are no doubt taking place within these envelopes.

In some such way, I think, we must encounter the difficulty, kindred in character if not so great in degree, which exists in Jupiter's case. We must admit the existence of intense heat throughout the gaseous surroundings of Jupiter, though we need not

* In the first twenty doublings equality with our atmospheric pressure would be attained, in the next twenty the pressure would be a million times greater, in the next a billion times, in the fourth twenty doublings the pressure would be a trillion times, and in the last twenty it would be raised to a quadrillion times the pressure at our sea-level. (I use the English system of numeration, according to which a million raised to the second power is a billion, to the third power a trillion, to the fourth a quadrillion, and so forth.)

imagine that they are as hot as the gaseous envelopes of the sun, or that their temperature even approaches solar temperatures. We must admit great freedom of motion within these gaseous and vaporous regions around Jupiter. So may we at once escape the difficulty which Jupiter assuredly presents, and be led to the conclusion which we had already reached from another side,—viz., that Jupiter's outer portions to a depth of many hundreds of miles within his visible surface do not belong to his real globe, but are mainly formed of gaseous, vaporous, and cloud-like matter.

From yet other directions the same result has been reached, as I pointed out in my "Other Worlds than Ours," many years before the great spot had appeared. No one now supposes that Jupiter is made of other materials than those which form the earth on which we live, nor does any one now suppose that Jupiter is a hollow planet, as Sir David Brewster insisted. Yet if we do not adopt one view or the other we cannot possibly explain the small mean density of Jupiter otherwise than by assuming that the globe we measure for Jupiter is very much larger than the planet itself. Jupiter is 1,250 times as large as the earth, but only 310 times as massive. This, alone, proves that the real globe of Jupiter lies far within the cloud-strewn surface we measure. With the enormous attraction residing in 310 times the earth's mass, a globe of the same materials as our earth would be considerably denser instead of less dense than the earth. Assigning to Jupiter a density only equal to the earth's, its diameter would be little more than 50,000 miles. Jupiter's diameter is fully 80,000 miles. The distance of the cloud-strewn surface we see, from the real surface of the planet, cannot then, it would seem, be less than 15,000 miles (the difference between 40,000 miles and 25,000 miles, the halves of the just-named diameters).

The telescopic aspect of Jupiter corresponds much better with this startling result than with the idea that he has an atmosphere in the least resembling our earth's.

CHAPTER XIV.

A DEAD WORLD.

THE ancients fell into strangely incorrect ideas about the heavenly bodies. They chose the most beautiful of all the planets, beautiful alike in symmetry of shape and delicacy of colouring, as the emblem of misery and gloom, regarding Saturday, the day sacred to that planet god, as one on which all work was unfortunate. They took, on the contrary, the most disappointing and unsatisfactory of all the sun's family as a fortunate orb, the emblem of love; and although, strange to say, the day devoted to this planet (Friday) also, was deemed unfortunate for beginning a great work, or starting on a long journey, that was only because the next day, devoted to the unlucky Saturn, compelled rest; and it is naturally unlucky to begin a great work if in a few hours you will have to rest from it.

In like manner the ancients looked at the full moon, and because she was pale, and seemed so "silently and with so wan a face" to climb the sky, they thought she was cold. "Ice-cold Dian" she seemed to them at the very time when her surface, as modern science shows, is hotter a good deal than boiling water. I say this of the full moon in perfect consciousness that an American physicist, Mr. Langley, with an instrument which he calls the bolometer, finds the full moon colder than ice. I reject the evidence of that too delicate heat-measurer, and prefer the wellattested teachings of the trustworthy old instrument, the thermopile, whose work has been tested and measured again and again and never found wanting in correctness. With too delicate a balance, you do not always know what moves it; a breath may make some light substance you are weighing seem twenty times as heavy, or as light, as it really is. And so, I suspect, it has been with Mr. Langley's unpleasantly named heat-measurer. Some unobserved change near at hand has made the bolometer tell of cooling, where it should have told of heating if it had really recorded the influence of the moon's beams. Once an astronomer who supposed his delicate heat-measurements were telling him of the heat of stars, found that in reality he had been carefully measuring the heat generated by friction as he turned his telescope towards the star. Mr. Langley was making, we may be well assured, a similar mistake. Of course he thoroughly believes in the results he has obtained; so fully does he believe in them that, supposing the cold he has found in the full moon to result from the thinness of the lunar air, or the absence of any air on the moon, he adopted the belief that rock surfaces at a great height above the sea-level do not get warm under the sun's rays, as science asserts. It was on a lofty peak of the Rocky Mountains, report says, that he maintained this argument. "*A priori* reasoning," he said, "may seem to show that these rocks around us must become hot under the sun's rays, but science should trust more in *à posteriori* evidence, the argument from observed facts,"—here he sat down, but for a singularly short time, on one of the rocks to which he had referred,—"I—I stand to it," he is reported to have continued, with some appearance of irritation, "despite of arguments *à priori* or — or otherwise, that the moon must be intensely cold when she is full; for my bolometer says so, and my bolometer is never mistaken."

Sir John Herschel had long since shown, by a process of simple reasoning, that at the time of lunar midday the moon's surface must become at least as hot as boiling water. The present Lord Rosse, using one of the fine telescopes which his father constructed (not, as has been mistakenly alleged, the great Parsonstown reflector *), arrived at a result corresponding to that which any one acquainted with the laws of physics could have anticipated. He ingeniously separated the heat which the moon reflects from the heat which she radiates—that is, which she gives out as a warm body will. He found that the surface of the moon at lunar midday is 500 degrees hotter than the same surface at lunar midnight. (I mean degrees Fahrenheit, of course; for the general reader, however intimate he may be with the rules for converting Centigrade or Réaumur, prefers to have no occasion to apply them.) Dividing these equally—as is only fair—on either side of nothing, we have a range from 250 degrees below nought, or 282 degrees below freezing, to 250 degrees above nought, or 38 degrees above boiling! We may get less cold, by dividing unequally; but then we get so much the more heat, and that would be quite unnecessary; or we may get less heat, but then we get so much the more cold, and 250 degrees below zero would be cold enough in all conscience.

* "Along whose tube a tall man may walk without stooping," it is the custom to add. Doubtless, for a tunnelling along which tall men may walk conveniently, the great Rosse telescope is the best in existence. But, regarded simply as an "instrument for observing the heavenly bodies," which, perhaps, is more nearly what it was meant for, the "mighty mirror of Parsonstown" is not so satisfactory. If Sir William Herschel's great 40-feet telescope "bunched a star into a cocked hat," Lord Rosse's still larger instrument played worse pranks still with the planets. "Zey show me somedings," said a well-known German astronomer, pathetically describing his experience at Parsonstown, "zey show me *somedings*, and zey say, 'He is *Saturn': and I believe zem*."

The stoutest among us would be killed by ten seconds of such cold, as surely as he would be killed by one second in boiling water. The moon, which passes through the whole range of this change in a fortnight, is assuredly not a desirable place for creatures so unfortunately sensitive as we are to changes of temperature.

All this, however, is not new but old. Till Mr. Langley came and set many doubting with his dreadful bolometer, no one imagined that there could be life on a planet undergoing the vicissitudes of temperature which Sir John Herschel had correctly indicated, and Lord Rosse had demonstrated. That the moon, whatever her past history may have been, must now pass monthly through amazing vicissitudes of heat and cold, is certain, let Mr. Langley's bolometer say what it may. It is with the moon's past history we are concerned at present, not with these effects of the sun's action on the moon's dead body.

For dead the moon assuredly is, *now*. It is as clear that there are none of the characteristics essential for life—as water, air, and reasonable ranges of temperature—on the moon at present, as it is that her surface has in the past been the scene of tremendous disturbance. That dead body of hers, carefully examined, with due regard to the evidence which our earth also can give about planetary existence, may tell us as much as a post-mortem examination might tell the keenly observant anatomist of the past life of a human being. What may have been the precise features of the various eras of lunar life we may no more be able to tell than the anatomist can tell what thoughts passed through the brain which he dissects with his scalpel. But the broad outline of human life we may trace as surely as that anatomist can follow the stages by which the body he dissects had reached its final condition before death invaded life's sanctuary.

It is here that, as it seems to me, new thoughts are suggested by knowledge recently acquired.

The argument from analogy has, I think, been somewhat too narrowly applied to the moon and other planets, when they have been compared with our earth. In assuming that each planet has its youth, its mid-life, its old age, and finally its death, astronomers have doubtless been right enough ; but I think it is by no means so clear that they have been right in assuming (tacitly, perhaps, but still confidently) that the various stages in the lifetime of one planet resemble the corresponding stages in the lifetime of another. A dog has stages of life corresponding to those of a man ; but a puppy is not a baby or even like one, a young whelp is unlike a lad, a dog is not a human being, and even a dead dog presents no very marked features of resemblance to a defunct man.

I propose to consider here some points in which, most probably, the moon's life-history must have been entirely unlike the life-history of our earth. The considerations I shall urge may be applied, it will be found, to other planets, as well to those which are larger than the earth as to those which, like the moon, are very much smaller.

I begin with some of the simple considerations involved, such as those relating to size, surface, substance, and so on.

Every one knows that the earth contains eighty-one times as much matter as the moon. I might dwell on the consideration that in gathering together her larger mass the earth must have become very much warmer than the moon ever was, even when the moon was at her youngest and hottest. For the celestial bodies owe their heat to their own energies in gathering their mass together ; and the greater the gathering energy the greater the developed heat, as certainly as the stronger a blacksmith's arm the greater the effect of his blows.

It would seem even that we have evidence of a still greater deficiency of original heat in the circumstance that the moon not only had less energy with which to gather together her substance, but that, having gathered it together, she has packed it less closely than the earth. If the moon were as compact as the earth she should have only an eighty-first part of the earth's volume. As a matter of fact she has fully a forty-ninth part. The earth put in a suitable balance (I cannot indicate any suitable place for setting it) would be found to weigh about five and a half times as much as a globe of water of the same size. But weighing the moon, three and a half globes of water as large as the moon would bring down the scale on their side.

Starting thus in her career of life with much less heat than the earth, the moon would cool also much more quickly. I do not mean by this that she would give out more heat, moment by moment, than the earth did at the same stage of her life ; but that she would be cooling faster in the same sense that a cupful of hot water cools faster than a bowlful, and a spoonful faster than a cupful. This is easily seen if we compare the moon when red-hot with the earth also red-hot, neglecting the effect of the air of either body in keeping in the heat, as clothing keeps in the heat of the human body. We may be sure that the consideration thus neglected does not affect the general result ; for certainly the moon was not better clothed (atmospherically) than the earth, at any corresponding stages of their lives—but the reverse. Our red-hot earth, then, had eighty-one times as much heat at that red-hot time than the moon at that (other) time when *she* was red-hot. And the earth was giving out thirteen and a half times as much heat, moment by moment, as the moon ; for in that degree her surface exceeds the moon's. Now, if a man has £81,000, while another has but £1,000, and expends daily £13 10s., while his poorer friend can only afford to expend £1 daily, the property of the former will last six thousand days, while that of the latter will in one thousand be completely exhausted. The richer man's money would last six times as long as that of the poorer man. The earth's heat would, in the same way, last six times as long as the moon's, at each stage of the cooling process.

In this way the moon would manifestly age very fast as compared with the earth. If we imagine the moon and the earth at the same stage of planet life six millions of years ago, then in a million years from that time, or five millions of years ago, the moon was where the earth is now. What will five millions of years do for us, or rather for our home? But even that way of putting it is not quite strong enough. Those five millions of years in the moon's history would correspond to six times as long—or to thirty millions of years—in the history of the earth! Our globe will show marked signs of advanced age by the end of that time, I venture to predict, in calm assurance that, at any rate, I shall not be contradicted by the evidence of observed facts. She would then be as far advanced in planetary life as the moon.

Like the well-known calculation about wine, made under (pretended) vinous influence for *All the Year Round* several years ago, this calculation may be modified, yet the result come out unchanged. If I remember rightly, that calculation began: "Let us suppose there are eighty casks, or it may be eight hundred, or, perhaps, eight thousand," and so on. We might have begun our calculation by saying, Imagine the moon and the earth at the same age six millions, or it may be sixty millions, or perhaps six hundred millions of years ago. It really does not matter. Take the longest period. Six hundred millions of years ago the earth and moon were in the same stage of planetary life. Then we find that five hundred millions of years ago the moon had reached the stage now reached by the earth, and three thousand millions of years hence the earth will have reached the same condition as the moon. Is not this, the reader may ask, a very different result from the former? On the contrary, it is precisely the same result! For, by our present assumption, the rate at which either planet ages is only one-hundredth of the rate we had before assumed. Hence the three thousand millions of years in our result indicate an amount of aging equivalent only to that resulting in thirty millions of years according to our former assumption.

Since, then, we are quite certain of this, that the time when earth and moon were equally advanced in planetary life must be set millions of years ago, we are at least certain also of this, that our earth will not be so old as the moon now is, she will not be so wretchedly decrepit (if not so utterly dead) a world until many millions of years have passed.

So far as this reasoning is concerned, the moon might have passed through a life much like that of our earth. She might well have had a life-bearing period akin to that through which the earth is passing at present. True, the various stages of her life would be very much shorter, and we can very well believe that, therefore, the various forms of animal life which have been developed on the earth would not have had the same chance of being properly developed on the moon. Or, considering the progress of a single race—our own—we can very well imagine that a being like man on the moon would not have sufficient time to pass through all the stages by which man has passed from the arboreal, hairy, pointed-eared, and four-legged ancestry now assigned him, to the civilized man of to-day, inventing every year more perfect instruments for destroying his fellows. The Lunarian, thus understood, may have been no better fighter than the man of the caves, or even than the more advanced fighters among the anthropoid apes, our cousins. In other words, he may have been a perfectly contemptible creature, instead of a being of imperially murderous instincts.

But now a consideration comes in which suggests the idea that at no time could the forms of animal and vegetable life on the moon have resembled those on the earth. We must apportion to the moon no more than her fair allowance of water, no more than her fair allowance of air. And when we have done this we find strong reason for thinking that, though that allowance of water and of air may have done very well for the Lunarians, it would not have done at all for us.

Let us begin with the water. The moon would have had one eighty-first part of the quantity of water which formed our earth's share. So far, good. That seems altogether fair. But observe. Our earth, with eighty-one times as much water, had a surface only thirteen and a half times as large over which to distribute that water in seas. It needs not even the ghost of Cocker to show that the earth had six times as much water per square mile. That of itself must have sufficed to make a very marked difference between the moon's condition *then* and our earth's condition now. Nor does it seem at all likely that at any stage, either earlier or later, the moon would have had a better chance of doing well in the universe than she had then—that is, at the time when she had reached the same stage of cooling which the earth has reached now.

But the want, or the short allowance, of water was as nothing compared with the thin air the Lunarians, if there ever were any, had to breathe.

Of course, as regards the quantity of air, the reasoning is the same precisely as for water. Only one-sixth part of the quantity of air which we have on this earth per square mile was (on the average) above each square mile of the moon. On the earth this would be a most serious matter. For the density of air depends on the weight of the total quantity above the surface; so that the density of the air would be reduced to one-sixth part if the quantity of air above each square mile were no greater here than it probably was on the moon, in the corresponding part of her planetary life. Now in the highest ascent above the sea-level which men have yet made —the celebrated balloon ascent by Messrs. Coxwell and Glaisher—the height attained was within a very few feet of the height of Mount Everest, the highest known peak of the Himalayas. At that height the air was reduced to little more than one-fourth its density at the sea-level. Mr. Glaisher fainted, and, if he had been alone, it would have been all up with him, even though the balloon might eventually have come down. Mr. Coxwell remained conscious, however. Nay, with creditable zeal for science, he even urged the fainting meteorologist to "make one other little observation, now—*do*," to which, however, Mr. Glaisher only responded by fainting dead away. Mr. Coxwell began, he tells us, to feel rather blue. Looking at his hands, he perceived that they were quite blue. They were also powerless, which was an even more serious matter; for it was necessary that the valve-strings should be pulled, if the conscious and the unconscious aeronauts were to be saved. Mr. Coxwell was equal to the occasion. Seizing hold of the valve-string with his teeth, he drew it—feebly indeed, but still so that it worked—and the balloon began to descend. He had saved himself and his companion, but only as by the skin of his teeth. Certainly this proved that no man could live, even for a few minutes, in air of only one-sixth of the density of the air at our sea-level—for that would be but two-thirds as dense as the air whose rarity so nearly killed our aeronauts.

Even this, however, would be as nothing compared with the tenuity of the lunar air, if we are right in supposing that, at the corresponding stage of her planetary life, the moon had the same allowance of air as compared with her mass that our earth has now. For, the smaller quantity of air would be drawn down with smaller force, gravity at the moon's surface being only one-sixth part of gravity at the surface of our earth. Instead of the lunar air having one-sixth, it would only have one thirty-sixth, of the density of our air at the sea-level. Air so thin would not only be unbreathable by creatures like ourselves; it would not support any kind of life known to us on earth, except such life as there is (if life it can be called) in rotifers and other such creatures, which can not only live with the smallest possible allowance of air, but resist with apparently unimpaired cheerfulness the action of a roasting heat and a much more than freezing cold, and can neither be boiled nor baked, nor drowned nor desiccated, to death.

Consider, again, some of the unpleasant results of such extreme rarity or tenuity of the air. It may seem rather a convenience than otherwise that the mercurial barometer would be only five-sixths of an inch in height. But the water barometer, instead of being, as with us, about thirty-three feet in height, would have a height of less than one foot. Now this in itself would not signify, but it would mean (and this *would* signify) that one foot would be the extreme height to which a suction pump would raise water. What a nuisance that would be! especially, too, where, as we have seen, water would not be very plentiful, and wells would have to be dug deeper than on the earth to reach it. Then drinking would be much more difficult—which might, however, be as well, where water would be so hard to get at—for in drinking we exhaust the air on one side of the water in our drinking-vessel (the air inside the mouth), and the air on the other side (outside the mouth) obligingly presses the water into the mouth, where we want it to go. But the air outside would not do this with sufficient energy if its density were reduced to one-sixteenth ; so that, in order to drink, one would have to tip the drinking-vessel up till the water ran out into the mouth, which would be, to say the least, an inelegant and unseemly way of drinking.

In passing we may notice that, were it not for the unpleasant deficiencies here mentioned, creatures much larger than any on the earth might exist on the moon. A Brobdingnag on the earth would be by no means the terrible monster imagined by the inventive Captain Gulliver. From what is now known about the relation between the strength and the size of muscles, a Brobdingnag ten times as tall (and also ten times as broad and as thick) as a man, would be one hundred times as strong ; but he would be one thousand times as heavy. Thus he would be ten times as heavy as he ought to be, and would be just about as active as a man among ourselves who, weighing 140 lbs. (10 stone), had his weight increased by overloading to 1,400 lbs. The extra 1,260 lbs., or rather more than half a ton, would certainly not conduce to activity, insomuch that, when Gulliver first saw a Brobdingnag, he might have been sure that the creature's show of giant size must be hollow, or else its weight so great that movement would be impossible. Now on the moon such a Brobdingnag would weigh only $233\frac{1}{3}$ lbs., and, so far as power of movement is concerned, would be like a man of 140 lbs. weighted down with only $94\frac{2}{3}$ lbs. Even this would be an awkward extra weight. But a lunar man six times as tall as one of ourselves would be all right, for he would be thirty-six times as strong, and also thirty-six times as heavy, so that he would be just as active as one of us. He would be no such contemptible giant as Jack the killer of giants dealt with so easily. He would stride six yards as easily as a man on earth strides one yard; he could leap over a height of twenty-four feet as easily as an active youth leaps over a four-feet stile. The work he could do as a lunar engineer or road-maker would be something stupendous. With as much ease as a man on earth can raise a block of stone six inches in length, height and breadth, our lunar man could raise a cubical block one yard in the side, for such a block which on the earth would be 216 times as heavy, would on the moon be but thirty-six times as heavy, and the lunar man would be thirty-six times as strong. A lunar Great Pyramid, representing the same amount of work as the Great Pyramid of Egypt, would be 1,500 yards in the side and 970 yards high. It would remain in the dry, thin air of the moon for as many hundreds of thousands of years as our Great Pyramid has lasted thousands; and as it would be quite easily discernible with our telescopes, even with those of moderate power, there might, after all, be nothing very stupendously absurd in old Gruithuysen's idea that some of the features on the moon were the work of former lunar inhabitants. But unfortunately these large lunar men would have wanted plenty of air and plenty of water, especially when at work on great lunar edifices; and if our estimate of the moon's past condition is sound, there would have been but very little water for them, and still less air.

Perhaps all this may seem very little worth considering. Of such speculations there is no end, said Sir John Herschel—after indulging in them to his heart's content. And certainly it seems somewhat idle to discuss the ways of lunar men, when we have every reason to think that, in a world whose various stages of life lasted so short a time as the moon's, no such creature as man could possibly have been developed, even if there had been the requisite supply of air and water. Let us rather consider, therefore, whether what is actually seen on the moon may not find its explanation in the circumstances we have been examining.

That the stages of the moon's life would be very much shorter than those of our earth's life, follows, as we have seen, from the consideration of her smaller mass. But the stages of her life would not only be shorter than those of the earth, they would be different in character, because of the different amount of air and water, and also because the lunar atmosphere, before it became air such as we have (only rarer), must have been very different in quality from our air in its old unbreathable state.

It has been shown by geologists that the various salts found in the sea must have belonged to it from the beginning. The familiar explanation that they were washed into the sea by rivers is no explanation at all; as a matter of fact the substances thus washed down by rivers came to be present in the solid crust by the drying up of former seas. We can form from the constitution of sea-water some idea of the horrible kind of atmosphere which our earth originally possessed. So also can we from many of the substances which we find in the earth's crust. There were sulphurous acid, and sulphuretted hydrogen (savouring of rotten eggs, though it could not have suggested their presence in days before as yet any chickens had appeared), carbonic acid, hydrochloric acid, and other highly disagreeable vapours. We have only to imagine what would happen if our earth were warmed up again, to see what must have been her state before she cooled. Nay, as she is kind enough to warm herself up locally at times, in sufficient degree to emit the very vapours which must of yore have been permanently outside her crust, we can tell by actual observation what they were. As the temperature beneath the earth's crust rises, the following gases and vapours are successively poured forth: carbonic acid gas (which chemists now call carbon dioxide), sulphurous acid, sulphuretted hydrogen, boracic acid and hydrochloric acid. How pleasant an abode our earth would have been in her youth (independently of her high temperature) for breathing animals, may be inferred from the state of things formerly existing in the Avernian Lake, across which no bird could fly with life. The showers falling from the hot air of those days would be by no means showers of pure water. Boracic acid in the liquid state may not sound very terrible, but boracic acid has played the very—— —a very important part, we would say—in modifying rock substances in volcanic districts, and when it fell in showers must have greatly altered the character of the earth's hot crust. Sulphuric acid might be as innocent as rose-water for anything that its name may perhaps imply to many; but when we speak of fiery hot vitriol, every one begins to recognize a substance that would probably have produced somewhat more marked changes on the hot crust of the earth than a drizzle of ordinary rain on the fields and plains of the earth produces to-day. Ammonia and various compounds of carbon, nitrogen and hydrogen, must have been present in the old atmosphere of the earth, and in various degrees of dilution with water must have been very effective as denuding agents.

Now it is easily seen that the various stages of the earth's vulcanian history must not only have been very different from those of the moon's, but that the records left in the crust must have been very differently treated. For example, the formation and the active existence of great volcanic craters on the earth probably preceded the formation of great mountain ranges. (I am not here referring to the various steps in the formation of a mountain range, but to the era of mountain forming as distinguished from the era of great crater formation.) On the moon there seems to have been no great era of range forming, and such mountain ranges as were formed probably began and ended their careers while the great craters were still active. But not only do we thus find a very different relation existing between mountain ranges and great craters on the moon and on the earth respectively, but we find that the records of the two forms of crust disturbance are in a very different state. On the earth all the original great craters have been so worn down and denuded that nothing but their basal wrecks remain; on the moon the great craters show their vast dimensions as originally formed, or where we see signs of important changes, the changes are those produced by subterranean not by subaerial action.

All this is readily explained as soon as we note the natural results of—1st, the relative shortness of the stages of the moon's life; 2ndly, the smaller relative quantity of water; and 3rdly, the smaller relative quantity of air.

The earlier forms of volcanic disturbance on the moon would resemble closely enough, in all probability, those which took place in the corresponding parts of the earth's history. Doubtless the earth had large craters like those which still remain in the moon. But then in the earth's case there has been a much more thorough wearing down of those upraised regions. There has been more water to do the work by sea action, by river action, and by glacial action; there has been more air to help in the work by the action of rain, wind, snow, and storm; and there has been much more time for all these processes to take effect. Similar considerations apply, indeed, to the earlier stages when our atmosphere was denser, more complex, and more destructive alike by its own action and by the action of the various fluids formed by condensation out of the unpleasant vapours present in it. Now even a planet cannot at once eat its cake and have its cake. If our earth has had water enough, air enough, and above all time enough, to wear down in great degree the records of the first stages of its vulcanian career, the immense outbursts of molten matter, the vast craters, the mighty wrinkles and corrugations, then naturally these records can no longer exist in their original dimensions on the earth. But it is equally clear that the corresponding records will remain on the moon, where there has been much less water, a far rarer air, and a much shorter time during which denuding forces of air and water could work.

We can at once see a reason then why on the moon the immense craters which are much earlier vulcanian products than mountain ranges, remain still extant. Many of the larger and older ones show signs of denudation, which we may probably regard as having been brought about when the moon's atmosphere was still in its early state, loaded with active acids, still intensely hot, and capable of producing marked effects on the intensely heated crust. But far the greater number of the lunar craters are manifestly in the state in which they were first formed. The denuding forces on the moon died out long before there had been time to wear any but the earliest craters down, or wholly to wear down any.

On the contrary, there are naturally few mountain ranges of great size on the moon. The mountain ranges are products formed out of the materials of previous formations; and if the great craters remain, or have been very little reduced by denuding action, it follows that there has been very little material available for mountain-making, and few mountain ranges therefore have been formed.

Still, the great range of mountains called the Lunar Apennines shows that we must consider mountain-making on the moon in some degree, unimportant though this part of the moon's vulcanian history may be, compared with the corresponding part of the earth's. We may expect to find that the great craters have in some degree been worn away to provide materials for that lofty range of mountains. And if we assume, as we fairly enough may, that the formation of the Lunar Apennines corresponded with the formation of terrestrial mountain ranges, then we may reasonably look for the traces of just such processes as geologists have recognized in the development of the Alps, Rocky Mountains, and Himalayas on the earth.

It will be found that the search for such evidence leads to some very curious results, not only justifying the belief that the Lunar Apennines were formed like the terrestrial mountain ranges, but enabling us to interpret features of the moon's globe which hitherto had remained unexplained.

In the first place we find the Lunar Apennines between two immense plains, the so-called Sea of Serenity on the east (or what would be the east to a lunarian), the so-called Sea of Showers on the west, each having an area of many hundreds of square miles. These great plains are manifestly great tracts on which finely divided matter has been deposited, as on a desert region like Sahara, or—more probably—as on the floor of a great sea. Regarding them as originally oceans, we see that the mountain range occupies such a position as would correspond with the formation of mountain ranges, as this process has taken place on the earth. We may suppose then that the matter out of which the Lunar Apennines were formed was deposited in a great troughlike depression, formed along a region where the crust had yielded in a far earlier stage, and whence molten matter had been extruded. Judging from the height of the Lunar Apennines, we see that the depth of sedimentary matter deposited in that depression, as it slowly sank below the level of an ancient Lunar Sea (originally extending over the whole space occupied by the Seas of Serenity and Showers), must have been several miles. This range then assuredly speaks to us of many hundreds of thousands of years, during which the waters of a vast sea beat on the shore lines of ancient lunar continents, receiving from lunar rivers the matter worn from those continents by lunar winds, and washed down by lunar rivers. (Of one of these rivers the immense mouth can still be recognized not far from the great crater Plato on the east of that ringed lake.*)

The steady sinking of the sea floor all round the immense seam thus formed, gradually pressed the matter deposited in a troughlike depression into the form of a ridgelike elevation. (The reader anxious for technical expressions—which have the advantage certainly of giving the appearance of much learning at a very cheap rate—may speak here, if he likes, of subclinal and anticlinal; but the actual shapes are perhaps as well indicated by speaking of troughs and ridges.) We may well believe that intense heat was developed in the process, and much of the sedimentary matter metamorphosed into crystalline rock. That the original summits of the elevated ridge were domed or rounded seems altogether probable. That the summits are now no longer rounded is clear from the shapes of the shadows cast

* We use the words "east" and "west" the reverse way from that employed in lunar maps. Picturing the moon as a planet, and comparing the processes taking place on her surface with those taking place on the earth, it seems reasonable to speak of east and west on the moon as we speak of them on the earth, not (as our maps of the moon do) as we speak of them onthe skies—for example, in speaking of a constellation.

on the floor of the surrounding plane when the sun is rising in the lunar east. Here, then, is clear evidence of denuding action continuing long after the ridge had been, as it were, shouldered out of the seas by the side pressure of the sinking sea floors on either side.

On this occasion, then, or rather during this special stage of the moon's history, the denuding processes went on long enough, and with sufficient energy, to do such work as took place many times over during the past history of our earth. "The hills are shadows," our poet sings, "and they flow from form to form and nothing stands; like mists they melt, the solid lands, like clouds they shape themselves and go." On the moon a poet would hardly have used such a comparison, even if any poet on the moon ever came to know so much of the past of his world as we have just indicated. For unquestionably the lands on the moon did not melt away like mists, but once only gave enough of their substance up to form one set of mountain ranges. The lunar poet could not echo the terrene poet's exclamation, "Oh earth, what changes hast thou seen!" once, and once only, he would have to admit, "hast thou, oh moon, seen change." Yet for the rest he might have sung (millions of years ago) with the Tennyson of our later world, "There rolls the deep where grew the tree; there where the loud street roars" (if ever they had loud streets in lunar cities), "hath been the stillness of the central sea."

But even of one such change the traces should remain. If the floors of the great seas around the Lunar Apennines have been covered with matter washed from the lunar continents, we may look for the signs on those floors, now that the great deep has retired from them (to deeper depths), for traces of the continents over which the waves of those seas once rolled. Such signs, though they have been little noticed, are clearly to be seen. There are the ghosts of great craters—craters which once stood out above the continental tracts to which they belonged, but have now been silted over until, like the forms of objects buried beneath a deep and level snowdrift, they show only through some slight difference in the texture of the sediment lying over the raised parts of the hidden surface. These buried craters were as large as any still outstanding. They tell us of stages of the moon's vulcanian life earlier than those of which the immense extant craters speak. And even these, the mighty Tycho, 54 miles across and 12,000 feet deep, Copernicus, 58 miles across, Kepler, Archimedes, Plato—these speak of a stage of vulcanian activity such as once existed on the earth, but of whose progress the material records were destroyed millions of years ago.

CHAPTER XV.

A ZONE OF WORLDS.

IN the latest edition of his collected essays, the many-sided Herbert Spencer considers the problem of that strange zone of small planets which travels between the paths of Jupiter and Mars. With his customary acumen, he considers the relations presented by the ring of small planets, their distribution as regards size, inclination, eccentricity, and so forth; and in the facts thus collected and analyzed he finds evidence that there was once a single planet travelling along the middle of the region now occupied by the zone of asteroids, and that this planet at some remote epoch burst into thousands of minute fragments. Of course, the theory is not advanced as a new one, but the evidence in its favour has never before been so fully presented.

I believe our great philosopher to have arrived at an entirely erroneous conclusion, ably and clearly though he has dealt with the evidence. I shall endeavour to show that while all the evidence offered by Mr. Herbert Spencer is consistent with another interpretation, there is some evidence not touched upon by him which will bear no other explanation than that which I shall indicate. Leaving, however, for the moment both the evidence he has adduced and the theory which I take to be the only one available, I wish at the outset to consider the complex nature of the solar system and the position apparently occupied in it by the asteroidal zone.

Until the Copernican theory was established, astronomers not only did not—they could not—form any idea whatever of the relative sizes of the different planets; for they had no notion of the relative distances of these bodies. Thus men's views as to the nature and character of the several planets were formed in the course of ages, during which it was not known whether Jupiter or Saturn were the larger, or whether Mercury or Venus or Mars might not be very much larger than those planets which we now know to be the giants of the solar system. This was unfortunate; because, when the Copernican theory was established, and when, later, the telescope showed the globe forms of all the planets and the systems of subordinate bodies which attend upon them, it did not seem to occur to any astronomers to indicate how completely these discoveries modified the aspect of the whole system. Later, other discoveries were made, including the recognition of the zone of asteroids and the discovery of Uranus and Neptune, which should still further have modified the views of astronomers. But settled as men's ideas were in a particular groove, no marked effect was produced; and beyond the suggestion that probably the zone of asteroids marked the place where a single planet had once travelled, astronomers were led to no new thoughts about the planetary system. The planet which was supposed to have burst illustrated, indeed, their unwillingness to change their views; for, by imagining such a planet, they were able to conceive the original condition of the solar system as even more uniform than it had appeared to be, either when as yet none of the asteroids had been discovered, or when the asteroids were not explained so conveniently.

Yet every one of the discoveries made by astronomers since the time of Copernicus, including the discovery of the truth of the Copernican theory itself, has given evidence of great variety and complexity of structure within the solar system. The theory of Copernicus, and the light which it threw on the dimensions of the solar system, proved that Jupiter and Saturn are bodies so much larger than the earth, Venus, Mars, and Mercury, that they must be set in a different class. The invention of the telescope, and the discoveries made by its means, proved that Jupiter and Saturn are the centres of systems resembling, though on a smaller scale, the solar system itself. The calculations by which the masses of the planets were determined proved that in mass, as well as in volume, enormous differences exist. The successive discoveries of the ring-system of Saturn, of the zone of asteroids, of the small moons of Mars, of the systems of meteors, of planetary comets, and others, pointed unmistakaby to a variety of structure within the solar system such as the earlier astronomers had never imagined. Yet no new survey of the solar system, no new effort to classify its members and to determine if possible their real nature and significance, was ever systematically attempted, or if attempted was held to belong rather to the region of speculation than of observation—as though anything could possibly have been more wildly speculative than the belief that the planets are such as they were imagined to be by the astronomers of Ptolemy's school.

Let us try to take such a view of the solar system as probably astronomers would have taken if their first ideas had been formed when they were in possession of the facts which have come to our own knowledge.

In the first place, suppose we could look at the solar system as it might be seen at some given moment, were our powers of vision as keen as those which are given to us by the most powerful telescopes yet made by man. We should see a great, glowing orb, containing more than 720 times as much matter as all the rest of the members of the solar system taken together. Around it travel four bodies relatively very small, at distances which may be represented by the numbers 4, 7, 10, and 16. The largest of these, at distance 10, has a companion body fairly comparable in size and mass with the rest of the group. Around this part of the system travel thousands, or more probably millions, of bodies in the form of a great ring whose entire breadth is greater than the diameter of the earth's orbit. The entire mass of all the asteroids together is not one-tenth the earth's mass. Then, at a distance more than three times as great as that of Mars, we come suddenly on a body entirely unlike any of those yet mentioned. A globe is found more than 300 times as massive as the earth, and not far from 200 times as massive as all the planets yet mentioned put together. It is girt round by a system of worlds, comparable rather with Mars and Mercury than with bodies of inferior class. The least of them has a surface equal to North and South America taken together, and is fit, therefore, to be the abode of many millions, nay, rather millions of millions, of living creatures. Oddly enough, the system of Jupiter and his satellites so closely resembles, though of course on a much smaller scale, the system of four bodies—Mercury, Venus, Earth, and Mars, around the sun as a centre, that a picture of one serves well as the picture of the other.* We next come, at a distance nearly twice as great from the

* Once when an assistant had unluckily broken a lantern slide showing the orbits of Mercury, Venus, Earth, and Mars about the central sun, I, for the occasion, substituted a slide showing the orbits of Jupiter's four moons about their central planet. No one detected the change.

sun, to a body less, indeed, than Jupiter, but belonging to the same class—the ringed giant Saturn, a hundred times as massive as the earth, and circled round by eight worlds, the largest of which exceeds Mercury in size, besides a ring-system, akin in some respects to the ring of asteroids. Again we pass over nearly as great a distance as we had already reached, and come upon the orbit of Uranus, with his family of four worlds as yet discovered; and though Uranus is much less than Saturn, he is a giant compared with the earth. Lastly, after passing over half the distance at which Uranus travels, we come on the orbit of the most distant planet, Neptune, a giant planet (brother to Uranus, in size and mass), attended by but a single yet discovered moon.

We have so fallen into the habit of regarding this system as represented by a central body with a series of circles set round it, that we find it difficult to picture it as it would appear to one who, visiting our sun's neighbourhood from outer space, should view the system as it would appear at any instant of time. He would not recognize that relationship of all the bodies in the system to a central mass, which appears to us the most striking feature of the planetary family. Instead of that, he would see five leading bodies, each attended by a family of small worlds. These five would not include our earth. They would be, first, the sun, attended by five worlds (two of which would be seen to form a double planet), Mercury, Venus, Earth and Moon, and Mars; secondly, Jupiter attended by a family of four worlds; Saturn with eight attendant worlds and a ring-system; Uranus with his family; and Neptune with his (for we may well believe that the discovered moon of Neptune has fellows which yet remain to be detected).

In what way would such a visitant of the solar system regard the asteroidal family? He could find nothing analogous to them throughout the five families, except only in the Saturnian system. There he would see a ring, or set of rings, consisting of multitudes of tiny bodies, like sands on the seashore for number. There can be no doubt, of course, that the Saturnian ring-system belongs to Saturn, for it lies within the orbit of the innermost of his eight moons. But the ring-system of asteroids regarding not only the paths of these small bodies but the position of the asteroids at any given instant, as manifestly belongs to the sun ; for he lies at the centre of the ring. Thus, our imagined visitor from interstellar space would at once decide that the asteroidal zone belongs to the first of the five leading bodies of the solar system.

Observe that thus viewing the asteroidal ring we find its character altogether natural. It has the features we might expect, after what we have found in the ring of Saturn (there unmistakably), to exist in a ring-system appertaining to a leading (in this case, the chief) member of the solar system. Regarded as part of a large system, including the giant planets, attending on the sun as the one great centre, the asteroidal zone is altogether abnormal, and in a sense inexplicable. Supposing we could find a natural explanation of the smallness of the inner planets, Mercury, Venus, and the Earth-Moon pair ; supposing, further, that we could explain the way in which, after showing a gradual increase in size and mass, thus far, with increase of distance from the sun, the system of primary planets next presents a marked falling off in both respects ; and supposing, further, that passing from the outside of the planetary system inward toward the centre, we could fairly account for the increase from the smaller giants, Neptune and Uranus, to the great ringed giant Saturn, three times as massive as all the just-named planets together, and thence to the still mightier Jupiter, which surpasses in mass Saturn and the rest together two and a half times ; yet how are we going to explain the strange anomaly that just inside the track of the prince of all the planets we come to a zone of bodies so insignificant that even if they are regarded as the fragments of a single planet which has burst, that planet could not have had a tenth part of the mass of our earth, and would probably have had a smaller mass than even Mercury?

Thus, while the asteroidal ring, regarded either as a zone of small bodies originally so constituted or as formed by the bursting of what was once a single planet, appears as an entirely abnormal feature in the solar system, it accords quite naturally with other characteristics of that system when regarded as occupying the same position in the sun's special family that Saturn's rings occupy in the Saturnian system. When we consider the solar system as a product of evolution (that is, as developed by processes akin to growth), we find the asteroidal system still more difficult to explain, unless we separate it, along with the sun's family of small planets, from the rest of the system, and regard it as forming part of the sun's special domain. It is certain that, according to the nebular theory of Laplace, there ought to be a certain uniformity in the dimensions and masses of the planets, as well as in the arrangement and distances of their orbits. For the process suggested in that theory is a uniform one, acting from the outside of the system where Neptune was made toward the interior where Mercury, the innermost planet, was formed. Uniformity there must have been, though what the law or laws of such uniformity, may not be clear. Now, apart from their utterly abnormal position, as just noted, the asteroids show an utter absence of uniformity in all the details of their system, as well as in their orbital movements. As Mr. Herbert Spencer justly notes, in considering that particular theory of their origin which Laplace himself suggested, if a nebulous ring broke up into numerous small portions revolving around the sun with approximately equal velocities, "their mean distances from the sun could scarcely differ so much that some are twice others; it could hardly happen that the annular space included between their most unlike mean distances would be more than one hundred millions of miles across, and that the space occupied by their widest excursions would be two hundred and seventy millions of miles across"; the parts of such a ring could not show the wide range of orbital inclination seen among the planetoid orbits; there could not be such eccentric orbits as are found among the asteroidal paths, one of which actually passes within the orbit of Mars; and, lastly (though this is indeed not a separate objection, being involved in the consideration of the enormous breadth of the system), "there could not arise any considerable differences between the times in which the discrete portions of such a ring revolved around the sun to the extent of some being thrice others."

These are, however, in the main, objections to the theory of Laplace itself; for it will be found on careful consideration that every process by which a single planet could be formed out of the fragments of what was once a complete ring, would actually require the variety of inclination, eccentricity, and mean distance, which appear thus inconsistent (to the eyes of one of our keenest modern reasoners) with the hypothesis of Laplace. A series of fragments travelling nearly at the same mean distance, in the same plane, and with but slight eccentricity, around even such an orbit as the earth's, could not gather up into a single mass in tens of millions of years; much less could such bodies travelling around orbital regions as wide as the paths of Uranus or of Neptune.

Of course, to any such theory of the origin of a single planet, from which, by a mighty outburst, the whole system of asteroids was formed, as it were, at a single stroke, there are other and still more striking objections. Those who advocated the theory of a burst planet in the time of Laplace were not greatly troubled by such objections as our Danas and Sterry Hunts, our Scropes and Mallets, recognize in the present day. These, while they know how tremendous may be the local action of the forces which are generated by the contest between the internal heat and the external pressures and movements of the earth, know also that the earth is neither strong enough nor weak enough to be even shattered into fragments by an explosion, such as Olbers, in the confidence of half-knowledge, could calmly enough suggest. For such an explosion to take place, the earth must be, on the one hand, far stronger than she is, to resist the action of such forces as would have to accumulate in order to be able finally to disrupt the whole globe; but, on the other hand, the earth would have to be so much weaker than she is as to give way simultaneously at all parts of her crust in the manner conceived by Olbers. The fact really is, that the earth, in the combined strength and weakness of plasticity, is as safe against explosion as though no subterranean energies or external forces of disturbance were at work upon and within her frame. The material which seems to us hard and breakable is absolutely plastic at a depth of less than thirty miles below her surface. It would be so were its substance the best tempered steel. And in this plasticity lies its safety; though, to say truth, the earth possesses no bursting forces which approach even to one-thousandth part of the energy which would be necessary for the shattering of her crust. The chief disturbing force seems to arise from the water which finds its way beneath the surface. This, no doubt, exposed to the heat of the earth's interior, presently assumes the form of superheated steam, and when gathered in sufficient quantities, produces irregular upheavals, earth-shakings, volcanic eruptions, and so forth. But these are forces affecting in reality merely the outer film of the earth's crust. If they gathered in ten times greater amount than in the most terrible earthquakes yet known, the result, disastrous though it might be to the human race, would not appreciably affect the earth as a whole. And constituted as the earth's inner frame is,—of matter which, however solid, is not rigid, but absolutely plastic,—there is no possibility of the earth's seas finding their way in, in sufficient quantity to seriously affect the real crust (as distinguished from the outer film we can examine), far less the interior mass.

But the explosion theory of the small planets is open to an absolutely fatal objection, the full force of which only the mathematician, perhaps, can appreciate. If a planet could burst, every fragment would start in its independent course from the scene of explosion. Its new path, therefore, however it might differ in eccentricity, inclination, and period from the path of the single original planet, would be a path passing through that point in interplanetary space where the explosion took place. During thousands of years after the explosion, the orbits of all the fragments would exhibit this evidence of their common origin. Perturbations would, of course, in the long run, carry all or most of the paths away from that point ; but still it would be possible for the mathematician to recognize the fact that once they had had a common point of intersection. The only class of perturbations with which mathematics could not thoroughly deal would be those arising from their mutual attractions. But even on this point the mathematician has something to say. The masses of the individual members of the asteroidal family are quite unknown. The numbers of the family, even, and their orbital movements, are as yet undetermined. Yet the whole family have been put in the scales of science and weighed, with the result that the entire mass of the zone is less than the mass of the planet Mars. Now the disturbing effect of Mars on the earth, its nearest neighbour, is so very small that it produces scarcely any measurable influence, even in long periods of time. The zone of asteroids is, however, far wider than the interval which separates the paths of Mars and the earth. The mutûal influences of bodies spread over so wide a region, and having an average mass certainly less than the three-hundredth part of the mass of Mars, may certainly be neglected in discussing the question whether the orbits of the asteroids ever intersected in one small region, such as the original imagined single planet could have occupied. This done, it is found that these orbits could not have had, at any time, that common region of intersection.

The theory of explosion is therefore as untenable on *à posteriori* as on *à priori* grounds. But perhaps the best disproof of this theory is to be found in the clearness of the evidence which exists in favour of another theory, flowing naturally from the one theory of the origin of the solar system which gives an account of the peculiar way in which the various masses within the solar system are distributed.

It has become clear within the last half century that the interplanetary spaces are traversed by millions of millions of streams of meteoric matter, travelling in orbits of every variety of eccentricity, inclination, and direction (unlike the planets, which travel all in one direction around the sun). Our earth passes in her yearly circuit around the sun through the orbits of some four hundred of these meteoric streams; though only through each stream of the number when the stream happens to be passing at the time that the earth crosses its orbit. It has been calculated by Professor Newcomb that in each year about four hundred millions of meteors of all orders down to the smallest which ordinary telescopes would show, fall on the earth's atmosphere. But the estimate now formed of the number of bodies so encountered is very much higher. All these bodies are eventually received in the form of microscopic dust on the earth's surface. In this lies the proof that the total number of these streams actually existing must be counted by millions of millions, the total number of individual meteors being simply uncountable.

Now, when these enormous numbers of meteoric systems exist, and when it is seen that year by year the earth still gathers in a large mass of meteoric matter (however small the mass may be in comparison with her own), we see that in the remote past the meteoric and cometic tenantry of our surrounding space must have formed a very important part of the solar system. Considering the millions of years proved to have elapsed since the system had its present general aspect, and the tens of millions of years preceding that time, we cannot but admit that the quantity of meteoric and cometic matter which could stand even the present rate of drain during so long a time must have been enormous. But it is obvious that the rate at which the meteors were withdrawn from circulation in far-back times must have been much greater than at present, for the simple reason that it would be proportional always to the number of free meteors, and this number has been constantly diminishing; and before the planets had assumed their present comparatively compact forms, their sweeping capacities would necessarily have been far greater than at present; for their widely extending, partly vaporous masses would range through much greater regions of the meteor-crowded interplanetary spaces.

Taking all these considerations into account, it becomes clear that no small portion of the present mass of each planet must have been derived from the process of meteoric aggregation. So much is certain. The whole subject of the origin of our solar system is full of difficulties and perplexities; but some few points are quite clear, and among them is this one. But when we take this at least partial meteoric origin of the solar system into account, we find an explanation of those general features of the arrangement of masses throughout the system of which the theory of Laplace gives no account whatever, and amongst those features the asteroidal zone finds its explanation.

Consider the conditions under which the process of aggregation would have gone on in the beginning:

From what we know of the present condition of the meteoric systems, we learn that, from the outset, there must have been a steadily increasing wealth of meteoric distribution with approach toward the sun. So that, were this point alone to be considered, we should expect to find the largest planet travelling next to the sun, and the planets growing less and less with increasing distance from him. But the movement of meteoric bodies around the sun would be more rapid the nearer they drew to him on their eccentric paths; and, apart from eccentricity of orbit, meteoric velocities would be greater near the sun than far away. Thus, at our earth's distance, the average velocity of a meteoric body would be about nineteen miles per second, its maximum velocity about twenty-seven miles. Close by the surface of the sun, the least velocity on a free orbit (necessarily circular, to give the least velocity there) would be about 260 miles per second, and the greatest velocity about 375 miles. At the distance of Jupiter, the maximum velocity would be only about twelve miles per second; less at Saturn's distance; and much less at Neptune's.

Now clearly the growth of a mass by the gathering in of meteoric matter would depend in great degree on the velocity with which such matter came by it. If its outskirting vaporous matter were actually interposed on the meteor's track, manifestly the chance of intercepting the meteor mass would be small if the velocity of the body were very great. If there were no actual collision at this passage, the power of the aggregating mass to perturb a passing body, and so alter its path as to make its eventual absorption a mere question of time, would depend in great degree on the length of time during which the meteoric mass was in its neighbourhood, and this would be greater or less according as that mass moved with less or greater velocity.*

Thus, approaching toward the sun, we find the formation of a planet encouraged by increase of material, but rendered more difficult by increasing velocities. It is evident that there must be some distance at which the conditions would, on the whole, be more favourable than at any other distance, a region where there would be a sufficient quantity of meteoric material, while the motions would not be too rapid to prevent steady aggregation of matter. At that most favourable distance, the largest subordinate aggregation would form. In other words, this would be the region where the giant mass would form from which, millions of years thereafter, Jupiter with his system of attendant worlds was to be developed.

Now, it is clear that outside the domain where Jupiter was formed, we should find planets less than he, at distances corresponding to the distance sepa-

* As it sometimes happens that speculative considerations are advanced in the definite and confident tone which should belong only to statements of fact, I deem it well to point out that what I have stated above is simply matter of fact, not speculative at all.

rating Jupiter himself from the central mass. Saturn, twice as far as Jupiter from the sun, and much less than he in mass; Uranus, twice as far away as Saturn, and still less than he, would thus be accounted for. And if there were but one more, we can see reasons why that outermost planet, not being disturbed in its growth by a gathering mass yet farther from the sun, would neither travel quite so far from the centre as would correspond with the law thus far noticed, nor show any further diminution of mass beyond that shown in the case of Uranus. At least, here seems to reside a not unreasonable explanation of the circumstance that, in the case of Neptune alone, Bode's Law (of distances doubling outward from Mercury's orbit) is found to fail, while his mass is not less than that of Uranus, but slightly greater. We may also find, in the exceptional circumstances of the outermost region of the solar system, an explanation of the peculiarity that the satellite systems of Uranus and Neptune, only, show a movement of circulation around their primary in a direction contrary to that observed throughout the rest of the solar system,—not only in the movements of the planets around the sun, and of the satellites around their primaries, but also in the rotation of individual planets (including our moon) upon their axes.

Turning to the region inside that swept through by the mighty gathering mass of Jupiter, we find on the line which we are now following an explanation of what before had seemed mysterious, the singular arrangement of the masses of those smaller planets which seem to own the sun as their special ruler.

It is clear that just inside the track of Jupiter, matter would either aggregate under great difficulties or fail to aggregate at all. His mighty disturbing influence, conjoined with that exerted by the sun, would here produce irregularities of movement akin in a sense to those which affect a sea where two cross sets of waves are travelling; just as in such a sea we find no mighty moving masses of water, but only broken waves, so in the region where the influences of the sun and Jupiter were combined, there could form no great aggregation, but only small pieces of what under other conditions would have been a planet. Be it noticed that the comparison here advanced is not merely metaphorical, as it might seem; it is demonstrable, as a mere matter of fact, that clouds of cosmical dust sweeping around the great central sun, under his influence solely or chiefly, would gather eventually into a single mass, while similar clouds under his influence, combined with a sufficiently powerful thwart influence, such as Jupiter would unquestionably have exerted on the region inside his track, would break up into a flight of small bodies circling on independent orbits around the central sun.

But we shall return presently to this point, and show the very marks which have been left by Jupiter on the asteroidal zone. For the moment we must consider the other members of the sun's special family.

Inside the zone where a planet failed altogether to attain the fulness of independent planetary life, we might expect to find a region where, though a planet formed with difficulty and acquired but a small mass, it yet succeeded, despite the perturbing influences of Jupiter, in gathering up its substance and becoming a primary though small member of the solar family. Possibly in the conditions prevailing here we may find an explanation, also, of the circumstance that Mars alone, of all the primary planets, has two bodies attending on him: and these, though we may call them moons, are in reality far too small to be set in the same class as the moons of Jupiter and Saturn,* and the companion planet of our earth.

* The idea has been advanced that because the smallest of Saturn's moons—Hyperion—required a very powerful telescope

On the other hand, in the region nearest to the sun, the velocities of meteoric on-rush would be so great that a planet would form with great difficulty, or not at all. Most probably—if we can judge from observations made during eclipses—no planet such as the Vulcan of our books of astronomy exists in the sun's immediate proximity. But there are good reasons for believing that a number of tiny bodies—pieces of planets, so to speak (unless we prefer to call them, with Humboldt, pocket planets)—travel around him within the orbit of Mercury. These, in the region of greatest disturbance near the sun, would correspond with the zone of asteroids in the region of greatest disturbance near the outskirts of his special domain.

Be this as it may, we certainly find near the sun a planet—Mercury—even smaller than Mars, affording thus evidence that that region is even less favourable for planetary development than the region inside the asteroidal zone, where the planet Mars with his two small moons was fashioned.

and the keen eye of Prof. G. P. Bond to detect it, therefore it may be regarded as comparable in smallness with the moons of Mars. A slight consideration of the circumstances under which these bodies are visible will show how very much larger Hyperion must be. Saturn's distance from the sun is, roughly, about six times as great as the distance of Mars; and, therefore, his satellites are illuminated with but about one thirty-sixth part of the light which illuminates those of Mars. When Mars is so placed that his moons are discernible in our telescopes, his distance from the earth is about the twentieth of Saturn's, corresponding to a diminution of the apparent size of Hyperion's disc 400 times greater than the diminution of the discs of Mars's satellites. Thus Hyperion, to appear as bright as one of the Martian moons, must have a surface 14,400 times as great, corresponding to a diameter 120 times as great, and a volume 1,728,000 times as great—which assuredly sets him in a different class of created orbs. Here I have taken no account of what is assuredly the case—that Hyperion is certainly a much easier telescopic object than either of the moons of Mars.

Between the planets Mercury and Mars lies a region where the sun's disturbing forces on the one hand, and those of Jupiter on the other, are neither so effective separately as in the regions traversed by Mars and Mercury, nor conjoin their influences disastrously as in the region of the asteroids. Here, therefore, we should expect to find the largest members of the sun's special family; and, accordingly, here we find Venus and the earth, Venus on the solar side less than the earth on the remoter side, just as Mercury is less than Mars.

It is noteworthy, also, how the largest member of the sun's special family, thus formed where the conditions seem most favourable, is attended by a companion body, the only secondary planet, properly so called, in the whole region inside the orbit of the giant Jupiter.

But now I would call attention to the way in which the asteroidal zone presents itself as part of the sun's own domain, in the same way that the Saturnian rings appear as part of the system of that noble planet. Not only are the asteroids thus recognized as occupying a natural position in the solar system, instead of an abnormal and scarcely explicable one, but we shall find on comparing the zones of asteroids with the rings of Saturn, that there is a most remarkable and significant feature common to both, by which not only are they brought into parallelism of condition, but may both be shown to have clearly imprinted on them the marks of the influences under which they reached their present condition.

Observe, I have spoken of the *zones* of asteroids, using the plural here for the first time. The description is correct, though no one would imagine it who should either look down a list of the minor planets with their mean distances, eccentricities, inclinations, and so forth, or should look at a picture presenting their paths as they really are. It required the keen insight and patient labour of Professor Kirkwood (of Bloomington, Ind.) to sift out from what seems a meaningless confusion of orbits the evidence of zone formation, which, though thus veiled, is clear and convincing enough so soon as the veil is lifted.

The asteroids, when duly arranged in the order of their mean distances, are found to be separable into distinct zones. At certain mean distances there are no asteroids at all, at others scarcely any; while at mean distances between these vacant or comparatively vacant zones, asteroids abound. This would be a remarkable circumstance, even though no law could be detected in the arrangement and dimensions of these zones—a circumstance not less striking than the existence of dark bands and zones in the solar spectrum. But as in this latter case the interpretation of the dark bands was of far greater importance than their mere detection, so is the interpretation of the vacant zones in the asteroidal region a matter of even more significance than their recognition. They lie at those very distances from the sun at which the perturbing action of Jupiter would be greatest. We know from the theory of planetary motion that when the movements of one planet synchronize with those of another, much greater perturbations arise from the mutual attractions of the two bodies than where there is no such harmonious relation existing. Thus Jupiter and Saturn disturb each other so effectively, owing to the simple relation between their orbital movements—two circuits of Saturn synchronizing almost exactly with five of Jupiter, the result being an irregularity of motion which long before it was explained was called the great irregularity of Jupiter and Saturn. Saturn is most affected, being the smaller; and if he were very small, Jupiter would scarcely be affected at all, but the supposed small Saturn greatly. Now, among the particles of clouds of cosmical matter travelling where the zones of asteroids now lie, would be many having periods synchronizing with Jupiter; for instance, the region would include bodies travelling in one-half, two-fifths, three-quarters, two-thirds, three-sevenths, and four-sevenths of the period of Jupiter. These he would most effectively disturb, and would work out of their paths into regions traversed by bodies on either side not having periods synchronizing so dangerously (for them) with his. So would he sweep clear, as it were, those mean distances, and in the fulness of time (a few tens of millions of years) no cosmic dust would remain there. Hence would have arisen the state of things shown by Professor Kirkwood—the Kepler of modern astronomy—to exist in the asteroidal region. Nor is it easy to conceive any other way in which this remarkable feature of the family of asteroids can be explained.

But if this explanation is the correct one, we might fairly expect to find in the Saturnian ring-system something akin to it. Now, not only is this the case, not only are the tiny satellites which form these rings cleared away precisely where the disturbing action of the Saturnian satellites would be most effective, but in their case the clearing away, instead of requiring the careful inquiry of a Kirkwood to reveal it, has been recognized for more than two centuries as a marked feature of the Saturnian system. The great division commonly called Ball's division, but really discovered by the elder Cassini, is simply the chief cleared space. It is all but absolutely clear of satellite dust to a width of about a thousand miles—not quite clear, for some little light comes from what was called erroneously the black division. Elsewhere are other divisions, not so broad nor so dark, but, like the great division, corresponding to the distance from Saturn's centre where the perturbing action of the nearer satellites would be most effective. Here, too, though the divisions were discovered with the telescope, we owe to Professor Kirkwood their simple and very beautiful interpretation.

No doubt, then, can remain. The asteroidal zones, though they may present, as Dr. Whewell quaintly put the matter, a case in which a planet has been spoiled in the making, yet show clearly the marks of their origin. They belong as certainly to the sun's special family of small planets, as the rings of Saturn belong to the Saturnian system. They bear, like these rings, the marks of the action of the other orbs which travel round their ruling body. They not only tell us of their own origin, but of the processes by which planets not spoiled in the making have been fashioned. They speak, moreover, of æons of æons of past time, during which the mighty action of Jupiter (already formed, be it noticed, and therefore telling us also of enormous antecedent periods of time) was at work upon them, clearing out the zones where asteroidal movements harmonized too closely with his own. Here, therefore, we may say that in this neglected wilderness of small worlds, we

> Have come on that which is, and caught
> The deep pulsations of the world,—
> Æonian music measuring out
> The steps of time."

CHAPTER XVI.

SATURN AND ITS SYSTEM.

IT has always seemed to me that the planet Saturn is the most interesting of all the orbs in the heavens. Independently even of his ring-system, which gives him so singularly beautiful an aspect in the telescope, he holds a remarkable position as the centre round which circle as many dependent orbs as those which constitute the primary members of the sun's family. There is something startling in the thought that in those remote depths, ten times farther away from the great centre of the system than we ourselves are placed, a system at once so beautiful and so elaborate should be pursuing its wide orbit. A universe is there, reduced by vastness of distance to a mere speck of dull light—a "miracle of design" which has existed for ages, during which none on this earth recognized aught that distinguished the planet from his fellows, save characteristics of inferiority.

I propose to give a brief sketch of some of the most interesting facts which have been ascertained respecting this wonderful planet. I may remark, in passing, that although I have on several occasions discussed matters connected with the subject of the planet's condition, this is the only occasion on which I have described Saturn and its system in a general way since the time when I wrote the work bearing that name.

It is not wonderful, when we consider the dull aspect and slow motions of Saturn, that the ancients should have associated with this planet ideas of gloom and of malign influences. The alchemist assigned the metal lead, heavy and poisonous, to the most distant known planet. The astrologer regarded Saturn as the most fatal of all the planets. Chaucer thus presents, in the address of Saturn to Venus, the characteristics of Saturnian influences: *

> "My dere doughter Venus, quod Saturne,
> My cours that hath so wide for to turne,
> Hath more power than wot any man.
> Min is the drenching in the see so wan,
> Min is the prison in the derke cote,
> Min is the strangel and hanging by the throte,
> The murmure and the cherles rebelling,
> The groyning, and the prive empoysoning.
> I do vengeaunce and pleine correction,
> While I dwell in the sign of the leon.
> Min is the ruine of the high halles,
> The falling of the toures and of the walles
> Upon the minour or the carpenter:
> I slew Sampson in shaking the piler.
> Min ben also the maladies colde,
> The derk tresons, and the castes olde:
> My loking is the fader of pestilence."

Travelling at a distance from the sun varying from 823 millions to 921 millions of miles, or from nine to ten times the earth's, Saturn accomplishes a complete revolution around the sun in rather less than thirty years. As the earth goes once round the sun while Saturn is traversing but about the thirtieth part of his orbit, it follows that year after year he is seen to advance but by a small distance along his track on the heavens. The earth comes between Saturn and the sun, and then Saturn is visible at night, and therefore favourably; then the earth makes a complete circuit, and has to advance but a little way farther before she is again between Saturn and the

* The word Saturnine sufficiently indicates the character ascribed to the planet's influence on the fates of men and nations.

sun. In fact, year after year, the return of Saturn to the midnight sky occurs about twelve days later, so that if in one year he is well seen in the summer, he will be well seen the summer after, and so on for several successive summers, before the year comes when he is well seen in autumn. Then for several more years (about seven) he is well seen in the autumn months. Next, for about seven years, he is well seen in the winter months. And lastly, for about seven successive years, he is well seen in the spring months. In 1887 he was actually at his nearest to the earth, and highest above the horizon, at midnight on January 9th. In 1888 he holds such a position on January 23rd. In 1889, on February 6th; and so on. For ordinary observation, however, Saturn is most favourably placed when he is an evening star; for it is only the astronomer who is willing to wait until midnight for his observations of any celestial body.

Two hundred and sixty-two years have passed since Galileo first examined Saturn with the telescope by which he had already discovered the moons of Jupiter. It must have been with singular interest that he prepared for his first telescopic view of the planet. Yet he could scarcely have expected what actually awaited him. There, in the small field of view of his telescope, was what appeared like a triple planet. Not a planet accompanied by two moons such as those which attend on Jupiter: these moons, seen in Galileo's telescopes, were the merest points of light, and scarcely to be distinguished from stars. What Galileo now saw was, however, very different. There seemed to be a central orb, and half overlapping it two others, somewhat smaller indeed, but still presenting considerable discs.* "When I observe

* What Galileo *thought* he saw may be represented by setting two shillings with a space an inch or so wide between them, and covering that space with a half-crown.

Saturn," says Galileo, writing to the Grand Duke of Tuscany, "he seems to be triformed; with a glass magnifying more than thirty times, the central body seems the largest; the two others, situated one on the east and one on the west, seem to touch the central body. They are like two supporters, who help old Saturn on his way, and always remain at his side. With a glass of lower magnifying power, the planet appears elongated and of the form of an olive."

In December, 1610, and again during the winter of 1611–12, these two attendant orbs seemed to grow smaller and smaller, though retaining their position unchanged.

In the winter of 1612–13, Galileo again examined Saturn, hoping to learn something more of these remarkable supporters. But to his intense astonishment they were not to be sēen. The planet shone with as fairly round a disc as Mars or Jupiter. Galileo was so startled at this strange event, that he began almost to doubt the evidence of his senses. "What is to be said concerning so strange a metamorphosis?" he wrote. "Are the two lesser orbs consumed, after the manner of the solar spots? Have they vanished and suddenly fled? Has Saturn perhaps devoured his own children? Or were the appearances indeed an illusion or fraud, with which the glasses have so long deceived me, as well as many others to whom I have shown them? Now, perhaps, is the time come to revive the well-nigh withered hopes of those who, guided by more profound contemplations, have discovered the fallacy of the new observations, and demonstrated the utter impossibility of their existence. I do not know what to say in a case so surprising, so unlooked for, and so novel. The shortness of the time, the unexpected nature of the event, the weakness of my understanding, and the fear of being mistaken, have greatly confounded me."

Galileo afterwards saw the smaller orbs return into view; but he noticed that as they grew larger and larger they changed strangely in shape, until he finally saw them lose their globular appearance altogether, each assuming the figure of two arms stretched round the planet.

I shall not describe here, at length, the gradual progress by which the true nature of the ring was recognized. Let it suffice to mention that Huyghens first, in 1656, announced that Saturn is not attended by two companion orbs, but by an immense ring,—flat, so that when turned edgewise towards the earth it cannot be seen in ordinary telescopes, and tilted towards the level of the path in which the planet travels, so that at two opposite parts of the path the ring, as seen from the earth, appears to attain its greatest opening.

In passing, it may be mentioned that in 1656 the ring was closed—that is, turned edgewise towards the earth—and that when it opened out after 1656 the flat side of the ring which was in view was the northern side. This side remained in view, the ring first opening out and then closing up, until December, 1671, when the ring was again turned edgewise towards the earth, after which the southern face came into view. Now it is well to notice that though so many years have elapsed since Huyghens discovered the nature of the rings, there have not been many of these changes by which the northern and southern faces of the ring are brought alternately into view. To my ideas, it gives a singularly impressive conception of the stately motion of Saturn in his orbit, to notice that during all the years which have passed since astronomers knew that Saturn is girdled about by a ring, the ring has swayed so slowly to and fro (as seen from the earth), that the northern side has been turned only eight times towards the sun, and the southern only seven. Two hundred and sixteen times the earth has circled round the sun, while Saturn has not yet swayed his ring through all its phases so many as eight times.

In 1675 Cassini found that the ring is divided by a strong dark band into two concentric rings. But the English astronomer, William Ball, had discovered this feature ten years earlier. The interest of Cassini's observation consisted in the fact that it proved the band to be a division between two distinct rings, and not a mere dark streak upon one face of a flat ring.

Huyghens had, in the meantime, discovered a satellite attending on Saturn. This orb, which has received the name Titan, is distinguished among all the secondary members of the planetary system by its superior size. It is larger than the primary planet Mercury, and little inferior to the planet Mars. Cassini discovered in 1671 another satellite (now called Japetus), also large, though not nearly so large as Titan. We have no other means of estimating the magnitude of these bodies than by considering their brightness. But assuming them to resemble our own moon and the moons of Jupiter, in their power of reflecting sunlight, it would follow that Titan is about 4,000 miles in diameter, and Japetus about 3,000.

But perhaps the most remarkable circumstance respecting these bodies, is the enormous distance at which they travel around their primary. The outermost of Jupiter's moons travels at a distance of 1,190,000 miles from Jupiter. The distance of Japetus from Saturn is nearly twice as great, being no less than 2,210,000 miles. Titan travels at a distance of 760,000 miles from Saturn.

It is impossible to consider these enormous distances in the case of Jupiter's outer moon, and the two chief moons of Saturn, without being led to consider in what degree these orbs seem likely to subserve the purpose of supplying light to their primaries. It may be very unphilosophical to reason from final causes; not because of the objection urged by some, that we have yet to demonstrate that there *is* design in the scheme of the universe, but for a reason which the believer may accept willingly,—namely, that however certain we may be that there is design in every portion of the universe, we are very far from being able to satisfy ourselves of the real purpose of any particular created object. Nevertheless, the mind of man is so constituted that it will inquire into final causes, even where the inquiry may be hopeless, and will be ready to recognize final causes where, perhaps, the evidence is much less satisfactory than it appears. Now, certainly, it is natural for the astronomer to consider the moons of Jupiter and Saturn either as intended to subserve the same purpose as our own moon, *or* (if it can be shown that they cannot subserve such a purpose) as created for some one or other of the special purposes which we seem to recognize among the celestial bodies of different orders. Taking Titan and Japetus as they are shown to us by our telescopes—two orbs together equal in bulk to two such planets as Mercury—it is very difficult indeed to imagine that they subserve no useful purpose at all.

Now, if we consider the amount of light which Titan and Japetus can supply to their primary when they are "full," we shall, I think, be led to doubt whether they can have been intended to serve the same purpose as our moon, and still less shall we be able to believe that they were meant, as Brewster and Chalmers have supposed, to compensate the Saturnians for their distance from the sun. Titan, when full, must appear to Saturn as an orb having somewhat less than two-thirds of our moon's apparent diameter. Such an orb, if as bright (intrinsically) as our moon, would no doubt be a useful light-giver. But it must be remembered that the moons of Saturn, being as far from the sun as Saturn is, are, like him, but faintly illuminated. They are illuminated by only about one-ninetieth part of the light which our moon receives! accordingly, whereas the disc of Titan must be about four-ninths of our moon's, the luminosity of this small disc is only one-ninetieth of our moon's, so that the total quantity of light supplied by Titan to Saturn is only 4-810ths, or about a 200th part of that which we receive from the full moon.

This would seem to show, at least to those who recognize design in the universe, that Titan was certainly not intended to serve the same purpose to Saturnians that our moon serves to the inhabitants of earth.

But if this seems strongly shown in the case of Titan, it is much more strongly shown in the case of Japetus. For Japetus is smaller than Titan, and almost exactly three times as far away. As a moon, it has a diameter equal to little more than one-seventh of our moon's. The disc it shows is but about the forty-third of our moon's, and its lustre being one-ninetieth of our moon's, the total quantity of light which it supplies to its primary is only equal to about the 3,850th part of that which we receive from the full moon. It may readily be shown, indeed, that our earth supplies more than eight times as much light to the planet Venus (when our earth is seen at her brightest from Venus) as Japetus supplies to its primary.

In passing, we may notice that another satellite called Hyperion—between Titan and Japetus—is so much smaller than Japetus (though probably about a thousand miles in diameter) as to supply less than half as much light to Saturn, probably about 1-9000th part of the light which we receive from the moon.

Thus we seem led to the conclusion that these moons, at any rate, are not intended to compensate the Saturnians for their great distance from the sun. That three orbs should have been created, to supply together about the 190th part of the light which our moon supplies, and that this provision should have been intended to compensate the Saturnians for the circumstance that they get from the sun only about one-ninetieth part of the light which we receive, are propositions too improbable, as it seems to me, to be reasonably entertained. When it is added, that all the eight Saturnian moons together would supply—if all full together—only about the sixteenth part of the light which we receive from the full moon, it seems abundantly demonstrated, I conceive, that whatever purpose the Saturnian satellite system was intended to subserve, it was *not* intended to compensate the Saturnians for the effects of their great distance from the ruling centre and luminary of the planetary system.

On December 23, 1672, Cassini discovered a satellite travelling within the orbit of Titan; and in March, 1684, he discovered two other satellites travelling yet nearer to Saturn. It affords striking evidence of the patience with which these astronomers of the seventeenth century worked, that in order to discover these two satellites Cassini had to employ telescopes one hundred and one hundred and thirty-six feet long (without tubes, however). In other words, the distance of the object-glass from the observer's eye was more than twice as great as the length of the gigantic tube of the Rosse telescope. Observing under such conditions must have been the most tedious work conceivable. It would be exceedingly difficult to get an object into the field of view, and even more difficult to keep it there. The modern observer, who, with well-appointed equatorial, has but to set his telescope by the divided circles, and can then, by setting the clock going, be saved all further trouble—the telescope simply travelling after the object by means of the clock motion—may look with some degree of contempt on the rough appliances of his predecessors: yet he has so much the better reason for regarding with cordial admiration the patient and zealous spirit with which the astronomers of former times conducted their labours.

More than a century passed before any further discovery of importance was effected. On August 19, 1787, Sir W. Herschel thought he could recognize a sixth satellite travelling very close to Saturn's rings. But it was not until he had completed his forty-feet reflector that he could assure himself on this point. On August 27, 1789, the first evening after this powerful instrument had been completed, Herschel turned it towards Saturn. As soon as the planet was brought into the field he plainly perceived six stars shining around Saturn. Five of these were the satellites already discovered; and in less than two and a half hours Herschel had satisfied himself that the sixth was also a satellite.* Soon after Herschel discovered a seventh satellite, travelling yet closer to the outer ring.

It remains only to be mentioned, in order to complete the record of satellite discovery, that an eighth satellite, travelling between the paths of Titan and Japetus, was discovered independently by Bond in

* It has been asserted that the sixth satellite was discovered with one of the 20-feet reflectors, and Sir John Herschel has been quite seriously taken to task for maintaining that the discovery was due to the 40-feet telescope. The above are the actual circumstances, as recorded by Sir W. Herschel himself. It seems wholly impossible to regard the doubtful view which he obtained in 1787 as the actual discovery of the satellite. According to all the rules usually adopted in these cases, the true discovery dates from those two and a half hours of observation, during which Herschel first satisfied himself that the satellite was not a fixed star.

America, and Lassell in England. It is probably the smallest of the whole family; and there is something so remarkable in the circumstance that this tiny orb should thus be found travelling between the two giant satellites, Titan and Japetus, that we may almost be permitted to entertain the suspicion, that in reality Hyperion is but one of a ring of small satellites travelling between the orbits of Titan and Japetus.

Before returning to the consideration of the Saturnian rings, it may be well for us to consider the nature of Saturn's family of satellites. We have in this scheme what may be regarded as no inaccurate picture, in miniature, of the Solar System itself. Of course, there are differences in points of detail, since Nature does not repeat herself detail for detail in such cases. Yet we find some striking features of resemblance. Thus the sun's family consists of eight members, and so also does Saturn's. Among the planets there are two which are prominent among the rest by their great bulk; and in Saturn's family we find also Titan which we can compare with Jupiter, and Japetus which we can compare with Saturn.

Certainly, if we consider what the Saturnian satellite family really is, that the orbs composing it are all large in reality, however minute they may appear either when viewed with the telescope or when considered with reference to such orbs as Jupiter or Saturn; that the span of the complete system is no less than 4,400,000 miles, or more than five times the sun's diameter; that even Japetus, which moves the slowest, circles on his orbits with a rapidity which exceeds a hundredfold the velocity of our swiftest express trains—we cannot but regard this system of secondary orbs as a most important portion of the scheme ruled over by the sun. If we are compelled to believe that the purpose intended to be fulfilled by these bodies is not the illumination of the

Saturnian nights—and for my own part I can arrive at no other conclusion—we seem bound to believe that they were created for some other purpose of importance. It does not seem at all unlikely, on this view of the subject, that they are themselves the abodes of living creatures of various orders. I have already, in these pages, shown reasons for believing that Saturn may be a source whence heat is supplied to these eight orbs, whereas it seems unlikely that he is himself a world fit to be the abode of living creatures. Again, though the satellites supply Saturn with very little light, yet they are capable of supplying each other with no inconsiderable amount, and must frequently present phenomena of great beauty and interest as viewed from each other. Thus a variety of reasons suggest the probability that we are to look among the Saturnian satellites, and not to Saturn himself, for places fit to be the abodes of living creatures.

We have seen that in the latter half of the seventeenth century, Saturn's ring had been found to be divided. Sir W. Herschel, notwithstanding the great telescopic power which he applied to the examination of the rings, was unable to do more than satisfy himself of the existence of the great division. It was suspected in his day that other divisions exist,—not that any had been seen, but that the discussion of the nature of the ring-system had led to the inference that it must consist of many distinct rings. But Herschel could not trace any signs of the existence of any other divisions than the great one.

But during the present century many skilful observers have recognized other divisions. One such division separates the outer ring into two of nearly equal width. This division seems to be permanent; but it is most difficult of detection, and can only be seen with telescopes of the first quality and on nights when the atmospheric conditions are very favourable. Other traces of division have not continued to be recognizable, and are therefore probably not permanent. It has been remarked that "if each division thus detected were considered as a satisfactory indication of a permanent division through a complete circumference, it would follow that the system consists not of two or three, but rather of thirty or forty concentric rings. Strange as such a conclusion might appear, and manifold as are the conditions of instability the complexity of such a system would introduce, we should have no resource (on the assumption of the solidity of the rings) but either to accept this solution of the question, or to reject the testimony of most accurate and skilful observers—of such men as Encke, the Struves, Captains Kater and Jacob, Mr. Dawes, and the astronomers of the Collegio Romano. The telescopes also through which such divisions have been repeatedly seen, have been among the most celebrated instruments of modern times."

But the most remarkable discovery yet made respecting this remarkable ring-system remains to be described. On November 15, 1850, Bond, of America, discovered a dark ring inside the inner bright one; and a few days later Dawes, in England, independently discovered this ring. The colour of the dark ring is a deep purple; or rather, since, as we shall presently see, the ring is semi-transparent, and therefore a portion of its apparent colour is that which it transmits, we may say, without committing ourselves to any theory as to the true seat of the colour, that the region occupied by the ring presents a deep purple colour. The nearer part of the dark ring can be traced over the disc of the planet, but the outline of the planet can be recognized *through* the ring. This portion of the ring does not show a purple tinge, but has been compared to a crape veil. A division has at times appeared in the dark ring, which also appears at times to be separated from the neighbouring bright ring.

Professor Bond, of America, noticed at about the same time a very remarkable darkening of the inner bright ring, on its inner edge, close to the dark ring. The peculiarity about this darkening was, that instead of being exactly similar in shape to the outlines of the several rings, its outline formed a longer oval, as though the darkened part were wider in those places which lie upon the seeming longer axis of the rings. If the rings were really oval, as they appear through the effects of foreshortening, this peculiarity would be explicable ; but as the rings are circular, and every part in turn comes into the position indicated, for the rings rotate, and moreover Saturn himself carries them into varying positions as seen from the earth, the appearance is altogether inexplicable as a mere shade or darkening. If, between the bright ring and the dark ring, there were a ring of an intermediate tint, that ring being concentric with the others (else it could not rotate) would present similar outlines, whether the whole system were more or less foreshortened. This not being the case, one outline of the darkened part being always more elliptical than the other, we must, in explaining the darkening, find an interpretation of a peculiarity which, in the nature of things, must be apparent. I believe the explanation enforced upon us by this consideration, is simply that the inner part of the bright ring is transparent, or rather, that we can see through this part, for, as will presently appear, we have no reason for believing that the actual substance of any part of the ring-system is transparent in the same sense that glass or crystal is transparent. I have shown in my treatise on Saturn that if the inner part of the bright ring consist of a multitude of concentric zones between which the dark sky beyond can be seen from our terrestrial station, the observed appearances would necessarily be seen. The explanation, to be adequately understood, requires such an illustration as is given in the ninth plate of that work; but its general principle may be understood by any one accustomed to drawing, who will attend to the following description. Suppose several white hoops to be lying one within the other (and quite concentrically) on a dark flat surface; the hoops being not shaped like those used by girls, but like the iron hoops which boys use. (A piece of cane bent into a circle would make such a hoop.) Now, if we looked at such a set of hoops from above, we should see so many white concentric circles on a dark ground. But let the point of view be not directly above, so that we look slantwise at the set of hoops. Then we shall see a number of similar white ovals on a dark ground. Now, if these ovals were mere oval *lines*, that is if the hoops were mere threads, the dark spaces between them would seem to grow narrow where the ovals are narrowed, and in just the same degree; but as the ovals are formed by stout hoops the case is altered, the dark spaces are *more* narrowed where the ovals are flattened. Nay, if the hoops are pretty close to each other, a very little foreshortening will make them seem actually to touch where the ovals are flattened, while where the ovals are lengthened out the dark background is visible. (If the reader will draw such a set of hoops, as they would actually be seen, he or she will at once perceive that this is so.) Now, without supposing that the rings of Saturn are composed of such hoops, I find myself led to the conclusion that the matter forming the rings runs into hoop-like shapes, and that where Bond saw the darkening, above described, these hoop-formed portions were far enough apart to let the dark sky beyond be seen where the ovals of the ring were most lengthened, the spaces closing up by foreshortening where these ovals are most narrowed. It is demonstrable, indeed, that, if the appearance observed by Professor Bond was not a mere illusion (which in the case of so practised an observer is altogether unlikely), it can be explained in this way and no other. So that the peculiar darkening seen by Bond is an independent proof of the multiple nature of the ring-system—a proof as complete as though Bond's telescope had been powerful enough to reveal the several rings forming this part of the bright inner ring.

Saturn's ring-system, regarded as a whole, is a structure so remarkable that we seem invited to consider it with a special degree of attention, in order that if possible we may form some idea of its real nature. If there were no other circumstance remarkable about it but its mere vastness, it would even then be well deserving of our closest scrutiny. But that a ring-system so symmetrical and beautiful should girdle a planet completely about, that it should accompany the planet on its path around the sun, that it should be as definite a portion of the planet's system as the belts on the planet's real globe, or as the satellites which circle about that orb, these are circumstances which render the study of the ring-system specially interesting. For we cannot but recognize the fact that such a system, swayed as it must be by the mighty attraction of Saturn's mass, must present a number of relations of a very complex and perplexing kind.

For a full account of the inquiries made by astronomers into this interesting system I may refer the reader to my treatise on "Saturn and its System."

CHAPTER XVII.

THE WORLD'S FIRST MERIDIAN.

DESPITE the opposition of the French, Brazilian, and Haytian astronomers (a rather singular combination), the meridian of Greenwich has been adopted as the astronomical and geographical reference meridian for the world, and hereafter we may expect uniformity to prevail in maps and charts, in nautical almanacs, and in tables of reference alike for terrestrial and celestial computations. Of course, there will be no noteworthy change in the ordinary measurement of time in different countries or cities. At New York and Washington, for instance, where when it is noon in England it is only about seven in the morning, and only seven in the evening when it is midnight in England, they will not, because of the adoption of the Greenwich meridian, call it noon or midnight when the sun gives them so different a time of day. Ordinary or civil time will always be reckoned pretty nearly by the sun—not exactly, of course, for the simple reason that in that case every journey east or west would involve a change of clock time. Just as Ireland has a different time from England, not because of any native cantankerousness on the other side of St. George's Channel, but because the sun gives different hours, so in the United States they must have their clocks and watches agreeing tolerably well with the sun, and so must have different local time from ours. In different sections of the States they will have, also, times differing by a full hour—earlier and earlier for more and more westward sections—an arrangement by which no place will have time much more than half an hour different from sun time.

Half an hour is not a matter of any great importance, as we may know by the fact that no one in the business of life recognizes the circumstance that sun time changes by more than half an hour in the course of each year in every part of the world. If we set a perfect clock or watch—that is, one steadily recording day after day 24 true hours of mean time, so that at the end of a hundred years or more it would be as near sun time as at the beginning—to show 12 noon when the sun was exactly south in February, then, tested by the sun, that clock would seem half an hour wrong at solar noon after about half a year had passed, which would *seem* to show that in a year it would be an hour wrong, and in six years would show six o'clock at 12, and in twelve years would show 12 noon for 12 midnight. Yet the discrepancy would be entirely due to a want of uniformity in the sun's motion, to which none except astronomers pay the slightest attention. In like manner, in the United States there are places where, judged by the sun (even when he is with the clock at Greenwich), the clocks seem half an hour too fast or too slow on the average all the time; yet business goes on undisturbed.

The same arrangements continue, now that the meridian of Greenwich is adopted as the reference meridian, which were in vogue before, except that the American hour system has been brought into correspondence with Greenwich time instead of Washington time—so that, for instance, a traveller from England to New York or Washington finds his watch *exactly* instead of *nearly* five hours fast by New York or Washington time. In this way the whole world may one day be divided into hour zones, so that every change of time for a voyager travelling westwards would be made by putting back his watch exactly an hour, and every change for a voyager travelling eastwards would be made by putting his watch forward one hour exactly. Though, even then, at sea, the present system would have probably to be retained, by which each noon the approximate local noon is adopted.

In what, then, it may be asked, does the importance of the recent change consist? The astronomer and the geographer do not need to ask the question, knowing as they do the multitudinous inconveniences which arise from the use, in the astronomical computations and the geographical charts made in different countries, of the longitudes of Greenwich, Vienna, Berlin, Paris, Washington, and so forth. I take up, for example, in the old time of the controversy about the transit of Venus, a treatise or paper written by Puiseux at Paris, or by Newcomb at Washington, and I find that before I can compare properly the results deduced or discussed by the French or American astronomer with my own, or with others dealt with by English astronomers, I must translate the French or American longitudes and times into Greenwich longitudes and times. Even in the case of a single treatise, the time thus wasted (there is no other word for it) is considerable; but when a great number of such works, on different astronomical subjects, pass through an astronomer's hands weekly or monthly, as is the case with me, the nuisance becomes quite serious; and when we remember that this is so in the case of one person alone, we see how large must be the total waste of time and trouble thus arising.

The geographer is similarly annoyed. In comparing French, German, or American maps with English maps, or geographical statements by geographers of other countries with similar ones made in England, the geographer finds that every detail depending on longitude has to be corrected or translated before the full significance of the foreign charts or statements can be appreciated.

In fact, this question of a meridian of reference may be regarded as affecting our view of the earth from without, as it were, more importantly than the view we take of the earth as residents in this or that part of her surface. It is the earth as a rotating planet which has now been definitely marked for reference, so that all astronomers and all geographers measure from one and the same mark, not each set from a mark of their own. Just as astronomers use a fixed meridianal marking on Mars by which to time the rotations of the planet, so in future will astronomers and geographers act with regard to the earth. Strange that they should have assigned a fixed meridian to a planet many millions of miles away, many years before they assigned a fixed meridian to their own planetary home!

It may be asked whether the adoption of a fixed meridian for the whole earth will affect the question many find so perplexing, as to what day of the week it is at particular places, and at particular times. Hitherto, it is to be noticed, the usage of astronomers and the usage of business folk has differed. To the astronomer there had been a definite series of days, the same all over the world. But, what people in England call, for instance, November 17, viz., the interval of time between midnight and midnight on either side of that day whose middle is noon November 17, had been, for the English astronomer, divisible into the last twelve hours of November 16 (which ended for the astronomer at noon November 17) and the first twelve hours of November 17, whose remaining twelve hours, numbered from 13 to 24, ended at noon November 18, civil time. So, all round the year,—December 25, for instance, in astronomical time, included for the astronomer the twenty-four hours from December 25, 12 noon Greenwich mean time, to 12 noon Greenwich mean time, December 26. Now, astronomers, all over the world, have the same way of numbering days as other folk.

But as to what day of the week it is at any particular place and time, the difficulty, which many imagine to exist only along the meridian half-way round the earth, west or east of Greenwich, has always existed, and will continue to exist, all over the earth. It is true that when we travel westwards or eastwards from Greenwich, we have to make a change of a full day, one way or the other, when near the meridian which lies 180 deg. east (or west) of Greenwich. But that is only because we have not made the necessary partial change at each successive stage of our journey west or east of Greenwich. Or it might, perhaps, be rather said that small partial changes are made stage by stage, in passing westwards or eastwards, which amount to half a day where two voyagers travelling westwardly and eastwardly at equal rates would meet; and these changes being in opposite directions, the two half days must be made into a whole day at the place where the voyagers cross each other, the westwardly voyager now taking the days (one ahead of those he had been using) of the eastwardly voyager, and *vice versâ*.

But the difficulty as to the day of the week exists all along, and is actually felt (which is different) wherever we pass across a line dividing two regions where different local time is used. Thus, suppose we are on a train travelling westwards from New York, and pass, at half-past twelve at night, a place where, along that railroad line, the change of an hour is made. It is, let us say, Tuesday morning early (half an hour after midnight) before we pass that place; but so soon as we have passed the place of change, it is no longer Tuesday morning but Monday night—half-past 11 P.M. By passing to and fro across the line of change, at any hour between 12 midnight and 1 A.M., for the eastward region, or (which is the same thing) between 11 P.M. and 12 midnight for the westward region, we can make the day of the week change as often as we please, or have any number of Mondays and Tuesdays, Tuesdays and Wednesdays, &c. (as the case may be), in the course of a single hour.

But the difference of day in such a case as this is a matter of no moment, and needs no correction, whereas it would be a matter of serious moment if every one who had circled around the earth either eastward or westward remained a whole day behind or in advance of those among whom he lived. It is obvious that as the westward traveller keeps on adding hour after hour to his time, he must add a full day by the time he has gone completely round, and unless he dropped a day somewhere he would be a day behind the friends whom he had left at home, by the time he rejoined them. The opposite change must be made by a traveller going eastwards; and clearly the proper place for the change is when either is half way round; for by making it there the discrepancy never exceeds half a day.

CHAPTER XVIII.

GREAT CIRCLE SAILING.

DOUBTLESS every reader of these pages is familiar with the theoretical advantages of the great circle track, from any given point on the earth to any other. This track is always the shortest which can be followed, subject only to a very slight correction on account of the earth not being a perfect sphere, a correction which will never be considered in practice. Nor is the mere shortening of the course by pursuing the great circle track the only theoretical advantage of the plan. Another advantage, often much greater, arises where a sailing ship is pursuing her journey against adverse winds; for if the rhumb course laid down on a Mercator's chart had to be followed under such circumstances, the nearest tack to that course might actually increase the ship's distance from her destination; but this could never happen if the course laid down for her were the great circle course. It may, indeed, be safely said, that while the advantages of a great circle course, apart from practical considerations to be presently noticed, are great for steamships, they are much greater for sailing ships, and the more unfavourable the winds the greater the advantages of the great circle course become.

But even where a great circle course would carry a ship into no unsuitable regions—neither into dangerous seas, nor into too high latitudes—there are practical difficulties about great circle sailing which have been found seriously to limit its application. Hitherto the choice of the master of a ship who wishes to shorten his route by availing himself of the great circle course, has lain between the use of charts specially devised for marking in such courses, but very ill adapted for the purpose, and the use of the excellent "Tables to Facilitate the Practice of Great Circle Sailing," published by Mr. J. T. Towson about twenty years ago.

With regard to the latter resource I need say very little. The calculations are not difficult, nor do they take very long. It is easy to determine the points where the great circle course from one port to another will cross conveniently chosen longitudes, 5° or 10° apart, and then the course so determined can be entered on a Mercator's chart as a series of short rhumb courses, indicating quite nearly enough for practical purposes the compound track which is the approximate great circle course. Nor is it at all difficult to determine with similar accuracy by the use of Towson's Tables, the composite course from one port to another by which first a given latitude is reached (the highest thought safe) on a great circle course, which is followed to a certain distance, and then left on another great circle course terminating at the destination sought.

All this is simple enough. But in practice masters of ships are not often found willing to make the necessary calculations. Moreover, the method only indicates a certain course, either great circle or composite. If through stress of weather, or from any other cause, that course is considerably departed from, either an entirely new series of calculations has to be made, or else the master may as well take the rhumb course from his new position. In the case of sailing vessels in stormy seasons, the great circle track determined by Mr. Towson's table would often be of very little use. What a master wants before he will care under such circumstances to adopt the great circle course, is a method by which in a few minutes, at the outside, he may be able to determine the proper course from whatever point he may have reached to the haven for which he is bound.

With regard to charts which have been prepared to facilitate great circle sailing, I have said that hitherto they have been ill adapted for the purpose. I refer especially to gnomonic charts. Sir George Airy long since devised a construction by means of which, and certain subsidiary tables, an approximation to the great circle course might be marked in upon a Mercator's chart; but the method was cumbrous, and I believe no sea captain ever applied it. He admitted, when gnomonic charts were first published for great circle sailing, that they were much more convenient than this method.

The gnomonic charts had one great advantage over all others, including those which I am about to propose as altogether better suited for the seaman's use. If you find your two ports in the chart, you have only to connect them by a straight line to get the great circle course. Nothing could be simpler or better.

Unfortunately this initial simplicity is compensated —and so far as comparison with my own plan is concerned is much more than compensated—by great disadvantages.

The gnomonic projection of the terrestrial globe may be conceived thus :—Imagine the globe to be of perfectly transparent crystal, the centre of the globe a point of light, the meridians, the parallels, and the outlines of continents being marked in upon the surface of the globe in opaque lines; then if the globe were set anywhere on a plane surface, the meridians, parallels, and continent outlines would be gnomonically projected as shadows on that surface (the central point being of course supposed to be the only source of light). It is easily seen that any great circle whatever, being a circle whose plane passes through the (luminous) centre of the sphere, must be projected into a straight line, the intersection, namely, of the circle's plane with the plane on which the shadows are cast or the projection is made.

But obviously the increase of scale as the distance from the centre of projection increases is enormous, and the distortion also becomes very great. For instance, a small area only 60° from the centre of projection is increased in surface six and three-quarter times, being extended in one direction to twice its breadth, and in the thwart direction to three and a half times its length.

The trouble about this rapid increase of scale is that it limits the extent of surface which can be presented in the projection. For, unless the scale at the centre of a map is exceedingly small, the size of the map would be enormous if only 75° or 80° of distance from the centre were brought in, and the maps would have to be infinite in size (which is not considered convenient on shipboard) to show a hemisphere. Now, if two ports, or places between which the great circle course is required, are not in the same chart, then the simplicity of construction mentioned above of course no longer exists. It *is* possible to have a set of gnomonic charts made, like the six maps of the heavens published by the Society for Diffusing Useful Knowledge, and to present a construction by which the great circle course from a port shown in one chart to another shown in a different chart can be obtained. But certainly the construction is not a simple one.

There are, however, other objections, which apply even where the two ports lie in the same gnomonic chart. It is desirable to see at once the "course" at every point of a great circle track. But a gnomonic chart does not show this. All the angles are altered in the projection,—as we see at once if we consider that a small circle on the sphere is projected gnomonically into an ellipse which for parts far from the centre of projection is exceedingly eccentric. Thus, lines which intersect at right angles on the globe may intersect at a very obtuse or acute angle in the projection. To get the "course" at any point of a great circle track, by construction, the seaman must plot the course in from the great circle chart to the Mercator's chart, in which all angles are correctly shown. This is inconvenient, and is made the more so, because owing to the great distortion in gnomonic charts, it is difficult to determine the precise longitude and latitude of points not actually on the intersections of the meridians and parallels.

The method by which I have proposed to obviate all these difficulties, and to make the determination of the great circle track from port to port, and the true course at every point of it a matter of extreme simplicity, is as follows:—

I have constructed two charts, one having the north pole the other having the south pole as its centre, on the stereographic projection, each extending 150° from its pole, or 60° beyond the equator all round. Either chart may be used as may be more convenient, or both may be employed, the same course being obtained in each projection, but on different scales, so that the two results serve to check each other.

The stereographic projection may be defined in the same way as the gnomonic, that is by conceiving a crystal globe standing on a plane, and a luminous point casting the meridians, parallels, and continent outlines as shadows on the plane; but instead of the luminous point being supposed to lie at the centre of the globe, it must be supposed to lie at the end of that diameter which passes through the point of contact of the globe and the plane on which the shadows are cast. Thus, if the globe be supposed to stand on a horizontal table, the highest point of the globe must be supposed luminous.

Now in the stereographic projection every circle on the sphere is projected into a circle. Thus the great circle track between two ports will be a circular arc. But how are we to determine the radius and the position of the centre of this arc? As a geometrical problem this would be rather difficult, till what may be called the geographical solution is pointed out, when it is seen to be exceedingly simple. A great circle through any point on a sphere necessarily passes through the opposite point, seeing that the diameter of the sphere, taken through the given point, is a diameter also of the great circle. Hence all we have to do to determine the great-circle course from one point to another, is to find (1) the port of departure, (2) the port of arrival, and (3) the antipodes of either; a circle swept through these three points will be the great circle required: (we can then see if the antipodes of the other port lies on the circle thus obtained, as of course it should, and so check our result).

The sweeping of a circle through these points on a map is, of course, a very easy thing to do, regarded as a problem in geometrical construction. We have only to connect the points, and draw the perpendicular bisectors of any two sides of the triangle thus obtained: these intersect in the centre of the circle required. But as a matter of fact the construction is not required. It is much easier to find the centre by trial: in ten or twelve seconds, or in half a minute at the outside, the centre is found, and then the great circle track is swept out in pencil or in ink as may be preferred, the marking end, after tracing the track, being carried round (without marking) to the antipodes *not* used in obtaining the circle, in order to see that all is right; for if so, that end will pass through both antipodes.

As the stereographic projection shares with Mercator's the property of showing correctly all angles whatsoever, the circular arc swept out in the way just described, not only shows the whole great circle track, but the exact "course" at every point of it. A "compass" may be set on every tenth meridian (that is, meridians 10° apart) on the chart, and the "course" athwart any meridian determined by means of a parallel ruler, just as the seaman obtains his "rhumb" course from the nearest "compass" on a Mercator's chart. Or, still more simply, a compass on table or on tracing paper may be set over the ship's place anywhere, the N—S line on the compass brought into coincidence with the meridian through the point, and the "course" at once seen. Or an ordinary protractor may be used to measure the angle at which the great circle track at any point crosses the meridian there. But a better plan is to join in pencil the centre of the great circle track and the ship's place, and measure the angle which this line makes with the meridian through the ship; this angle is the same as that at which the great-circle track crosses the latitude-parallel.

Of course the *vertex*, or highest latitude reached, is given at once by this method.

A great advantage of this method is that wherever a ship is found to be after a few days of stormy or cloudy weather, the great circle track from that point to her haven can be at once pencilled in. The seaman has already marked on the chart the antipodes of his haven, that haven itself, and the ship's place. All he has then to do is to sweep a circle through these three points, or rather to mark in, in pencil, such part of a circle through these points as connects the ship's place and the haven.

Thus, if the weather continues rough, with contrary winds, he knows at once what is the true "course" for the great circle track from his place to his haven, and on what tack to lie; where, without that knowledge, sailing as close to the wind as he could on the wrong tack, he might be actually going away from instead of towards his port.

Determining the composite course, where a given latitude is to be touched in passing from one port to another, is exceedingly simple by my method. It depends on the principle that a great circle which touches one small circle on a sphere, touches also the antipodal small circle, that is to say, the small circle every point of which is the antipodes of a circle on the other. Hence, having the port of departure and the latitude-parallel to be touched, we take also the antipodal latitude-parallel—the great circle arc required, will, in the chart, be part of a circle through the point and touching these two latitude-parallels. (The parallels are concentric circles, and of course the touching circle must go *outside* the inner latitude-parallel, in order to touch it on the antipode of the point of contact with the other—the latitude-parallel to be touched by the great circle track.) This is the simplest possible geometrical construction, because the radius of the required circle is at once given, and also the distance of its centre from the centre of the map; opening the compass to the required radius, and putting one point on the port of departure, we mark with the other a point at the determined distance from the pole; this is the centre of the required great circle track *to* the limiting latitude. The same opening of the compass, a point being put on the haven, gives the centre of the great-circle track *from* the limiting latitude.

Even the distance along the great circle track between any two points can be determined very closely by a construction requiring but two or three minutes. No other chart method does anything with the distance.

I propose shortly to publish, if possible, the northern and southern maps for this method, on a convenient scale.

CHAPTER XIX.

HOW EARTHQUAKES ARE CAUSED.

WHEN a great volcanic outburst takes place, or the earth is shaken by tremendous throes, men are apt to suppose that some unusual condition prevails beneath the earth's crust. But in reality, although subterranean disturbances may be the true cause of all great earthquakes and eruptions, there can be little doubt that the occasion of those subterranean disturbances is often, if not always, to be sought outside the earth's crust. It is doubtful whether the process of contraction, which is going on all the time with greater or less activity, although generating enormous supplies of subterranean heat, might not, nevertheless, proceed without producing great subterranean disturbances were it not for external changes which intensify its action, sometimes assisting its effects, sometimes resisting them, and so making their disturbing energies much greater than they otherwise would be. Of some of these external causes of subterranean disturbance I propose briefly to treat before considering the earth's internal activity. They have received much less attention than they deserve.

Let us first consider a cause of disturbance which might very well be overlooked—the changes of atmospheric pressure which are taking place all the time. When we hear that the barometer has risen or sunk half an inch, we do not commonly attach much importance to the change, nor, in most parts of the earth, is such a change likely to produce any remarkable effects. Even in regions where the crust of the earth is notably unstable, a change of half an inch in the height of the mercurial column is not ordinarily of great importance. Yet it might under certain conditions make such a change in the conditions of equilibrium as to bring about an earthquake. Consider what it really means. When the barometer rises half an inch over an area of 10,000 square miles, less than a sixth of the area of England, the pressure on that area is increased by 4,260,000,000 tons. If a wave of atmospheric pressure passed over the United States in such sort that over the eastern half of the States the barometer were first half an inch lower than in the western half, and then half an inch higher, the effect would be as though a mass of about seven hundred thousand millions of tons were shifted from the western to the eastern half of the United States. We know that such changes—nay, changes considerably greater—take place, and they do no particular harm in most cases. But certainly such changes of pressure are not to be neglected in considering the cause of subterranean disturbances. They must affect the equilibrium of the crust even of the most stable parts of the earth in marked degree. Rightly considering the matter, the wonder is, not that changes of atmospheric pressure, seemingly so slight that we scarcely notice them at all, may bring about subterranean disturbances, but that the disturbances they produce are so seldom observed.

That changes of atmospheric pressure do affect the earth's crust in recognizable degree has been observed even in England, where earthquakes are infrequent, and where destructive earthquakes scarcely ever occur. It may surprise many to learn that while earthquakes occur but seldom in England, vibratory undulations, or earth-shakes, as they may conveniently be called, are occurring all the time. No less than 217 were noted in Great Britain during the fifteen years from 1868 to 1882 inclusive. The eastern side of Britain is the more disturbed, and England and Scotland are much more disturbed than Ireland. The connection between these earth-shakes and changes of atmospheric pressure has been abundantly shown in a remarkable paper read by Mr. W. Walton Brown before the North of England Institute of Mining and Mechanical Engineers. Other causes are recognized too, but this cause is distinguishable from the rest.

An increase of one inch in the height of the mercurial barometer corresponds to a weight of 650 pounds to each square foot, or about 852,000 tons on each square mile, of surface. This cannot but prove a most effective addition to the pressures constantly exerted upon the regions beneath the crust, and when the pressure fluctuates by such an amount, increasing here and diminishing there, we cannot wonder if the effects of such changes show themselves in a marked way in the weaker portions of the earth's surface. Now in times of great storm the mercury changes rapidly in height, and this corresponds to the rapid addition or removal of many thousands of millions of tons to and from the areas of rising and falling barometer. In regions like the British Isles the effects of such changes, though sensible to scientific observation, are only recognizable otherwise (that is, in a way to attract general observation) by the occurrence of great colliery explosions. This is not due, I think, as my friend Mr. W. Mattien Williams supposes, to the formation of fissures in the crust enclosing the fire-damp, and the consequent escape of the gas, but to the diminution of the pressure of the air over colliery regions, and the increase of pressures elsewhere. If, for instance, over a region a few hundreds of square miles in extent where there are coal mines, the atmospheric pressure in a time of great storms is reduced so that the mercury sinks an inch, while all around the pressure is high, we have for the time a condition of affairs which cannot but result in the forcing out of enormous quantities of gas. For over a region where outlets already exist, or where the crust has at least been so weakened that it forms but a weak enclosure for the gas usually imprisoned, a pressure of hundreds of millions of tons has been removed, while all around the pressures are enormously increased, so that gas is driven toward the region of outlet from all sides.

In considering this particular point, as, indeed, always in dealing with disturbances affecting large regions of the earth's crust, we must remember how plastic the crust must be, let its thickness and the strength of its materials be what they may. Many imagine that because the earth's crust presents enormous areas of solid matter, its capacity of resisting pressure is therefore very great. But it is through its very extent that the earth's crust becomes weak and plastic. Just as the lengthening of any kind of horizontal support, beam, bridge, or the like, makes it weaker to resist vertical pressure, so the broader and wider the areas of the earth's surface exposed to any strain, the greater the effect produced. Nay, as we know that a bridge formed on the same plan as one of ample strength, but on a very much larger scale, would not only be weaker to resist external strains, but unable to support even its own weight, so we may be well assured that many extensive portions of the earth's crust have no sustaining power whatever, afford no resistance to increased pressure, nay, are retained in a position of equilibrium (under normal conditions) only by the reaction of the earth's interior supplementing such strength as they may themselves possess. If a portion of the earth's crust thus needs even but a small additional supporting force below, it can be well understood how the addition of thousands of millions of tons on an area only a few thousand square miles in extent may utterly destroy equilibrium.

We need not be surprised to find, then, that earthquakes have very often been preceded by remarkable atmospheric phenomena. Usually great earthquakes have not followed tremendous storms, but a condition of portentous calm. The air has been found oppressive for hours, perhaps days, before the earthquake occurred. Remembering afterward the sense of oppression which had preceded the subterranean disturbance, the ordinary observer has been apt to infer that the dull, heavy calm, the unrestful stillness, was nature's pause before the mighty throes in which her imprisoned energies found vent. But in reality the oppressive stillness has been simply the result of increased atmospheric pressure, and this increased pressure brings about the earthquake as its direct consequence. Those who have had experience of earthquake shocks are apt, when the air is heavy and a sense of oppression and tension is felt by all men, and even apparently by the animal world, to say, "I fear this stillness is ominous, and that we shall have an earthquake," but in reality they should rather say, "This stillness means a high barometer and increased atmospheric pressure; I fear the earth's crust, weak as it is here, will not be able to bear the additional strain, and that we shall have an earthquake, or some other form of subterranean disturbance." But there is something impressive in the sense of mystery, something strangely suggestive in the thought of nature, like some live creature, pausing before a mighty effort. The idea of causation, which lies at the root of all scientific inquiry, and leads men to look for the proximate and then for the remote causes of observed events, has no attraction for those who have little care for scientific research : they are disposed to think that a certain charm disappears from nature's work when its mechanism is too closely examined. But in reality there is something even more striking in the thought of what nature is really doing than in vague fancies about what she seems to be doing. A true poet, though he may find the gloomy pause of nature before her earth-throes suggestive and impressive, finds far more to move him in the thought of the vast waves of weight which the unseen air is constantly carrying over the earth's surface, and in the fluctuations, the pulsations, and the mighty throbs which move the broad bosom of the earth in response to the passage of those atmospheric waves.

It was asked not long since whether the hurricanes which followed the Spanish earthquakes were not produced by those subterranean disturbances; and all-explaining electricity has been called upon to explain how earth-throes might have caused atmospheric disturbances. I know of no way in which such consequences could have followed from a displacement of the earth's crust. To me it seems far more natural to conclude that the hurricanes and earthquakes were alike produced (the hurricanes chiefly, the earthquakes partially) by the atmospheric compression which preceded the subterranean disturbances. This compression indicated a heaping of air over the disturbed region ; the earth's crust yielded under this increase of pressure, combined with the action of other forces, and earthquakes followed ; the compressed air swept away to regions of less pressure, and the rarefaction following led in the usual way to the indraught which precedes a cyclonic disturbance.

But while the action of atmospheric pressure in helping to excite subterranean activities must not be overlooked, the varying pressure exerted by seas and oceans is a more potent disturbing factor. Atmospheric pressure is distributed in such a way that though the weight of air on any given area is continually changing, there are no sharply defined lines, at any time, which separate regions of less pressure from regions of greater pressure. It is otherwise with the sea along a shore-line. Here we have the sea acting with constantly varying intensity, as its level changes, on the seaward side of the shore-line, while on the landward side there are no such variations of pressure. Let us consider what this means. Take a tolerably straight shore-line 500 miles in length, and suppose that along this shore-line a region of ocean 100 miles broad rises through a height of three feet under the combined action of sun and moon raising a tidal wave, and favouring strong winds urging the water shoreward. Then we have 50,000 square miles of sea-water, three feet deep, added as so much dead-weight to that part of the earth's crust which underlies the seas along that shore. Each square mile contains in round numbers 3,000,000 square yards, or 27,000,000 square feet. The additional weight corresponds, then (as the added layer is three feet deep), to 50,000 times 81,000,000 cubic feet of water, each weighing $64\frac{1}{3}$ pounds, or to 116,000,000,000 tons. It is clear that the addition of so enormous a weight as this to the submerged part of the earth's crust, outside the shore-line, may well produce strains too great to be resisted. It must be remembered that the very existence of a precipitous shore-line (as distinguished from one where the land above water and the parts submerged form one great slope) indicates the comparative weakness of the crust along that coast. It has yielded on one side to pressure thrusting it upward above the sea-level, and on the other side to the pressure of the water forcing it down. It is true, the actual line of yielding may not coincide with the existent shore-line. For the action of the sea waves may (and generally must) have altered the position of the coast from that which it occupied when first formed. But it may be taken for granted that not far from every precipitous shore-line lies a line of weakness, where the crust has given way in the past, and may give way again. In this consideration undoubtedly we find a part of the explanation of the observed fact that almost all the great regions of subterranean activity on the earth lie near the sea-shore

But while the changes of atmospheric and oceanic pressure are potent factors in the production of earthquakes, and are probably in the great number of cases their direct occasion, it is, of course, to the subterranean regions themselves that we must look for the forces at work in upheaving the crust of the earth. The forces acting from the outside are as the pull on the trigger; the imprisoned gases and vapours generated by internal heat are as the powder by whose explosion the missile is ejected.

Yet even in considering the earth's subterranean activities we still have to look outside for a part at least of the causes of disturbance. The air perhaps may in this respect be neglected, but the water is all-important. It has been said, indeed, and probably with a nearer approach to truth than usual in the case of generalizations of the sort, "Without water there can be no volcano," and a similar rule (not quite so general) applies to earthquakes: few probably occur, possibly none, save through the action of water in some way or other. All active volcanoes except one (in mid-Asia) are by the sea-shore. Nearly all the great earthquakes recorded by history have taken place, and have apparently had their centre of disturbance, near the sea.

There can be very little doubt, indeed, that the direct cause of every great subterranean disturbance is water in the form of steam—steam superheated, under great pressure, and therefore possessing much greater expansive power than steam at ordinary temperatures. We have, then, two points to consider in dealing with the causes of earthquakes—first, the conditions under which water finds its way into the interior of the earth, and secondly, the cause of the intense heat by which that water is turned into steam.

Of course what I have already said respecting the fluctuations of pressure at and near the coast-line goes far to explain how water can there find its way through the earth's crust. Not only does the fluctuation of pressure disturb the equilibrium of the crust, it also tends to form cracks and fissures. The alternate inflow and outflow of water along a shore-line subjects the crusts to an alternation of pressure akin to the alternate bendings of a wire or plate by which the workman succeeds in breaking it. There must be a bending to produce openings or cracks running parallel to the coast-line. Although the strength of the crust might usually withstand the effects of this constantly varying strain, there must be certain of the many thousands of miles of coast-line on the earth's surface where the changes of strain would at times become too great to be resisted, and submarine fractures would follow.

But if water merely finds its way beneath the crust into cavities communicating with the open air, or, indeed, with the ocean-waters outside, no very great disturbances could be produced by the conversion of this water into steam; for the steam would find ready egress, in one case by passing directly into the air, in the other, by rising through the water in the form of large steam-bubbles. It must be by the closing up of fissures as much as by the formation of fissures that the alternations and irregular variations of pressure do their most destructive work. When water has found its way into some widely extending cavity beneath the crust, so long as it is converted gradually into steam, passing away as fast as it is formed, no serious harm can happen. But when, owing to movements of the crust, waters under the earth are imprisoned, and then turned into steam at high pressure, we have the elements of most active disturbance. The imprisoned steam probably forces its way at first into widely ranging cavities beneath the crust. As more and more is generated, the subterranean regions occupied by steam become larger and larger. Internal barriers are broken through, with premonitory noises and rumblings, telling how the imprisoned vapour is gathering its forces. When there is no room for further extension, the continual generation of steam adds steadily to the pressure. If all this happens in the neighbourhood of a volcanic crater, the steam eventually forces its way through, and an eruption of greater or less energy takes place. But if there is no possibility of escape in that way, the internal disturbances continue, become more and more active, and eventually break their way through stronger subterranean barriers than they had before overcome, so passing into larger cavities, and perhaps to regions whence the imprisoned steam can pass away. This process cannot but be accompanied by earth-shakings of greater or less energy according to the strength of the internal barriers thus broken through. And probably the passages of escape thus formed only remain open while the pressure from the region of chief disturbance is very great. As the pressure diminishes, the barriers close again till fresh forces are brought to bear on them. And so shock succeeds shock until at length the region of disturbance has been relieved from excessive pressures, after which for a long time there may be rest.

We can understand, then, why the sea-shore should be the region of chief disturbance, and the fluctuations of oceanic pressure among the most potent disturbing forces. We can understand also how it has come to pass that nature seems "to have provided," as a modern writer puts it, "against the inroads of the ocean by setting the earth's upheaving forces where they were most wanted." As usual in such cases, we find that nature's apparent purpose is in reality a result of direct causation. The forces at work in removing the upraised parts of the earth's crust along shore-lines are the very forces which, working in another direction, cause the earth's crust to be raised along the shores, or, at any rate, so changed in position that the amount of land surface remains practically unchanged. In this sense the remark I have just quoted is scarcely more intelligent than that of the old lady who was enthusiastic about nature's wisdom and beneficence in making rivers run beside towns; but as a recognition of the constant action and reaction at work in this particular field, as in others, of nature's workings, the remark is sensible enough. The crust has yielded along particular lines, *therefore* there the seas are at work upon the upraised shore-line, and in turn the regions thus undergoing encroachment are those also where the subterranean energies necessary to repel the attacks of the sea are most readily developed. Or, putting the case the other way, *because* the earth has yielded along these lines, there lie the shores of the great deep, and there the sea-waves beat upon the capes, headlands, and cliffs which mark where the crust of the earth gave way.

But though we have drawn a step nearer to the true cause of earthquakes in passing from the changes of water pressure to the introduction of water beneath the surface and its conversion into steam, we have yet another step to make. Whence comes the heat by which the water is vaporized and other changes produced which—though probably in a less degree—have their part to play in producing earthquakes? It used to be supposed that this question was sufficiently answered by referring to the earth's internal heat. But in reality it is the earth's internal heat we have to explain, or rather we have to explain how it is that now after millions of years, during which the earth's store of internal heat has been drawn upon, it still remains so great even near the outer surface. What maintains the earth's internal heat?

The answer is that this heat is maintained, especially in the outer layers of the earth's crust, by the process of contraction which goes on all the time under the action of terrestrial gravity.

There are three stages of this process of contraction, two of which are past, while the third is in progress. First, the crust of the earth, still intensely hot, shrinks more quickly than the central mass, because exposed more freely to the cold of outer space. The crust continually deepens, too, besides shrinking as a whole. In this process the reaction of the central mass must cause the crust to give way along vast fissures, which are presently filled up by the inrush of molten matter from within. Next comes the stage when the central mass shrinks from the enclosing crust, still plastic enough to follow it bodily, forming, in so doing, series of wrinkles or corrugations — the mountain ranges of the earth. Lastly comes the stage when the crust yields chiefly in certain places, varying with the progress of time, and when the resulting process of contraction leads to the generation of intense heat under those places, and the consequent occurrence from time to time of eruptions, earthquakes, and other forms of subterranean disturbance. It has been shown by Mallet in England, and by Sterry Hunt and Dana in America, that the process of contraction amply suffices to account for all the heat indicated during these convulsive throes within the earth's crust.

In order rightly to understand how the process of contraction acts, we must consider what the earth's crust actually is (so far as can be judged), and what the probable nature of the region below the crust. If we regarded the crust as a rigid shell, and considered its strength to be such as its vast size and great thickness seem at first sight to suggest, we might well fail to comprehend how the crust can possibly be affected by any process of contraction. I have already pointed out that the extent of the crust, and even its thickness, mean weakness, not strength. But it is not till we recognize how absolute this weakness is that we can understand the real nature of the work going on underneath. If I were to say that the earth's crust has no supporting power *at all*, I might seem to be pronouncing the most utterly paradoxical opinion that can be imagined. The earth—and when we speak of the earth we mean really the earth's crust—seems the most appropriate emblem of stability. The earth's crust supports the most massive buildings man can erect upon it, and (which means much more) the earth's crust supports the everlasting hills, the great mountain ranges, whose summits range six miles above the sea-level, which is far from the lowest level of the earth's solid surface. Yet the crust has so little real supporting power, so little real rigidity, that practically it may be said to support nothing, except in the sense in which, without stability or rigidity, the sea surface supports the stately ship. A bridge is said to have supporting power, because a weight placed on the bridge is sustained above the surface which the bridge spans ; a cloth on a table is not said to have supporting power, because, though heavy weights may stand upon it, their pressure is transmitted undiminished to the solid surface of the table. In one sense, of course, the table itself has no supporting power, for it transmits pressures to the floor, and thence to the earth. In like manner the bridge transmits pressures to its piers, and thence earthward. But the table and the bridge have that kind of supporting power which depends on relative rigidity; they transmit the pressures in altered directions. The cloth is without rigidity, and does not appreciably alter the directions of pressure. The earth's crust resembles the cloth in this respect. The pressures resulting from the masses apparently supported by the earth's outer crust are transmitted directly to the regions below. To the very centre of the earth, probably, all pressures are transmitted with scarcely any change, insomuch that the centre of the earth, where gravity vanishes, is the place where pressure attains its maximum amount.

It is this absence of rigidity in the earth's frame, regarded as a whole, which causes the process of contraction to be so effective an agent in generating heat. Pressure results in compression, and compression forcibly produced generates heat. But here arises a difficulty which many find confusing enough. It is a principle in physics that where work is done, heat is lost, and it seems as though a process of compression, due to the action of gravity, being a process in which work is done, must be one in which heat is lost instead of gained. The work is done, however, *upon* the matter compressed, not *by* it, and so the compressed matter gains the heat which corresponds to the work done, instead of losing it. Work is done when matter expands, but this work is done by the expanding matter, and is accompanied, therefore, by loss of temperature. In reality a process of contraction may be said to involve the employment of a certain amount of available work. If one imagines the state of things before contraction to be the result of a withdrawal of the matter to be acted upon by gravity to a greater distance from the centre of gravity, then contraction means the undoing of that work; and as when work is done heat is lost, so when work is undone heat is gained. A thousand examples in nature might be cited to show how constant is the operation of this law. Work is done and heat is employed in raising from the sea the vapour which eventually as rain supplies the great lake region between Canada and the United States. This store of work is drawn upon where Niagara (in rapids and falls alike) restores a portion of the raised water to lower levels, and heat results from this undoing of nature's former work. Or, where man chooses, he gets work from Niagara instead of heat, the work done in driving machinery being the equivalent of just such work as heat can be made to do when employed to drive engines of various forms. And so in multitudes of other instances.

Now the example just cited affords a suggestive illustration of the tremendous energies residing in the contractive power of the earth. Indeed, I have always found in this suggestion the most impressive effect of the Niagara Falls on my own mind. We see terrestrial gravity at its work at Niagara, because there it has work to do on such a scale as to afford some idea of the real meaning of gravity, yet within such compass that we can grasp the idea of the work that gravity is doing. To think that a portion only of the rain-fall which supplies the lake system of North America, drawn downward continuously by the force of gravity, should produce this ceaseless noise and turmoil, suggests how greatly we may be deceived respecting the forces of nature, for gravity is constantly doing work which we scarcely notice, yet which is so vast in amount that all the work done at Niagara is nothing by comparison. To the mere accident (in a sense) that the water raised from the seas has here fallen on upraised regions instead of on the lower levels, to the mere difference of height between the places on which they fall and the sea-level from which the sun's heat raised them, we owe the tremendous forces represented by the Niagara Falls and Rapids. But we must go farther before we see the real meaning of such processes, or therefore of the much more energetic processes of which I simply take Niagara as a convenient illustration. The clouds which float in the air over the lake region contain within them potential energies enormously exceeding all the forces at work in Niagara. A small portion only of these energies is concentrated at Niagara into the tremendous exhibition of force which is so impressive—nay, so appalling—to all who stay long enough near Niagara to apprehend its significance aright. Now the clouds represent work done by heat. The falls and rapids represent the undoing of the work so done, gravity undoing the work which has been done upon parts of the earth's material by forces external to the earth—those, namely, which reside in the rays of the mighty sun.

Finding in the processes of contraction taking place continually within the earth's crust the sources of the heat by which water reaching the interior is converted into steam and other disturbing changes are produced in subterranean regions, we are brought to recognize in terrestrial gravity the real cause of all forms of subterranean disturbance. We had already recognized the pressure, and especially the changes of pressure, of air and water as effective disturbing causes, and these are directly due to gravity. Now we find, further, that to gravity is due the internal heat by which matter beneath the crust is changed from a state of quiescence to a state of activity.

Directly and indirectly all the forms of disturbance by which the earth's crust is affected are due to gravity, yet not all, be it observed, to terrestrial gravity. For in some of the changes affecting the atmosphere and the ocean we recognize the power of solar heat, the cause of all atmospheric changes, of rain-fall, of the action of frost and thaw in disintegrating the earth's crust: and solar gravity is the cause of solar heat. The same force raises two-sevenths of the tidal wave. Lunar gravity, again, raises the remaining five-sevenths of the tidal wave. All subterranean activity is due, then, to gravity in one form or another.

Thus finally we recognize that the true cause of terrestrial disturbances is that most mysterious of all the properties of matter, the force of attraction. We speak glibly of gravity as explaining what had seemed inexplicable before the law of gravity was recognized. We tell how when "nature and nature's laws lay hid in night, God said, Let Newton be, and all was light." But how much more profound the mystery revealed than the mystery removed! There is naught in all that science has disclosed to man more utterly—one might say more hopelessly—mysterious than that power by which in an instant, throughout the whole universe, matter acts on matter. We seem here to stand in the very presence of the Godhead, for it seems as though were but this last veil lifted, and the mystery of gravity removed, we should see revealed the great first cause of all phenomena. All the energies of the universe, Light, Heat, nay, Life itself, have their origin in this mysterious quality of matter —a quality so inconceivable that the very philosopher who discovered it, or first recognized its meaning, asserted that no man with competent power of philosophizing could for a moment believe such a power to exist as gravity seems to be, or that matter *can* act on matter at a distance without some intermediary. But passing from a mystery which may never be explained, we recognize in gravity's work on the earth's crust an agency which, though it appears at a first view to be a destructive one, is in reality a source of life. For were the work of terrestrial gravity in this direction to cease, solar gravity, acting by its heat-generating power on the waters of the earth and the air, would in the course of time, through the action of rain and river, of wave and of wind, level all the upraised parts of the earth beneath the seas. But the earth's gravity constantly renovates the earth, making it present, for periods of time which seem endless, those varieties of land and water which are essential to the existence of the forms of life now existing upon the surface of our planet home.

CHAPTER XX.

PARENTS AND CHILDREN.

A NOTE ON THE LAW OF HEREDITY IN ITS RELATION TO THE TRAINING OF CHILDREN.

THE principle of heredity has now been generally accepted, but many of the conclusions which follow from it seem to have been little thought of by many of those who most fully accept the principle itself. Amongst the most important of these may be mentioned the changed view of parental duties and parental responsibilities which presents itself when we consider how the character of each child born into the world depends almost wholly on influences derived from the child's parents. In old times men's ideas respecting the training of children, the duties of children to their parents, and the duties of parents to their children, were comparatively simple. The child's mind was regarded as a blank page on which anything could be written that the child's teachers might desire. The child's good qualities were regarded as involving merit which deserved reward; the child's bad qualities were regarded as involving offence which deserved punishment. The duty of the child to its parents was held to be very distinct and definite, while, beyond the duty of maintaining the child, the parent's responsibility, according to old-fashioned ideas, was limited to the inculcation of moral and social duties (by precept rather than by example), and the employment of a system of rewards and punishments for the development of the child's good qualities and the correction of its bad ones.

All this is practically very little changed, though the absurdity and unfairness of the old system have been demonstrated over and over again in recent years. Children are not only unlike, instead of being like, as the old system implied, but unlikeness is their most striking characteristic. Instead of its being probable that a well-chosen system of training will suit ninety-nine children out of a hundred, the chances are that no system of training could be devised which would really be suitable for any two children out of a hundred. The children of the same family differ strangely from each other. Though all their qualities are derived from the same source, the proportions in which these qualities have been received are so different that, as a rule, no two children, even in a large family, are closely alike in character. If this is so in one and the same family, as every one who has observed such families must have noticed, how absurd must be the attempt to select any system of training which shall suit scores of boys or girls (or, as in America, of boys *and* girls) belonging to different families. With a recognition of the laws of heredity, the old-fashioned system of training ought in this respect to have been entirely altered.

But not only has the position of the trainer and teacher of children been altered with the new lights under which character now presents itself, but the relations of the child to the parents and of the parents to the child have been entirely altered. In all the various stages of a child's life from babyhood to manhood or womanhood, the character, however much it may change naturally or be affected by external influences, is in the main a product of development. It is as hopeless to apply a system of rewards and punishments to modify the essentials of character, at any stage of child-life, as it would be to attempt to alter by an elaborate system of watering or manuring the fruit of the pear-tree into the fruit of the walnut-tree, or *vice versâ*. Moreover, the character of the child at the different stages of child-life is very differently related to the parental character. In early childhood the character is not only remoter from the parental type, but partakes even of many of the characteristics of animal types. The very young child is in reality wanting in most of the essential attributes of human types of character, in most of the features which distinguish man from the lower animals. A baby is an engaging animal, but still it is little more than an animal. It cannot be said to reason, more at any rate than a clever dog or monkey seems to reason. It has no distinct ideas of right or wrong. It has appetites and wants, and nearly all that it does is ruled by those appetites and wants—at first almost wholly, later with such limitations as are suggested by the effects of experience more or less consciously acted upon. The system of training appropriate at this stage of child-life—in fact, the only system available—is akin to the system of training used for animals. The tender nurse and the loving mother may object to this statement, but she acts on this principle. Moreover, a parent can more fairly act in this way to the very young child than to one that begins to show peculiarities of character more nearly approaching those of either parent, or of others of the child's near kindred.

Parents can hardly feel responsible for those faults of character in the baby which, according to the principles of heredity, have not been directly handed down to the infant by them, but belong to much more remote progenitors. Similar remarks apply to the following stage of early childhood, the stage when the child resembles in character the savage rather than the mere animal. It is at these two stages, chiefly, that the old-fashioned system of training can alone be adopted, though even at those early stages discrimination is required, because of the different degrees in which animal or savage peculiarities of character are recognized. Some babies are good little animals, though they have animal-faults which require correction ; others, on the contrary, are bad little animals, and require for their own good (and even for their own safety) a severer system of treatment. So with young children a stage or so later. Some are very pleasant little savages, though they have some savage tricks which must not be encouraged, but checked ; others are terrible little barbarians, and unless ruled with a rather strong hand will do mischief to others, and (probably) still more serious mischief to themselves.

For these earlier stages of child-life, a system of training, and, where necessary, of control and even severity, has to be adopted ; and the only considerations to be attended to in selecting the most appropriate measures are those depending on the individual traits of character observed at this stage of the growing child's life. At this time it may sometimes happen that the old-fashioned system of severity—the old-fashioned doctrine that he who spareth the rod hateth his child—may be unfortunately appropriate.

Even in the animal and savage stages of a child's life, however, gentleness and kindness are nearly always better than sternness and severity. Nearly always it is the weakness of the parent rather than the fault of the child which calls for correction, though correction falls on the child, not on the parent. The child, seeing examples of ill-temper and obstinacy, falls into obstinate and ill-tempered ways, and is presently punished, more because its faults excite anger than because, when wisely considered, they are held to require such correction as may lead to their being gradually eliminated from the character.

It would be difficult to say what proportion of the faults of manhood have their origin at this state of life, because the faults then springing into existence are afterwards commingled with those inherited from the parents or through the parents. But there can be little doubt that for want of patient and judicious training, and occasional correction, erring rather on the side of pity than of severity, many characters are seriously impaired before the inherited traits have begun to show themselves with any degree of distinctness.

It is, however, later in life, in boyhood and girlhood, young manhood and young womanhood, that we recognize the more difficult part of parental training. Many parents, indeed, nay most, overlook the special considerations to which they ought to attend, now that the development of the law of heredity has made the origin of individual peculiarities of character clear; but this does not affect the argument. In every family we see, at one or another part of the child's life, the faults and good qualities of the parents or of other near relations showing themselves with greater or less distinctness. Faults may be so punished that the child conceals them: yet they are there. It very seldom happens that they are not at some time or other shown, in such sort that the parents can see what manner of man or woman the child will grow up to be.

Now here a very difficult question of responsibility and duty presents itself. A father, we will say, recognizes in his child a fault which he knows to be inherited both from and through himself—in other words, what is called a family failing. The consciousness that he himself has the fault does not in any degree diminish the annoyance caused by it; rather the reverse, seeing that faults in others are all the more provoking if we are ourselves liable to them.

But the right to punish and the duty of punishment are curiously affected by consideration of the hereditary nature of the fault. I am, let us say, prone to violent fits of temper, or to moroseness, or to obstinacy; some fine day, a son or daughter of mine exhibits, in a marked degree, the same failing, which I know to be mine, which I know I have inherited, and which I equally know I have transmitted. I know that in his or her career my child will suffer from the effects of this family failing, unless every pains be taken either to eradicate it or to bring it under mastery, making of it a servant instead of a tyrant.

Of old, my course would have been clear enough, though painful. I should have felt it my obvious duty to use correction of such degree of severity—and no more—as was necessary to compel my child to master his fault of temper. (Of course I am considering here the case of a parent who recognizes his duty in such matters; one who does not would probably thrash his son or punish his daughter in such severe ways as might occur to him, with no other object but to get rid of the annoyance caused by the child's fault.) But when the parent recognizes the fault of disposition or of temper as in reality his own, though manifested by the child, the position becomes difficult and painful. A parent may be obliged by a sense of duty to punish his own fault in his child, being all the while conscious that for the existence of the fault in the child he is himself responsible.

It is easily seen, too, that in the case of far-seeing persons (those farthest removed from the savage state), the sense of duty, or rather the feeling of doubt and difficulty in this matter, would extend further. The father who, knowing that some fault of temper has been a source of sorrow or misery to himself feels that, let it have come how it may (not that he has any doubt whence it came), this fault in his child must be corrected, might very well consider that, since children could only be born to him at the risk of inheriting this source of sorrow and misery, it would not be well that children should be born to him at all. The old argument against Malthusian doctrines, that a child born into the world may possibly become one in the choir of heaven, singing God's praises everlastingly, so that all doctrines by which the number of such children may be diminished (as by late marriage, &c.) are sinful, might be met, even in the general case, by the answer that a child born into the world may possibly have a quite different future; but in particular cases the probabilities are so enormously against the happier, and in favour of the less happy, fate, that the argument (*if it is worth anything at all*) might be applied very effectively, at any rate, against early marriage. Be this, however, as it may, it is evident that due consideration of the doctrine of heredity should lead a parent, who recognizes his own faults in his offspring, to be very careful and tender, however earnest, in his endeavour to eradicate such faults. When I hear of, or see parents harshly punishing their children for faults which they must *know* that they themselves possess and have transmitted to their offspring, I am inclined sometimes to wonder whether they will be able to look their children in the face in after years, when the real origin of such faults has become as clear to the younger as to the older possessors of them. If the younger were not, happily, much more forgiving as a rule than their elders, how much might the peace of families be disturbed in after years by the recollection of past severities inflicted by parents on children for faults which would have had no existence but for the parents, and which the parents show in at least as marked a degree as their offspring. And again, how singularly would the lives of young people be affected if parents considered carefully the precept, "Let him that is without fault," &c.

CHAPTER XXI.

LIVING DEATH-GERMS.

THE conquests made by science are varied in character, sometimes seeming to promise a domain more hurtful (on the whole) than fruitful; a sort of intellectual Afghanistan. In other cases a land of promise seems before us, but the way to it is not clear. As an instance of the former kind may be mentioned the progress which science is making in the study of explosive substances and the recognition of their power. Of the latter kind no more marked instance could be cited than the researches of Pasteur and others into the nature of the germs of various diseases, and the power of cultivating these germs so that their character may be modified.

Let us for a moment suppose it proved (though at present we have only promise of proof) that the disease-germs which produce vaccinia (the disease—if so it can be called—following vaccination) are the same in species as those which produce small-pox, but that during the residence of those germs in the heifer their power has undergone a certain modification which renders them innocuous while yet they produce that particular change which results in what we call protection from small-pox. Then it would follow, as as least highly probable, that in the case of any other illness produced by living germs, we may learn how the disease-germs can be so cultivated as to lose their power for serious mischief, while retaining the power of producing protective ailment akin to the more dangerous illness produced by the unmodified germs. So that typhus, scarlet fever, diphtheria, and a host of other ailments, which are more or less certainly known to be due to the presence of living organisms in the blood or tissues, would be treated as we now treat small-pox. People inoculated with the specific "matter" for each of these diseases, once perhaps in every six or seven years, would be safe from them, or safe at any rate from severe attacks. Epidemics of such diseases would be rendered almost impossible; but, when they occurred, sensible people could find protection even as they now find protection from an epidemic of small-pox. Of course there would follow effects similar to those which have led many to imagine that vaccination has done more mischief than good, because so many weakly lives which would otherwise have succumbed to the unmodified disease would be saved. Just as in a race of warlike savages the type is improved by the constant weeding out of the weaker in battles and through the hardships of campaigning, so in a people exposed to many dire forms of disease the stronger only survive, and the race seems improved. But precisely as men of sense would object to see their nation improved in physique by the thinning out resulting from constant wars, so should they advocate every method by which the action of the more fell diseases may be modified, even at the risk of the survival of many weaker members who would otherwise have been weeded out by disease.

This, then, is the promised, or rather suggested, future,—protection for those who are wise enough to accept protection, possibly even compulsory protection, from those diseases which now produce so much misery and sorrow. Let us see how the matter stands, examining the evidence by experiments made on creatures of comparatively smaller worth, and, be it noted, not made on them that man alone may gain, but directly for the protection of the lower animals from disease.

Let us take first a disease which has been proved to be produced by living germs,—by creatures capable of reproducing their kind, so that once a suitable abode is found, their numbers may increase until they kill their unwilling host.

In the twenty years ending 1853 the silk culture of France had more than doubled, and there seemed every reason to believe that it would continue to increase for many years to come. The weight of the cocoons produced in 1853 amounted to no less than 52 millions of pounds. But on a sudden the aspect of affairs changed. A disease appeared which rapidly spread, and in a little more than half the time during which the silk culture had doubled, it was reduced to less than the sixth part of its amount in 1853. In 1865 the cocoons only weighed eight millions of pounds. The loss in revenue, in this single year, amounted to four million pounds sterling.

The disease which had produced these disastrous results has received the name of *Pébrine.* It shows itself in the silkworm by black spots (whence the name). When it is fairly developed the worms become distorted and stunted, their movements are languid, their appetites fail them, and they die prematurely. But the disease does not necessarily become fairly developed in the worm. On the contrary, it may be only incipient during this stage of the silkworm's life. The worm may even produce a fine cocoon. Yet the disease incipient in the worm will be developed in the moth, and the eggs produced by the diseased moth will be diseased too!

It was in 1849 that the characteristic feature of the disease was first recognized. In that year

Guérin Méneville noticed small vibratory bodies in the blood of silkworms. It was shown that the vibrations were not due to independent life; and the error was made of supposing that the corpuscles belonged to the blood of the worm. In reality they are capable of indefinite multiplication. They are the real germs of the disease. These living bodies "first take possession of the intestinal canal, and spread thence throughout the body of the worm. They fill the silk cavities," says Tyndall, "the stricken insect often going automatically through the motions of spinning, without any material to work upon. Its organs, instead of being filled with the clear viscous liquid of the silk, are packed to distension by the corpuscles."

The case of the silkworms may be regarded as closely similar to that of a nation attacked by plague or pestilence. If anything, the case of the silkworms seemed even more difficult to deal with. At any rate, no plague which has fallen on man ever gave rise to so many suggestions for the remedy of the mischief. "The pharmacopœia of the silkworm," wrote M. Cornalia, in 1860, "is now as complicated as that of man. Gases, liquids, and solids have been laid under contribution. From chlorine to sulphurous acid, from nitric acid to rum, from sugar to sulphate of quinine, all has been invoked in behalf of the unhappy insect." "Pamphlets were showered upon the public," says Tyndall; "the monotony of waste paper being broken at rare intervals by a more or less useful publication." The French Minister of Agriculture signed an agreement to pay 500,000 francs for a remedy, which, though said by its inventor to be infallible, was found on trial to be useless.

It was when matters were in this state that Pasteur was invited by Dumas, the celebrated chemist, to investigate the disease. Pasteur had never even seen a silkworm, so that it was not because of any special experience in the habits of the creature that Dumas considered him likely to achieve success where so many had failed. Yet he attached extreme importance to Pasteur's compliance with his request. "Je mets un prix extrême," wrote Dumas, "à voir votre attention fixée sur la question qui intéresse mon pauvre pays; la misère surpasse tout ce que vous pouvez imaginer." For it was in Dumas's own district that the disease prevailed most terribly.

Pasteur first studied the worm at various stages of its life. Most of our readers are doubtless aware of the nature of these stages; and doubtless many have had practical experience, as we have, of the ways of the creature as its life progresses. First the eggs, neatly arranged by the mother moth on some suitable surface provided by the worm-keeper, are watched until in due course comes forth a small dark worm. This grows, and as it grows casts its skin three or four times, becoming lighter at each such moulting. After the last moulting the worm has its characteristic white colour. It continues to grow (feeding on mulberry-leaves), until, the proper time having arrived, it climbs into whatever suitable place has been provided for it (silkowners use small brambles, but our schoolboys use little paper cups) and there spins its cocoon. When this is completed and the silk has been wound off, the chrysalis is found inside, which becomes a moth, and the moth laying her eggs, the cycle is recommenced.

It was Pasteur who showed that the disease germs might lurk in the egg, or might first appear in the worm, and in either of these stages might escape detection. But the destructive corpuscles in the blood grow with the growing worm. In the chrysalis they are larger than in the full-grown silkworm; and finally, in the moth (assuming the germ to have begun either in the egg or the young worm) the corpuscles are easily detected. He therefore said that the moth and not the egg should be the starting point of methods intended for the destruction of the seeds of disease. For in the egg or the young worm the germs might escape detection; in the moth, he affirmed, they could not.

When Pasteur, in September, 1865, announced these views, physicists and biologists agreed in rejecting them. He was told he knew nothing about silkworms, and that his supposed discoveries were old mistakes long since shown to be such.

He answered by the simple but impressive method of prediction. Parcels of eggs, regarded by their owners as healthy, were inspected by him, the moths which had produced them being submitted to his examination. He wrote his opinion in 1866, placing it, in a sealed letter, in the hands of the Mayor of St. Hippolyte. In 1867 the cultivators communicated their results. Pasteur's letter was opened, and it was found that in twelve cases his prediction was fulfilled to the letter. He had said that many of the groups would perish totally, the rest almost totally; and this happened in all except two cases, where, instead of almost total destruction, half an average crop was obtained. The owners had hatched and tended these eggs in full belief that they were healthy: Pasteur's test applied for a few minutes in 1866 would have saved them this useless labour.

Again, two parcels of eggs were submitted to Pasteur, which, after examination of the moths which had produced them, he pronounced healthy. In their case an excellent crop was produced.

Pasteur carefully investigated the development of the disease germs. He took healthy worms by 10, 20, 30, 40, and 50, and placed matter infected with the germs on their food. "Rubbing a small diseased worm in water, he smeared the mixture," says

Tyndall, "over mulberry-leaves. Assuring himself that the leaves had been eaten, he watched the consequences from day to day. Side by side with the infected worms he reared their fellows, keeping them as much as possible out of the way of infection. On April 16, 1868, he thus infected thirty worms. Up to the 23rd they remained quite well. On the 25th they seemed well, but on that day corpuscles were found in the intestines of two of them. On the 27th, or eleven days after the infected repast, two fresh worms were examined, and not only was the intestinal canal found in each case invaded, but the silk organ itself was charged with corpuscles. On the 28th the twenty-six remaining worms were covered by the black spots of pébrine. On the 30th the difference of size between the infected and non-infected worms was very striking, the sick worms being not more than two-thirds of the bulk of the healthy ones. On May 2 a worm which had just finished its fourth moulting was examined. Its whole body was so filled with the parasite as to excite astonishment that it could live. The disease advanced, the worms died and were examined, and on May 11 only six out of the thirty remained. They were the strongest of the lot, but on being searched they also were found charged with corpuscles. Not one of the thirty worms had escaped; a single meal had poisoned them all. The standard lot, on the contrary, spun their fine cocoons, two only of their moths being proved to contain any trace of the parasite, which had doubtless been introduced during the rearing of the worms."

He examined the progress of infection still more carefully, counting the number of corpuscles, which, as the disease increased, rose from 0 to 10, to 100, and even to 1,000 or 1,500, in the field of view of his microscope. He also tried different modes of infection. "He proved that worms inoculate each other by the infliction of visible wounds with their claws." He showed that by the simple association of diseased with healthy worms the infection spread. He demonstrated, in fine, that "it was no hypothetical infected medium—no problematical pythogenic gas —that killed the worms, but a definite organism."

Thus did Pasteur teach the worm-cultivator how to extinguish the pestilence which had destroyed his egg crops. The plans for extirpating the diseased worms had failed before his researches, for the very sufficient reason that no sufficient means had been devised for distinguishing the diseased from the healthy. As Pasteur himself stated the matter— "the most skilful cultivator, even the most expert microscopist, placed in presence of large cultivations which present the symptoms described in my experiments, will necessarily arrive at an erroneous conclusion if he confines himself to the knowledge which preceded my researches. The worms will not present to him the slightest spot of pébrine; the microscope will not reveal the existence of corpuscles; the mortality of the worms will be null or insignificant; and the cocoons leave nothing to be desired. Our observer would, therefore, conclude without hesitation that the eggs produced will be good for incubation. The truth is, on the contrary, that all the worms of these fine crops have been poisoned; that from the beginning they carried in them the germ of the malady, ready to multiply itself beyond measure in the chrysalides and the moths, thence to pass into the eggs and smite with sterility the next generation. And what is the first cause of the evil concealed under so deceitful an exterior? In our experiments we can, so to speak, touch it with our fingers. It is entirely the effect of a single corpusculous repast; an effect more or less prompt according to the epoch of life of the worm that has eaten the poisoned food."

His plans for the elimination of diseased worms, and for the isolation of the healthy from contagion in any possible form, met with full success. The disease has not been eradicated, because the silk-producing districts cannot be completely isolated; but its ravages have been so far reduced that the cultivation of silk promises soon to reach something like the position which had been hoped for before the disease had shown itself.

Now between the ideas which had prevailed respecting pébrine before Pasteur's researches, and those which still prevail respecting many contagious diseases, there is a striking analogy. Just as Pasteur was assured by many experienced silk-growers that the disease was due to some deleterious medium, rendered more or less poisonous at different times by some mysterious influence, so epidemic diseases, we are assured by many experienced medical men, are due to occult influences arising spontaneously in foul air. It matters not that as certainly as an animal produces creatures of its own kind, and not of some other kind, so the poison of one fever produces always that fever, and not some other fever. In this they find no evidence of anything akin to what Dr. Budd has called parentage. The followers of Pasteur in the silk districts, and those who have benefited by others of his researches, presently to be described, would as soon believe in the spontaneous generation of pébrine and kindred diseases as in the spontaneous generation of cats and dogs. But many still believe respecting diseases affecting the human race in which precisely the same phenomena of reproduction are presented, that they arise from some spontaneous fermentation (unlike every form of fermentation on which experiments have yet been made).

But before we pass to consider other and even more decisive evidence, we may note that, so far as the researches of Pasteur on pébrine are concerned, we have not yet seen the way to any means of safety from the contagious diseases which affect human beings. We cannot kill all diseased persons in order that we may get rid of the disease-germs within them.

Even more remarkable than his investigation of the silkworm disease was Pasteur's investigation of the disease known as splenic fever, which affects horses, cattle, and sheep on the continent. In the rapidity of its action this disease (known also as "anthrax," and "charbon") resembles the black plague. In bad cases death ensues in the course of twenty-four hours. In less severe cases the creature attacked suffers greatly, and retains the traces of the attack during the rest of its life. It is stated that between the years 1867 and 1870 no less than 56,000 deaths occurred among horses, cattle, and sheep in the district of Novgorod, in Russia, while 568 human beings perished, to whom the disease had been somehow communicated. In France the disease is very prevalent, and many proprietors have been ruined by the entire destruction of their flocks and herds. It is said that a malady which occurs among the woolsorters at Bradford (often proving fatal) is a modification of anthrax communicated by the wool of sheep which have suffered from splenic fever.

In 1850 MM. Rayer and Devaine discovered minute transparent rod-like bodies in the blood of animals which had suffered from this disease. Koch, a German physician, then scarcely known, showed that these objects are of a fungoid nature, and traced the various stages of their existence. Cohn obtained similar results, as did Ewart in England. The growth of the disease-producing rods, as studied under microscopic examination, is as follows :—First, germs of extreme minuteness are seen in the form of simple tubes with transverse divisions; next, minute dots appear, which enlarge into egg-shaped bodies lying in rows within the tubes; lastly, the rods break up, freeing the ovoid germs. It has been shown that "the minutest drop of the fluid containing these germs, if conveyed into another portion of cultivated fluid, initiates the same process of growth and reproduction; and this may be repeated many times without any impairment of the potency of the germs, which, when introduced by inoculation into the bodies of rabbits, guinea-pigs, and mice develop in them all the characteristic phenomena of splenic fever. Koch further ascertained," continues Dr. Carpenter, from whom the above passage is quoted, "that the blood of animals that succumbed to this disease might be dried and kept for four years, and might even be pulverized into dust, without losing its power of infection."

Pasteur's first steps in inquiring into this disease were characterized by the same keenness of judgment which he displayed in investigating *pébrine.* He ascertained that "charbon" would often appear in its most malignant form among sheep feeding in seemingly healthy pastures, where there were no known causes of infection. He found on inquiry that animals which years before had died in those regions had been buried ten or twelve feet below the surface, so that it seemed obvious they could have had nothing to do with the reappearance of the malady. But in inquiries such as these Pasteur has taught us that what obviously *cannot be* has an unfortunately perplexing fashion of turning out to be precisely what *is.* He quickly became persuaded that in some way the germs of disease supposed to be buried out of the way three or four yards beneath the soil reached the surface and originated fresh attacks of the "charbon" pestilence. He found in earth-worms—those creatures which Darwin has recently shown to be such important workers in the earth's crust—the cause of the trouble. He was ridiculed, of course. But he has a troublesome way of turning ridicule upon those who laugh at him. Collecting worms from pastures where the disease had reappeared, "he made an extract of the contents of their alimentary canals, and found that the inoculation of rabbits and guinea-pigs with this extract gave them the severest form of 'charbon,' due to the multiplication in their circulating current of the deadly anthrax-bacillus" (this is the pleasing way science has of describing the disease germs), "with which their blood was found after death to be loaded."

Our countryman, Professor Brown Sanderson, discovered another way in which "anthrax" has been communicated. He found that herds affected with it had been fed with brewers' grains supplied from a common source, "and on examining microscopically a sample of these grains, they were seen to be swarming with the deadly bacillus, which, when once it has found its way among them, grows and multiplies with extraordinary rapidity."

But now comes the point which renders this inquiry important to ourselves. The poison germs are small, visible only in the microscope, but they are fungoid, and the laws of their growth and development are as determinable (with suitable care) as the laws of the growth and development of the monarchs of a forest. Now whatever lives and grows and produces creatures after its own kind, whether animal or vegetable, can be cultivated. With due care and watchfulness it may be altered in type and character, just as the wild plants of the hedgerow may be altered into plants producing the flowers and fruits of our gardens and hothouses. The methods of cultivation are not precisely the same, because as yet microscopists do not know how to select the less from the more destructive germs, so as to propagate from the former only. But, as Dr. Carpenter puts the matter, two modes of "culture" suggest themselves: first, "the introduction of the germs into the circulating current of animals of a different type, and its repeated transfusion from one animal into another;" and secondly, "cultivation carried on out of the living body, in fluids (such as blood-serum or meat-juice) which are found favourable to its growth, the temperature of the fluid, in the latter case, being kept up nearly to blood-heat. Both these methods have been used by Pasteur himself and by Professor Burdon Sanderson; and the latter especially by M. Toussaint of Toulouse, who, as well as Pasteur, has experimented also on another bacillus which he had found to be the disease-germ of a malady termed 'fowl cholera,' which proves fatal among poultry in France and Switzerland. It has been by Pasteur that the conditions of the mitigation of the poison by culture have been most completely determined; so that the disease produced by the inoculation of his 'cultivated' virus may be rendered so trivial as to be scarcely worth notice. His method consists in cultivating the bacillus in meat-juice or chicken-broth, to which access of air is permitted while dust is excluded; and then allowing a certain time to elapse before it is made use of in inoculation experiments. If the period does not exceed two months the potency of the bacillus is little diminished; but if the interval be extended to three or four months, it is found that though animals inoculated with the organism take the disease, they have it in a milder form, and a considerable proportion recover; whilst if the time be still further prolonged, say to eight months, the disease produced by it is so mild as not to be at all serious, the inoculated animals speedily regaining perfect health and vigour."

Now, if we consider what has been done in this case we shall recognize the probability, if not the absolute promise, of protection being obtained against some of the most terrible of the diseases which affect the human race. We see that in some cases, at any rate, the germs of a deadly disease may be so "cultivated" that the disease, though communicable by the altered germs, is no longer fatal. Now we know that the milder attacks of scarlet fever, measles, whooping-cough, diphtheria, and other such diseases, produce as completely protective a change in the constitution of the patient as the severest forms short of absolutely fatal attacks. We see, then, that even had no experiments been made to determine whether the disease communicated by cultivated germs is protective, there would be good reason to believe that it is so.

But such experiments have been made. What Pasteur calls the "vaccination" for the "anthrax" disease has been shown by repeated experiments to be absolutely protective. Prof. Greenfield has vaccinated cattle from rodents (gnawing animals like rats, squirrels, &c.) with the "anthrax disease," and has found that they remain free from all disorder, local or constitutional. The same result has attended M. Toussaint's experiments with the bacillus "cultivated" in special fluids, not in the living body of any creature: sheep and dogs inoculated with this cultivated poison showing no form of the deadly "anthrax" disease.

The experiment was conducted on a large scale under the auspices of the provincial agricultural societies of France. A flock of fifty sheep was placed at M. Pasteur's disposal. Of these he vaccinated twenty-five with the cultivated "anthrax" poison on May 3, 1881, repeating the operation a fortnight later. All the animals thus treated passed through a slight illness, but at the end of the month were as well as their fellows, the twenty-five which had not been vaccinated. On May 31 all the fifty were inoculated with the strongest anthrax poison. "M. Pasteur predicted that on the following day the twenty-five which were inoculated for the first time would all be dead, whilst those protected by previous 'vaccination' with the mild virus would be perfectly free from even mild indisposition. A large assemblage of agricultural authorities, cavalry officers, and veterinary surgeons, met on the field the next afternoon to learn the result. At two o'clock twenty-three of the unprotected sheep were dead; the twenty-fourth died an hour later, and the twenty-fifth at four. But the twenty-five 'vaccinated' sheep were all in perfectly good condition; one of them, which had been designedly inoculated with an extra dose of the poison, having been slightly indisposed for a few hours, but having then recovered."

These experiments are important in themselves. The French owners of flocks and herds have now an infallible protection against the deadly "charbon" poison, which had caused serious loss to nearly all of them, and ruinous loss to not a few. But such experiments are infinitely more important in what they promise. If the law which they seem to indicate is general, if every kind of disease-germ can be "cultivated" so as to be deprived of its malignancy, but not of its protective agency, then we may hope to see cholera, diphtheria, measles, scarlatina, and other diseases brought as thoroughly under control as one which formerly was the most deadly of them all—small-pox.

Let us here pause for a moment to consider some inquiries which have been made by two American doctors, Messrs. Wood and Formad, under the direction of the American National Board of Health, into the nature of the poison which is active in diphtheritic epidemics. Read in the light of what Pasteur, Toussaint, and Greenfield have done with diseases affecting the lower animals, the inquiries of Drs. Wood and Formad are full of promise that before long complete protection will be found against the fatal disease diphtheria.

They had shown long ago that shreds of diphtheritic membrane, taken from the throats of human patients and used for the inoculation of rabbits, produced tubercular disease, and also that the false membrane supposed to be characteristic of diphtheria appears as a result of severe inflammation of the trachea, however produced. But now they have found that in every case of true diphtheria the membranes are loaded with minute organisms (micrococci), while the blood and the internal organs of patients dying from the disease are similarly infected. They have ascertained also how these micrococci destroy life. They attack the white corpuscles, or leucocytes in the blood. These lose their form, and eventually burst, giving exit to an irregular transparent mass packed with micrococci. Hence a new and multiplied crop of blood foes, and, with the increased destruction of the white corpuscles of the blood, the destruction of the person in whose veins the contaminated blood flows. They showed also that the disease can readily be communicated artificially from animal to animal. Another fact detected by Drs. Wood and Formad is of extreme importance, as showing how epidemics of diphtheria may be brought about as a development of the malignancy of sore throats not hitherto regarded as akin to diphtheria. They showed that in ordinary sore throats as well as in the diphtheritic sore throat the micrococci are present, differing only in development and activity. In other words, diphtheria may be regarded as due to naturally cultivated micrococci, the cultivation being of such a kind as to increase their destructiveness.

Some experiments by Pasteur illustrate the kind of cultivation just mentioned. "It is not a little curious," writes Dr. Carpenter, "that, as culture of one kind can mitigate the action of the poison germs, so culture of another kind may restore or even increase their original potency. It has been found by Pasteur"—in the case of the "anthrax" or "charbon" poison—"that this may be effected by inoculating with the mitigated virus a new-born guinea-pig, to which it will prove fatal; then using its blood for the inoculation of a somewhat older animal; and repeating this process several times. In this way a most powerful virus may be obtained at will." "This discovery," proceeds Dr. Carpenter, "is not only practically available for experimental purposes, but of great scientific interest, as throwing light upon the way in which mild types of other diseases may be converted into malignant." Dr. Grawitz has, indeed, recently asserted that even some of the most innocent of our domestic forms of disease-germs may be changed by artificial culture into disease-germs of the most destructive nature.

Of the importance of such researches as those made by Wood and Formad some conception may be formed when we note that the deaths from diphtheria in England and Wales during the last ten years have amounted to nearly 30,000, or to more than half as many again as have been caused by small-pox.

We have seen that in diseases known to be due to living germs the circumstances under which propagation of the disease takes place are precisely those which medical science recognizes in the propagation of small-pox, measles, scarlet fever, and other so-called zymotic diseases. We have seen further that a modified form of "anthrax" (as of "fowl-cholera") can be produced which, while by no means destructive of life, exerts a perfectly protective influence. We should be justified in inferring that the protective influence of vaccination is similar in character, were it not that in such matters science requires proof, not surmise, or even highly probable inference. For, as we have seen, one disease can no more be produced by the germs of another disease than cats from dogs (to use an apt illustration of Miss Nightingale's); nor can one disease, so far as any experiments yet made seem to show, exert a protective influence against another entirely distinct. If this last rule were absolutely certain, instead of being but exceedingly probable, we might at once argue that the germs which produce vaccinia (the disturbance following vaccination) are simply the germs of small-pox "cultivated" by residing for a while in the blood of the heifer. For vaccination exerts a protective influence gainst small-pox, and, if such influence can only be exerted by the small-pox disease-germs, it follows that the disease-germs in the case of vaccination are the same in kind as those to which small-pox is due, differing only in the energy with which they attack the springs of life.

But science is not content to take such matters for granted. The relationship between small-pox and vaccination has been definitely put to the test. Unfortunately the results hitherto obtained have not been in satisfactory agreement. Dr. Thiele of Kasan, forty years ago, repeatedly succeeded (according to a report issued under Government authority) in producing genuine vaccination by inoculating heifers with small-pox poison; and having done this he used this artificial vaccine matter in vaccinating human beings, "its protective power being found fully equal to that of the natural vaccinia." But not only so—at that comparatively remote date, Dr. Thiele unconsciously cultivated the small-pox poison germs after the second manner described above. According to his own account, and his own erroneous idea as to the meaning of what resulted, he diluted the small-pox poison with warm milk, or, as Pasteur would say, he cultivated the living germs in warm milk; and with the poison thus modified, he produced vaccinia, without passing the small-pox poison through the blood of the cow at all. Now this was thought so unlikely to be true, in those days, that Dr. Thiele's other statements were by many physicians discredited, and this particular result was simply ignored by subsequent workers. But now, at any rate, the very improbability of what he achieved, according to the views prevalent in this day, should cause us to regard with all the more confidence his account of his experiments. For no man, still less a skilful physician as Dr. Thiele undoubtedly was, would invent experiments with improbable results. If he invented at all he would at any rate invent what seemed likely to be true, especially if the experiments were such as could be very readily repeated. In our own time this particular experiment might be invented by a dishonest person, the result being altogether likely to be right: others might be left to make the experiments and the credit claimed by him who asserted that he had made them himself. But in Thiele's time it was very unlikely that this would be done. It seems, therefore, exceedingly probable, as far as his account is concerned, that in the first place a modified form of the true small-pox poison is communicated in vaccination, and in the second, that a suitably modified form can be obtained without the use of the cow at all, by simply cultivating the small-pox disease-germs in warm milk.

But simultaneously with Dr. Thiele's researches others were made in this country by Mr. Ceely, of Aylesbury, which led to results not exactly contrary to those by Dr. Thiele, but which were certainly less satisfactory. He was able to produce an eruption in cows inoculated with small-pox virus, and the disease was transmissible to the human subject; but it resembled small-pox rather than vaccinia, and its transmission by inoculation did not produce what the best judges considered as genuine cow-pock. It was allowed to die out.

We may suggest in passing, as a possible cause of the difference thus observed between Ceely's and Thiele's results, some difference in the length of time allowed to elapse after the small-pox virus was transmitted to the cow. It may be necessary, in making such experiments, to recall Pasteur's experiments with "fowl-cholera," when it was found that the potency of the bacillus was only sufficiently reduced after the lapse of a considerable time.

On the contrary, the experiments made a few years later than Ceely's by Mr. Badcock, of Brighton, were similar in their results to those made by Dr. Thiele. Dr. Carpenter, who has been able to examine the record kept by Mr. Badcock's son, states that Mr. Badcock "inoculated his cows with small-pox virus furnished to him from an unquestionable source, and that this inoculation produced vesicles which were pronounced by some of the best practitioners of Brighton to have the characters of genuine vaccinia, while the lymph drawn from these vesicles, and introduced by inoculation into the arms of children, produced in them vaccine vesicles of the true Jennerian type." "Free exposure of some of these children to small-pox infection," adds Dr. Carpenter, "showed them to have acquried a complete protection, and the new stock of vaccine has been extensively diffused through the country, and has been fully approved by the best judges of true vaccinia, both in London and the provinces. Mr. Simon, writing in 1857, stated that from the new stock thus obtained by Mr. Badcock (not only once but repeatedly), more than 14,000 persons had been vaccinated by Mr. Badcock himself, and that he had furnished supplies of his lymph to more than 4,000 medical practitioners. And I learn from Mr. Badcock, jun., who is now a public vaccinator at Brighton, that this stock is still in use in that town and its neighbourhood."

These results seem decisive. But against them we must set the failures of attempts made by Professors Chauveau and Burdon Sanderson, and by Belgian physicians who have recently conducted experiments in this direction, as well as the earlier experiments of Ceely. But as Dr. Carpenter well remarks, failures cannot be regarded as negativing the absolute and complete successes obtained by Thiele and Badcock. We can perhaps learn from a careful study of the failures the conditions on which success and failure may depend. But a single success is absolutely decisive; because, as we have seen, persons inoculated with the poison germs obtained from the cows experimented on by Thiele and Badcock were found to be fully protected against the deadly small-pox poison—a result which there can be no mistaking.

It is gratifying to know that neither Chauveau nor Burdon Sanderson consider their failure as negativing decisively the results obtained by Thiele and Badcock. A reinvestigation of the matter is to be carried on before long, and as Mr. Badcock, sen., himself is able and willing to give all necessary information as to the way in which his researches were carried on, there is every prospect that the secret of success in such researches will be discovered. We venture to predict with considerable confidence that the new researches will unmistakably confirm those of Badcock and Thiele.

In the meantime let us note some experiments which are full of promise in another direction.

Anti-vaccinationists, not concerned by the terrible mischief which has followed the attempts of their followers to escape vaccination, continue their outcry against what they call legalized poisoning, and often with success, especially in America, where there is no settled system of compulsory vaccination. But, when there are outbreaks of malignant small-pox, those who have seemed to agree with the anti-vaccinationists are found singularly ready to seek the protection which vaccination affords; and in America they are not only willing to be vaccinated themselves in such cases, but eager to pass municipal enactments for compulsory vaccination. It seems, however, that even independently of the vaccination of the healthy, there is a resource by which safety can be secured in cases of epidemic small-pox, and the disease quickly stamped out. The importance of this will be recognized when we consider the probability that protective means will before long be found in the case of other diseases, and the extreme unlikelihood that (for many years to come) all adults would consent, except perhaps in times of epidemics, to be inoculated with the specific poisons of other diseases than small-pox.

Dr. Payne, late Professor of the Theory and Practice of Medicine in the Southern Medical College, Atlanta, noticed, as far back as 1846, when at the Small-pox Hospital in New York, that the initial fever of small-pox can be detected by the pulse for some time before any other symptom appears. The pulse is peculiar, and difficult to describe; "but recognizable by any physician who will patiently and carefully investigate the subject until his finger becomes educated." "When once recognized," says Professor Payne, "it can *never* be forgotten, any more than the peculiar thrill imparted to the finger by the pulse of a patient who has lost large quantities of blood by hæmorrhage can be forgotten by a physician who has once learned to detect it."

Now Dr. Payne, whenever he recognizes the initial fever in this way, at once vaccinates the patient. If this is done within ten or twelve hours after the initial fever of small-pox has set in, the patient will have but a slight illness, will show no trace of eruption, and will be thenceforth as perfectly safe from a recurrence of the disease as if he had had small-pox in its most malignant form. A still more remarkable feature of the case is this, that if the patient is vaccinated after the initial fever sets in, he can go about where he pleases without any fear of imparting the disease to others. The ingrafting of the vaccine matter upon the primary small-pox fever seems to destroy its ability of reproduction or propagation entirely. (Here, of course, it is to be noted, that its power of reproduction by actual revaccination remains, but that its power of reproduction in the ordinary way in which small-pox spreads is destroyed, just as in vaccination.) "Another peculiarity," says Dr. Payne, "is this; if an unprotected patient is vaccinated before the beginning of the fever, and the vaccine takes, but does not prevent, only modifies the disease, the eruption will be like that of variola in its appearance and characteristics. But if vaccinated after the commencement of the initial fever, and too late to entirely prevent an eruption, the eruption will resemble in size and character the small-pox eruption. There is," he adds, "as great a difference in the appearance of the varioloid and small-pox eruption as there is between grey and yellow."

Dr. Payne relates a very interesting case illustrating his method of dealing with cases of small-pox, first where the patient had not been vaccinated in good time, and later with those who showed signs of the initial fever. In 1873 an epidemic of small-pox broke out in Virginia, the small-pox being of the variety known as *variola nigra*, and when not modified by some benign influence was invariably confluent. Both in and around Manassas the cases were of the same kind. Being called on to attend a coloured servant-girl, who was ill in a room over the kitchen of a large hotel near his own dwelling, he recognized in her the pulse peculiar to small-pox, and next day the eruption appeared. "I saw," he says, "it would never do to remove this woman, and I determined to isolate the case, and abide the consequences, be they what they might. If I have her removed the poor woman will die, and the prevailing winds will blow the poison for miles down the valley below, and the disease will spread beyond control. But should she die (of which there is strong probability) my plans will be defeated. Firm in faith of the greatest good to the greatest number, I said to myself, 'If she dies, I will wrap her from her toes to the crown of her head in double linen, and with the aid of some one who has had the small-pox, I will bury her.'" Luckily she recovered. "Three persons who were in the room at the time were ordered to report to the Doctor twice daily. One showed the peculiar pulse on the 24th; he was then vaccinated, and after being indisposed for two days (but without eruption) recovered. The others, who had been vaccinated before, did not take it."

In one case, a family of eight persons, "poor and shiftless coloured people," occupied a house in which there was only one room, and where good air and cleanliness were impossible. The father suffered from a very malignant attack of varioloid, and was terribly scarred, but the rest of the family, none of whom had ever been vaccinated before, were vaccinated after the initial fever began, and escaped with slight attacks. In another case, where a whole family were exposed to the infection, he vaccinated the father and two sisters; but an old aunt who had not been vaccinated for many years refused to be vaccinated, being attacked by varioloid. The day after vaccinating the father and sisters a brother who had returned showed the peculiar pulse. Dr.

Payne vaccinated him at once, and the next day his arm looked as if he had been vaccinated eight days before ; it rapidly became sore ; he was indisposed for two or three days, and recovered without a single sign of eruption. These cases are taken from a report of Dr. Payne's experiments in the *Scientific American.* Dr. Payne's plan has been tried in more than a hundred cases, extending over a period of thirty-four years, without a single failure.

Supposing that what has been shown to be true of small-pox is true also of other malignant diseases, a haven of safety is in view, though it may be that some time must elapse before it can be reached. The germ peculiar to each disease has to be made the subject of special study. The proper habitat for such "cultivation" as shall result in mitigating the virulence of its action has to be determined, and the degree of protective power remaining after cultivation has to be ascertained. Next the indications of the initial stage of each form of disease have to be recognized,* and the effects of inoculation with the mitigated disease determined. When this has been done (always on the assumption we have made that what seems most probably true is really so), "plague and pestilence" will no longer be feared as they now are. Isolation of those first attacked from the rest will go a great way to diminish the risk of the infection spreading. A careful watch for the signs of the initial fever among those exposed to infection will do the rest, if due measures are taken in every case when the initial fever shows itself.

And as the inquiries of Pasteur and his fellow-workers seem thus to indicate a haven of safety, so also do they show the presence of concealed rocks,

* It may well be that in many cases, instead of the comparatively rough test of feeling the pulse, the use of the sphygmograph, or some other instrument for determining minute changes in the character of the pulse, may be required.

of dangers heretofore unnoticed. What Pasteur showed respecting the deadly "anthrax" has its analogue, we may be sure, in diseases affecting the human race. Dangers lurk where none would suspect them, and where only the keen eyes of the trained science-worker can find them. The poison-germ may attack through the alimentary canal in the food we eat, through the lungs in the air we breathe, as well as directly through the blood-current. Disease and death may lurk in a dress, a child's toy, a lock of hair, a letter, or a carpet. Neither time nor distance avails to destroy the fatal infection.

We may note lastly a point to which attention has been directed by Dr. Andrew Wilson, in *Knowledge*, that the practical and actual benefits which have flowed to human health, and which are likely to flow in the future as well—"the saving of life by the prevention and extermination of disease"—have arisen from a simple study in natural history. So-called practical minds are often given to loudly express their disapproval of any science which deals with what to them seem mere abstractions. Doubtless to such minds the study of the development of the "rods" of splenic fever under a watch-glass must seem a piece of scientific *dilettantism*, just as information respecting the solar system may seem despicable enough, because its results cannot be measured by a profitable currency, or, in plain language, because it does not seem to pay. The best answer to such reasoning is found in the recital of the results to human and animal life, to which studies in an apparently unimportant field of research in natural history have led and seem likely to lead mankind.

CHAPTER XXII.

THE MISUSED H OF ENGLAND.

THERE are few subjects more curiously interesting, alike to the student of science and to those who do not care specially for scientific inquiry, than the peculiarities of a language as spoken by different sections of the same people. There can be little doubt that by rightly understanding these localisms we are enabled to advance a step towards the interpretation of those wider diversities which distinguish the speech of races not forming the same nation, but having a common origin. Any one who, in England, for example, studies the peculiarities of dialect in the southern, western, eastern and northern counties—peculiarities still so great that the inhabitants of certain localities find it difficult to comprehend standard English (so to describe English as spoken by the most cultured classes)—will be well on his way to understand how the various languages of Europe had their origin from a common stock. In particular we learn to recognize *how*, though we may not so easily understand *why*, in some communities changes of a certain kind in consonantal and vowel sounds systematically prevail. Thus in certain northern English counties the *i* is systematically pronounced *oi*, in others *ee;* so that as far as this vowel sound is concerned we can always translate the county language into common English by changing the *oi*'s or the *ee*'s, as the case may be, into *i*'s. In Wiltshire and Somersetshire, the *s*'s are all turned into *z*'s; elsewhere, the *o*'s are changed into *oa*'s, the broad *a*'s into *Æ*'s, and so forth. In the Celtic parts of Great Britain, as in the Highlands and in Wales, we find wider examples of Grimm's law in the altering of *b*'s into *p*'s, *d*'s into *t*'s, *v*'s into *f*'s, and so forth. But in the majority of cases it has not been found possible to explain why some sections of a race should modify certain sounds in particular ways, any more than it has been found possible to explain why the Teutonic and Latin races should have modified the language which was once common to both in such diverse directions as to produce languages whose kinship becomes manifest only under close and careful study.

The misuse of the letter *h* in England and the correct use of the aspirate in English-speaking communities outside the old home may be regarded as affording an instructive example of the modifications a language thus undergoes, under varying circumstances. Of course, it has not yet come to pass, and we may hope it never will, that the omission of *h* where it ought to be sounded, and its introduction where it has no right to be, have become so universal in England as to be regarded as justified by custom,—

> "Quem penes arbitrium est et jus et norma loquendi."

On the contrary, in England, the misuse of the letter *h* is much more unpleasant than in America or Australia, where, I notice, people are more amused than disgusted by unaspirated or "exaspirated" *h*'s; very much as we find a foreigner's mistakes in speaking English rather pleasant than otherwise, while the mistakes of an ignorant native sound coarse and vulgar by comparison. I have heard Americans say that they find something quaint in what they are good enough to call "the Hinglish haccent." In

England we scarcely view the matter that way. Occasionally some peculiar collocation of dropped and forced *h*'s may raise a laugh among us, as when dear old Leech makes the veterinary doctor tell the owner of a horse that "it hain't the 'unting as 'urts 'im ; it's the 'ammer, 'ammer, 'ammer, along the 'ard 'igh road."* But as a rule the *h* malady is regarded as a most unpleasant one in Old England, however funny it may seem to Americans and Australians. It is instructive and interesting, however ; and I propose here to consider its nature and origin, the laws of its propagation, and the reason why in certain English-speaking communities it has never shown itself, and probably never will. Mr. Grant White has dealt with the *h* malady in England, recently, in a highly interesting paper, wherein, however, he presents views which are, I think, entirely incorrect. The evidence he adduces in support of his views seems to

* So far as I can judge, few Americans, and certainly none who have not been in England, understand precisely how the *h* is "exaspirated" in the old country. When an American novelist, newspaper-writer, speaker, or actor tries to present the Hinglish haccent, he invariably (at least I have never noticed an exception) puts in *h*'s which would never appear in really English talk. Thus the above sentence would be given in an American newspaper (see the way Greenfield's evidence in the recent slugging trial was dealt with), "Hit hain't the 'unting has 'urts 'im ; hits the 'ammer, 'ammer, 'ammer, halong the 'ard 'igh road." But no one ever talks that way in England. Here is a hint which American novelists may find useful : English ill-users of the *h* *never* insert an extra *h* except in a word on which emphasis or semi-emphasis falls. For instance, you will hear a man, so ignorant as to say "eddicated" for "educated," who yet will not say "heddicated," long though the word is, and therefore inviting the extra aspiration, unless he is emphasizing the word, so that in one and the same sentence such a word will come in both ways, thus : "Squire Brown were a well-eddicated man, sir, but he wor n't nothing to Dr. Jones for eddication. Heddicated ! Why, sir, Dr. Jones were that well heddicated there wor n't any one down in our parts could hold a candle to him," and so forth, drawled out in the customary down-country fashion.

me, indeed, to point in precisely the opposite direction from that which he indicates.

In the first place, it is perfectly true, as he asserts, that the purity of American speech in this particular respect is remarkable, because America is the younger nation, and in some respects less cultivated (Mr. Grant White says "less cultivated" without any reservation), and produces the smaller part of the literature common to the two, though I cannot go on with him to say that her part of the literature is much inferior in quality as well as in quantity. He omits, however, to notice, or at least does not dwell upon, the fact that a similar but much more remarkable contrast has long existed between England and Ireland with regard to the letter *h*. A labouring-man in Ireland, who cannot write or even read English, and who talks with a brogue as broad as his spade, will never drop or misplace an *h*, and ridicules the *h*-less Saxon as heartily as would a New York newspaper critic. Whatever theory we are to adopt respecting English misuse of the *h* must account for the circumstance that this delicate aspiration, so slight that if it becomes more than barely perceptible it is as offensive as its omission would be, is given by the most ignorant Irishman, while it is a dead letter (or else becomes a disagreeably live letter) with Englishmen of fair average education. In certain positions, indeed, presently to be touched on, the *h* is almost universally dropped in England, insomuch as almost to justify its omission by persons of culture, in obedience to the authority of recognized custom.

A large majority of Englishmen drop the *h* in nearly all words in which it ought to be sounded. So far I go with Mr. White. But I do not agree with him that most Englishmen put in an *h* where it ought not to be. I should imagine, from my own observations,—and I have had exceptional means of testing the matter in my lecture travels,—that about one-third of the people in England throw into their talk, now and then, a wrongly placed aspirate, but not more. My observations do not include the criminal classes, but they would not raise the proportion very much, certainly not to one-half. Even those who do throw in "exaspirated" *h*'s, however, are not quite so bad as most Americans imagine, and as Mr. Grant White assumes. It would indeed be a difficult problem to deal with, if the worse-speaking Englishmen always precisely inverted the use of the aspirate; omitting it where it should be, and systematically introducing it where it has no business. It would require as much skill and as good a knowledge of spelling to practise such consistent blundering as to speak correctly. Those who have studied English talk in this respect know that the very worst misusers of the aspirate give the *h* correctly at times,—always, indeed, with some words; and also that they are as likely to offend by "exaspirating" an *h* which is perfectly in place when duly aspirated as by inserting an *h* where no *h* should be. I remember an American whom I met in Boston, in 1873—rather an ignorant man I need hardly say—who remarked to me when he heard that I had recently met Mr. Emerson, "I guess you said, ''Ave hi the '*h*onour hof haddressing Mr. Hemerson?'" He was not altogether jesting, as I at first thought, for he supposed that all Englishmen came over to America affected with the *h* disease, but that some quickly caught the purer "American accent," as he called it. Now every Englishman knows that an "exaspirator" of *h*'s, when '*h*onoured by an introduction, is more apt than usual to display the strength of his aspirations; but even in such an accession of the malady as is thus brought on, he would not speak as my Boston friend imagined. No one could, in fact, without breaking up his talk into gasps. He would say, "'Ave I the

Hhonour of addressing Mr. Hemerson?"—a sentence which has no gasps in it, because the extra *h*'s come in along with the exaggerated emphasis which the much-oppressed person wishes to introduce. It is indeed noteworthy that these maltreaters of the aspirate always use a word into which they can fling a strong aspiration, when expressing great respect. "'Ave I the pleasure," etc., would not suit them at all. Note also (what Mr. Grant White overlooks, by the way) that a big rough *h*, which sounds very unpleasant even in a word beginning with *h*, under ordinary conditions, does not sound ill at all when the chief emphasis of a sentence falls on such a word. A man may say *Hhome* so as to offend as much as though he said *'ome*, or even more; but one would find it very difficult to pronounce the *h* with disagreeable strength in such a sentence as this: "I am *heartily* at your service." Note again the difference in the aspiration of the *h*'s in the following sentence, if earnestly emphasized: "I am right glad to see you here; you must look on my house as your *home*." A strong aspiration on *here* and *house* would be offensive, but the aspirate in *home* could not well be too strong.

While the number of those who use unauthorized *h*'s is great, and of those who drop their *h*'s much greater, it must be admitted that the number of those who in some way or other maltreat the aspirate is so large that one may doubt whether one in a hundred in England can be excepted. Mr. Grant White says that he has heard highly educated men, scholars and men of scientific attainments, men who write capital letters after their names (though *that* counts for absolutely nothing), drop their *h*'s in England, "just as in America men of like position have a nasal twang, say *Mu'ica* for America, and the like." I have myself heard "men of like position" in America, including a professor who earnestly advocates the continued study of classical literature in American colleges, say "you *was*," "I don't know as," "I remember of," "unbeknown," and make other like mistakes. Yet I doubt not that offences against the laws of aspiration in England are as common as "you was" and "we was" in America. The omission of the *h* in such words as *he*, *him*, *her*, etc., may be occasionally noticed in the rapid speaking of even the best-bred men in England; but here America has no advantage, for in these words I have repeatedly noticed the *h* slurred or lost in quick speaking in America. The *h* in words beginning with *wh* is so often dropped in England that it is doubtful whether custom does not justify its omission altogether. Yet I am inclined to think that many Americans rather overaspirate the *h* in *wh*. The Irish unquestionably do so. *Wh* with them is altered into *hw*, a fault of speech which more than one English novelist has noted and ridiculed. I see that Mr. Grant White regards *hw* as the proper way of presenting the aspirated *w*. This seems to me erroneous. I have, indeed, very little doubt that in old times *hw* was the uniform and therefore correct way of giving the consonantal sound in *what*, *which*, *whale*, etc. In Saxon times, certainly, the sound was *hw*, and the spelling accordingly. Very likely the Irish retain the old-fashioned sound in this case, as in several others in which modern English pronunciation has departed from it. But at present English folk must not say *hwat*, *hwale*, and so forth, if they wish their breeding (for in England these points are matters of breeding rather than of education) to remain unquestioned. There is a legitimate way of aspirating the *h* in *wh*, which to a good ear perfectly differentiates *whale* from *wail* or *wale*, *which* from *witch*, and so forth. We may say *hwat*, *what* or *wat* (which we must spell *wot*, though), and only one of these is right. Among the best-bred Englishmen the delicate aspiration corresponding to the middle sound is consistently given. To one accustomed to the rough *hwat*, *what* properly pronounced sounds like *wot;* but to a good ear, not spoiled by constantly hearing the rougher sound, the distinction is nearly as great as that between *house* and *'ouse*—albeit I must admit that many Englishmen who never drop an *h* when it is alone, neglect the aspirate when it comes in company with *w*.

Still more often is *h* neglected in words beginning with *rh*. But this fault is quite as common in America as in England. I have oftener heard of *Road Island* than of *Rhode Island*.

Lastly come the cases where there is divided authority as to whether an *h* should be sounded or not. Mr. Grant White dwells on the omission of the aspirate in the word *hospital*, remarking with surprise that he has often seen the words *an hospital* in books published within the last ten years. He says this in answer to Mr. Ellis's remark that the omission of the *h* in this word is an archaism. But there are many who consider that to sound the *h* in this word is as improper as to sound it in *honour* or *hour*. I myself constantly sound the *h* now in *hospital* and *humour* (at least, when in America), but it appears to me incorrect. So in the word *humble*, which, since Dickens made Uriah Heep so constantly call himself "a numble" person, has gained an aspirate to which it is not legitimately entitled. We must all sound the *h* in *humble* now, I suppose; but before "David Copperfield" was written a clergyman who should have substituted "a humble and contrite heart," in the "Dearly beloved brethren," for the legitimate "an humble" would have been regarded as a very ignorant and vulgar parson indeed.

And now to consider the origin and progress of the *h* disease in all its varied forms :—

There seems no room for doubting that the *h* disease had its origin in London. Walker speaks of it as specially prevalent in London in his day, and even now it is more common in the pure cockney dialect (the most hateful form of the English language in existence) than anywhere in England. Moreover, its prevalence in other places than London is greater or less according as such places are nearer to or farther from the metropolis. We find no trace of it in Cornwall or Wales ; very little in Cumberland, Northumberland, and Yorkshire. In the midland counties it is less common than in the southern. It is at its maximum in the heart of London. In this respect it is like the *v*-and-*w* malady, which, even when at its height (it has now nearly died out), was never so badly felt in the provinces as in the metropolis ; though of course, like all metropolitan defects, it spread in greater or less degree over the whole country.*

This being the case, we are justified in assuming that the disease had at first that form which is characteristic of the faults of language found at great centres of population, and especially in the chief city of the nation. If you wish to hear French clipped and slurred you should go to Paris, and German suffers like treatment in Vienna and Berlin. It is the same with English in London. In a great and busy city men shorten their words and sentences as much as possible, being assured that what they say will be understood, because all speak the same language and adopt the same convenient abbreviations. Thus, just as in Paris *cette femme* becomes *c'te f'me*, and *Voila ce que c'est* becomes *V'la c' q' c'est*, so in London *City Bank* becomes *C'ty B'ak ;*

* In Shakespeare's time there appears to have been a *v*-and-*f* disease, insomuch that in a sonnet attributed (wrongly) to him we find "vades" for "fades." This still lingers in parts of England, but I suspect it had its origin in London.

halfpenny is abridged first to *ha'penny*, and then to *hapny* or *'apny*. *Omnibus* is shortened into *'bus;* every one in it addresses the *conductor* as *'ductor;* the conductor shortens the cry of *all right* into *ry*, announces the threepenny fare as *thripns*, and so forth. In fact, it may be laid down as a general proposition that, although a language becomes modified in provincial places and in colonies, it is only in busy cities, and chiefly in capital cities, that a language is modified by clipping and slurring. Take any forms of county *patois* in England, for example, and you find the modifications of English tending towards increased stress on the various tones; in other words, the reverse of the clipping and slurring which is always going on, though under varying forms, in the metropolis. Consider the northern dialect, for example, as truthfully presented in Tennyson's "Northern Farmer":—

> "Wheer 'asta beän saw long and meä liggin' 'ere aloän?
> Noorse? thoort nowt o' a noorse; whoy, Doctor's abeän an' agoän
> Says that I moänt 'a naw moor aäle: but I beänt a fool:
> Git ma my aäle, for I beänt a-goin to breäk my rule."

Here there are shortenings of the more familiar words *'asta* for *hast thou*, and so on; but the tones are all lengthened, and the throwing in of *a*'s separately, or as additions to vowel sounds, shows well how the comparatively slow-going life of provincial places leads to prolixity in pronunciation as well as in speech. In great cities all this is reversed. Men have not time for drawling or vain repetitions. We might be tolerably certain, apart from other evidence, that such a peculiarity as the dropping of the letter *h* had its origin in cities, and not in country places. It saves time and it saves breath to omit the aspirate, and one might safely have guessed that in London the *h* would be dropped in the long run, merely because of the convenience of the omission as a ready form of word-clipping. Of course we may believe (for a *raison de plus*) that, as I suggested several years since in my essay on "English and American-English," the London fogs had something to do with the omission of the *h*. It is something, when a real "London particular" prevails (a fog which no one who has ever experienced its delights can forget)*, to avoid an aspirate; and though the denser fogs last usually but two or three days altogether throughout the year, it is seldom so clear and pleasant in the heart of London that one would be apt to luxuriate in many aspirations. Still, it appears to me more probable that the *h* was dropped in clipping the language, after customary city fashion, than that its loss was (at least, primarily) due to London fog and mist.

But if we assign the suppression of the *h* to slurring and laziness, how are we to account for the introduction of a forced *h* where no *h* ought to be? At first sight it seems as though any explanation of this fault must of necessity be inconsistent with the interpretation I have assigned to the omission of the *h*. For every one who has ever heard the forced *h* in its native home—I mean specially in London—knows that it is emitted after a fashion entirely opposed to the idea of laziness.

Yet in reality there is nothing more inconsistent

* I have been asked, when what is considered dense fog has prevailed in New York, whether it is not pretty nearly as bad as a London fog. The densest fog I have ever seen in New York, or indeed in America, would have to be thickened at least tenfold, and then coloured a strong yellow-brown, and loaded with acid vapours and heavy smoke, to approach in effect the true London particular. In such a fog a cab-driver cannot see his horse's head or shoulders; sometimes a man cannot see his own feet. I have passed two steps outside my own door, have stopped, and (unwisely) turned round to consider my whereabouts, and have then been scarcely able to find my way back.

between the "exaspirated" *h* and the suppressed *h* than there is between the volubility of a London cabman when anxious to convey his meaning very forcibly and his customary brevity. The same man who says "Jump in, guv'nor," to his fare at the beginning of a ride shall vituperate him in well-chosen but objectionable objurgations for ten minutes at a stretch, when the journey is over, and the right fare offered him. We need not wonder if, in like manner, the same people who drop their *h*'s under ordinary conditions throw in more *h*'s than are necessary when they wish to emphasize their conversation. It is indeed noteworthy, and in it we find, I think, the key to the problem we are dealing with, that the *true* sound of the aspirate is *never* given by cockneys, and by those who have adopted their speech in this respect, to the extra *h* thrown in on words beginning with a vowel or a silent *h*. No cultured person ever pronounces the aspirate on unemphasized words as the cockney pronounces it in words which have not properly any initial aspirate. It becomes clear, then, that the false aspirate of the cockney is in reality thrown in only for emphasis. Of course there must be cases, also, where an Englishman who slurs his *h*'s endeavours to set matters right by throwing in extra *h*'s at random. In such a case he is not as often right as wrong, for the simple reason that it is usually after a mistake in the suppression of an *h* that he throws in an extra strong *h*, and the chances are in favour of its falling where no *h* is wanted; but in any case, one would not notice an *h* that fell rightly, whereas one would at once remark an erroneous *h*. Hence arises the quite mistaken notion that the Englishman who both suppresses and "exaspirates" his *h*'s *invariably* goes wrong. Any one who carefully follows the talk of such a man will notice that he quite often gets an *h* in the right place, and correctly pronounces a word which has no *h*.

That *h* should fall out of words beginning with *wh* is obviously explained by the theory here advanced; and the circumstance that even the most inveterate maltreater of the *h* never throws one in where it should not be in words beginning with *w* (never, for instance, says *which* for *witch*, however steadily he may say *witch* for *which*) corresponds well with my theory. For there is no gain in emphasis by aspirating a word beginning with *w*, as there is by aspirating a word beginning with a vowel or a silent *h*.

Mr. Grant White, who takes a different view, oddly overlooks the circumstance that the view which he does take, so far from being, as he supposes, "an explanation of the phenomenon," would, if accepted, add enormously to the difficulty of the problem. It will be observed that if my theory is correct we can at once understand why the *h* is not maltreated in Ireland, America, and Australia, which is the really remarkable point. If the *h* disease is a defect due to slurring, and of comparatively recent origin, we can readily understand why it has not made its appearance outside the old home of the English-speaking people. There, and there alone, would the people slur (at least, in the busy centres of population) the language common to all, and which all spoke with equal readiness. Elsewhere the language would be more carefully dealt with.

In Ireland, for example, to begin with that case, the English language was not so common that it could be readily slurred. Irish folk had to speak it and hear it spoken with distinctness in order to understand it readily. There the modes of pronunciation, and such matters in particular as aspiration, had to be attended to more carefully than in England, and especially in London. A very slight difference in this respect would make a great alteration in the result, for all changes in a language *originate* in very slight differences. But, it may be urged, the contrary is demonstrably the case in Ireland; for there the English language has undergone great changes: they say *raison* and *saison* for *reason* and *season*, *hwat* and *hwy* for *what* and *why*, *goold* for *gold*, *obleege* for *oblige*, and so forth; to say nothing of certain changes in consonantal and vowel sounds, which may be attributed to peculiarities in the vocal organs. This, however, affords strong evidence in favour of my theory, that a language is less modified at a distance than in the heart of its native home. For there can be no manner of doubt that all those peculiarities of pronunciation which are regarded as especially Irish are in reality old English. The letters *ea* in old English stood originally for the sound which they still represent in the word *great*. (Pepys spells *skates* indifferently *skeats* and *skates*.) The French *raison* and *saison* were altered into *reeson** and *seeson* only through cockney laziness, reducing all broad vowel sounds to narrow ones. So *tea* stood for the same sound as the French *thé*, but has been narrowed into *tee*. As for *hwat* and *hwy*, I remember distinctly how my grand-aunt, a lady of eighty-four, belonging to the old school, used, in 1848 and 1849, to complain that I, as a schoolboy, was not better taught than to say *w'at* and *w'y*; she herself pronounced the words with all the aspirational emphasis employed by the Irish of to-day. She systematically said *goold* for *gold*, *obleege* for *oblige*. (So did Lord John Russell, still later.) The Irish, then, retained the old English sounds here, and doubtless, also, in the initial *h*.

As the colonization of America was a later affair than the occupation of Ireland, we do not find so

* Thus Falstaff's play upon words has been lost where he says, "What! a reason on compulsion! Not though reasons were as plenty as blackberries." Pronounce "reason" as the word was certainly pronounced in Shakespeare's time, and we see at once the play on the words "reasons" and "raisins."

many archaisms in America as in Ireland. The *h* disease in England must of course be set later still, or that would doubtless have spread in America too. As matters actually happened, the Americans started free of all trace of this malady, and have been able, notwithstanding importations of great numbers of *h*-dropping Englishmen in later times, to keep the malady from spreading in the new country. Probably not a single Englishman or Englishwoman who has landed here with the *h* disease has been cured of it; for it seems incurable in the adult. But probably in not a single case have the children caught their parents' malady.*

Mr. Grant White considers that he has found evidence showing that the *h* was suppressed in England by all classes seventy-five years ago. "This *h* breathing," he says, "is a fashion in speech which, I venture to say, is, among the 'best people' in modern England, hardly more than seventy-five years old." Now, apart from the circumstance that the contrary is known,—I myself can vouch strongly for this, because I have heard the conversation of hundreds of persons who were past middle life at the time Mr. Grant White mentions, and know that they were as careful and correct about their *h*'s as I was taught from my childhood upwards to be,—apart, I say, from this, Mr. Grant White's idea, even if accepted,

* I have had occasion to notice (1) how English-born children (my own) catch the *h* malady from nurses and servants, eventually losing it; (2) how American-born children (step-children of my own) are affected by it; and (3) how children of mixed American and English parentage (my own, again) are affected. On the first point I need not speak, nor specially on the third, except to say that my youngest boy, with an American mother and a father who uses the *h* correctly, said a "'orse" and a "'ouse" in England quite naturally; but my American daughter (actually step-daughter) of five, took the worst form of the *h* malady in a business-like way. "Mamma," she said one day, "you say oven, don't you? Well, *I* say *hoven!*"

would give no explanation of the suppression of the *h*, still less of its forcible and wrongful introduction. So far from that, it would leave us two problems of immense difficulty to deal with.

First, it would be very much harder to explain the difference between England, on the one hand, and Ireland, America, and Australia, on the other; for how could correctness of speech have been derived from a people who *all* spoke incorrectly in this respect? Secondly, we should have to explain how it is that, although the *h* malady is incurable when once fairly established, there are, nevertheless, thousands of Englishmen, using their *h*'s correctly, who were lads at the time when Mr. Grant White says all Englishmen, even the best bred, dropped their *h*'s. To this must be added the truly surprising circumstance that we should have amended a fault thus universal without even a passing note in our general literature or in the press that so wide-spread an evil existed.

These considerations should suffice to overthrow Mr. Grant White's theory, which every well-bred Englishman of middle age and beyond knows to be entirely erroneous. (Men of over eighty in England can tell Mr. White—I know it, because they have told me—that at good schools in England, seventy-five years ago, the same care was taken in teaching the correct use of the *h* as at the present day.) I need not, therefore, occupy much time in considering the evidence which he regards as establishing his position. Still it may be interesting to touch on his chief points.

He notes that no English writer of novels, tales, or humorous sketches, seventy-five years ago, makes fun of the *h* peculiarity. This proves, if it proves anything, that the suppression of the *h* was less common then in England than it is now; and this is well known (in England) to have been the case. The *h* malady has spread as the *v*-and-*w* malady has died out,—*why*, I cannot say, but the fact is certain. The *h* malady existed, of course, but was not common enough to attract the attention of humorists, as it does now. (Nor were humorists such close observers then as now.)

Mr. Grant White's attempt to prove that the *h* malady was common, because in the Bible "an" is often written before *h*, fails, when we consider that the distinctive use of *an* and *a* is a comparatively modern rule. Many still regard it as an unsatisfactory rule,—at least in its present general form. Any one who will repeat aloud, and with full voice, the sentence "I stayed at a hotel commanding a horizon eighty miles away" will see that the Englishman who writes "an hotel," as I often do, or "an horizon," as I almost always do, does not necessarily suppress the *h*. (I have often to use the word *horizon*, in lecturing, preceded by *a* or *an*, and nearly always I find that to give the *h* softly and correctly it is far easier to say "an horizon" than "a horizon.") He considers Miss Burney must have said an 'osier, because she wrote "an hosier;" on the contrary, we may recognize in her use of *an* instead of *a* her care to avoid a gasping utterance of the aspirate. As for the Bible writers, the very existence of the letter *h* in the words Mr. Grant White quotes as preceded by *an* shows, when we consider the practical origin of spelling in English, that the *h* was sounded. It was probably sounded originally even in the words *hour*, *humble*, *honest*, etc.

I think, however, it has been sufficiently shown that the suppression of the *h* was a fault of slurring, a liberty arising from what may be called undue familiarity with the language; while the converse fault arose from a reaction against the other, and showed itself (as it still shows itself) only where an attempt was made at undue emphasis.

CHAPTER XXIII.

THE LANGUAGE OF WHIST.

WITH SPECIAL REFERENCE TO THE AMERICAN LEADS.

To the language of whist, already full and clear, an important and probably a final addition has recently been made in what are called the American Leads, really only an extension of a system already adopted. The time seems fitting, then, to consider the scientific interest and significance of whist language. But first I would make a few preliminary remarks on book-learning in games of skill.

There are some who argue, and not without a show of reason, that the royal game of chess has been impaired by book-learning. Nearly all the principal lines of opening at chess have been traced out to the tenth or twelfth, many to the twentieth move on either side, and a player, however skilful, who does not know the various attacks and defences, the traps for the opponent and pitfalls for self, which lie along the different paths thus pioneered, plays at so great a disadvantage with one who knows them all well, that often he is beaten before the game is fairly begun, and certainly before his opponent has had occasion to exert any original chess power. Of course the answer is that the progress of chess play could not fail to disclose these various lines of attack and defence, with the pitfalls and traps belonging to each. Even if no books of chess strategy were ever published, chess players could not fail to learn from games actually played the resources of the various openings. The books only present in a conveniently accessible form the information obtained during actual play. Any one who chooses may acquaint himself with at least so much of book knowledge as is necessary to enable him to hold his own against *mere* book knowledge. If he does not choose to do so, he must not complain if he gets beaten over and over again by players whom he is assured, though sometimes such assurance is a mere illusion, that he could defeat every time were it not for their book-learning. It is not at all necessary, as many imagine, to know all the features of a hundred different openings, to master all the intricacies of such brilliant openings as the Evans, the Ruy Lopez, the Allgaier-Thorold, and so forth. A man might be a successful player without knowing anything whatever of the Evans' Gambit, for example, seeing that he need never proffer it himself, nor as second player accept it when proffered him. Nor need the King's Gambit be ever accepted. Though I should say the man was no chess player who merely for safety avoided these and other fine lines of opening, which soon lead to the finest opportunities for brilliant play on either side. Indeed, I cannot understand how any one who really loves chess can fail to take interest in the study of the openings, in which the finest possible chess play is presented for study.

Something similar to the objections which many brilliant but uncultured chess players raise against book-learning in chess is raised by whist players—many of the really skilful—against what is called conventional whist play, and in particular against the various signals which within little more than the last quarter of a century have taken their place among whist conventions. Good players of the old-fashioned schools consider that they had quite enough to look out for as whist used to be played without being asked to notice and respond to a number of small signals which, they say, may not be wanted or employed in one game out of ten, yet must be looked out for in all games, often at the cost, they think, of diminished attention to more important whist matters. Then they go further, and maintain that the various signals are bad policy, and may cause the loss of a game by showing the adversary just what was necessary to indicate the sole line to success.

Before considering the signals properly so called—which are more numerous, by the way, than many whist players seem to suppose—I would call attention to the circumstance that when whist was quite in its infancy similar questions must have been suggested, and, for aught I know, may have been raised, over matters about which there is now no doubt or cavil. I suppose that before Hoyle's time most players of whist ran much upon the lines which many players of a common form of home whist—called by Pembridge Bumblepuppy—still follow. In particular, I imagine that they paid very little attention to the small cards. A Bumblepuppy player who holds the two, three, and four of a suit of which the ace has been led, will play the three or the four quite as readily as the two, not because the three cards are all of the same value, but because *all* small cards are to him of equal value, *i.e.*, of no value at all. With two, five, and seven he would be as like as not to throw the seven to the ace, instead of the proper card, the two. Now one can readily imagine that to one of these careless players it would be a new and an unpleasant light, that proper play of the small cards might and usually would give useful information to his partner. A seven, for instance, played by one who invariably played his lowest card when he could not take, or help to take, a trick, would mean, certainly, that he had not the two, three, four, five, or six, and this might, and probably would, throw useful light on the position of other cards in the suit. Again, suppose a player, third in hand, with only small cards, and that on a six led, to which second player has dropped a two, third player puts a seven, this, if he invariably played his lowest when unable to take a trick, would mean that he had no more cards in the suit, often a most useful piece of information. But if he were careless about small cards, he might still hold three, four, or five, or all three of them.

In this, and in multitudes of other ways, careful play of the small cards, from the lowest upwards, invariably, where nothing can be done to take or help to take a trick, may convey useful information to the partner and also to the enemy. Now, one can easily imagine that when the best whist players of that early whist age called attention to this point, the careless players would say that they could not make a toil of pleasure by attending to such details; and if the bad effects of carelessness were pointed out, they would probably agree that playing thus according to a definite rule gave at least as much information to the enemy as to partner, and might often set the enemy on the only line for saving or winning a game. The argument would be sound enough, though absolutely insufficient to establish the careless player's position. There can be no doubt that many a game has been saved or won by noting the indications of the enemy's play in such matters as this, and that the regular player is exposed to this particular disadvantage, that those playing against him can always, or nearly always, draw sure inferences from the play of his small cards. Yet, nowadays, no one for a moment thinks of advising irregular play of small cards as a fit way of avoiding this disadvantage. Every whist player knows that the advantages of regular play in this respect in the long run far more than counterbalance the disadvantages. Very seldom indeed does any player see, or imagine, occasion for departing from the rule of playing his small cards in order from the lowest upwards, when he has to play them to tricks already won, or which he cannot help to win.

It is the same with other rules of play, which are not, like this one, based on a consideration of the actual value of the cards,* but are purely conventional. I am not yet speaking of signals.

Let us consider some of these rules, which every whist player is supposed to know, and which every sound whist player knows and follows.

In playing to a trick from cards in sequence, whether low or high, the lowest is invariably played first, unless it is specially intended to deceive the enemy (or to signal, a point presently to be considered). As good players very seldom think it better to deceive the enemy than to play so that their partner shall understand them—though, of course, occasions arise when partner is so weak in cards that it is useless to tell him anything—you seldom see a good player put down a ten when he holds the knave, a knave when he holds the queen, a queen when he holds the king, or a king when he holds the ace. When he does, you are sure that he has recognized special occasion for deceiving the enemy. A player who, without such occasion, puts the higher card of a sequence down to a trick, you may at once set down as either unacquainted with the conventions of the game, and, therefore, for the nonce a bad partner, or a bad player, which is not quite the same thing. If he errs through ignorance, he may yet have in him the makings of a good, nay even of a fine player; but if

* For in good whist play it very often happens that a trick or two at the end of a hand, and therefore often a game or a rubber, will depend on the value of very small cards in the different hands.

he deliberately neglects the customary rule, without solid reason for believing that it is useless to enlighten his partner, and may be useful to confuse the enemy, then he is a hopelessly bad player.

In leading, again, it is customary, in almost every case, to lead the highest card, if any, of a head sequence. This is true whether it is the first or a later round in the suit. Thus, if you lead a queen, your partner knows certainly that you do not hold the king; if you lead a knave, he knows almost certainly that you do not hold the queen. You *may*, in this case, hold knave, queen, king, and two or more small ones; for with such cards you would play the knave for a sound reason, corresponding (conversely) to the reason which leads to the general rule that the highest card should be played first. You lead the best of a top sequence in nearly every case, that your partner may not suppose that the enemy on his left—fourth player—has that card (which he might very well suppose if you had played the next lower), and may not, therefore, take a trick which is already yours. Thus, if, holding queen, knave, and others, you led the knave, and your partner held the king, he would infer that queen lay to his left, and cover your knave with the king, a card which, of course, he would reserve for future more effective use if you led the queen. The exceptions to this general rule are two. From ace, king, and others you lead king, not ace; for even if your partner has no cards of the suit he would not trump your king (unless a trick were wanted to save the game); he would know you had either led from ace king or from king queen (and others), and that in one case trumping would mean winning his partner's trick, while in the other it would stop his partner from getting command of the suit by drawing the ace.* The other case is

* A player who would trump a king led by his partner, first

where you *want* your partner to take your trick with the card (if he has it) in sequence with your high cards. Suppose, for instance, you have king, queen, knave, and two or more little ones. To establish this strong suit you must get out the ace, and it is best to get it out first round even if your partner holds it. If you play the king he will certainly hold up the ace; but if you lead the knave, your partner plays the ace, and with the two best cards still left in your hand, besides two small ones, four out of nine cards (probably) left in the suit, you have (probably) complete command.

Now, these conventional rules are, in a sense, a form of signalling. All the recognized leads, with play second round, &c., are forms of signalling. No one can be regarded as a decent whist player who does not know these rules, which really are much simpler than they seem. I have shown (as "Five of Clubs") in the pages of *Knowledge*, and in my little book on whist, that instead of some one hundred rules for leading, as often given, there are not quite a score when the leads are properly systematized. Every player who leads correctly may be regarded as telling his partner something about his hand. To refuse to lead correctly because some little trouble may be involved in committing the rule to memory is, of course, refusing to play whist respectably. No one now, again, uses, in regard to these rules for leading, the argument which many fine players still use against the signals properly so called, that often correct play shows the adversary just how to save or win the game; yet the argument applies certainly as much to the rules for leading as to the signalling.

round of a suit (except in the one case of a cross ruff being possibly set up), must, of course, be hopelessly bad. Yet, where a player has the atrocious habit of leading from a singleton, he could not complain if a king were trumped. *Such* a player ought always to lead ace from ace king and others, lest his king be trumped by a partner knowing his evil ways. But we are speaking of men who play *whist*.

Again, there are many rules, readily learned when regarded as all depending on one or two principles, for play second hand and third hand. Even the old-fashioned and sound, though by no means universal, rules, Second hand play your lowest, and Third hand play your best, are in a sense signals; they cause the play second and third hand to have a definite meaning, and so become part of the conversation of the game. But other and more precise rules, especially for the play second hand, are still more definite in their meaning. Thus, if a queen falls from second player on a small card led, it is known that either the player of the queen holds king and one small one, or ace and ten, or no more, unless the queen has been played on a knave led, when he may have two small ones, but not more, left. If in the next round a small one falls from this hand, it is almost certain that he holds the king left. If in the first round the king is played on the queen, it is most probable that the queen was played from ace, queen, ten—though till next round it remains uncertain whether the queen may not have been held single. Similarly with other cards which may fall from the second player. They have a definite meaning, or one of two or three definite meanings, and so form part of whist conversation. The like, but in less degree of the play of third hand, and in still less degree (usually) of the fourth hand's play.

We see then that, even in the ordinary run of play, whist is full of points to be noticed, and that each card which falls has a definite meaning if the players are all sound, and all careful and steady.

But we recognize this still more clearly when, passing from rules for the play of particular cards to a round, we consider those which belong to the principles of the game.

Thus, it is a principle not absolutely general, but very nearly so, that the first lead should be from the longest suit. A player who recognizes this principle may be said in leading to say, "This is my longest suit"—subject to a few exceptions, as, for instance, when he has no long suit outside trumps, or when his long suit (say a four-card one) is exceedingly weak. Now, this conversational point is held by many players of the modern school, and in my opinion justly, to be worth so much in itself (with hands of average strength) as to override considerations based on the strength of suits. Many still hold that it is better to lead from a short strong suit than from a long weak one; and in many cases it doubtless is. But the advantage, such as it is, does not usually equal the disadvantage of failing to inform partner of the chief constituent of your hand. In the case of the original first lead it very seldom happens that a player should lead from a short strong suit rather than open a long weak one; for the short suit thus opened may be the long suit of one of the opponents. The odds are indeed two to one that it is so. In this case opening it means giving up the command of an enemy's suit, and may turn out disastrously. I remember a case where a player who held four small trumps (hearts), four clubs to the knave, ace king, queen of diamonds, and ace king of spades, led out the king and queen of diamonds, following with the ace. (This, indeed, is the way in which most of the opponents of the modern whist system would play from such a hand.) The third round was ruffed with the king of hearts by the second player, Y,—third player, B, discarding a club, and fourth player, Z, throwing a small diamond, so that it was clear that three diamonds remained with Z. In the first two rounds Z had not signalled for trumps, because though he held ace knave, ten, and a small one, he had not strength outside trumps to justify a signal. Yet he signalled in the second and third rounds, for he held six diamonds, ten, eight, and four others, and the knave fell second round on his right. He had, therefore, his suit established completely, and to the second round played the five instead of the four, his lowest left. When the opening player, A, instead of stopping at sight of the mischief he had already done, went on with the ace, Z completed the late signal by dropping the four. His partner, Y, led the ten of hearts, his best trump of two left, and Z, finessing, played his small trump. Another trump led from Y brought down the queen of trumps to Z's ace, who drew A's remaining trumps, played his three long diamonds, and made three tricks, which, with two by honours, gave Y-Z a treble. (The score was "Love all.") Now, this dismal result was entirely due to A's bad play in giving up command of a short suit, in which, for ought he could tell, the enemy might be strong. As the cards happened to lie, A-B would have made a fair score had A led a club, despite the enemy's superior trump strength; for B held ace, king, queen, and a small club, with queen, knave, ten, and five of spades. Z would certainly not have established his long diamonds but for A's care in handing over the command to him.

Usually, however, players attend to the rule which Pole has expressed in the doggrel rules of whist, where he says, "There is necessity the strongest, Your first lead should be from the suit that's longest." Thus the first lead says in whist language, "This is my longest suit."

Then there is another important bit of whist conversation in the return of the lead. If partner returns the lead at once, he says, in clearest whist language, "I have no suit of my own which is worth leading. I am leaving to you the command of this hand—at least until I see whether the suit you have opened is a strong one or not." Few whist points are more important than this. There can, to my mind, be nothing much more annoying than to have a partner who, when he has taken the first round in your suit, immediately returns it, though he has a strong suit of his own, which, so far as he can tell, may be better worth playing out than your own. If players would always bear in mind that returning your partner's lead at once means weakness outside his suit, or *should* mean that, they would not fall into the error of omitting to give information about their own suit in response to what they have been told by their partner. But, of course, if they really are weak, it is better to return partner's suit, even at the risk of making the same confession to the rest of the table.

It is, however, in the selection of the right card that the return of partner's lead speaks most clearly. Not to know, or, knowing, not to follow, the right rule in this respect, is to play whist very badly, to speak false whist language. Many a game has been lost through partner returning a three instead of a two, or a two instead of a three. In the former case, if the preceding play has shown that the two must certainly be in hand as well as the three, the return of the three means, "I have no other card but the two left;" in the latter case, if the play has shown that the three must certainly be in hand as well as the two, the return of the two means, "I have still another card in the suit besides the three." Many unobservant players imagine that it is only once in a long while that the leader can be deceived in this way; but, as a matter of fact, it is a thing of frequent occurrence, or rather, the occasion for care in the return of the right card lest such mistake should be made, occurs frequently. Thus I lead five of clubs (trumps), having arrived rather early at that critical stage of a hand where I see that if I can get out trumps I may bring in a long suit. My partner takes the trick with the king, seven and four falling from the opponents. Not having in my hand either the two or the three, and knowing that the opponents, who are steady and accurate players, would certainly not have played seven and four if either of them had held three or two, I know that both these cards are with my partner. He carelessly returns the three, though besides the two he holds the knave. I take the trick with trump queen, six and eight falling from the enemy. I then lead the ace, to which fall the nine, the two, and the king. Now in reality all the trumps are extracted from the enemy, and, moreover, I and my partner are in reality two by honours. But I know nothing of this, for my partner has in whist language told me the contrary. His return card said, "I hold no other trump besides the three I thus play, and the two which you know I hold because the enemy have not played it." Now suppose that the previous play has shown me that if the player on my left gets the lead, he must lead from a suit of which I hold the king card, enabling me to bring in my established suit. I jump at the opening (naturally enough, but not wisely, for the only sure course is to go on with the established suit), lead trumps to give him that lead, and find I have brought down my partner's trump on mine instead of extracting the enemy's. My partner has no card in my suit, leads the wrong suit of the enemy's, knowing nothing of the position of the king card I hold, and the enemy are able to work that suit to the bitter end, scoring perhaps the odd trick and the game, though we may want but two to win, and hold these in honours alone. Such mishaps are continually happening through incorrect whist speech.

In the discards again, much whist talk may be held.

In the first place, it is to be noticed that the first discard has chief significance, like the first lead. Later discards have their meaning, of course, according to the state of the game, but not the same definite meaning as the first. Just as the first lead is in nearly every case from the longest suit, so the first discard is usually from the shortest suit. Just as the first lead says to partner, usually, "This is my best suit, and if you have no suit of your own, you should return me this;" so the first discard says, usually, "This is my weakest suit, and is the one you should not lead from, unless it chances to be your own."

Of course exceptions arise which, under ordinary conditions, override the general rule. Thus, if you have king and another in one hand and three or four small cards in another, when you have to make your first discard, you would not unguard the king, but rather play one of the small cards of the longer suit. So if you have ace and another, or only a single card, of a suit which you may afterwards have occasion to lead to your partner, you would not discard from such a short suit, but choose another. Or, to speak generally, you would not cripple yourself in any way, merely to convey to your partner information as to your shortest suit. Yet even in such cases as these, if your partner has given evidence of great strength, you would sacrifice your own hand, even by playing from such cards as these, rather than mislead him as to the position of your shortest suit.

On the other hand there is a general rule respecting the first discard of almost equal importance as the general rule just given—nay, perhaps, when the value of sound defence is considered, of even greater importance. It is this: if the enemy have led trumps, or signalled for them, or have otherwise indicated strength in trumps, you should generally discard from your best protected suit. For, when the enemy are playing a forward game, your whole system of defence depends on keeping the command of the plain suit; and if you and your partner are careful to discard only from well-protected suits, you may so keep the command of the enemy's best suits as to avoid disaster even though you may not be able to achieve any signal triumph. Be this as it may, the first discard when the enemy have shown strength in trumps may usually be interpreted to signify the best protected or usually the longest, instead of the shortest suit, as when the enemy have not shown such strength. This indication is not quite so clear as that given by the discard from the short suit; for the choice of a defensive course is not usually so simple as the choice of the proper method of conducting a forward game. Thus, with a strong game the first discard nearly always signifies a short suit; but with a weak game the first discard cannot so confidently be supposed to be from a long suit; you may see your way to safety best by clearing your partner's long suit, or by discarding from a short suit of weak and useless cards.

Still in every case the first discard has its meaning in whist language, either a simple and unmistakable meaning, or a meaning which is one of two or three possible indications.

I pass now to the "signal," properly so called—viz. the signal for trumps, and will then consider "the Echo of the Signal," and afterwards the "American Leads," which include, and will eventually obliterate, what have been called the Penultimate and Antepenultimate Leads.

The signal or Peter consists in playing an unnecessarily high card to a trick, as the five when you hold the four. The fall of the lower card later completes the signal. This signal means, I have sufficient strength in trumps and outside trumps to warrant me in playing a forward game, and in calling on partner to lead trumps. The signal is imperative; though, of course, a good player would not respond if signalled by a partner whose skill he mistrusted. But to be imperative, the signal must be made at the first opportunity. Made later, it is only to be regarded as a suggestion, not as a command, though I have seen more games obviously made by a late signal than by an ordinary Peter. I remember a case where I saw at about the sixth trick, and after a round of trumps had already been taken out by the enemy, that a game could only be saved (and as it chanced won also) if I were led through in trumps, and made a successful finesse. I held best, third-best, and fifth-best trump. My partner (my life-partner as it happened) saw the signal, and led the fourth-best trump, to which second player dropped the seventh best. I knew my left-hand opponent, original trump leader, had not led from five; so, as six trumps were now played, my partner could hold no more *if* the second best lay on my right; * and if the second best lay on my left it did not matter how I played, for the game was gone. I therefore had, of course, no hesitation in taking my partner's fourth best with my third best, which took the trick. The second best fell next round to the best; and my fifth best drew the last trump from my opponent on the left. I led then a winning card in a plain suit, and to my partner's major tenace in a suit in which my opponent on the right had shown weakness. The game was thus saved by a late signal, and could have been saved on no other line. Another example is given in "How to Play Whist."

The general idea among average whist players is that the signal is of infrequent occurrence, and many tire of looking for it, because in its positive form it appears perhaps in but one of ten or twelve games. But, as a matter of fact, the signal is shown in one form or another in almost every game. This part of whist language is scarcely ever out of use. Unless

* My partner could have but one more card, and that could not possibly be the second best, as in that case it would have been the proper card to lead.

trumps are led out very early, independently of any signal, it scarcely ever happens that the opportunity for the signal does not occur in plain suits to one or other if not to all of the players. Every player who, having the chance of signalling, does not signal, says to his partner, and indeed to the whole table, "I have not strength enough to justify a signal." To look out for information of *this* sort is no idle task, but one which has its reward in nine games out of ten played. In saying this, I refer of course to the watch for the signal all round, not in partner's play only.

To look for the signal in this way systematically becomes, after a short time, quite as easy and simple as attending to the various leads and discards. Just as you note your partner's suit and the suit of either adversary, and have no trouble in retaining them in your mind, so is it with the evidence derived from the signal whether withheld or given. Thus the notes made as the first few tricks are played might run somewhat like this:

My right-hand adversary (A) is strong in hearts. My adversary on the left (B) has not signalled. Neither A nor B is very strong in trumps (spades). My partner (Z) does not signal. Probably trumps are pretty equally divided. B's suit is clubs. Since Z passes a doubtful card, he probably holds four trumps. His own suit is diamonds, which is also mine, and I also have four trumps. The time seems ripe for leading trumps, trusting to diamonds being established between us—from what the first round in diamonds has shown, &c.

Looking out for the signal, shown or withheld, should be considered as normal a part of whist watching as noting leads, discards, and cards returned, whether by partner or by the adversaries in their respective suits. The "echo" is a signal which should in like manner be looked for whenever trumps have been either signalled for or led. It is the token by which the partner of the signaller or trump-leader tells him that he himself holds either less or more than four trumps. When he holds four or more, he signals either in trumps or in a plain suit at the first opportunity. This means in whist language, "I hold four trumps." Failing to give the "echo" when partner has led trumps or signalled for trumps, means in whist language, "I do not hold so many as four trumps."

The "echo" is regarded by many old-fashioned whist players as a useless elaboration of what they call the signalling system. They suppose that not one game in a hundred can give occasion for the "echo." As a matter of fact, the "echo" signal or convention appears either in the positive or in the negative form in quite a large proportion of those games—and they are many—where trumps are led early, or are signalled for. The idea of an opponent of this particular development of the signalling system is that for the "echo" to be played it is necessary, first, that his partner should be long and strong in trumps; secondly, that he should signal; thirdly, that he himself should hold four trumps; fourthly, that he should have an opportunity of echoing; and fifthly, that this opportunity should come in the first few tricks. But, as a matter of fact, the conditions necessary for the "echo" signal to appear effectively in a game are only the two following:—First, one of the four players must be strong enough in trumps to lead them or signal for them; and secondly, his partner must have the opportunity of signalling if he wants to. It is just as important to show by not echoing that you do not hold four trumps as it is to show by echoing that you do; and it is even more important to watch for the "echo" from the enemy, than to note it in your partner, or to proclaim it yourself; for defence is always somewhat more important at whist than attack.

We come, lastly, to the so-called American leads, which constitute in reality but the development of the leads called the Penultimate and the Antepenultimate.

The Penultimate lead was invented by Cavendish (Major Jones), and is itself a development of the lead of a middle card (the lowest of a mid-sequence), as the ten from the king, knave, ten, and others, the nine from king, knave, ten, nine. It consists in leading the lowest card but one from a suit of five or more, not headed by high cards requiring a different lead. Thus from queen, ten, seven, four, three, you lead the four; but from ace, four others (not including the king) you lead the ace, not the penultimate; from king, queen, and three others, not including the knave, you lead the king. But whenever, according to the old-fashioned play, you would lead the lowest from a suit of five cards, the penultimate rule requires that you should lead the lowest but one.

The rule originally required the lead of lowest but one from six cards, or from seven or more. But it was suggested (by Drayson, first I believe) that from six cards the lowest but two, or the antepenultimate, should be led. And some players extended this plan to the lead of the lowest but three from a suit of seven cards, and so on.

Now, no one who has ever fallen into the habit of showing a five-card suit by the penultimate, or a six-card suit by the antepenultimate, and noting also when his partner or the enemy lead in that way, can have failed to recognize a number of cases where he seems to have an opportunity of indicating a five-card or six-card suit in other ways, when the first lead cannot be the penultimate or the antepenultimate. Drayson had already mentioned one case, viz., where you lead ace from ace, five small ones, suggesting that in this case, after leading the ace, you should follow with the lowest but one. But such cases are numerous. Thus you lead king from king, queen, three small ones; and, the king making, you follow with a small one. You feel in this case that as you would lead your lowest if you had had originally but two small ones, the proper course is to lead your lowest but one when you have three, or originally a five-card suit. So with four little ones, you feel that the lowest but two is the proper card to follow with.

Now the first class of American leads makes the system of leading from long suits uniform.

First we have the rule: From a long suit, in which you have to lead a small card, lead *always* the fourth-best card. This rule includes the penultimate, the antepenultimate, and, if one may invent such a word, the pre-antepenultimate from seven cards.

Mr. N. B. Trist, of New Orleans, to whom the systematizing of long-suit leads is due, calls the fourth card of a long suit the "card of uniformity."

Secondly: From a long suit in which you have to open with a high card or cards, and follow with a low one, follow with the original fourth-best card, that is, let your first small card lead be in this case, as in the preceding, the "card of uniformity."

Both these rules are included in this general and simple rule: Play your long suit always as if it were a four-card suit, whatever be the number of cards you may have held in it.

But Mr. Trist has added another development of a system by which Cavendish had shown how long suits may be indicated. It used formerly to be the rule that from ace, queen, knave, and others (one or more) you led ace and followed with queen. But Cavendish noticed in actual play that this sometimes led to your long suit being blocked by partner, who, if he holds king guarded second round, drops the small card to the queen, and when the suit is led again, and he has won with the king, has no card with which to return the suit. If you held originally only four cards of the suit this would be right, because one of the enemy might hold four in the suit, headed by the ten, and capturing your queen with the king, would leave the fourth trick to the adversaries. But if you held originally five, and your partner three, it is so unlikely that either of the enemy holds four, that the chances are in your favour if partner captures the second trick with his king. To induce him to do this, you lead the knave instead of the queen. This means in whist language, "I hold the queen and two more at least." By capturing the knave, and leading either at once or when the right time comes a small card, your partner often enables you to make the three remaining tricks of the suit.

Here, again, I suppose that every one who has employed this device must have felt repeatedly that it would be very convenient if the system were extended, so that whenever it is a matter of indifference, so far as (1) the mere strength of the card, and (2) the information it conveys as to your high cards, which of two high cards you play first, then the play of the lower first should indicate that you held originally more than four. Observe that this rule can never affect the original lead, since the rules for leading are so definite that it can never be a matter of indifference which of two high cards in sequence is first led. And again, in many cases where you have choice of two high cards equal in strength for your second lead, it is by no means a matter of indifference which you select. Thus from ace, king, queen, knave (plain suit), with or without others, the leads are, king first, then knave. This is the only way in which you can say, in whist language, "I led from ace, king, queen, knave." Here it is only on the third round that you can show you had four only, or more, originally, viz., by leading the ace in the former case, and the queen in the latter.

This is the development actually proposed by Mr. Trist. His third rule, which includes the whole of his leads of the second class, may be thus worded—of two high indifferent cards, lead the higher with a suit of four originally, the lower with a suit of five.

Now these American leads have been subjected to a good deal of criticism. Whist players may be divided into two classes, those who think every change an improvement by which the language of the game is made fuller and clearer; those who think such changes undesirable. Cavendish and his followers consider that with a good partner the American leads are a gain; the others (and be it understood that I am speaking of good players on this side also) consider that against observant enemies the American leads do more harm than good.

For my own part, I cannot but think that Cavendish and his school pay too little attention to the diversity of cases which may arise, when they advocate the uniform acceptance of the American leads. With regard to the echo, the signal, the rules for discarding, leading, returning leads, and so forth, no exception is to be taken against the general principle that plain sailing is best. But I have always felt that in the case of the penultimate and antepenultimate a fixed rule is a little dangerous; and I feel this still more strongly in the case of the American leads. That they should replace the penultimate, which is indeed included within them, is sufficiently obvious, but that they should be followed uniformly has not, I think, been fully shown. Cavendish has tried pretty often in the *Field* to show that we should above all things adopt a uniform system in such matters. But it might as reasonably be urged that we should always discard from our shortest suit rather than attempt to guard ourselves against declared strength of the enemy's.

I would advocate a modified acceptance, only, of the American leads. Since it is clear that a long suit can be of little value against strong hands, and especially trump strength, with the enemy, it appears to me that the American leads (certainly those of the first class) should be employed only when the strength of the rest of the hand justifies a certain degree of confidence. I would regard the use of the American system as a signal of more than length in that particular suit, as showing in fact either a strong hand, or recognition of strength through partner's trumps and strong cards.

Used with this understanding and limitation, I believe the American leads to be a great gain and improvement to the system of playing long suits.

Lastly, I would suggest one other saying in whist language, the utility of which I have more than once noticed in playing with a partner who has accepted the principle. In discarding from a long suit (that is, when strength has been declared by the enemy) the original fourth-best of the suit should be selected, in my opinion, for the first discard. In Game XXIII. of my little treatise on whist, the penultimate is discarded in such a case, and the game saved through the information thus conveyed. But the card of uniformity would be the proper card to throw out in all such cases.

CHAPTER XXIV.

BUT IS WHIST-SIGNALLING HONEST?

A RESOLUTE attack has been made upon the methods of signalling which have now been long in vogue in whist circles. Not only is their value for the whist-player questioned, but doubt is thrown on their honesty. In an article by a well-known player of great strength (over the initial "M." of his *nom de plume*, but quite unmistakably from Mogul's resolute whist pen), we have this last point so strongly insisted upon that the article, which bears the title "Whist—Rational and Artificial," might well have been called "Whist—Honest and Dishonest." "We do not see," says M., "why a game like whist (usually played for money) should be altered and spoilt for the sole benefit of the best players, and feel that Cavendish's views bring us face to face with the question, Are signals legitimate play? This point has never been thrashed out, and it is quite time it should be. No one will dispute that for players to say, by word of mouth or by finger-signals, what signals say, would be unfair. But we can see no difference between such signals and preconcerted modes of playing the cards to convey the same information. Signals are in no way more legitimate because every one at the table knows their significance than giving the information orally or using finger-signals of which every one knows the meaning; the two things are in principle identical. Let us test the question in this way. Suppose two of our signallers went to a French club, could they honourably use all their signals without first explaining them and intimating that they intended to use them? And suppose our Gallican friends replied, 'That is all very well; but we object to your using them for two reasons—the one that until we have constantly practised them they will give you an advantage, and the other that we consider them in direct contravention of one of the corner-stones of the game, viz., that players shall not by preconcerted signals give their partners any information as to their hands; if, therefore, you insist on using them, we shall consider it unfair play, and act accordingly.' Can any one say the Frenchmen would not be justified in using such language? And, if justified, is it not because signals are essentially improper? Even if within the letter, they are absolutely opposed to the spirit of the established rule of etiquette, which says, 'No intimation whatever, by word or gesture, should be given by a player as to the state of his hand.'"

The considerations here advanced appear to me to merit most careful consideration, nay, not merely to merit or require, but imperatively to demand such consideration.

It will be known to many readers that, in my little treatise "How to Play Whist," and also in my much smaller "Home Whist," I have described, and by silence may seem to have adopted, the various signals and conventions now in vogue, except only what is called by Cavendish "the echo in plain suits." (Of this last method I may simply remark in passing that I believe it about as likely to take its place in whist-play as a system by which each player should be allowed, when it is his turn to play, some half an hour's time for calculating the chances of the various lines of play open to him.) In both works, but definitely in the smaller, I have dealt with whist as a recreation. It is because whist is such a fine game for recreation and for rest, when played properly, that I have thought it well to address a class of readers for whom Cavendish can hardly be said to write, endeavouring to show them how, by giving a very little time to the study of certain points, they can turn the dreary inanity of family whist (as too commonly played) into brightness and pleasantness. But of whist as a means of gambling I take no account whatever. I should have imagined that whist would scarcely be more suited for money-play than chess or draughts, had I not learned that there are many good whist-players who value it for this quality more than for any other, if not for this quality alone. There is, I am told, a certain satisfac-faction in sitting down to a game which may be made, by suitable stakes and wagers, as lively as baccarat, while yet the players have all the satisfaction of being engaged in what is ostensibly a game depending on skill. Besides, since the game does depend on skill, insomuch that, as Cavendish himself has shown, the skilful player has points in his favour, and must in the long run come out ahead, there is the further satisfaction for the gambling mind (always affected by a slight strain of moral weakness) that the whist-player may become to some degree the controller of his own fortune. But whether whist is a good or bad gambling game was assuredly a point which I have had very little in my thoughts when writing about this fine game. All gambling whatsoever is for me simply detestable. I hold it to be immoral, though I by no means hold all gamblers to be immoral; for many of them are justified, as many according to the teaching of Catholic ethics are saved, by the plea of "invincible ignorance." (This plea may signify either that the right view of the matter has never occurred to them, or that, when it has been presented to them, they have honestly been unable to see it.) Still, even in dealing with whist from a non-gambling point of view, the question of the fairness of signalling is one which must be carefully considered—while, of course, from the gambling point of view, it becomes one affecting common honesty.

Now when Cavendish, who may be regarded as the chief priest of the signalling-whist religion, first presented the various conventions and signals, he was very careful to describe them as developments of whist principles. "Pembridge" has shown that no actual principles were involved, and that the conventions were not developments. But these are details. In all other respects Cavendish was right enough. He was especially right in trying to show, even though unsuccessfully, that the conventions had their origin in points of play; for in this there was an earnest endeavour to show that they are honest.

Let us consider some of these defences.

When your partner has led a suit in which you are weak numerically, it is often a point of whist strategy to return him your best as a strengthening card. This helps him in more ways than one. It forces out good cards from the enemy; it enables him to finesse as if your card were one of his own; and it has the further advantage of showing him, but quite legitimately, that you are short in the suit. But while this lead of a strengthening card of two cards left, say a knave or ten instead of a small one, is manifestly legitimate as a part of whist strategy, the case is surely altered when having two small cards left you lead the higher as a conventional way of showing that you hold only two. Strategy does not require you to lead the three, for instance, from three, two, or even the five from five, two. You cannot possibly help your partner by so doing—at least, not in one case out of a thousand. No whist-player would ever think of returning the five rather than the two, because of any superiority in strength which the five possessed over the two. But of course the lead of the five from five, two, or of the three from three, two, according to the present conventional system, is a matter of considerable importance. It means either when the lower card falls, or if through the previous play your partner knows that you hold the lower card—"I have but these two cards left in the suit." Therefore your partner has a hint as to the hands of the opponents. Now this conventional meaning is necessarily a matter of preconcerted arrangement. It matters not whether it is arranged just before the game to which you are sitting down, or last year, or fifty years ago; it does not even matter whether it is arranged between A and B for a single game, or known to all the players of a club, or of a county, or of a continent. It is a prearranged convention in no sense depending on whist principles. Mogul says it is dishonest, and there is a good deal to be said for that view; but even if it be regarded as legitimate, it involves a change in the game. The rules ought definitely to say it shall be (or shall not be) lawful for a player to make use of this particular convention for the sake of informing his partner that he has not more than two left after the first round in his partner's suit.

Take next a more important question, the signal *par excellence*, more important as affecting the lead of trumps in cases where on the leading of trumps the question of making or failing to make a great game may altogether depend.

Suppose that a player, A, who has a strong hand, especially in trumps, holds the knave and a small one in a suit which is led by the enemy, he being fourth player. If the trick is won by third player with the king or queen, A may perhaps deem it well to play the knave rather than the small one. For, while the knave will fall, and most probably fall in-effectively next round, its play first round may lead the enemy to suppose A holds no more in the suit, and therefore to lead trumps lest one of their strong suits be ruffed, or lest perhaps a cross-ruff fatally injurious to them should be established. Now supposing the enemy not thus entrapped to lead trumps, A's partner, if he is an old hand, will naturally observe A's attempt to get trumps led by the enemy, and will therefore at the first opportunity lead them himself. It would be the same if A played knave from knave, ten, instead of from knave and a small card not in sequence with it, except that in this case the device, as costing nothing, would not imply quite so strong a wish for a trump lead as in the other case. In every such case, where a player obviously played a higher card where a lower one would have done as well, or—if not in sequence—even better, to induce the enemy to lead trumps, there has been an expression of a wish that trumps should be led. And this wish has been expressed in a manner strictly in accordance with whist strategy. The player has done what seemed good for his game and his partner's, and the partner, if a player, seeing what has thus been held good strategy, makes his inferences accordingly, precisely as he does from the play of his partner, or of either opponent when leading, or when second, third, or fourth hand. This is part of the game, and the issues of such manœuvres are among the *gaudia certaminis*.

There is all the difference in the world, however, between a piece of strategy like this and the signal or Peter as now established. To see this, one has only to consider what the signal would look like to a keen player not knowing its conventional significance, and seeing it for the first time. He would say to himself, What on earth can my partner (or either enemy) mean by playing the four when he held the two? He could gain nothing by it. It must have been sheer carelessness. I must ask him (if partner) at the end of the hand, or (if opponent) at the end of the rubber. Now if the meaning of the signal were thereupon explained to him and he were invited always to employ the signal when needed, and always to respond to it when displayed by a trustworthy partner, in what respect would this explanation differ from a direct attempt to introduce means of communication between players depending on matters entirely outside the game? If he asked an opponent, Why did you cough twice just before playing? and the opponent said, In our club that means, "The card I am playing is my last in the suit;" but two coughs followed by a sneeze imply that trumps are to be led *instanter*, he would probably say, "I would rather not play in your company." But really there is not much to choose between the two methods of signalling. And I think, with Mogul, there is absolutely nothing to choose, so far as fairness is concerned, between the Peter and a system (generally admitted, if that makes any difference) by which opening a suit of a different colour from trumps should be understood to mean all-round strength.

As for the signal (though thus deduced from a strategic detail) being a development of a principle, that is in truth all nonsense. It is not a principle at whist that you should play a high card to deceive the enemy into leading trumps when you want trumps led. This is but a device, often found effective, but assuredly no principle. And the play of the higher of two indifferent cards is not even a development of this device. It is something entirely different; for it is play which, of itself, could not possibly induce the enemy to lead trumps. One might as well take for a signal the play of all cards with the right hand when trumps are not wanted, and with the left hand when they are.

As for the "echo of the call," that is so essentially artificial a signal that there is something suggestive of audacity in the attempt to treat it as the development of any principle. Your partner has signalled, or has led trumps from strength, and you desire to show him that you too are numerically strong. You can do this by holding up four or five fingers, by coughing violently, by sneezing, or in a hundred different ways, the objections against which are scarcely more obvious than those against the method actually adopted. Certainly nothing in whist strategy would lead any one to play the higher of two cards, either in a plain suit or in trumps, before the lower—without intending to take a trick—because he had as many as four trumps. This, however, is the return signal or "echo of the signal"—play at the first opportunity an unnecessarily high card before a lower one, when you wish to show your partner that you hold four trumps, after he has already indicated great trump strength, either by signalling or by a trump lead.

When we consider the penultimate and its recent development in the American leads, from the same point of view, we find equally strong reasons for regarding this method of leading as an unfair dodge, suitable only for such a work as Sharper Shuffle's "Private Treatise on Signs at Whist, by Way of Counter-Treatise to Hoyle's" (see "The Humours of Whist"). The only shadow of a principle suggested by Cavendish is found in the occasional lead, as a part of whist strategy, from the lowest of an intermediate sequence in a strong suit. If you are leading from queen, ten, nine, eight, two, for instance, you may very properly lead the eight, because by that lead you diminish the chance of the trick being taken very cheaply. This chance, be it observed, is rather greater than usual when you hold three such intermediate cards as ten, nine, eight; your partner's cards in the suit may all be below the seven, the six or even the five, any one of which may take the trick if you lead the two. So, again, the ten is the regular lead from king, knave, ten, and others, though some whist-players regard the lead as of doubtful validity. But it is not by any means a general principle at whist that you should lead the lowest of an intermediate sequence when you have one. Nor, if it were, could the lead from an intermediate card, *not* the lowest of a sequence, be regarded as a development of a principle depending on the lead from an intermediate sequence. Yet we have long been invited to regard the lead of a four, for instance, from queen, ten, seven, four, two, as the same in principle as the lead of eight from queen, ten, nine, eight, two, and have been advised accordingly to lead the lowest but one of a suit of five cards or more. A point of whist strategy has been made to suggest a whist sign such as Shuffle would have rejoiced in; and we are told that the sign is fair because it is the development of a principle. The penultimate signal cannot thus, I think, be reasonably justified.

Then "come you in" the American leads, which are certainly a development of the penultimate (and therefore are *not* the development of the development of a principle, as is claimed for them), and have the advantage of simplifying the lead from long suits. For the rule they assign for leading from such suits when you do not lead a high card is simply, Lead the original fourth best. But inasmuch as the only reason which can conceivably be assigned for this plan is that thereby you give your partner information, for there can be no strategic reason for leading a five instead of a two, I venture to assert that there is no whist justification for the convention. That every one at the table has an opportunity of reading the signal as your partner can is certainly no justification. Every one at the table can read an exposed card, or a card led or played in error; but whist laws do not allow you, therefore, to lead out of turn, or in any other way to show your partner and the table that you hold such and such cards. They impose a penalty, which often has a most damaging effect, for all such misdoings; and I may express here my opinion that to make even home whist enjoyable such penalties should be carefully enforced. One cannot, of course, impose any penalty on a player for leading four or three before two; for careless players never make any distinction between the small cards, and so signal promiscuously all the time they are playing. But we can make signalling an offence against whist etiquette, only to be condoned on the plea of incorrigible carelessness. The argument that the American leads, the penultimate, and so on, though not resulting from any considerations of whist strategy, are legitimate because they belong to the play of the cards in accordance with whist law, would apply to a system by which in leading from a four-card suit one would place the card with its length directly towards partner, from a five-card suit place the length on a line passing between partner and left-hand opponent, with a six-card suit place the card with its length from opponent to opponent, and with a seven-card suit place it with its length extending in a line passing between left-hand opponent and yourself; while the lead from a short suit might be indicated by putting the first card at least a foot from the centre of the table. If this were understood by all a little attention would enable every one at the table to avail himself of the information; but this kind of attention would assuredly not be whist.

There is a certain point of strategy in another department of the American leads. Cavendish was once playing from a long suit headed by ace, queen, knave (six cards in all), and—following the ace with the queen, as formerly was the recognized rule—had his long suit blocked by his partner. The queen made, and when the third round was played Cavendish's partner took the trick with his king, and, having no small card left in the suit, had to lead a losing card, so that opponents made their strong suit and won the game. Here manifestly there was defective whist strategy, and the lesson taught should have been, *not* that a conventional or recognized system of going on with long suits should be introduced for the sake of conveying information to partner, but rather that to all recognized rules there are exceptions. Seeing the opportunity of making a great game with his long suit, and the risk that if his partner had the king and failed to play it soon the chance would be lost, Cavendish ought certainly to have led the knave, thereby either forcing out the king from the enemy, or drawing it from his partner, who, following the sound rule that you should seldom attempt to finesse in your partner's suit, would have played king on his partner's knave. Whether in the actual game Cavendish's partner had any opportunity of throwing away his king, and so clearing his partner's suit—the nature of which, after the second round, should have been clear to him—is not mentioned in the story as usually told. Possibly even that would not have saved the suit, as the third lead in it may only have been open to the partner, not to the original leader. If there was a chance of discarding the king, Cavendish being sure of re-entry, then the partner played ill too. Be this as it may, Cavendish noted the experience, and corrected his manner of leading from ace, queen, knave, to five at least, thereafter.

That was whist. The great value of whist as a game is in the opportunities it gives the player of learning from experience, and there is always good promise in a player who is thus ready to learn.

But unfortunately Cavendish and Mr. Trist (the inventor of the American leads) have made a general principle out of this point of strategy, and having developed this imaginary principle, have deduced the cut-and-dried convention that in the second round of a long suit, the two best cards of which are in your hand, you should play the lowest of the two best if you had originally more than four cards in the suit. Observe that, as we have just seen, leading the third best second round when you have both the second and the third best, is a point of whist strategy, if you are long in the suit; and there is, therefore, no objection to that: it conveys information in a perfectly legitimate way, because you have no occasion to explain beforehand to any whist-player the meaning of your play. He sees it at once, because he reasons: "My partner would assuredly not have played the lowest of his top sequence, knowing I should be likely to waste my king-card upon it, unless he wanted this card out of his way; he must, therefore, know in some way that when the king-card has been thus drawn he will have complete command in the suit; he doubtless has two or three cards left besides the remaining card of the head sequence from which he is leading. Therefore I may play my king, and if I have a small card left in the suit I either lead it to him at once, or diligently keep it, to return him the suit when trumps are out or the right time has otherwise obviously come." But if, going on second round with the two best declared in his hand, my partner leads the lower rather than, as is customary, the higher, I have no indication from any recognized principle of whist strategy as to the meaning of his departure from a rule based on the simplest whist principles. It might be simply a slip of the fingers, or carelessness, or an attempt to mislead the opponents. Only if the nature of the convention has been explained to me can I find any meaning in it; and so far as trick-making is concerned, the convention might have any meaning whatever given to it. Leading the lowest of two indifferent high cards (when the highest is naturally played) might mean "I have four trumps, or I am short in right-hand opponent's suit, or I play for a cross-ruff;" or, for that matter, it might simply be a way of saying to partner and opponents, "I have a bad headache, and play accordingly."

But the American leads, which are little short of an abomination of desolation in themselves (considered as a whole), have led to further developments, which, if admitted, must utterly ruin whist as a game. As I put the matter long ago in the pages of *Knowledge*, Cavendish seems to be in no way troubled that he is spoiling the game by knocking the brains out of it, so long as he can bring in some new additions to the developments, which are no developments of principles, nor even real developments of certain devices which were never more than occasional points of strategy. Mogul goes farther, and says of Cavendish's "Whist Developments" that it makes him exclaim, "It were better for whist if Cavendish had never been born." But, for my own part, I rather rejoice at the appearance of that most unattractive work, for I know that it will do more to destroy the growing conventionalities of whist than any amount of direct opposition. Gamblers, as a rule, are a foolish nation, and though they will learn easy tricks quickly enough, they have not in the main capacity for such developments as Cavendish is trying to introduce under the pleasing title of "The Echo in Plain Suits." Those who love whist for itself, seeing the lengths to which professionals are prepared to go in arranging a system of signs, will cease to play with them at a game which will no longer be whist. In rejecting the new developments whist-players of the unprofessional sort (*amateurs* are so called because they love the game) will be led to question the propriety of other so-called developments. When they do so, I doubt if even the Blue Peter will escape, long though it has lasted. I have done something in my "How to Play Whist" to kill the signals, by showing what a quantity of signalling has to be attended to when not only the positive but the negative aspect of the signals is taken into account. I believe that every one of these conventions has injured the game. At any rate, as I wrote last year in *Knowledge*, if not the active causes of decay, they are its signs and tokens. Let the game be restored to its original purity. It assuredly should be seen that it needs some cleansing when players are asking whether, when whist is played for money, the system of signalling is *honest*

APPENDIX.

OUR GALAXY.

Letter to Sir J. Herschel.

BY RICHARD A. PROCTOR.

THE following correspondence will be found chiefly interesting as introducing the latest ideas which Sir John Herschel expressed about the star depths. The reader may find some interest also in noting the earliest inquiries made by myself into the same subject.

London, *July* 24, 1869.

I have for several months had it in my thoughts to write to you respecting certain views which seem to me to result from what has been discovered respecting stars and nebulæ, and in particular from the unrivalled series of observations which the scientific world owes to your father and yourself. I have, however, had several reasons for delaying. I have not cared to bring the matter before you until I had gathered a sufficient amount of evidence—and also I have had some diffidence in making a statement which sounds at a first hearing as though intended for a correction of the impressive theories of Sir W. Herschel; though all who have really studied his successive essays on the universe will recognize in my work merely an attempt to advance on the road which he pioneered. If the attempt is a failure you will not the less readily recognize its object.

Although the evidence I have now gathered is far from being so complete as could be wished, yet it is, I think, sufficient to support many important general conclusions. If it might not seem so to others I think it will to you, who have the means of forming a much readier decision than any man of science now living could do. This is, in fact, one of my reasons for submitting my views to you rather than to any other. There are many eminent mathematicians, many profound physicists, but I know of no one but yourself who is at once mathematician, physicist, and observer, while possessing besides that knowledge of the whole range of astronomical facts which seems required in deciding on matters such as those I wish to submit to you.

My object is, briefly, to show that there are reasons for modifying our views as to the distribution of stars and nebulæ throughout space. Our own galaxy appears to be rich in a variety of forms of matter or of aggregation not hitherto ascribed to it. It would seem that within a single region of our sidereal system there may be collected—stars of every variety of magnitude, star-groups of every degree of resolvability, star-streams, gaseous masses, gaseous systems, and in fact all the features which we have been in the habit of looking upon as characteristic (not of any finite portion of our system but) of that quasi-infinite expanse of discernible space whereof our own galaxy has been thought to occupy but a corner.

I will present the points of the evidence as they occur to me, without in general indicating their bearing on my views (to save time and also because you will see in a moment how each part of the evidence bears on the case). Many of the facts are of course already well known to you: but I mention them to make the case complete.

1°. When the irresolvable nebulæ in your large catalogue are isographically distributed (in the manner carried out by you on a smaller number) we find that they show a marked preference for extra-galactic space. Their withdrawal from a given great-circular zone, if not accidental, indicates their association with the sidereal scheme by *some* law. And, as the separate nebulæ have every variety of position, the coincidence of our own galaxy (assumed to be *one* of the nebulæ) with the vacant region—both in place and in position or direction—is too remarkable to be readily accepted as but an accident.

2°. The very easily resolvable nebulæ affect the galactic zone very decidedly, the less easily resolvable nebulæ affect that zone less markedly, the irregular nebulæ (gaseous) are all on or close to that zone, the planetary (also gaseous) affect its neighbourhood. I need not point out the separate significance of these facts, or their yet greater significance when taken in combination with the fact that irresolvable nebulæ withdraw from the neighbourhood of the galaxy.

3°. Wherever there is a large extra-galactic space singularly clear of lucid stars there also irresolvable nebulæ are singularly few in number.

4°. But where amidst a number of small stars there is a small space singularly clear of stars, there nebulæ are abundant (as both your father and yourself testify).

[These are not contradictory, as they seem at first sight. If we suppose irresolvable nebulæ to be simply aggregations of small stars and star material in place of single (relatively) large stars, we can understand that nebulæ and lucid stars should be wanting *together*, or that in small-star neighbourhoods the formation of nebulæ should *drain* the district. Both facts require to be accounted for as not accidental, and no other explanation seems possible, while this one seems at once natural and satisfactory.]

5°. There are in places well-marked star-streams—such as the River Eridanus, which the moderns have carried farther towards the South Pole, and the stream from the water-can of Aquarius, which the moderns have carried over the back of Grus towards the south.

6°. The two last-named streams of stars lead up to the two Magellanic Clouds.

7°. There are well-marked nebular streams in both hemispheres; but two singularly distinct streams in the southern.

8°. The two last-named streams of nebulæ lead up to the Magellanic Clouds.

9°. One of these streams agrees with one of the star-streams named above, the other with the other!

10°. In the Magellanic Clouds are mixed up stars and nebulæ.

[The Clouds and the evidence they give have been so clearly treated in your "Outlines of Astronomy" that I have no reason for dwelling on the significance of 10°. In conjunction with 5°–9° the evidence seems to me absolutely conclusive as to the association between stars and nebulæ.]

11°. Every one of the irregular nebulæ is strangely associated with stars, which seem (but only seem) to lie much nearer to us. Your maps (in "Southern Observations") and descriptions appear to me conclusive on this point, even where you seem to avoid enforcing the conclusion or to adopt a contrary one. If the coincidence could be accidental in one case could it in so many?

12° The Orion nebula has long faint branches extending to ϵ and ι Orionis and condensing around those stars. Can this be accidental? And if not, does it not prove these important points: first, that a nebula may be no farther off than lucid stars, and secondly, that lucid stars so far apart as ϵ and ι Orionis may belong to a single system?

13°. All those stars mixed up with the Orion nebula whose proper motions have been ascertained, have proper motions singularly small, and coincident in direction—indicating (in a new way) their belonging to one system.

14°. The other stars in Orion have for the most part the same characteristic proper motions (nor need exceptions at all perplex us, considering that we ought to expect them in such cases).

15°. All the stars in Orion (says Secchi) have similar spectra. (Even if this remark is inexact the general significance of what Secchi has observed cannot be misunderstood.)

[The last points go to prove the real oneness of the Orion stellar and nebular system.]

16°. In particular regions of the heavens particular spectral types prevail—indicating the existence of *star-systems* among the constellations—*i.e.*, among lucid stars. The evidence given by this fact, looked on as subsidiary to the evidence for star-streams, seems important.

17°. In particular regions of the heavens the stars seem to be drifting in a mass towards the same directions.

[I send a rough copy of a portion of a map of proper motions I am preparing for next meeting of the Royal Astronomical Society.]

18°. The proper motions of stars of smaller magnitudes are not small enough (on the average) for the accepted views as to star-distances. I have taken the proper motions in Main's Catalogue of 1,167 stars common to Bessell's Catalogue and Greenwich Catalogue, and using the formula :—

$$\text{Mean proper motion} = \sqrt{\text{sum of square of proper motions.}}$$

I get :—

Mean proper motion of stars of 1st 3 mags. $= 0''\cdot301$ } !!
" " " next 3 mags. $= 0''\cdot302$ }

[I believe this result to be much more than sufficient to prove 18° ; but I do not by any means accept it as it stands —or as otherwise than accidental so far as its significance in regard to detail is concerned. Its general significance is as trustworthy as it is obvious.]

19°. The same result follows from the small effect of the correction due to the sun's proper motion, when estimated with reference to the commonly-assumed stellar distances.

[I venture to point out that it is doubtful whether Mr. Dunkin's result on this point is, as you suggest, that which was to have been *expected.* Does not your reasoning, founded on the number of stars which are moving hither and thither with their *real* proper motions, apply both ways? Every one of those stars is affected by the sun's proper motion. I find by the ordinary rules for determining means that the correction due to the sun's motion ought to be exactly one-half of the sum of the squares of the

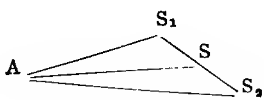

FIG. A.

proper motions, if the sun's motion is equal to the average of stellar motions. There is a simple geometrical proof of this, the chief points of which are as follows :—Let S A Fig. A = displacement due to sun's motion, $S\,S_1 = S\,S_2$ = motion of two stars in opposite directions from S, *foreshortened* and really equal to S A. Then,

Sum of squares of opposite motions $= S_1 A^2 + S_2 A^2$,
Sum of squares of ditto *uncorrected* $= S S_1^2 + S S_2^2$, and former sum exceeds latter by $2 S A^2$ (one $S A^2$ for each).

And the effect of foreshortening on all motions from S, precisely corresponds to the effect of change of position of S in diminishing S A. Hence for the whole sphere there is an exact equality between the effect due to the stars' own motions and the effect due to the sun's motions, assuming,—

1. Sun's proper motion = stars' average proper motion.
2. Motions equally distributed in all directions.
3. Stars equally distributed through space.

The evidence (obtained by Mr. Dunkin) that the sun's motion does *not* correct the stars' motions in this proportion—*i.e.*, by one-half, establishes the probability that the distances of stars of smaller mags. have been underrated—and that therefore the extent of the effect of the sun's motions upon such stars has also been underrated.

20°. The stars of smaller mags. are not so numerous as they should be according to the theory of uniform distribution.

[Professor Nichol speaks of their number as "according with their theoretic distances," but Struve's hypothesis of the extinction of light was devised to account for a marked want of accordance in this respect.]

21°. The general appearance of the Milky Way is not that which either the theory of a cloven disc, or that of a flattened ring, requires.

[Sir W. Herschel was gradually modifying the disc theory—necessarily the *first* to be adopted after his gauging processes; and you have spoken in many places of the evidence which appears in form of a modified view.]

22°. The well-defined edges of parts of Milky Way prove we are outside its streams; for no cluster of objects within which (cluster) the observer is situated can anywhere appear with well-defined edges.

23°. The great gap in Argo is not explicable on either the disc or the flat-ring theory.

24°. The "Coalsack," though more readily explicable on the latter than on the former, remains a difficulty with either.

25°. The singular want of lucid stars in the "Coalsack" and between the branches in Scorpio, &c., is another difficulty.

26°. The break in the second stream of the Milky Way is a great difficulty.

27°. The knots and clustering aggregations along the Milky Way seem to require a different theory.

[The view I would suggest as roughly explaining observed phenomena is the following*:—This explanation accounts

FIG. B.—A perspective view of Milky Way supposed to be depicted on a crystal globe.

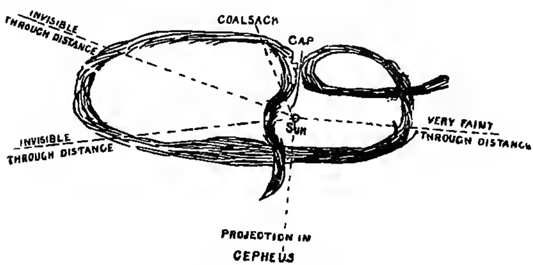

FIG. C.—Suggested general figure of Milky Way spiral in space.

for the singular diminution of brightness from Cygnus to Ophiuchus, and the equally singular increase from Cygnus to Aquila.]

28°. The lucid stars near the Milky Way seem to be intimately associated with it—especially in certain regions.

29°. Along that part of the Milky Way which I have put

* The figures are copied directly from the sketches in the rough draft of my letter.

on the nearest part of the spiral the stars have in many instances singularly large proper motions—also 61 Cygni, and α Centauri, the nearest known stars are on this branch.

30°. The proper motion of the Sun is such as the dynamical conditions suggested by my spiral seem to require.

31°. There are in many parts of the Milky Way outlying streamers. On either the flat disc or the flat ring theory we cannot readily conceive these to be "columnar excrescences bristling up from the general level." Yet were it not for the strong antecedent improbability thus indicated, this would be the natural conclusion, because it is clearly unlikely that *several* sheets of stars would be seen edgewise, even though *one* might be.

32°. In places, a projection of the Milky Way has its apex coincident with a bright star.

Many other points might be dwelt upon—as

33°. The fact that certain forms of aggregation are singularly common in certain regions (double stars are less common in Southern heavens, you have noted).

34°. Variable stars are more common in some places than others—and sometimes variables in certain districts are characterized by the same law and mode of variation, &c.

35°. The "access to the Nubeculæ* on all sides is through a desert" (Why? unless those regions have been *drained* to form the Nubeculæ).

36°. The fact that many regular nebulæ are singularly associated with single, double, or multiple stars.

37°. The existence of those faint "mottlings" (forming streams) which you detected in the southern heavens.

38°. The fact that in such clusters as Præsepe and Perseus we find nebular tracts (in the former two distinct nebulæ), and that the probability is enormously against this being accidental.

39°. The breaking out of new stars on the borders of the Milky Way.

40°. The indicated existence of dark orbs (causing the irregular proper motions of Sirius and Procyon).

41°. The occurrence of such a change in a star as would lead to the appearance of a so-called "new star"—since it

* Another name for the Magellanic Clouds.

seems far more reasonable to believe that a really minute star has thus suffered a fire change than that a sun resembling ours has suddenly blazed out to many times its usual brilliancy.

42°. The variability and disappearance of many nebulæ.

But for the present the facts before cited seem sufficient for my purpose.

Per contra I know of very few definite facts. There is the general feeling of experienced observers that the nebulæ are far beyond our sidereal system. But on such a point experience avails nothing—or rather no one has yet obtained any experience enabling him to form an opinion, *from observation*, on this point. Experience can tell an observer whether a star apparently single is likely to be doubled with a moderate increase of power; whether an unresolved nebula is or not resolvable and so on: because on these points an observer *can have* experience. But the opinion an observer may form as to the distance of a nebula can never be confirmed or disproved, and so he can never acquire real *experience* on this subject.

Many view the careful study and analysis of observations, work which they call "paper astronomy," with contempt. You are not likely to share that feeling, remembering that Copernicus, Kepler, and Newton—to name no more—were paper astronomers. I feel that there is room for a good deal of paper astronomy on the subject of this letter; and that it would be good service to enlist fellow-labourers in *paper-work.* It is not that I undervalue observation—rather that others do. A large mass of observations is available to modern thinkers—but most men prefer to make new observations. In this way the real worth of observation is lost; and further, many who take part in it would do better work in other directions.

My object in writing is very much as follows:—I have been trying to show that useful results could be obtained if a few who have time would overhaul the accumulated mass of observations now lying at the disposal of astronomers. I have myself very little time to work at this subject. If I can save an hour or two a week for such work it is as much as I can do without injustice to my family. I should like to see many of those now engaged in accumulating pre-

D D

posterously useless observations at work in the field over the fence of which I have looked. A hundred modes of inquiry should be followed up; observed facts should be co-ordinated and rendered significant; new modes of observation should be devised. Thus may clear views, perhaps, be obtained respecting that which it has hitherto seemed hopeless to inquire about—the arrangement and configuration of the star-groups in our own neighbourhood; the architecture of the nearer portions of the sidereal heavens. —Yours very truly,

R. A. PROCTOR.

THE UNIVERSE.

LETTER TO RICHARD A. PROCTOR.

BY SIR JOHN HERSCHEL.

COLLINGWOOD, *Aug.* 1, 1869.

I hope you will not regard my delay in replying to your letter of the 24th [ult.] as indicating any absence of interest on my part in its subject matter, or in the views you have been led to base on the constitution of our sidereal system. On the contrary, it has been precisely by reason of the number and variety of the striking facts you have brought together, and the evident bearing of a great proportion of them on the great problem it offers to human speculation, that I have been unwilling to make a hasty reply.

I have never participated in that feeling which you designate as "a contempt for astronomy on paper" in the sense of repudiating such speculations as aim at grouping together known but apparently disconnected facts, within one general view, in consonance with known laws—holding that in the midst of so much darkness we ought to open our eyes as wide as possible to any glimpse of light, and utilize whatever twilight may be accorded us, to make out, though but indistinctly, the forms that surround us.

Under this impression I should not feel so bigoted to the "ring" or the "disc theory" of the Milky Way as to reject your proposed system of convolutions, which certainly seems to give a plausible, if not a fully satisfactory account of the gap in Argo, and the break in the following branch of the double stream about Ophiuchus; though I confess that the "Coalsack" does not at all give me the idea of the perspective view of a loop, formed by the visual ray passing over one portion, and under another,* of one and the same continuous belt: and the disappearance by the simple effect of remoteness of the interrupted stream in Ophiuchus, would involve, in consequence, a regular progressive diminution in magnitude of the component stars, in approaching the break from either side, down to unresolvable nebulosity and thence to evanescence.†

I do not see the absolute necessity for placing the sun fairly out of the Belt. Distant portions of the Belt from which the sun's position is quite remote may still appear sharply terminated—and it is a fact that abundance of stars of all magnitudes *are seen* in situations inclined at all angles to the galactic plane, their density increasing gradually and regularly down to that plane.‡

The considerations you adduce relative to the proper motions of the stars are exceedingly curious and interesting.§ Of late years catalogues have gone into so much

* I am quite with Sir John Herschel on this point, since I have seen the Southern heavens.—R. A. P.

† By no means necessarily. It is easy to conceive a star-cloud or stream in which the arrangement of the stars of various orders would be such that no order would ever present unresolvable nebulosity at any distance, while the several orders would present the same appearance at all distances, the brighter orders taking the place of the fainter (apparently) as the distance increased.—R. A. P.

‡ On trying Sir John Herschel's gauges in the Southern heavens on a plan proposed by me, and accepted as trustworthy by Sir J. Herschel, Mr. Sydney Waters found the increase of density to be—as I had anticipated—by no means gradual and regular, but rapid, and, as it were, sudden, in the neighbourhood of the Milky Way, and irregular over the visible cloud-like gatherings in the galaxy.—R. A. P.

§ Sir George Airy, in a letter addressed to me in February, 1873, confounded my discovery of Star Drift with what Pond, his predecessor as Astronomer-Royal, had already noticed. But Pond's observation applied solely to the general drift of all the stars in the heavens, on account of the sun's proper motion through space. This is a very different matter from the drift of star groups which I had indicated in my maps of stellar proper motions.—R. A. P.

detail, and with such accuracy, that these motions are, of course, much better known to us than some twenty or thirty years ago. The community of proper motions over large areas, of which you give a picture in Gemini and Cancer, is most remarkable, and the coincidence of proper motion in β, γ, δ, ϵ, and ζ, Ursæ Majoris is most striking. Your promised paper on this subject cannot fail to be highly interesting.

I cannot say that I am at all surprised at its being found that the average proper motions of stars of small magnitude is not less than of large, considering (as I have always done) that the range of individual magnitude (*i.e.*, lustre) must be so numerous that multitudes of *very* minute stars may in fact be our very near neighbours. But your remark (in your No. 19.) on the conclusion I have been led to draw relative to the small effect of the correction due to the sun's proper motion will require to be very carefully considered, and I shall, of course, give it every attention.

In my Isographic Projection of the Nebulæ I made no distinction or separation into classes, &c. You appear to have done so (in your second proposition); and the connection of the great mass of globular clusters with the Milky Way in the region about Sagittarius and Scorpio has led me to conclude, as you seem to have done, a decided connection with, and, in fact, inclusion, in that system, of the clusters in question.

A remark which this connection, and the structure of the Magellanic Clouds, has often suggested to me, has been strongly recalled by what you say of the inclusion of every variety of nebulous or clustering form within the galaxy:—viz., that if such be the case (*i.e.*, if these forms belong to and form part and parcel of the galactic system), then that system includes *within itself miniatures of itself* on an almost infinitely reduced scale: and what evidence, then, have we that there exists a universe beyond—unless a sort of argument from analogy that the galaxy with all its contents may be *but one* of these miniatures of that* more vast universe;

* Possibly a mistake for "a," "that" being anticipated from the emphatic "that" presently required, and, as it were, *felt coming*. But the reference may, of course, be to the previously mentioned "universe beyond."—R. A. P.

and so on *ad infinitum;* and that there may be in *that* [more vast universe] multitudes of other systems on a scale as vast as *our* galaxy; the analogues of those other nebulous and clustering forms which are not miniatures of our galaxy.*

I hope you will not be deterred from dwelling more consecutively and closely on these speculative views by any idea of their hopelessness which the objectors against paper astronomy may entertain—or by the real slenderness of the material threads out of which any connected theory of the universe (at present) has to be woven. *Hypotheses fingo* in this style of our knowledge is quite as good a motto as Newton's *Non fingo*—provided always they be not hypotheses as to modes of physical *action* for which experience gives no warrant.—I remain, dear sir, yours very truly,

J. H. W. HERSCHEL.

* Here, as elsewhere in this letter, Sir John Herschel writes in a condensed form, very suitable for the occasion (addressing as he was one who was already fully engaged upon and with the subject), but not quite so fully, or therefore so clearly, as he would have deemed necessary had he been writing to one viewing the subject from outside. The idea of the universe here presented, one of the grandest that could be imagined, may be illustrated thus:—Imagine insects, examining twigs from some distance, at first under the mistaken idea that they were really boughs, much farther away than they seemed to be. If now any evidence suggested that these objects were not so far away, and being near were *really* smaller than the boughs, a thoughtful insect might say, They seem to belong to the bough, and some of them to be miniatures of it; if so, he might continue, What evidence have we of a bough beyond, unless a sort of argument from analogy that this bough with all its twigs may be but such a miniature of a more vast kind of bough—[a *tree* as our wider knowledge tells us]—and that there may be in that [tree] multitudes of other boughs, on a scale as vast as ours, those other boughs corresponding on a larger scale to the other twigs on our bough which are not miniatures of it?—R. A. P.

Letter to Sir J. Herschel.*

BY RICHARD A. PROCTOR.

London, *Aug.* 8, 1869.

I should have written sooner to thank you for your very kind and encouraging letter, but for great pressure on my time. I wish also to mention, when writing, certain points I had accidentally omitted from my former letter.

You are not to suppose that I write now to meet the objections you have mentioned against my notions respecting the Milky Way, though I may have occasion to do so as I proceed. I do not wish to regard my views as something to be defended. I should have studied your writings and example to little purpose if I took this line. I may not hope, perhaps, to attain easily that placidity with which you are able to urge or to consider objections *against* hypotheses whose strong points you had shortly before exhibited; but I have at any rate definitely set that quality before me as the one which is of all others the most valuable to the searcher after truth.

First of the omitted facts is the discovery made by Lieut. Herschel that rich star-clusters sometimes show—besides the continuous spectrum—the bright-line spectrum of the gaseous nebulæ. This fact seems to me to be of the utmost importance. We find in it a fresh bond of union between all the members of the nebular family. Mr. Huggins had shown that the planetary nebulæ are all gaseous. Your own researches had confirmed this and had rendered it highly probable that all the irregular nebulæ are also gaseous. On the other hand, it had seemed that the clus-

* This letter, like that in No 1, New Series, is from the rough draft of the letter actually sent; and I am unable to say in what degree the latter differed from the first form. I am under the impression that I sent a much condensed letter. It is so unusual with me to make a draft of any letter I may have to write that I feel sure I eventually sent a letter differing much from the present in form; but, on the other hand, the circumstance that I kept the rough draft convinces me that in substance this letter presents what I actually wrote.

ters are a class apart, while the irresolvable nebulæ (not belonging to planetary or irregular classes) seemed to occupy a position midway between the two classes of gaseous and stellar nebulæ; since they showed in about equal proportions bright-line or continuous spectra. I was so convinced that this separation of the nebulæ into sets would be done away with after awhile, that I asked Mr. Huggins if it had occurred to him to look for signs of the bright-line spectrum superposed on the continuous spectrum given by the stellar nebulæ. He answered that his own view was similar to mine, but he had hitherto been unsuccessful in proving its justice. Lieut. Herschel's observations have supplied the necessary evidence. It is scarcely necessary to dwell on the significance of the facts thus brought together (I repeat some facts already referred to) :—

1. Irregular and planetary nebulæ affect neighbourhood of the Milky Way.

2. Irresolvable nebulæ segregate themselves from the Milky Way.

3. Clusters imitate the behaviour of the gaseous nebulæ.

4. The first and third sets of nebulæ belong to the extreme classes as respects gaseity or non-gaseity.*

But, 5. All the classes of nebulæ are brought together again by the discovery that their gaseity is simply a question of proportionate degree.

6. All the classes (including the irregular class) are brought together in the Magellanic Clouds.

* When we arrange the nebulæ (I.) in order of their apparent stellarity, (II.) in order of their gaseity, and (III.) in order of their association with the Milky Way, we get the following three columns :—

I.	II.	III.
Clusters.	Planetary and Irregular Nebulæ.	Clusters.
Irresolvable Nebulæ.	Irresolvable Nebulæ.	Planetary and Irregular Nebulæ.
Planetary and Irregular Nebulæ.	Clusters.	Irresolvable Nebulæ.

Mr. Huggins' discovery (confirmed by Lieut. Herschel's observations) that the gaseous nebulæ show a faint continuous spectrum as well as the bright-line one; and Lieut. Herschel's discovery of the converse fact that stellar nebulæ show a bright-line spectrum as well as the continuous one make tables I. and II. highly significant.

The main inference from this little set of facts is that the gaseous matter so common in the nebulæ exists in all parts of the galactic system, and serves as a sort of index of the oneness of the sidereal and nebular systems.

The second omitted fact is the discovery made in recent times of the singular complexity of the solar system. Forced to take the solar system with which we suppose ourselves familiar as presenting in a general way a type of the wider system of which it is a part, we clearly are led to form different views now than analogy suggested when as yet astronomers knew so little of our system. The discovery of the asteroids has done much towards this change of view; but I think the knowledge recently acquired respecting meteor-systems tends much more importantly to give us just conceptions of the richness and complexity of our solar scheme. I do not so much refer to the strange discoveries recently made respecting the orbits of the meteors, the association of these bodies with comets, and so on; though the evidence on all these points seems too definite and complete to leave any room for question respecting them. The point to which Prof. Herschel has called so much attention—I mean the large number of meteoric systems which our earth traverses—and the consequent argument from probability that there exist millions of such systems within the solar scheme—seems to me far more important. It teaches us to look for an enormous wealth of relatively minute bodies in other systems, and therefore prepares us to look on the "suns" in the sidereal scheme as relatively few, the minute orbs and the groups of minuter orbs as relatively numerous.

Your father had no such analogy to guide him; but he was, I believe, so steadily progressing towards a change of view respecting stellar distribution (I judge from some of his later papers) that I think he might soon have been able to reverse the analogy, and *infer* the existence of multitudes of minute bodies within the solar system from the analogy which the sidereal scheme presents.

Lastly, I should have referred to the question of the extinction of light. It seems to me to have escaped notice that the arguments in favour of the extinction of light and those against it are equally irresistible on the accepted theories of stellar distribution.* The *argument* on which Struve founded his formula

$$\text{brightness of a star} = \frac{1}{(\text{dist})^2}\,(0{\cdot}990651)^{(\text{dist}-1)}$$

has never, I believe, been disposed of; though arguments of equal force have disposed effectually of his extinction *theory*. Now I think that when irresistible arguments can be urged, on a given hypothesis, both *for* and *against* a certain theory, we may reasonably assume that there is something wrong about the given hypothesis.

You disposed of Struve so completely, for instance, that I believe every one has since looked on the theory of extinction as exploded; yet it was rather by arraying stronger arguments against his than by destroying the force of what he had put forward that this was done. *You* put the matter directly on this footing; but others who have quoted the result have quite forgotten that a difficulty was admittedly left unaccounted for. (I am not referring to Struve's misinterpretation of some words of your father's, but to his argument drawn from the insufficient number of faint stars.)

According to the views I have been led to form the question of the extinction of light seems an open one—but several facts seem to suggest that there *is* appreciable extinction even within such a distance as separates α Centauri from us.

I think on a reconsideration of my views you will see that (according to them) increase of distance would not necessarily lead to nebulous light. It seems to me that neither does the presence of irresolvable nebulous light necessarily indicate extreme distance, nor the converse. If we have a certain group of stars, and that group be supposed to move continually away, the question whether it will ever become nebulous (with *any* power if there be no extinction, or with a given power if there be extinction) depends wholly on the relation between the size of the com-

* When I speak of accepted views, I refer to those which are continually described in treatises on astronomy as the direct fruits of your father's researches and your own. I know well that there is not a single line either of your own or your father's writing in which the question of stellar distribution is spoken of as one on which we are in a position to form a definite theory.

ponent stars and the distances which separate them. If the two stars A and B, in the cluster shown in Fig. D, are of such size and so distant that when just disappearing to the naked eye they are clear of each other in appearance, they will be equally so when just disappearing for any power whatever, unless there is extinction.

We know there are parts of the Milky Way where there is irresolvable nebulosity: and the question at once suggests itself, Have we any evidence whether this is to be looked upon as a proof of indefinitely vast extension of the galaxy in direction of the nebulosity? It seems to me that we have positive proof that this is *not* the case. I will take the case of the clustering aggregation in Perseus. Your father's account of this spot shows that every accession of

FIG. D.—A Star-Group.

power is followed by the resolution of more and more of the spot, but that the highest powers fail to resolve some parts. Also the spot contains stars of the seventh magnitude. If, then, we are to look on irresolvability with given powers as a test of distance, the true figure of the system which appears to us as a spot is a frustum of a cone—as A B C D, Fig. E—having the sun, S, at its vertex. And from the size of the largest telescope used by your father (irresolvability under which implied, he considered, a distance which light would take 20,000 years in travelling) we should have A B at least 100 times greater than C D. This seems utterly contrary to all reasonable probability; and I think we have precisely the same sort of argument here which you have drawn from the Magellanic Clouds—

the conclusion being, that within limits of distance which are as about seventy to seventy-one (remembering the small area of the spot in Perseus) there may coexist stellar arrangements resulting in all degrees of resolvability, from star-groups almost resolvable by the naked eye down to absolute irresolvability in the largest telescopes man has yet constructed. Irresolvability being thus shown to be no test of distance, it seems to follow, *e converso*, that absence of nebulous light under given powers is no proof of relative nearness.

If the gap in Ophiuchus is really due to distance, however, there are certainly some indications we might look for along the fading extremities of the two arms which here extend towards each other. One is, as you point out, the diminution of star-magnitude; but it is to be noticed that all we could look for is the absence of stars of the brighter telescopic orders. The background of this part of the Milky Way would not differ from the background elsewhere. There would be stars of all orders from those just visible up to a certain magnitude—elsewhere there would be stars of all orders from those just visible up to a certain higher order of magnitude. The difference could only be determined by careful and systematic observation directed to that special end. If there is no such difference my notion about the Milky Way would have to be abandoned, or at least looked on as not probably correct.

FIG. E.—Imagined—but impossible—shape of a Star Cluster like that in Fig. D.

You will notice that my theory indicates the possibility that an infinite variety of constitution may exist in different parts of the Milky Way.

I am not sure that one could expect the Milky Way near Crux and thence to Argo to present any obvious signs of the structure I suggest—supposing my suggestion correct. The apparent difference between the nearer and farther streams would be very slight. Even in our own neighbourhood questions of relative distance are so difficult to determine that one might expect this one to defeat any save the most systematic inquiry, directed to the special purpose of determining whether there are any signs to guide us here. I am perplexed by the appearance given to the Milky Way in this neighbourhood in the large maps of the S. D. U. K. The rest of the Milky Way is shaded to a uniform tint, and is obviously incorrect in many places. But in the south-polar map the artist has suddenly indulged in the strangest variations of shade. What authority he had, if any, is not stated. One cannot help feeling he must have had some—perhaps notes by Lacaille or others. *Inter alia*, the Milky Way is shaded much more darkly along one side of the Coalsack than along the other; and presents in an exaggerated form all the appearances my theory would lead one to expect. Whether some southern traveller noticed a slight difference of brilliancy, which has somehow been expanded into the singular arrangement I have mentioned, or whether the whole matter is the result of carelessness I cannot say: but one actually seems to see one stream crossing the other. This I noticed after my theory was formed, so that I was naturally rather interested in the peculiarity: but if there *is* any such difference of brilliancy it must be very slight. Would it be possible that such a feature could have escaped your notice? You remember Faraday used to say when an experiment was to be shown him, "What am I to *look for?*" And it seems just possible that, not having the thought of looking for such a feature, or of attaching any importance to it if seen, it might have escaped even your unrivalled powers of observation. I put forward this view with extreme diffidence, and am quite prepared to accept your opinion as decisive as to the appearance of the Milky Way here, if you feel clear that the delicate difference in question could not have escaped you.

According to my theory, the proper motions of the stars in Crux should be larger towards the α end of the cross than towards the other. I try the experiment, though without putting very much faith in it—as the proper motions assigned to southern stars seem to require revision.

One cannot make much, I fear, from what is shown in Fig. F. Of course it is part of my theory that the bright stars, such as those in Crux, sway the Milky Way wisps, and are mixed up with them.

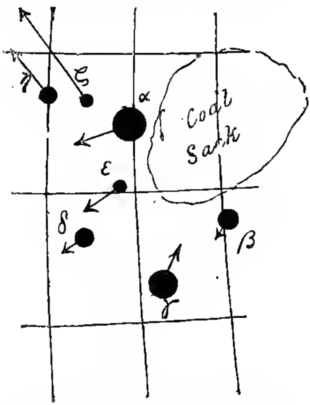

FIG. F.—Proper motions (in 36,000 years) of stars in the Southern Cross.

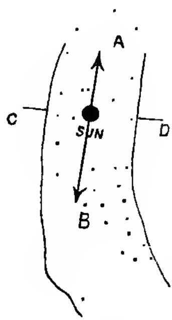

FIG. G.—Illustrating the effects of our sun being in the midst of a stream of stars.

I did not mean to suggest that the sun is altogether out of the plane rich in stars within which the Milky Way streams have (according to my view) been formed. But I cannot find any other interpretation of a well-defined edge *anywhere* along the Milky Way than this, that the sun is out of that particular gathering to which the edge belongs. He might be on one part of the stream and we should still see such an edge when looking at other parts. But there is this difficulty :—If the sun is altogether out of the stream, but near its medial plane, we should see precisely the sort of increase towards the galactic plane that we actually observe, the stream only being the nuclear line (if one may speak so incorrectly) of a spiral region rich in small stars. But if the sun were *in* a stream, we ought to see an aggregation of small stars towards two opposite points on the heavens—as towards A and B (Fig. G)—and a zone wanting in small stars towards C and D.

I am not so fond of my twisted-stream theory as not to recognize its weak points. But it seems to me we are forced to take *some* stream-theory, when we apply the laws of probability to the appearances presented by the Milky Way. I think, too, that we are bound to look on the stars composing the Milky Way as really minute in comparison with Aldebaran and its like. Projecting the Milky Way to the distance due to the apparent smallness of its component stars assumed nearly equal to lucid stars, I cannot understand the many singular correspondences between its arrangement and that of the brighter stars. But looking on it as a stream swayed by the leading stars, one seems to get a general conception of its nature according satisfactorily with observed appearances.

Have you ever noticed the singular contrast between the poverty of the heavens (in lucid stars) from Centaurus to Monoceros on one side of the Milky Way, and the extreme richness from Crux to Orion on the other side?

Your suggestion that the Galaxy contains within itself miniatures of itself is very beautiful, and doubtless points to a great truth, as yet but dimly seen.—Yours very truly,

R. A. PROCTOR.

EXTRACTS FROM LETTERS TO RICHARD A. PROCTOR.

BY SIR JOHN HERSCHEL.

COLLINGWOOD, *Aug.* 20, 1869.

One of the arguments advanced in favour of spatial extinction of light was that *if not* the whole heavens ought to be one blaze of solar light—admitting the universe to be infinite, because it was contended that there could then be no direction in space in which the visual ray would not encounter a star—*i.e.*, a sun. This argument is fallacious; for it is easy to imagine a constitution of a universe literally infinite which would allow of any amount of such directions of penetration as *not* to encounter a star:—

Granting that it consists of systems subordinated according to the law that every higher order of bodies in it should be immensely more distant from the centre than the next inferior order, this would happen. Thus—in our own—the moon is very near the earth and the satellites to their primaries. These primaries are immensely more distant from the sun, *their* centre. The fixed stars again still more immensely more remote from the sun. Suppose *our* system to terminate with the visible fixed stars. Then imagine a system of such systems as remote from each other *in comparison with their own dimensions*, as the distance of the fixed stars in comparison with the diameter of the solar system. Such systems seen from each other would subtend no greater angle than a star seen from the sun,* and so on.

May 11, 1870.

Among the innumerable ways in which an almost infinite multitude of luminaries of all sizes and brightnesses, from [10,000 suns] down to [an ounce of red-hot stone]† may be

* Wherefore it follows, we may note in passing, that the nebulæ which subtend much larger angles than this are utterly unlike the neighbouring galaxies as analogy would lead us to expect that these would appear to us—that is as mere points in apparent size, and as also exceedingly faint in apparent intrinsic lustre.—R. P.

† The brackets here are used as in mathematics. Sir John intended me to understand that each luminary might be anything from a group of 10,000 suns down to an ounce of red-hot stone. Without the first pair of brackets I might have understood him to represent a multitude of luminaries, instead of a single luminary, by the 10,000 suns. The second pair of brackets was rendered necessary—logically —by the use of the former pair, which could not be avoided. This condensed way of writing is very convenient and useful when the writer knows that his correspondent will not misunderstand him; but, of course, it would be very unsuitable in addressing the general public on scientific subjects.—R. P.

distributed in space, so as to appear to our eyes and telescopes just as our Milky Way and sidereal firmament does, I see no distinct reason for or against a spiral, discoid

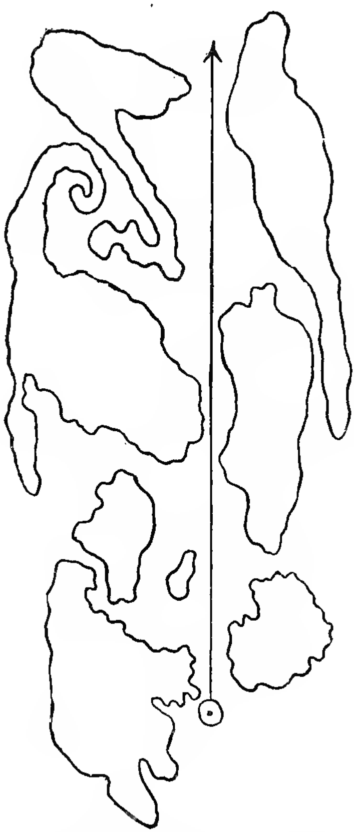

FIG. H.—Showing how a "Coalsack" in the Milky Way may be explained.

annular, or cellular arrangement. As regards the openings in the form of "Coalsacks," I do not quite see that what you say ("Other Worlds than Ours," 1st Edition, p. 256) as to a channel having a particular direction and perfectly straight, is necessary.

Imagine, for instance, such a form (not merely plane, but tridimensional) as *this* (Fig. H) for the star-groups and galactic masses. This would leave quite as good a passage for the visual ray out into space as the neatest cut chimney.*

COLLINGWOOD, *April* 1, 1870.

I have been examining my star-gauges in reference to the very curious and interesting statistical relations your letter communicates [these are given in my essay on a novel way of gauging the star-depths, "Essays on Astronomy."—R. P.]; but I see that in so far as the relation of the grouping to the plane of the Milky Way is concerned, the results there arrived at [that is in Sir J. Herschel's star-gauges] are applicable only for stars below 8 mag., and for stars 8 mag. and upwards those gauges afford no ground for any conclusion one way or other,—*i.e.*, in the mode in which they are there grouped.

It would perhaps be worth while to regroup them [be it understood that Sir John Herschel is here referring to his own gauges] for the regions within and without your boundary-line, which—if I understand you right—divides the globe of stars 6 mag. and upwards into two very unequal segments—a rich and a poor one—having to each other the radii of about 5 to 2 in area, with no reference to the Milky Way, but cutting straight across it!—certainly a very startling fact, and none the less so that it should have the *Nubecula Major* for its centre. Still it seems almost too sudden to jump to a conclusion as to a real concentricity resulting from a physical connection—the more especially as the B. A. Catalogue can hardly yet be taken as effectively defining the limit of demarcation between 6 mag. and 7 mag.

* The only objection is that the overlapping star-clouds would give marked variations of brightness around a "coalsack," whereas a general uniformity is observed.—R. P.

COLLINGWOOD, *July* 28, 1870.

I thank you for calling my attention to that section in my "Outlines."* Undoubtedly there *is* a discordance of statements which requires correction. But the appeal there is rather to the statistical result of actual enumeration; and assuredly, on a cursory view of the heavens on a clear night, stars down to the 7th and 8th magnitude *do* affect the eye, though we cannot *fix* them by reason of that strange law which curtails a star *directly* looked at of a very large aliquot part of its photometric effectiveness.

I am very glad you are taking an independent line, and utilizing the immense additions which have been made to Uranography in the way of numerical accumulation and improved knowledge of proper motions, &c. We may—indeed must—form theories as we go along [what else do we observe *for?*—R. P.] and they serve as guides for inquiry, or suggestions of things to inquire—but as yet we must hold them rather loosely, and for many years to come keep looking out for side-lights.

[I had submitted to Sir John Herschel my idea of a method of presenting his gauges of the southern heavens (and also, if possible, his father's) on an equal surface projection. I may remark that the idea was never carried out by myself, because Mr. Sidney Waters kindly undertook to carry it out: and placed his results, published in the Monthly Notices of the Astronomical Society, at my disposal. His valuable chart will be found in my "Universe of Stars." What prevented me from attending to that work was that I had entered on another work of a far more laborious nature, and calculated to throw much clearer light on the subject of stellar distribution in space. I refer to my equal-surface chart of the stars in Argelander's series of forty large charts. This chart, which I was not able even to begin (so few were my leisure hours) during Sir John Herschel's life, occupied me about 400 hours, which I could only with great difficulty make free out of my working time.]

* I had noticed that Sir John Herschel's recognition of a band of bright stars running nearly on the line of the Milky Way was not in accordance with his remark that stars of the higher orders are not more richly strewn in the Milky Way than elsewhere.—R. P.

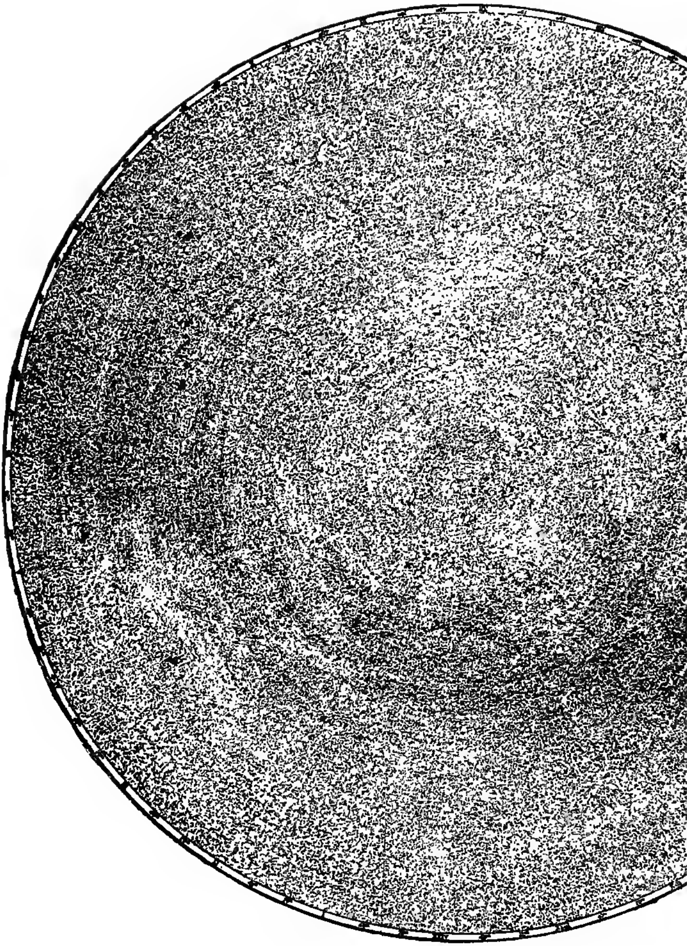

FIG. J.

FIG. K.

Fig. L.

[*To face page 418.*

COLLINGWOOD, *July* 28, 1870.

I have been thinking over plans for laying down the star-gauges, and I can light on none that seems to promise better than that which you suggest. The chief obstacle to carrying it out will, I suppose, be found in the fact that often all the gauges do not cover the whole surface, and that there are necessarily considerable areas ungauged.

July 31.—I have kept this unfinished in hopes of being able to lay my hands on the mass of graphical projections, &c., which formed the groundwork of what is said about the distribution of stars in galactic parallels in my Cape observations, thinking they might save you some trouble. I know they exist, but after a great search and turning over the contents of many portfolios and boxes, I have been unable to lay hands on them—so I will not longer delay replying to your letter—and doubt not that you will quite as well accomplish your object from the registered statement of gauges—both those of my father and my own.

COLLINGWOOD, *Feb.* 7, 1871.

The two star-charts you have been so good as to send me (representing the northern and southern heavens isographically), are very interesting. The contrast between the star density under the arch of the Via Lactea to the right of the *Nubecula* in the southern chart as compared with that in the diametrically opposite place in the northern is striking.

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