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ASTRONOMICAL DISCOVERY

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ASTRONOMERS ROYAL.

ASTRONOMICAL DISCOVERY

BY

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WITH PLATES

TO
EDWARD EMERSON BARNARD
ASTRONOMICAL DISCOVERER
THESE PAGES ARE INSCRIBED IN MEMORY OF
NEVER-TO-BE-FORGOTTEN DAYS SPENT WITH HIM AT THE
YERKES OBSERVATORY
OF
THE UNIVERSITY OF CHICAGO

PREFACE

The aim of the following pages is to illustrate, by the study of a few examples chosen almost at random, the variety in character of astronomical discoveries. An attempt has indeed been made to arrange the half-dozen examples, once selected, into a rough sequence according to the amount of "chance" associated with the discovery, though from this point of view Chapter IV. should come first; but I do not lay much stress upon it. There is undoubtedly an element of "luck" in most discoveries. "The biggest strokes are all luck," writes a brother astronomer who had done me the honour to glance at a few pages, "but a man must not drop his catches. Have you ever read Montaigne's essay 'Of Glory'? It is worth reading. Change war and glory to discovery and it is exactly the same theme. If you are looking for a motto you will find a score in it." Indeed even in cases such as those in Chapters V. and VI., where a discovery is made by turning over a heap of rubbish—declared such by experts and abandoned accordingly—we instinctively feel that the finding of something valuable was especially "fortunate." We should scarcely recommend such waste material as the best hunting ground for gems.

The chapters correspond approximately to a series of six lectures delivered at the University of Chicago in August 1904, at the hospitable invitation of President Harper. They afforded me the opportunity of seeing something of this wonderful University, only a dozen years old and yet so amazingly vigorous; and especially of its observatory (the Yerkes observatory, situated eighty miles away on Lake Geneva), which is only eight years old and yet has taken its place in the foremost rank. For these opportunities I venture here to put on record my grateful thanks.

In a portion of the first chapter it will be obvious that I am indebted to Miss Clerke's "History of Astronomy in the Nineteenth Century"; in the second to Professor R. A. Sampson's Memoir on the Adams MSS.; in the third to Rigaud's "Life of Bradley." There are other debts which I hope are duly acknowledged in the text. My grateful thanks are due to Mr. F. A. Bellamy for the care with which he has read the proofs; and I am indebted for permission to publish illustrations to the Royal Astronomical Society, the Astronomer Royal, the editors of *The Observatory*, the Cambridge University Press, the Harvard College Observatory, the Yerkes Observatory, and the living representatives of two portraits.

H. H. TURNER.

UNIVERSITY OBSERVATORY, OXFORD,
November 9, 1904.

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LIST OF PLATES

ERRATA

Page 133, line 27, *for* "200 stars" *read* "200 stars per hour."

" 145, See note on page 220.

" 146, bottom of page. This nebulosity was first discovered by

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ASTRONOMICAL DISCOVERY

CHAPTER I

URANUS AND EROS

Popular view of discovery.

Discovery is expected from an astronomer. The lay mind scarcely thinks of a naturalist nowadays discovering new animals, or of a chemist as finding new elements save on rare occasions; but it does think of the astronomer as making discoveries. The popular imagination pictures him spending the whole night in watching the skies from a high tower through a long telescope, occasionally rewarded by the finding of something new, without much mental effort. I propose to compare with this romantic picture some of the actual facts, some of the ways in which discoveries are really made; and if we find that the image and the reality differ, I hope that the romance will nevertheless not be thereby destroyed, but may adapt itself to conditions more closely resembling the facts.

Keats' lines.

The popular conception finds expression in the lines of Keats:—

> Then felt I like some watcher of the skies
> When a new planet swims into his ken.

Keats was born in 1795, published his first volume of poems in 1817, and died in 1821. At the time when he wrote the discovery of planets was comparatively novel in human experience. Uranus had been found by William Herschel in 1781, and in the years 1800 to 1807 followed the first four minor planets, a number destined to remain without additions for nearly forty years. It would be absurd to read any exact allusion into the words quoted, when we remember the whole circumstances under which they were written; but perhaps I may be forgiven if I compare them especially with the actual discovery of the planet Uranus, for the reason that this was by far the largest of the five—far larger than any other planet known except Jupiter and Saturn, while the others were far smaller—and that Keats is using throughout the poem metaphors drawn from the first glimpses of "vast expanses" of land or water. Perhaps I may reproduce the whole sonnet. His friend C. C. Clarke had put before him Chapman's "paraphrase" of Homer, and they sat up till daylight to read it, "Keats shouting with delight as some passage of especial energy struck his imagination. At ten o'clock the next morning Mr. Clarke found the sonnet on his breakfast-table."

> SONNET XI
> *On first looking into Chapman's "Homer"*
>
> Much have I travell'd in the realms of gold,
> And many goodly states and kingdoms seen;
> Round many western islands have I been
> Which bards in fealty to Apollo hold.
> Oft of one wide expanse had I been told
> That deep-brow'd Homer ruled as his demesne;
> Yet did I never breathe its pure serene
> Till I heard Chapman speak out loud and bold:
> Then felt I like some watcher of the skies
> When a new planet swims into his ken;
> Or like stout Cortez when with eagle eyes
> He star'd at the Pacific—and all his men
> Look'd at each other with a wild surmise—
> Silent, upon a peak in Darien.

Comparison with discovery of Uranus.

Let us then, as our first example of the way in which astronomical discoveries are made, turn to the discovery of the planet Uranus, and see how it corresponds with the popular conception as voiced by Keats. In one respect his words are true to the life or the letter. If ever there was a "watcher of the skies," William Herschel was entitled to the name. It was his custom to watch them the whole night through, from the earliest possible moment to daybreak; and the fruits of his labours were many and various almost beyond belief. But did the planet "swim into his ken"? Let us turn to the original announcement of his discovery as given in the Philosophical Transactions for 1781.

XXXII.—Account of a Comet
By Mr. Herschel, F.R.S.
(Communicated by Dr. Watson, jun., of Bath, F.R.S.)
Read April 26, 1781

Original
announcement.

"On Tuesday the 13th of March, between ten and eleven in the evening, while I was examining the small stars in the neighbourhood of H Geminorum, I perceived one that appeared visibly larger than the rest; being struck with its uncommon magnitude, I compared it to H Geminorum and the small star in the quartile between Auriga and Gemini, and finding it to be so much larger than either of them, suspected it to be a comet.

"I was then engaged in a series of observations on the parallax of the fixed stars, which I hope soon to have the honour of laying before the Royal Society; and those observations requiring very high powers, I had ready at hand the several magnifiers of 227, 460, 932, 1536, 2010, &c., all which I have successfully used upon that occasion. The power I had on when I first saw the comet was 227. From experience I knew that the diameters of the fixed stars are not proportionally magnified with higher powers as the planets are; therefore I now put on the powers of 460 and 932, and found the diameter of the comet increased in proportion to the power, as it ought to be, on a supposition of its not being a fixed star, while the diameters of the stars to which I compared it were not increased in the same ratio. Moreover, the comet being magnified much beyond what its light would admit of, appeared hazy and ill-defined with these great powers, while the stars preserved that lustre and distinctness which from many thousand observations I knew they would retain. The sequel has shown that my surmises were well founded, this proving to be the Comet we have lately observed. [Pg 5]

"I have reduced all my observations upon this comet to the following tables. The first contains the measures of the gradual increase of the comet's diameter. The micrometers I used, when every circumstance is favourable, will measure extremely small angles, such as do not exceed a few seconds, true to 6, 8, or 10 thirds at most; and in the worst situations true to 20 or 30 thirds; I have therefore given the measures of the comet's diameter in seconds and thirds. And the parts of my micrometer being thus reduced, I have also given all the rest of the measures in the same manner; though in large distances, such as one, two, or three minutes, so great an exactness, for several reasons, is not pretended to."

Called first
a comet.

At first sight this seems to be the wrong reference, for it speaks of a new comet, not a new planet. But it is indeed of Uranus that Herschel is speaking; and so little did he realise the full magnitude of his discovery at once, that he announced it as that of a comet; and a comet the object was called for some months. Attempts were made to calculate its orbit as a comet, and broke down; and it was only after much work of this kind had been done that the real nature of the object began to be suspected. But far more striking than this misconception is the display of skill necessary to detect any peculiarity in the object at all. Among a number of stars one seemed somewhat exceptional in size, but the difference was only just sufficient [Pg 6]

Other
observers
would not
have found
it at all.

to awaken suspicion in a keen-eyed Herschel. Would any other observer have noticed the difference at all? Certainly several good observers had looked at the object before, and looked at it with the care necessary to record its position, without noting any peculiarity. Their observations were recovered subsequently and used to fix the orbit of the new planet more accurately. I shall remind you in the next chapter that Uranus had been observed in this way no less than seventeen times by first-rate observers without exciting their attention to anything remarkable. The first occasion was in 1690, nearly a century before Herschel's grand discovery, and these chance observations, which lay so long unnoticed as in some way erroneous, subsequently proved to be of the utmost value in fixing the orbit of the new planet. But there is even more striking testimony than this to the exceptional nature of [Pg 7] Herschel's achievement. It is a common experience in astronomy that an observer may fail to notice in a general scrutiny some phenomenon which he can see perfectly well when his attention is directed to it: when a man has made a discovery and others are told what to look for, they often see it so easily that they are filled with amazement and chagrin that they never saw it before. Not so in the case of Uranus. At least two great astronomers, Lalande and Messier, have left on record their astonishment that Herschel could differentiate it from an ordinary star at all; for even when instructed where to look and what to look for, they had the greatest difficulty in finding it. I give a translation of Messier's words, which Herschel records in the paper already quoted announcing the discovery:—

"Nothing was more difficult than to recognise it; and I cannot conceive how you have been able to return several times to this star or comet; for absolutely it has been necessary to observe it for several consecutive days to perceive that it was in motion."

No

We cannot, therefore, fit the facts to Keats' version of them. The planet did not majestically

reveal itself to a merely passive observer: rather did it, assuming the disguise of an ordinary star, evade detection to the utmost of its power; so that the keenest eye, the most alert attention, the most determined following up of a mere hint, were all needed to unmask it. But is the romance necessarily gone? If another Keats could arise and know the facts, could he not coin a newer and a truer phrase for us which would still sound as sweetly in our ears?

I must guard against a possible misconception. I do not mean to convey that astronomical discoveries are not occasionally made somewhat in the manner so beautifully pictured by Keats. Three years ago a persistent “watcher of the skies,” Dr. Anderson of Edinburgh, suddenly caught sight of a brilliant new star in Perseus; though here “flashed into his ken” would perhaps be a more suitable phrase than “swam.” And comets have been detected by a mere glance at the heavens without sensible effort or care on the part of the discoverer. But these may be fairly called exceptions; in the vast majority of cases hard work and a keen eye are necessary to make the discovery. The relative importance of these two factors of course varies in different cases; for the detection of Uranus perhaps the keen eye may be put in the first place, though we must not forget the diligent watching which gave it opportunity. Other cases of planetary discovery may be attributed more completely to diligence alone, as we shall presently see. But before leaving Uranus for them I should like to recall the circumstances attending the naming of the planet. Herschel proposed to call it *Georgium Sidus* in honour of his patron, King George III., and as the best way of making his wishes known, wrote the following letter to the President of the Royal Society, which is printed at the beginning of the Philosophical Transactions for 1783.

A Letter from William Herschel, Esq., F.R.S.,
to Sir Joseph Banks, Bart., P.R.S.

“Sir,—By the observations of the most eminent astronomers in Europe it appears that the new star, which I had the honour of pointing out to them in March 1781, is a Primary Planet of our Solar System. A body so nearly related to us by its similar condition and situation in the unbounded expanse of the starry heavens, must often be the subject of conversation, not only of astronomers, but of every lover of science in general. This consideration then makes it necessary to give it a name whereby it may be distinguished from the rest of the planets and fixed stars.

“In the fabulous ages of ancient times, the appellations of Mercury, Venus, Mars, Jupiter, and Saturn were given to the planets as being the names of their principal heroes and divinities. In the present more philosophical era, it would hardly be allowable to have recourse to the same method, and call on Juno, Pallas, Apollo, or Minerva for a name to our new heavenly body. The first consideration in any particular event, or remarkable incident, seems to be its chronology: if in any future age it should be asked, *when* this last found planet was discovered? It would be a very satisfactory answer to say, ‘In the reign of King George the Third.’ As a philosopher then, the name Georgium Sidus presents itself to me, as an appellation which will conveniently convey the information of the time and country where and when it was brought to view. But as a subject of the best of kings, who is the liberal protector of every art and science; as a native of the country from whence this illustrious family was called to the British throne; as a member of that Society which flourishes by the distinguished liberality of its royal patron; and, last of all, as a person now more immediately under the protection of this excellent monarch, and owing everything to his unlimited bounty;—I cannot but wish to take this opportunity of expressing my sense of gratitude by giving the name *Georgium Sidus*,

Georgium Sidus
——jam nunc assuesce vocari,
Virg. Georg.

to a star which (with respect to us) first began to shine under his auspicious reign.

“By addressing this letter to you, Sir, as President of the Royal Society, I take the most effectual method of communicating that name to the literati of Europe, which I hope they will receive with pleasure.—I have the honour to be, with the greatest respect, Sir, your most humble and most obedient servant,

W. Herschel.”

This letter reminds us how long it was since a new name had been required for a new planet,—to find a similar occasion Herschel had to go to the almost prehistoric past, when the names of heroes and divinities were given to the planets. It is, perhaps, not unnatural that he should have considered an entirely new departure appropriate for a discovery separated by so great a length of time from the others; but his views were not generally accepted, especially on the Continent. Lalande courteously proposed the name of Herschel for the new planet, in honour of the discoverer, and this name was used in France; but Bode, on the other hand, was in favour of retaining the old practice simply, and calling the new

planet Uranus. All three names seem to have been used for many years. Only the other day I was interested to see an old pack of cards, used for playing a parlour game of Astronomy, in which the name Herschel is used. The owner told me that they had belonged to his grandfather; and the date of publication was 1829, and the place London, so that this name was in common use in England nearly half a century after the actual discovery; though in the "English Nautical Almanac" the name "the Georgian" (apparently preferred to Herschel's *Georgium Sidus*) was being used officially after 1791, and did not disappear from that work until 1851 (published in 1847.)

Uranus finally adopted.

It would appear to have been the discovery of Neptune, with which we shall deal in the next chapter, which led to this official change; for in the volume for 1851 is included Adams' account of his discovery with the title—

<div align="center">"ON THE PERTURBATIONS OF URANUS,"</div>

and there was thus a definite reason for avoiding two names for the same planet in the same work. But Le Verrier's paper on the same topic at the same date still uses the name "Herschel" for the planet.

The discovery of Neptune, as we shall see, was totally different in character from that of Uranus. The latter may be described as the finding of something by an observer who was looking for anything; Neptune was the finding of something definitely sought for, and definitely pointed out by a most successful and brilliant piece of methodical work. But before that time several planets had been found, as the practical result of a definite search, although the guiding principle was such as cannot command our admiration to quite the same extent as in the case of Neptune. To explain it I must say something of the relative sizes of the orbits in which planets move round the sun. These orbits are, as we know, ellipses; but they are very nearly circles, and, excluding refinements, we may consider them as circles, with the sun at the centre of each, so that we may talk of the distance of any

Bode's law.

planet from the sun as a constant quantity without serious error. Now if we arrange the planetary distances in order, we shall notice a remarkable connection between the terms of the series. Here is a table showing this connection.

<div align="center">TABLE OF THE DISTANCES OF THE PLANETS FROM
THE SUN, SHOWING "BODE'S LAW."</div>

Name of Planet.	Distance from Sun, taking that of Earth as 10.	"Bode's Law" (originally formulated by Titius, but brought into notice by Bode).
Mercury	4	4 + 0= 4
Venus	7	4 + 3= 7
The Earth	10	4 + 6= 10
Mars	15	4 + 12= 16
( )	( )	4 + 24= 28
Jupiter	52	4 + 48= 52
Saturn	95	4 + 96= 100
Uranus	192	4 + 192= 196

If we write down a series of 4's, and then add the numbers 3, 6, 12, and so on, each formed by doubling the last, we get numbers representing very nearly the planetary distances, which are shown approximately in the second column. But three points call for notice. Firstly, the number before 3 should be 1½, and not zero, to agree with the rest. Secondly,

Gap in the series suggesting unknown planet.

there is a gap, or rather was a gap, after the discovery of Uranus, between Mars and Jupiter; and thirdly, we see that when Uranus was discovered, and its distance from the sun determined, this distance was found to fall in satisfactorily with this law, which was first stated by Titius of Wittenberg. This third fact naturally attracted attention. No explanation of the so-called "law" was known at the time; nor is any known even yet, though we may be

said to have some glimmerings of a possible cause; and in the absence of such explanation it must be regarded as merely a curious coincidence. But the chances that we are in the presence of a mere coincidence diminish very quickly with each new term added to the series, and when it was found that Herschel's new planet fitted in so well at the end of the arrangement, the question arose whether the gap above noticed was real, or whether there was perhaps another planet which had hitherto escaped notice, revolving in an orbit represented by this blank term. This question had indeed been asked even before the discovery of Uranus, by Bode, a young astronomer of Berlin; and for fifteen years he kept steadily in view this idea of finding a planet to fill the vacant interval. The search would be a very arduous one, involving a careful scrutiny, not perhaps of the whole heavens, but of a considerable portion of it along the Zodiac; too great for one would-be discoverer single-

Search for it.

handed; but in September 1800 Bode succeeded in organising a band of six German astronomers (including himself) for the purpose of conducting this search. They divided the Zodiac into twenty-four zones, and were assigning the zones to the different observers, when they were startled by the news that the missing planet had been accidentally found by Piazzi in the constellation Taurus. The discovery was made somewhat dramatically on the

first evening of the nineteenth century (January 1, 1801). Piazzi was not looking for a planet

Accidental discovery.

at all, but examining an error made by another astronomer; and in the course of this work

he recorded the position of a star of the eighth magnitude. Returning to it on the next night, it seemed to him that it had slightly moved westwards, and on the following night this suspicion was confirmed. Remark that in this case no peculiar appearance in the star suggested that it might be a comet or planet, as in the case of the discovery of Uranus. We are not unfair in ascribing the discovery to pure accident, although we must not forget that a careless observer might easily have missed it. Piazzi was anything but careless, and watched the new object assiduously till February 11th, when he became dangerously ill; but he had written, on January 23rd, to Oriani of Milan, and to Bode at Berlin on the following day. These letters, however, did not reach the recipients (in those days of leisurely postal service) until April 5th and March 20th respectively; and we can imagine the mixed feelings with which Bode heard that the discovery which he had contemplated for fifteen years, and for which he was just about to organise a diligent search, was thus curiously snatched from him.

More curious still must have seemed the intelligence to a young philosopher of Jena named Hegel, who has since become famous, but who had just imperilled his future reputation by publishing a dissertation proving conclusively that the number of the planets could not be greater than seven, and pouring scorn on the projected search of the half-dozen enthusiasts who were proposing to find a new planet merely to fill up a gap in a numerical series. [Pg 16]

The sensation caused by the news of the discovery was intensified by anxiety lest the new planet should already have been lost; for it had meanwhile travelled too close to the sun for further observation, and the only material available for calculating its orbit, and so predicting its place in the heavens at future dates, was afforded by the few observations made by Piazzi. Was it possible to calculate the orbit from such slender material? It would take too long to explain fully the enormous difficulty of this problem, but some notion of it may be obtained, by those unacquainted with mathematics, from a rough analogy. If we are given a portion of a circle, we can, with the help of a pair of compasses, complete the circle: we can find the centre from which the arc is struck, either by geometrical methods, or by a few experimental trials, and then fill in the rest of the circumference. If the arc given is large we can do this with certainty and accuracy; but if the arc is small it is difficult to make quite sure of the centre, and our drawing may not be quite accurate. Now the arc which had been described by the tiny planet during Piazzi's observations was only three degrees; and if any one will kindly take out his watch and look at the minute marks round the dial, three degrees is just *half* a single minute space. If the rest of the dial were obliterated, and only this small arc left, would he feel much confidence in restoring the obliterated portion? This problem gives some idea of the difficulties to be encountered, but only even then a very imperfect one. [Pg 17]

Briefly, the solution demanded a new mathematical method in astronomy. But difficulties are sometimes the opportunities of great men, and this particular difficulty attracted to astronomy the great mathematician Gauss, who set himself to make the best of the observation available, and produced his classical work, the *Theoria Motus*, which is the standard work for such calculations to the present day. May we look for a few moments at what he himself says in the preface to his great work? I venture to reproduce the following rough translation (the book being written in Latin, according to the scientific usage of the time):—

<div align="center">

Extract from the Preface to the
Theoria Motus.

</div>

"Some ideas had occurred to me on this subject in September 1801, at a time when I was occupied on something quite different; ideas which seemed to contribute to the solution of the great problem of which I have spoken. In such cases it often happens that, lest we be too much Distracted From the Attractive Investigation On Which We Are Engaged, We Allow Associations Of Ideas Which, If More Closely Examined, Might Prove Extraordinarily Fruitful, To Perish From Neglect. Perchance These Same Idea-lets of Mine Would Have Met With This Fate, If They Had Not Most Fortunately Lighted Upon a Time Than Which None Could Have Been Chosen More Favourable For Their Preservation and Development. For About The Same Time a Rumour Began To Be Spread Abroad Concerning a New Planet Which Had Been Detected On January 1st of That Year at the Observatory Of Palermo; and Shortly Afterwards the Actual Observations Which Had Been Made Between January 1st And February 11th by the Renowned Philosopher Piazzi Were Published. Nowhere in All The Annals of Astronomy Do We Find Such an Important Occasion; and Scarcely Is It Possible To Imagine a More Important Opportunity for Pointing Out, As Emphatically As Possible, the Importance Of That Problem, As at the Moment When Every Hope of Re-discovering, Among the Innumerable Little Stars of Heaven, That Mite of a Planet Which Had Been Lost To Sight for Nearly a Year, Depended Entirely on an Approximate Knowledge Of Its Orbit, Which Must Be Deduced From Those Scanty Observations. Could I Ever Have Had A Better Opportunity for Trying Whether Those Idea-lets Of Mine Were of Any Practical Value Than If I Then Were To Use Them for the Determination Of The Orbit of Ceres, a Planet Which, in the Course of those forty-one days, had described around the earth an arc of no [Pg 18]

[Pg 19]

more than three degrees? and, after a year had passed, required to be tracked out in a region of the sky far removed from its original position? The first application of this method was made in the month of October 1801, and the first clear night, when the planet was looked for by the help of the ephemeris I had made, revealed the truant to the observer. Three new planets found since then have supplied fresh opportunities for examining and proving the efficacy and universality of this method.

"Now a good many astronomers, immediately after the rediscovery of Ceres, desired me to publish the methods which had been used in my calculations. There were, however, not a few objections which prevented me from gratifying at that moment these friendly solicitations, viz. other business, the desire of treating the matter more fully, and more especially the expectation that, by continuing to devote myself to this research, I should bring the different portions of the solution of the problem to a more perfect pitch of universality, simplicity, and elegance. As my hopes have been justified, I do not think there is any reason for repenting of my delay. For the methods which I had repeatedly applied from the beginning admitted of so many and such important variations, that scarcely a vestige of resemblance remains between the method by which formerly I had arrived at the orbit of Ceres and the practice which I deal with in this work. Although indeed it would be alien to my intention to write a complete history about all these researches which I have gradually brought to even greater perfection, yet on many occasions, especially whenever I was confronted by some particularly serious problem, I thought that the first methods which I employed ought not to be entirely suppressed. Nay, rather, in addition to the solutions of the principal problems, I have in this work followed out many questions which presented themselves to me, in the course of a long study of the motions of the heavenly bodies in conic sections, as being particularly worthy of attention, whether on account of the neatness of the analysis, or more especially by reason of their practical utility. Yet I have always given the greater care to subjects which I have made my own, merely noticing by the way well-known facts where connection of thought seemed to demand it." [Pg 20]

These words do not explain in any way the methods introduced by Gauss, but they give us some notion of the flavour of the work. Aided by these brilliant researches, the little planet was found on the last day of the year by Von Zach at Gotha, and on the next night, independently, by Olbers at Bremen. But, before this success, there had been an arduous search, which led to a curious consequence. Olbers had made himself so familiar with all the small stars along the track which was being searched for the missing body, that he was at once struck by the appearance of a stranger near the spot where he had just identified Ceres. At first he thought this must be some star which had blazed up to brightness; but he soon found that it also was moving, and, to the great bewilderment of the astronomical world, it proved to be another planet revolving round the sun at a distance nearly the same as the former. This was an extraordinary and totally unforeseen occurrence. The world had been prepared for *one* planet; but here were *two*! [Pg 21]

Rediscovery of Ceres.

Another planet found.

Hypothesis of many fragments.

The thought occurred to Olbers that they were perhaps fragments of a single body which had been blown to pieces by some explosion, and that there might be more of the pieces; and he therefore suggested as a guide for finding others that, since by the known laws of gravitation, bodies which circle round the sun return periodically to their starting-point, therefore all these fragments would in due course return to the point in the heavens where the original planet had exploded. Hence the search might be most profitably conducted in the neighbourhood of the spot where the two first fragments (which had been named Ceres and Pallas) had already been found. We now have good reason to believe that this view is a mistaken one, but nevertheless it was apparently confirmed by the discovery of two more bodies of the same kind, which were called Juno and Vesta; the second of these being found by Olbers himself after three years' patient work in 1807. Hence, although the idea of searching for a more or less definitely imagined planet was not new, although Bode had conceived it as early as 1785, and organised a search on this plan, three planets were actually found before the first success attending a definite search. Ceres, as already remarked, was found by a pure accident; and the same may be said of Pallas and Juno, though it may fairly be added that Pallas was actually contrary to expectation. [Pg 22]

Minor Planets, 1801 to 1850.

Number	Name.	Discoverer.	Date.
1	Ceres	Piazzi	1801
2	Pallas	Olbers	1802
3	Juno	Harding	1804
4	Vesta	Olbers	1807
5	Astraea	Hencke	1845
6	Hebe	Hencke	1847
7	Iris	Hind	1847
8	Flora	Hind	1847

9	Metis	Graham	1848
10	Hygeia	De Gasparis	1849
11	Parthenope	De Gasparis	1850
12	Victoria	Hind	1850
13	Egeria	De Gasparis	1850

Here now is a table showing how other bodies were gradually added to this first list of four, but you will see that no addition was made for a long time. Not that the search was immediately abandoned; but being rewarded by no success for some years, it was gradually dropped, and the belief gained ground that the number of the planets was at last complete. The discoverers of Uranus and of these first four minor planets all died before any further addition was made; and it was not until the end of 1845 that Astraea was found by an ex-postmaster of the Prussian town of Driessen, by name Hencke, who, in spite of the general disbelief in the existence of any more planets, set himself diligently to search for them, and toiled for fifteen long years before at length reaping his reward. Others then resumed the search; Hind, the observer of an English amateur astronomer near London, found Iris a few weeks after Hencke had been rewarded by a second discovery in 1847, and in the following year Mr. Graham at Markree in Ireland (who is still living, and has only just retired from active work at the Cambridge Observatory) found Metis; and from that time new discoveries have been added year by year, until the number of planets now known exceeds 500, and is steadily increasing.

Hencke's long search.
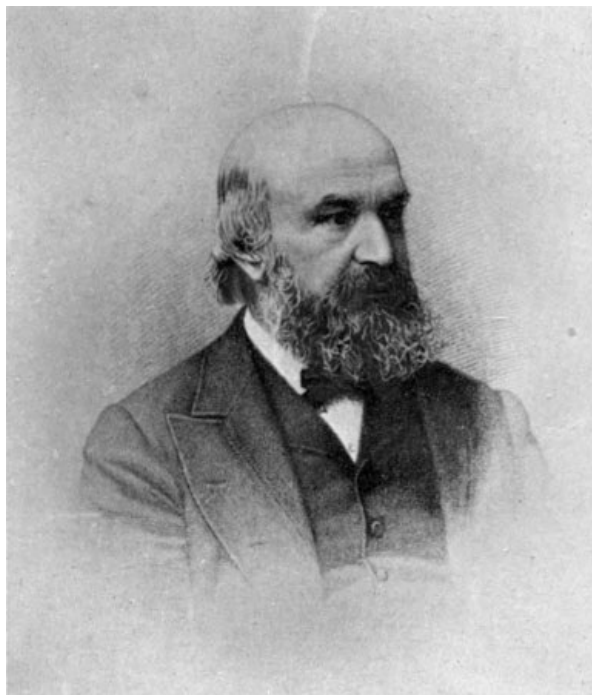
By permission of Messrs. Macmillan & Co.
I.—J. C. ADAMS.
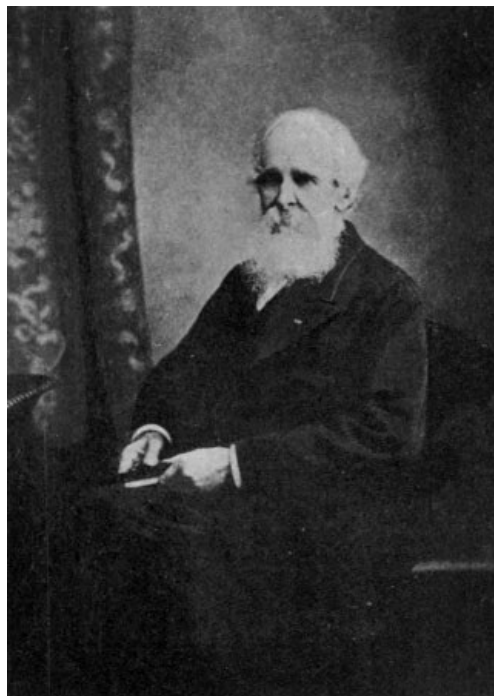
II.—A. Graham.
discoverer of the ninth minor planet (metis).

You will see the great variety characterising these discoveries; some of them are the result of deliberate search, others have come accidentally, and some even contrary to expectation. Of the great majority of the earlier ones it may be said that enormous diligence was required for each discovery; to identify a planet it is necessary to have either a good map of the stars or to know them thoroughly, so that the map practically exists in the brain. We need only remember Hencke's fifteen years of search before success to recognise what vast stores of patience and diligence were required in carrying out the search. But of late years photography has effected a great revolution in this respect. It is no longer necessary to do more than set what Sir Robert Ball has called a "star-trap," or rather planet-trap. If a photograph be taken of a region of the heavens, by the methods familiar to astronomers, so that each star makes a round dot on the photographic plate, any sufficiently bright object moving relatively to the stars will make a small line or trail, and thus betray its planetary character. In this way most of the recent discoveries have been made, and although diligence is still required in taking the photographs, and again in identifying the objects thus found (which are now very often the images of already known members of the system), the tedious scrutiny with the eye has become a thing of the past.

<div style="margin-left:3em">The photographic method.</div>

[Pg 24]

Table showing the Number of Minor Planets Discovered
in each Decade since 1850.

1801 to 1850—	altogether	13	discoveries.
1851 to 1860—	"	49	"
1861 to 1870—	"	49	"
1871 to 1880—	"	108	"
1881 to 1890—	"	83	"
1891 to 1900—	"	180	announcements
In 1901	"	36	"
" 1902	"	50	"
" 1903	"	41	"
		Total 609	

[*N.B.*—Many of the more recent announcements turned out to refer to old discoveries.]

The known number of these bodies has accordingly increased so rapidly as to become almost an embarrassment; and in one respect the embarrassment is definite, for it has become quite difficult to find *names* for the new discoveries. We remember with amusement at the present time that for the early discoveries there was sometimes a controversy (of the same kind as in the case of Uranus) about the exact name which a planet should have. Thus when it was proposed to call No. 12 (discovered in 1850, in London, by Mr. Hind) "Victoria," there was an outcry by foreign astronomers that by a subterfuge the name of a reigning monarch was again being proposed for a planet, and considerable opposition was manifested, especially in America. But it became clear, as other discoveries were added, that the list of goddesses, or even humbler mythological people, would not be large enough to go round if

Scarcity of names.

[Pg 25]

we were so severely critical, and must sooner or later be supplemented from sources hitherto considered unsuitable; so, ultimately, the opposition to the name Victoria was withdrawn. Later still the restriction to feminine names has been broken through; one planet has been named Endymion, and another, of which we shall presently speak more particularly, has been called Eros. But before passing to him you may care to look at some of the names selected for others:—

No.	Name.	No.	Name.
248	Lameia	389	Industria
250	Bettina	391	Ingeborg
261	Prymno	433	Eros
264	Libussa	443	Photographica
296	Phaëtusa	457	Alleghenia
340	Eduarda	462	Eriphyla
341	California	475	Ocllo
350	Ornamenta	484	Pittsburghia
357	Ninina	503	Evelyn
385	Ilmatar		

Bettina.

In connection with No. 250 there is an interesting little history. In the *Observatory* for 1885, page 63, appeared the following advertisement:—"Herr Palisa being desirous to raise funds for his intended expedition to observe the Total Solar Eclipse of August 1886, will sell the right of naming the minor planet No. 244 for £50." The bright idea seems to have struck Herr Palisa, who had already discovered many planets and begun to find difficulties in assigning suitable names, that he might turn his difficulty into a source of profit in a good cause. The offer was not responded to immediately, nor until Herr Palisa had discovered two more planets, Nos. 248 and 250. He found names for two, leaving, however, the last discovered always open for a patron, and on page 142 of the same magazine for 1886 the following note informs us how his patience was ultimately rewarded:—"Minor planet No. 250 has been named 'Bettina' by Baron Albert de Rothschild." I have not heard, however, that this precedent has been followed in other cases, and the ingenuity of discoverers was so much overtaxed towards the end of last century that the naming of their planets fell into arrears. Recently a Commission, which has been established to look after these small bodies generally, issued a notice that unless the naming was accomplished before a certain date it would be ruthlessly taken out of the hands of the negligent discoverers. Perhaps we may notice, before passing on, the provisional system which was adopted to fill up the interval required for finding a suitable name, and required also for making sure that the planet was in fact a new one, and not merely an old one rediscovered. There was a system of *numbering* in existence as well as of *naming*, but it was unadvisable to attach even a number to a planet until it was quite certain that the discovery was new, for otherwise there might be gaps created in what should be a continuous series by spurious discoveries being struck out. Accordingly it was decided to attach at first to the object merely a *letter of the alphabet*, with the year of discovery, as a provisional name. The alphabet was, however, run through so quickly, and confusion was so likely to ensue if it was merely repeated, that on recommencing it the letter A was prefixed, and the symbols adopted were therefore AA, AB, AC, &c.; after completing the alphabet again, the letter B was prefixed, and so on; and astronomers began to fear that they had before them a monotonous prospect of continually adding new planets, varied by no incident more exciting than starting the alphabet over again after every score.

The provisional letters.

Eros.

Fortunately, however, on running through it for the fifth time, an object of particular interest was discovered. Most of these bodies revolve at a distance from the sun intermediate between that of Mars and that of Jupiter, but the little planet which took the symbol DQ, and afterwards the name of Eros, was found to have a mean distance actually less than that of Mars, and this gave it an extraordinary importance with respect to the great problem of determining the sun's distance. To explain this importance we must make a small digression.

Transit of Venus.

About the middle of the last century our knowledge of the sun's distance was very rough, as may be seen from the table on p. 32; but there were in prospect two transits of Venus, in 1874 and 1882, and it was hoped that these would give opportunities of a special kind for the measurement of this important quantity, which lies at the root of all our knowledge of the exact masses and dimensions of not only the sun, but of the planets as well.
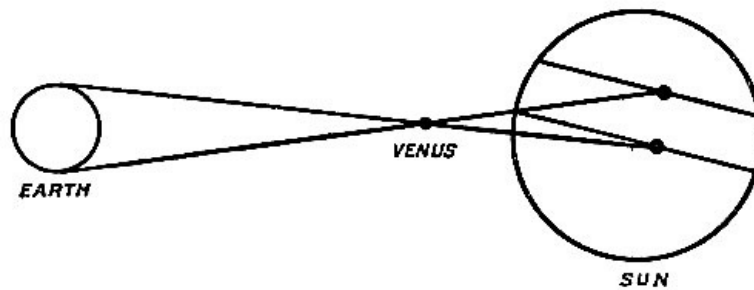
F<small>IG</small>. 1.

The method may be briefly summarised thus: An observer in one part of the earth would see Venus cross the disc of the sun along a different path from that seen by another observer, as will be clear from the diagram. If the size of the earth, the distance of the sun, and the *relative* distance of Venus be known, it can be calculated what this difference in path will be. Now the relative distance of Venus *is* known with great accuracy, from observing the time of her revolution round the sun; the size of the earth we can measure by a survey; there remains, therefore, only one unknown quantity, the sun's distance. And since from a knowledge of this we could calculate the difference in path, it is easy to invert the problem, and calculate the sun's distance from the knowledge of the observed difference in path. Accordingly, observers were to be scattered, not merely to two, but to many stations over the face of the earth, to observe the exact path taken by Venus in transit over the sun's disc as seen from their station; and especially to observe the exact times of beginning and ending of the transit; and, by comparison of their results, it was hoped to determine this very important quantity, the sun's distance. It was known from previous experience that there were certain difficulties in observing very exactly the beginning and end of the transit. There was an appearance called the "Black Drop," which had caused trouble on previous occasions; an appearance as though the round black spot which can be seen when Venus has advanced some distance over the sun's disc was reluctant to make the entry and clung to the edge or "limb" of the sun as it is called, somewhat as a drop of ink clings to a pen which is slowly withdrawn from an inkpot. Similarly, at the end of the transit or egress, instead of approaching the limb steadily the planet seems at the last moment to burst out towards it, rendering the estimation of the exact moment when the transit is over extremely doubtful.

The "Black Drop."

These difficulties, as already stated, were known to exist; but there is a long interval between transits of Venus, or rather between every pair of such transits. After those of 1874 and 1882 there will be no more until 2004 and 2012, so that we shall never see another; similarly, before that pair of the last century, there had not been any such occasion since 1761 and 1769, and no one was alive who remembered at first hand the trouble which was known to exist. It was proposed to obviate the anticipated difficulties by careful practice beforehand; models were prepared to resemble as nearly as possible the expected appearances, and the times recorded by different observers were compared with the true time, which could, in this case of a model, be determined. In this way it was hoped that the habit of each observer, his "personal equation" as it is called, could be determined beforehand, and allowed for as a correction when he came to observe the actual transit. The result, however, was a great disappointment. The actual appearances were found to be totally different in character from those shown by the model; chiefly, perhaps, because it had been impossible to imitate with a model the effect of the atmosphere which surrounds the planet Venus. Observers trained beforehand, using similar instruments, and standing within a few feet of each other, were expected, after making due allowance for personal equation, to give the same instant for contact; but their observations when made were found to differ by nearly a minute of time, and after an exhaustive review of the whole material it was felt that all hope of determining accurately the sun's distance by this method must be given up. The following table will show how much was learned from the transits of Venus, and how much remained to be settled. They left the result in doubt over a range of about two million miles.

Failure.

S<small>UN</small>'<small>S</small> D<small>ISTANCE</small>, <small>IN</small> M<small>ILLIONS OF</small> M<small>ILES</small>, <small>AS FOUND BY</small> D<small>IFFERENT</small> O<small>BSERVERS</small>

Before the Transits of Venus estimates varied between **96** million miles (Gilliss and Gould, 1856) and **91** million (Winneche, 1863), a range of 5 million miles.

The Transits of 1874 and 1882 gave results lying between **93¼** million (Airy, from British observations of 1874), **92½** million (Stone, from British observations of 1882), and **91½** million (Puiseux, from French observations), a range of 1¾ millions.

Gill's Heliometer results all lie very near **93** millions. The observations of Mars in 1877 give about 100,000 miles over this figure: but the observations

of Victoria, Iris, and Sappho, which are more trustworthy, all agree in giving about 100,000 miles *less* than the 93 millions.

It became necessary, therefore, to look to other methods; and before the second transit of 1882 was observed, an energetic astronomer, Dr. David Gill, had already put into operation the method which may be now regarded as the standard one.

We have said that the *relative* distance of Venus from the sun is accurately known from observations of the exact time of revolution. It is easy to see that these times of revolution can be measured accurately by mere accumulation. We may make an error of a few seconds in noting the time of return; but if the whole interval comprises 10 revolutions, this error is divided by 10, if 100 revolutions by 100, and so on; and by this time a great number of revolutions of all the planets (except those just discovered) have been recorded. Hence we know their relative distances with great precision; and if we can find the distance in miles of any one of them, we can find that of the sun itself, or of any other planet, by a simple rule-of-three sum. By making use of this principle many of the difficulties attending the direct determination of the sun's distance can be avoided; for instance, since the sun's light overpowers that of the stars, it is not easy to directly observe the place of the sun among the stars; but this is not so for the planets. We can photograph a planet and the stars surrounding it on the same plate, and then by careful measurement determine its exact position among the stars; and since this position differs slightly according to the situation of the observer on the earth's surface, by comparing two photographs taken at stations a known distance apart we can find the distance of the planet from the earth; and hence, as above remarked, the distance of the sun and all the other members of the solar system. Or, instead of taking photographs from two different stations, we can take from the same station two photographs at times separated by a known interval. For in that interval the station will have been carried by the earth's rotation some thousands of miles away from its former position, and becomes virtually a second station separated from the first by a distance which is known accurately when we know the elapsed time. Again, instead of taking photographs, and from them measuring the position of the planet among the stars, we may make the measurements on the planet and stars in the sky itself; and since in 1878, when Dr. Gill set out on his enterprise of determining the sun's distance, photography was in its infancy as applied to astronomy, he naturally made his observations on the sky with an instrument known as a heliometer. He made them in the little island of Ascension, which is suitably situated for the purpose; because, being near the earth's equator, it is carried by the earth's rotation a longer distance in a given time than places nearer the poles, and in these observations for "parallax," as they are called, it is important to have the displacement of the station as large as possible. For a similar reason the object selected among the planets must be as near the earth as possible; and hence the planet Mars, which at favourable times comes nearer to us than any other superior planet[1] then known, was selected for observation with the heliometer.

And now it will be seen why the discovery of the little planet Eros was important, for Mars was no longer the known planet capable of coming nearest to us; it had been replaced by this new arrival.

Further, a small planet which is in appearance just like an ordinary star has, irrespective of this great proximity, some distinct advantages over a planet like Mars, which appears as a round disc, and is, moreover, of a somewhat reddish colour. When the distance of an object of this kind from a point of line such as a star is measured with the heliometer it is found that a certain bias, somewhat difficult to allow for with certainty, is introduced into the measures; and our confidence in the final results suffers accordingly. After his observations of Mars in 1878, Dr. David Gill was sufficiently impressed with this source of error to make three new determinations of the sun's distance, using three of the minor planets instead of Mars, in spite of the fact that they were sensibly farther away; and his choice was justified by finding that the results from these three different sets of observations agreed well among themselves, and differed slightly from that given by the observations of Mars. Hence it seems conclusively proved that one of these bodies is a better selection than Mars in any case, and the discovery of Eros, which offered the advantage of greater proximity in addition, was hailed as a new opportunity of a most welcome kind. It was seen by a little calculation that in the winter of 1900-1901 the planet would come very near the earth; not the nearest possible (for it was also realised that a still better opportunity had occurred in 1894, though it was lost because the planet had not yet been discovered), but still the nearest approach which would occur for some thirty years; and extensive, though somewhat hasty, preparations were made to use it to the fullest advantage. Photography had now become established as an accurate method of making measurements of the kind required; and all the photographic telescopes which could be spared were pressed into the service, and diligently photographed the planet and surrounding stars every fine night during the favourable period. The work of measuring and reducing these photographs involves an enormous amount of labour, and is even yet far from completed, but we know enough to expect a result of the greatest value. More than this we have not time to say here about this great problem, but it will have been made clear that just when astronomers were beginning to wonder whether it was worth while continuing the monotonous discovery of new minor planets by the handful, the 433rd discovery also turned out to be one of the greatest importance.

To canons for the advantageous prosecution of research, if we care to make them, we may therefore add this—that there is no line of research, however apparently unimportant or monotonous, which we can afford to neglect. Just when we are on the point of relinquishing it under the impression that the mine is exhausted, we may be about to find a nugget worth all our previous and future labour. This rule will not, perhaps, help us very much in choosing what to work at; indeed, it is no rule at all, for it leaves us the whole field of choice unlimited. But this negative result will recur again and again as we examine the lessons taught by discoveries: there seem to be no rules at all. Whenever we seem to be able to deduce one from an experience, some other experience will flatly contradict it. Thus we might think that the discovery of Eros taught us to proceed patiently with a monotonous duty, and not turn aside to more novel and attractive work; yet it is often by leaving what is in hand and apparently has first claim on our attention that we shall do best, and we shall learn in the next chapter how a failure thus to turn flexibly aside was repented.

CHAPTER II

THE DISCOVERY OF NEPTUNE

Search for definite objects.

In the last chapter we saw that the circumstances under which planets were discovered varied considerably. Sometimes the discoveries were not previously expected, occurring during a general examination of the heavens, or a search for other objects; and, on one occasion at least, the discovery may be said to have been even contrary to expectation, though, as the existence of a number of minor planets began to be realised, there have also been many cases where the discovery has been made as the result of a definite and deliberate search. But the search cannot be said to have been inspired by any very clear or certain principle: for the law of Bode, successful though it has been in indicating the possible existence of new planets, cannot, as yet, be said to be founded upon a formulated law of nature. We now come, however, to a discovery made in direct interpretation of Newton's great law of gravitation—the discovery of Neptune from its observed disturbance of Uranus. I will first briefly recall the main facts relating to the actual discovery.

Disturbance of Uranus.

After Uranus had been discovered and observed sufficiently long for its orbit to be calculated, it was found that the subsequent position of the planet did not always agree with this orbit; and, more serious than this, some early observations were found which could not be reconciled with the later ones at all. It is a wonderful testimony to the care and sagacity of Sir William Herschel, as was remarked in the last chapter, that Uranus was found to have been observed, under the mistaken impression that it was an ordinary star, by Flamsteed, Lemonnier, Bradley, and Mayer, all observers of considerable ability. Flamsteed's five observations dated as far back as 1690, 1712, and 1715; observations by others were in 1748, 1750, 1753, 1756, and so on up to 1771, and the body of testimony was so considerable that there was no room for doubt as to the irreconcilability of the observations with the orbit, such as might have been the case had there been only one or two, possibly affected with some errors.

It is difficult to mention an exact date for the conversion into certainty of the suspicion that no single orbit could be found to satisfy all the observations; but we may certainly regard this fact as established in 1821, when Alexis Bouvard published some tables of the planet, and showed fully in the introduction that when every correction for the disturbing action of other planets had been applied, it was still impossible to reconcile the old observations with the orbit calculated from the new ones. The idea accordingly grew up that there might be some other body or bodies attracting the planet and causing these discrepancies. Here again it is not easy to say exactly when this notion arose, but it was certainly existent in 1834, as the following letter to the Astronomer Royal will show. I take it from his well-known "Account of some Circumstances historically connected with the Discovery of the Planet exterior to Uranus," which he gave to the Royal Astronomical Society at its first meeting after that famous discovery (Monthly Notices of the R.A.S., vol. iii., and Memoirs, vol. xvi.).

Suspicion of perturbing planet.

No. 1.—*The* Rev. T. J. Hussey *to* G. B. Airy.
[*Extract.*]

"'Hayes, Kent, 17*th November* 1834.

"'With M. Alexis Bouvard I had some conversation upon a subject I had often meditated, which will probably interest you, and your opinion may determine mine. Having taken great pains last year with some observations of *Uranus*, I was led to examine closely Bouvard's tables of that planet. The apparently inexplicable discrepancies between the ancient and modern observations suggested to me the possibility of some disturbing body beyond *Uranus*, not

taken into account because unknown. My first idea was to ascertain some approximate place of this supposed body empirically, and then with my large reflector set to work to examine all the minute stars thereabouts: but I found myself totally inadequate to the former part of the task. If I could have done it formerly, it was beyond me now, even supposing I had the time, which was not the case. I therefore relinquished the matter altogether; but subsequently, in conversation with Bouvard, I inquired if the above might not be the case: his answer was, that, as might have been expected, it had occurred to him, and some correspondence had taken place between Hansen and himself respecting it. Hansen's opinion was, that one disturbing body would not satisfy the phenomena; but that he conjectured there were two planets beyond *Uranus*. Upon my speaking of obtaining the places empirically, and then sweeping closely for the bodies, he fully acquiesced in the propriety of it, intimating that the previous calculations would be more laborious than difficult; that if he had leisure he would undertake them and transmit the results to me, as the basis of a very close and accurate sweep. I have not heard from him since on the subject, and have been too ill to write. What is your opinion on the subject? If you consider the idea as possible, can you give me the limits, roughly, between which this body or those bodies may probably be found during the ensuing winter? As we might expect an eccentricity [inclination?] approaching rather to that of the old planets than of the new, the breadth of the zone to be examined will be comparatively inconsiderable. I may be wrong, but I am disposed to think that, such is the perfection of my equatoreal's object-glass, I could distinguish, almost at once, the difference of light of a small planet and a star. My plan of proceeding, however, would be very different: I should accurately map the whole space within the required limits, down to the minutest star I could discern; the interval of a single week would then enable me to ascertain any change. If the whole of this matter do not appear to you a chimæra, which, until my conversation with Bouvard, I was afraid it might, I shall be very glad of any sort of hint respecting it.'

[Pg 41]

[Pg 42]

"My answer was in the following terms:—

<div align="center">

No. 2.—G. B. Airy *to the* Rev. T. J. Hussey.
[*Extract.*]

"'Observatory, Cambridge, 1834, *Nov.* 23.

</div>

<div style="margin-left:2em">Airy's scepticism.</div>

"'I have often thought of the irregularity of *Uranus*, and since the receipt of your letter have looked more carefully to it. It is a puzzling subject, but I give it as my opinion, without hesitation, that it is not yet in such a state as to give the smallest hope of making out the nature of any external action on the planet … if it were certain that there were any extraneous action, I doubt much the possibility of determining the place of a planet which produced it. I am sure it could not be done till the nature of the irregularity was well determined from several successive revolutions.'"

[Pg 43]

Although only a sentence or two have been selected from Airy's reply (he was not yet Astronomer Royal), they are sufficient to show that the problem of finding the place of such a possible disturbing body was regarded at that time as one of extreme difficulty; and no one appears seriously to have contemplated embarking upon its solution. It was not until many years later that the solution was attempted. Of the first attempt we shall speak presently, putting it aside for the moment because it had no actual bearing on the discovery of the planet, for reasons which form an extraordinary episode of this history. The attempt which led to success dates from November 1845. The great French astronomer Le Verrier, on November 10, 1845, read to the French Academy a paper on the Orbit of Uranus, considering specially the disturbances produced by Jupiter and Saturn, and showing clearly that with no possible orbit could the observations be satisfied. On June 1, 1846, followed a second paper by the same author, in which he considers all the possible explanations of the discordance, and concludes that none is admissible except that of a disturbing planet exterior to Uranus. And assuming, in accordance with Bode's Law, that the distance of this new planet from the sun would be about double that of Uranus (and it is important to note this assumption), he proceeds to investigate the orbit of such a planet, and to calculate the place where it must be looked for in the heavens. This was followed by a third paper on August 31st, giving a rather completer discussion, and arriving at the conclusion that the planet should be recognisable from its disc. This again is an important point. We remember that in the discovery of Uranus it needed considerable skill on the part of Sir William Herschel to detect the disc, to see in fact any difference between it and surrounding stars; and that other observers, even when their attention had been called to the planet, found it difficult to see this difference. It might be expected, therefore, that with a planet twice as far away (as had been assumed for the new planet) the disc would be practically unrecognisable, and as we shall presently see, this assumption was made in some searches for the planet which had been commenced even before the publication of this third paper. Le Verrier's courageous announcement, which he deduced from a consideration of the mass of the planet, that the disc should be recognisable, led immediately to the discovery of the suspected body. He wrote to a German astronomer, Dr. Galle (still, I am glad to say, alive

<div style="margin-left:2em">Le Verrier's papers.</div>

<div style="margin-left:2em">Planet to be detected by disc.</div>

<div style="margin-left:2em">Galle's discovery of</div>

[Pg 44]

and well, though now a very old man), telling him the spot in the heavens to search, and stating that he might expect to detect the planet by its appearance in this way; and the same night Dr. Galle, by comparing a star map with the heavens, found the planet. [Pg 45]

To two points to which I have specially called attention in this brief summary—namely, the preliminary assumption that the planet would be, according to Bode's Law, twice as far away as Uranus; secondly, the confident assertion that it would have a visible disc—I will ask you to add, thirdly, that it was found by the aid of a star map, for this map played an important part in the further history to which we shall now proceed. It may naturally be supposed that the announcement of the finding of a planet in this way, the calculation of its place from a belief in the universal action of the great Law of Gravitation, the direction to an eminent observer to look in that place for a particular thing, and his immediate success,— this extraordinary combination of circumstances caused a profound sensation throughout not only the astronomical, but the whole world; and this sensation was greatly enhanced by the rumour which had begun to gather strength that, but for some unfortunate

circumstances, the discovery might have been made even earlier and as a consequence of totally independent calculations made by a young Cambridge mathematician, J. C. Adams. Some of you are doubtless already familiar with the story in its abridged form, for it has been scattered broadcast through literature. In England it generally takes the form of emphasising the wickedness or laziness of the Astronomer Royal who, when told where to [Pg 46] look for a planet, neglected his obvious duty, so that in consequence another astronomer who made the calculation much later and gave a more virtuous observer the same directions where to look, obtained for France the glory of a discovery which ought to have been retained in England. There is no doubt that Airy's conduct received a large amount of what he called "savage abuse." When the facts are clearly stated I think it will be evident that many of the harsh things said of him were scarcely just, though at the same time it is also difficult to understand his conduct at two or three points of the history, even as explained by himself.

There is fortunately no doubt whatever about any of the *facts*. Airy himself gave a very clear and straightforward account of them at the time, for which more credit is due to him than he commonly receives; and since the death of the chief actors in this sensational drama they have been naturally again ransacked, with the satisfactory result that there is practically no doubt about any of the facts. As to the proper interpretations of them there certainly may be wide differences of opinion, nor does this circumstance detract from their interest. It is almost impossible to make a perfectly colourless recital of them, nor is it perhaps necessary to do so. I will therefore ask you to remember in what I now say that there is almost necessarily an element of personal bias, and that another writer would probably give a [Pg 47] different colouring. Having said this, I hope I may speak quite freely as the matter appears in my personal estimation.

Airy's account was, as above stated, given to the Royal Astronomical Society at their first meeting (after the startling announcement of the discovery of the new planet), on November 13, 1846, and I have already quoted an extract from it. He opens with a tribute to the sensational character of the discovery, and then states that although clearly due to two individuals (namely, Le Verrier and Galle), it might also be regarded as to some extent the consequence of a movement of the age. His actual words are these: "The principal steps in the theoretical investigations have been made by one individual, and the published discovery of the planet was necessarily made by one individual. To these persons the public attention has been principally directed; and well do they deserve the honours which they have received, and which they will continue to receive. Yet we should do wrong if we considered that these two persons alone are to be regarded as the authors of the discovery of this planet. I am confident that it will be found that the discovery is a consequence of what may properly be called a movement of the age; that it has been urged by the feeling of the scientific world in general, and has been nearly perfected by the collateral, but independent labours, of various persons possessing the talents or powers best suited to the different [Pg 48] parts of the researches."

I have quoted these words as the first point at which it is difficult to understand Airy's conduct in excluding from them all specific mention of Adams, knowing as he did the special claims which entitled him to such mention; claims indeed which he proceeded immediately to make clear. It seems almost certain that Airy entirely under-estimated the value of Adams' work throughout. But this will become clearer as we proceed. The "account" takes the form of the publication of a series of letters with occasional comments. Airy was a most methodical person, and filed all his correspondence with great regularity. It was jestingly said of him once that if he wiped his pen on a piece of blotting-paper, he would date the blotting-paper and file it for reference. The letters reproduced in this "account" are still in the Observatory at Greenwich, pinned together just as Airy left them; and in preparing his "account" it was necessary to do little else than to have them copied out and interpolate comments. From two of them I have already quoted to show how difficult the enterprise of finding an exterior planet from its action on Uranus was considered in 1834. To these may be added the following sentence from No. 4, dated 1837. "If it be the effect of any unseen body," writes Airy to Bouvard, "it will be nearly impossible ever to find out its place." But the first letter which need concern us is No. 6, and it is only necessary to explain that [Pg 49] Professor Challis was the Professor of Astronomy at Cambridge, and in charge of the Cambridge Observatory, in which offices he had succeeded Airy himself on his leaving

Cambridge for Greenwich some eight years earlier.

<div align="center">No. 6.—Professor Challis <i>to</i> G. B. Airy.
[<i>Extract.</i>]</div>

<div align="right">"'Cambridge Observatory, <i>Feb.</i> 13, 1844.</div>

"'A young friend of mine, Mr. Adams of St. John's College, is working at the theory of *Uranus*, and is desirous of obtaining errors of the tabular geocentric longitudes of this planet, when near opposition, in the years 1818-1826, with the factors for reducing them to errors of heliocentric longitude. Are your reductions of the planetary observations so far advanced that you could furnish these data? and is the request one which you have any objection to comply with? If Mr. Adams may be favoured in this respect, he is further desirous of knowing, whether in the calculation of the tabular errors any alterations have been made in Bouvard's *Tables of Uranus* besides that of *Jupiter's* mass.'

"My answer to him was as follows:—

<div align="center">No. 7.—G. B. Airy <i>to</i> Professor Challis.
[<i>Extract.</i>]</div>

<div align="right">"'Royal Observatory, Greenwich, 1844, <i>Feb.</i> 15.</div>

"'I send all the results of the observations of *Uranus* made with both instruments (that is, the heliocentric errors of *Uranus* in longitude and latitude from 1754 to 1830, for all those days on which there were observations, both of right ascension and of polar distance). No alteration is made in Bouvard's *Tables of Uranus* except in increasing the two equations which depend on *Jupiter* by $\frac{1}{50}$ part. As constants have been added (in the printed tables) to make the equations positive, and as $\frac{1}{50}$ part of the numbers in the tables has been added, $\frac{1}{50}$ part of the constants has been subtracted from the final results.'

<div align="right">[Pg 50]</div>

"Professor Challis in acknowledging the receipt of these, used the following expressions:—

<div align="center">No. 8.—Professor Challis <i>to</i> G. B. Airy.
[<i>Extract.</i>]</div>

<div align="right">"'Cambridge Observatory, <i>Feb.</i> 16, 1844.</div>

"'I am exceedingly obliged by your sending so complete a series of tabular errors of *Uranus*.... The list you have sent will give Mr. Adams the means of carrying on in the most effective manner the inquiry in which he is engaged.'

"The next letter shows that Mr. Adams has derived results from these errors.

<div align="center">No. 9.—Professor Challis <i>to</i> G. B. Airy.</div>

<div align="right">"'Cambridge Observatory, <i>Sept.</i> 22, 1845.</div>

"'My friend Mr. Adams (who will probably deliver this note to you) has completed his calculations respecting the perturbation of the orbit of *Uranus* by a supposed ulterior planet, and has arrived at results which he would be glad to communicate to you personally, if you could spare him a few moments of your valuable time. His calculations are founded on the observations you were so good as to furnish him with some time ago; and from his character as a mathematician, and his practice in calculation, I should consider the deductions from his premises to be made in a trustworthy manner. If he should not have the good fortune to see you at Greenwich, he hopes to be allowed to write to you on this subject.'

<div align="right">[Pg 51]</div>

"On the day on which this letter was dated, I was present at a meeting of the French Institute. I acknowledged it by the following letter:—

<div align="center">No. 10.—G. B. Airy <i>to</i> Professor Challis.</div>

<div align="right">"'Royal Observatory, Greenwich, 1845, <i>Sept.</i> 29.</div>

"'I was, I suppose, on my way from France, when Mr. Adams called here; at all events, I had not reached home, and therefore, to my regret, I have not seen him. Would you mention to Mr. Adams that I am very much interested with the subject of his investigations, and that I should be delighted to hear of them by letter from him?'

"On one of the last days of October 1845, Mr. Adams called at the Royal Observatory, Greenwich, in my absence and left the following important paper:—

<div align="right">[Pg 52]</div>

"'According to my calculations, the observed irregularities in the motion of *Uranus* may be accounted for by supposing the existence of an exterior planet, the mass and orbit of which are as follows:—

Mean distance (assumed nearly in accordance with Bode's Law)	38.4
Mean sidereal motion in 365.25 days	1° 30′.9
Mean longitude, 1st October 1845	323 34
Longitude of perihelion	315 55
Eccentricity	0.1610.
Mass (that of the sun being unity)	0.0001656.

For the modern observations I have used the method of normal places, taking the mean of the tabular errors, as given by observations near three consecutive oppositions, to correspond with the mean of the times; and the Greenwich observations have been used down to 1830: since which, the Cambridge and Greenwich observations, and those given in the *Astronomische Nachrichten*, have been made use of. The following are the remaining errors of mean longitude:—

Observation—Theory.

	"		"
1780	+0.27	1813	-0.94
1783	-0.23	1816	-0.31
1786	-0.96	1819	-2.00
1789	+1.82	1822	+0.30
1792	-0.91	1825	+1.92
1795	+0.09	1828	+2.25
1798	-0.99	1831	-1.06
1801	-0.04	1834	-1.44
1804	+1.76	1837	-1.62
1807	-0.21	1840	+1.73
1810	+0.56		

The error for 1780 is concluded from that for 1781 given by observation, compared with those of four or five following years, and also with Lemonnier's observations in 1769 and 1771.

"'For the ancient observations, the following are the remaining errors:—

Observation—Theory.

	"		"		"
1690	+44.4	1750	- 1.6	1763	- 5.1
1712	+ 6.7	1753	+ 5.7	1769	+ 0.6
1715	- 6.8	1756	- 4.0	1771	+11.8

The errors are small, except for Flamsteed's observation of 1690. This being an isolated observation, very distant from the rest, I thought it best not to use it in forming the equations of condition. It is not improbable, however, that this error might be destroyed by a small change in the assumed mean motion of the planet.'

"I acknowledged the receipt of this paper in the following terms:—

No. 12.—G. B. Airy *to* J. C. Adams, Esq.

"'Royal Observatory, Greenwich, 1845, *Nov.* 5.

"'I am very much obliged by the paper of results which you left here a few days since, showing the perturbations on the place of *Uranus* produced by a planet with certain assumed elements. The latter numbers are all extremely satisfactory: I am not enough acquainted with Flamsteed's observations about 1690 to say whether they bear such an error, but I think it extremely probable.

"'But I should be very glad to know whether this assumed perturbation will explain the error of the radius vector of *Uranus*. This error is now very considerable, as you will be able to ascertain by comparing the normal

equations, given in the Greenwich observations for each year, for the times *before* opposition with the times *after* opposition.'

"I have before stated that I considered the establishment of this error of the radius vector of *Uranus* to be a very important determination. I therefore considered that the trial, whether the error of radius vector would be explained by the same theory which explained the error of longitude, would be truly an *experimentum crucis*. And I waited with much anxiety for Mr. Adams' answer to my query. Had it been in the affirmative, I should at once have exerted all the influence which I might possess, either directly, or indirectly through my friend Professor Challis, to procure the publication of Mr. Adams' theory.

"From some cause with which I am unacquainted, probably an accidental one, I received no immediate answer to this inquiry. I regret this deeply, for many reasons."

Adams'
silence.

Here we may leave Airy's "account" for a few moments to consider the reason why he received no answer. Adams was a very shy and retiring young man, and very sensitive; though capable of a great resolution, and of enormous perseverance in carrying it out. We know (what is not indicated in the above account), how steadily he had kept in view the idea of solving this great problem. It was characteristic of him that as early as 1841 he had formed a resolution to undertake it, although at the time he was not able to enter upon its accomplishment. The following memorandum, which is still in existence, having been found among his papers after his death, records these facts:

[Pg 55]

"1841, July 3. Formed a design, in the beginning of this week, of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus, which were as yet unaccounted for: in order to find whether they may be attributed to the action of an undiscovered planet beyond it, and if possible thence to determine the elements of its orbit, &c., approximately, which would probably lead to its discovery."

Accordingly, "as soon as possible after taking his degree" he embarked upon the enterprise, and the first solution was made in the long vacation of 1843, assuming the orbit of the unknown planet to be a circle with a radius equal to twice the mean distance of Uranus from the sun (an assumption which, as we have seen, was also made by Le Verrier). Having satisfied himself that there was a good general agreement between his results and the observations, Adams began a more complete solution; indeed from first to last he made no less than six separate solutions, the one which he announced to Airy in the above letter being the fourth. Hence he had already done an enormous amount of work on the problem, and was in his own mind so justly convinced of the correctness and value of his results that he was liable to forget that others had not had the same opportunity of judging of their completeness; and he was grievously disappointed when his announcement was not received with full confidence.

[Pg 56]

His
disappointment
at
Greenwich,

But perhaps it should first be stated that by a series of mischances Adams had been already much disappointed at the failure of his attempts to see the Astronomer Royal on his visits to Greenwich. This does not seem to have been exactly Airy's fault; he was, as may well be supposed, an extremely busy man, and was much occupied at the time on a question of great practical importance, at the direct request of the Government, namely, the settling of the proper gauge for railways throughout the country. The first time Adams called to see him, he was actually in London sitting on the Committee which dealt with this question, and Adams was asked to call later; when the visit was repeated, Airy was unfortunately at dinner (and it may be added that his hours for dinner were somewhat peculiar), and the butler, acting somewhat in the manner of his kind, protected his master's dinner by sending away one whom he doubtless regarded as a troublesome visitor. There is, as I have said, little doubt about any of the facts, and it seems well established that Airy himself did not learn of Adams' visits until afterwards, and it would scarcely be just to blame him for a servant's oversight. But Adams had left the paper above reproduced, and Airy with his business-like habits ultimately proceeded to deal with it; he wrote the answer given above asking Adams a definite question, filed a copy of it with the original letter, and then dismissed the matter from his thoughts until the reply from Adams, which he confidently expected should again bring it under notice.

[Pg 57]

and at
Airy's
question.

This further disappointment was, however, too much for Adams; he regarded the question put by Airy as having so obvious an answer that it was intended as an evasion, though this was far from being the case. Airy was thoroughly in earnest about his question, though it must be admitted that a more careful study of the problem would have shown him that it was unnecessary. Later, when he learnt of Le Verrier's researches, he put the same question to him, and received a polite but very clear answer, showing that the suggested test was not an *experimentum crucis* as he supposed. But Adams did not feel equal to making this reply; he shrank into his shell and solaced himself only by commencing afresh another solution of the problem which had so engrossed his life at that time.

[Pg 58]

The merits
of Airy's
question.

I have heard severe or contemptuous things said about this question by those who most blame Airy. Some of them have no hesitation in accusing him of intellectual incompetence: they say that it was the question of a stupid man. I think that in the first place they forget

the difference between a deliberate error of judgement and a mere consequence of insufficient attention. But there is even more than this to be said in defence of the question. The "error of radius vector" came before Airy in an entirely independent way, and as an entirely independent phenomenon, from the "error of longitude," and there was nothing unnatural in regarding it as requiring independent explanation. It is true that, *as the event proved*, a mere readjustment of the orbit of Uranus got rid of this error of radius vector (this was substantially Le Verrier's answer to Airy's question); but we must not judge of what was possible before the event in the light of what we now know. The original possibilities were far wider, though we have forgotten their former extent now that they have been narrowed down by the discovery. If a sentry during war time hears a noise in a certain direction, he may be compelled to make the assumption that it is the movement of an enemy; and if he fires in that direction and kills him, and thus saves his own army from destruction, he is deservedly applauded for the success which attends his action. But it does not follow that the assumption on which he acted was the only possible one. Or, to take a more peaceful illustration, in playing whist it sometimes becomes apparent that the game can only be won if the cards lie in a certain way; and a good player will thereupon assume that this is the fact, and play accordingly. Adams and Le Verrier played to win the game on the particular assumption that the disturbance of Uranus was due to an external planet revolving at a distance from the sun about twice that of Uranus; *and won it*; and we applaud them for doing so. But it is easy to imagine a rearrangement of the cards with which they would have lost it; and Airy's question simply meant that he was alive to these wider possibilities, and did not see the need for attempting to win the game in that particular way.

One such alternative possibility has already been mentioned. "Hansen's opinion was, that one disturbing body would not satisfy the phenomena; but he conjectured that there were two planets beyond *Uranus*." Another conceivable alternative is that there was some change in the law of gravitation at the distance of Uranus, which, it must be remembered, is twice as great as that of any planet previously known. Or some wandering body might have passed close enough to Uranus to change its orbit somewhat suddenly. We now know, for instance, that the swarm of meteorites which gives rise to the well-known "November meteors" must have passed very close to Uranus in A.D. 126, assuming that neither the planet nor the swarm have been disturbed in any unknown manner in the meantime. It is to this encounter that we owe the introduction of this swarm to our solar system: wandering through space, they met Uranus, and were swept by his attraction into an orbit round the sun. Was there no reaction upon Uranus himself? The probabilities are that the total mass of the swarm was so small as to affect the huge planet inappreciably; but who was to say that some other swarm of larger mass, or other body, might not have approached near Uranus at some date between 1690 and 1845, and been responsible at any rate in part for the observed errors? These are two or three suppositions from our familiar experience; and there are, of course, limitless possibilities beyond. Which is the true scientific attitude, to be alive to them all, or to concentrate attention upon one?

But we are perhaps wandering too far from the main theme. It is easy to do so in reviewing this extraordinary piece of history, for at almost every point new possibilities are suggested.
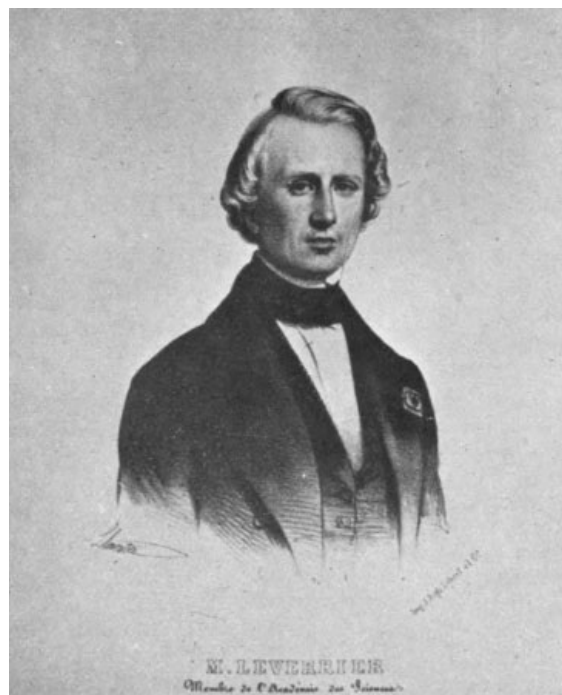
III—U. J. Le Verrier.
(*From a print in the possession of the Royal Astronomical Society.*)
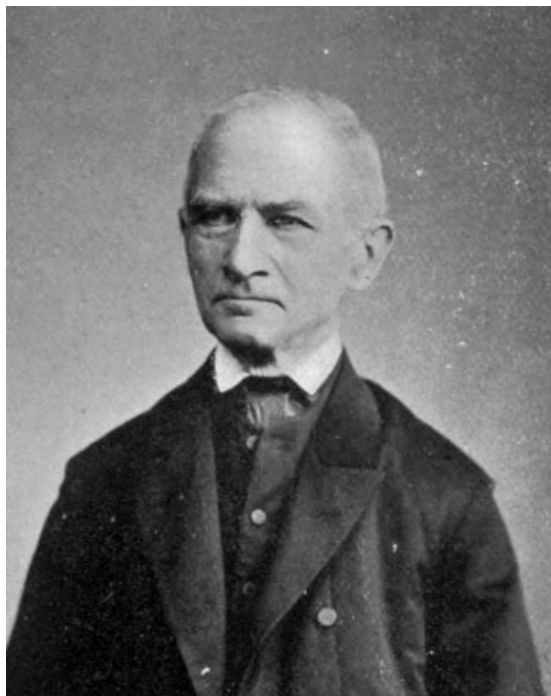
IV—J. G. GALLE.
WHO FIRST SAW THE PLANET NEPTUNE

We must return, however, to Airy's "account." We reached the point where he had written to Adams (on November 5, 1845), asking his question about the radius vector, and received no reply; and there the matter remained, so far as he was concerned, until the following June, when Le Verrier's memoir reached him; and we will let him give his own version of the result.

Airy receives Le Verrier's memoir.

> "This memoir reached me about the 23rd or 24th of June. I cannot sufficiently express the feeling of delight and satisfaction which I received from it. The place which it assigned to the disturbing planet was the same, to one degree, as that given by Mr. Adams' calculations, which I had perused seven months earlier. To this time I had considered that there was still room for doubt of the accuracy of Mr. Adams' investigations; for I think that the results of algebraic and numerical computations, so long and so complicated as those of an inverse problem of perturbations, are liable to many risks of error in the details of the process: I know that there are important numerical errors in the *Mécanique Céleste* of Laplace; in the *Théorie de la Lune* of Plana; above all, in Bouvard's first tables of *Jupiter* and *Saturn*; and to express it in a word, I have always considered the correctness of a distant mathematical result to be a subject rather of moral than of mathematical evidence. But now I felt no doubt of the accuracy of both calculations, as applied to the perturbation in longitude. I was, however, still desirous, as before, of learning whether the perturbation in radius vector was fully explained. I therefore addressed to M. Le Verrier the following letter:—

No. 13.—G. B. Airy *to* M. Le Verrier.

"'Royal Observatory, Greenwich, 1846, *June* 26.

He puts the "radius-vector" question to Le Verrier,

"'I have read, with very great interest, the account of your investigations on the probable place of a planet disturbing the motions of *Uranus*, which is contained in the *Compte Rendu de l'Académie* of June 1; and I now beg leave to trouble you with the following question. It appears, from all the later observations of *Uranus* made at Greenwich (which are most completely reduced in the *Greenwich Observations* of each year, so as to exhibit the effect of an error either in the tabular heliocentric longitude, or the tabular radius vector), that the tabular radius vector is considerably too small. And I wish to inquire of you whether this would be a consequence of the disturbance produced by an exterior planet, now in the position which you have indicated?'"

There is more of the letter, but this will suffice to show that he wrote to Le Verrier in the same way as to Adams, and, as already stated, received a reply dated three or four days later. But the rest of the letter contains no mention of Adams, and thus arises a second difficulty in understanding Airy's conduct. It seems extraordinary that when he wrote to Le Verrier he made no mention of the computations which he had previously received from Adams; or that he should not have written to Adams, and made some attempt to understand his long silence, now that, as he himself states, he "felt no doubt of the accuracy of both calculations." The omission may have been, and probably was, mere carelessness or

but makes no mention of Adams.

forgetfulness; but he could hardly be surprised if others mistook it for deliberate action.

However, attention had now been thoroughly attracted to the near possibility of finding the planet. On June 29, 1846, there was a special meeting of the Board of Visitors of Greenwich Observatory, and Airy incidentally mentioned to them this possibility. The impression produced must have been definite and deep; for Sir John Herschel, who was present, was bold enough to say on September 10th following to the British Association assembled at Southampton: "We see it (the probable new planet) as Columbus saw America from the shores of Spain. Its movements have been felt trembling along the far-reaching line of our analysis with a certainty hardly inferior to that of ocular demonstration." Airy discussed the matter with Professor Challis (who, it will be remembered, had originally written to him on behalf of Adams), suggesting that he should immediately commence a search for the supposed planet at Cambridge. It may be asked why Airy did not commence this search [Pg 64] himself at Greenwich, and the answer is that he had no telescope which he regarded as large enough for the purpose. The Royal Observatory at Greenwich has always been, and is now, better equipped in some respects than any other observatory, as might be expected from its deservedly great reputation; but to possess the largest existing telescope has never been one of its ambitions. The instruments in which it takes most pride are remarkable for their steadiness and accuracy rather than for their size; and at that time the best telescope possessed by the observatory was not, in Airy's opinion, large enough to detect the planet with certainty. In this opinion we now know that he was mistaken; but, again, we must not judge his conduct before the event in the light of what we have since discovered. It may be recalled here that it was not until Le Verrier's third paper, published on August 31, that he (Le Verrier) emphatically pointed out that the new planet might be of such a size as to have a sensible disc; and it was this remark which led immediately to its discovery. Until this was so decisively stated, it must have seemed exceptionally improbable; for we saw in the last chapter how diligently the Zodiac had been swept in the search for minor planets,—how, for instance, Hencke had searched for fifteen years without success; and it might fairly be considered that if there were a fairly bright object (such as Neptune has since been found to be) it would have been discovered earlier. Hence Airy not unreasonably considered it [Pg 65] necessary to spread his net for very small objects. On July 9 he wrote to Professor Challis as follows:—

No. 15.—G. B. Airy *to* Professor Challis.

"The Deanery, Ely, 1846, *July* 9.

"You know that I attach importance to the examination of that part of the heavens in which there is ... reason for suspecting the existence of a planet exterior to *Uranus*. I have thought about the way of making such examination, but I am convinced that (for various reasons, of declination, latitude of place, feebleness of light, and regularity of superintendence) there is no prospect whatever of its being made with any chance of success, except with the Northumberland telescope.

"Now, I should be glad to ask you, in the first place, whether you could make such an examination?

"Presuming that your answer would be in the negative, I would ask, secondly, whether, supposing that an assistant were supplied to you for this purpose, you would superintend the examination?

"You will readily perceive that all this is in a most unformed state at present, and that I am asking these questions almost at a venture, in the hope of rescuing the matter from a state which is, without the assistance that you and your instruments can give, almost desperate. Therefore I should be glad to [Pg 66] have your answer, not only responding simply to my questions, but also entering into any other considerations which you think likely to bear on the matter.

"The time for the said examination is approaching near."

Professor Challis did not require an assistant, but determined to undertake the work himself, and devised his own plan of procedure; but he also set out on the undertaking with the expectation of a long and arduous search. No such idea as that of finding the planet on the first night ever entered his head. For one thing, he had no map of the region to be examined, for although the map used by Galle had been published, no copy of it had as yet reached Cambridge, and Professor Challis had practically to construct a map for himself. In these days of photography to make such a map is a simple matter, but at that time the process was terribly laborious. "I get over the ground very slowly," he wrote on September 2nd to Airy, "thinking it right to include all stars to 10-11 magnitude; and I find that to scrutinise thoroughly in this way the proposed portion of the heavens will require many more observations than I can take this year." With such a prospect, it is not surprising that one night's observations were not even compared with the next; there would be a certain economy in waiting until a large amount of material had been accumulated, and then [Pg 67] making the comparisons all together, and this was the course adopted. But when Le

Verrier's third paper, with the decided opinion that the planet would be bright enough to be seen by its disc, ultimately reached Professor Challis, it naturally gave him an entirely different view of the possibilities; he immediately began to compare the observations already made, and found that he had observed the planet early in August. But it was now too late to be first in the field, for Galle had already made his announcement of discovery. Writing to Airy on October 12, Challis could only lament that after four days' observing the planet was in his grasp, *if* only he had examined or mapped the observations, and *if* he had not delayed doing so until he had more observations to reduce, and *if* he had not been very busy with some comet observations. Oh! these terrible *ifs* which come so often between a man and success! The third of them is a peculiarly distressing one, for it represents that eternal conflict between one duty and another, which is so constantly recurring in scientific work. Shall we finish one piece of work now well under way, or shall we attend to something more novel and more attractive? Challis thought his duty lay in steadily completing the comet observations already begun. We saw in the last lecture how the steady pursuit of the discovery of minor planets, a duty which had become tedious and apparently led nowhere, suddenly resulted in the important discovery of Eros. But Challis was not so fortunate in electing to plod along the beaten track; he would have done *better* to leave it. There is no golden rule for the answer; we must be guided in each case by the special circumstances, and the dilemma is consequently a new one on every occasion, and perhaps the more trying with each repetition.

Such are briefly the events which led to the discovery of Neptune, which was made in Germany by direction from France, when it might have been made in Cambridge alone. The incidents created a great stir at the time. The "Account" of them, as read by Airy to the Royal Astronomical Society on November 13, 1846, straightforward and interesting though it was, making clear where he had himself been at fault, nevertheless stirred up angry passions in many quarters, and chiefly directed against Airy himself. Cambridge was furious at Airy's negligence, which it considered responsible for costing the University a great discovery; and others were equally irate at his attempting to claim for Adams some of that glory which they considered should go wholly to Le Verrier. But it may be remarked that feeling was not purely national. Some foreigners were cordial in their recognition of the work of Adams, while some of those most eager to oppose his claims were found in this country. In their anxiety to show that they were free from national jealousy, scientific men went almost too far in the opposite direction.

Airy's conduct was certainly strange at several points, as has already been remarked. One cannot understand his writing to Le Verrier in June 1846 without any mention of Adams. He could not even momentarily have forgotten Adams' work; for he tells us himself how he noticed the close correspondence of his result with that of Le Verrier: and had he even casually mentioned this fact in writing to the latter, it would have prepared the way for his later statement. But we can easily understand the unfavourable impression produced by this statement after the discovery had been made, when there had been no previous hint on the subject at all. Of those who abused him Cambridge had the least excuse; for there is no doubt that with a reasonably competent Professor of Astronomy in Cambridge, she need not have referred to Airy at all. It would not seem to require any great amount of intelligence to undertake to look in a certain region for a strange object if one is in possession of a proper instrument. We have seen that Challis had the instrument, and when urged to do so was equal to the task of finding the planet; but he was a man of no initiative, and the idea of doing so unless directed by some authority never entered his head. He had been accustomed for many years to lean rather helplessly upon Airy, who had preceded him in office at Cambridge. For instance, when appointed to succeed him, and confronted with the necessity of lecturing to students, he was so helpless that he wrote to implore Airy to come back to Cambridge and lecture for him; and this was actually done, Airy obtaining leave from the Government to leave his duties at Greenwich for a time in order to return to Cambridge, and show Challis how to lecture. Now it seems to me that this helplessness was the very root of all the mischief of which Cambridge so bitterly complained. I claimed at the outset the privilege of stating my own views, with which others may not agree: and of all the mistakes and omissions made in this little piece of history, the most unpardonable and the one which had most serious consequences seems to me to be this: that Challis never made the most casual inquiry as to the result of the visit to Greenwich which he himself had directed Adams to make. I am judging him to some extent by default; because I assume the facts from lack of evidence to the contrary: but it seems practically certain that after sending this young man to see Airy on this important topic, Challis thereupon washed his hands of all responsibility so completely that he never even took the trouble to inquire on his return, "Well! how did you get on? What did the Astronomer Royal say?" Had he put this simple question, which scarcely required the initiative of a machine, and learnt in consequence, as he must have done, that the sensitive young man thought Airy's question trivial, and did not propose to answer it, I think we might have trusted events to right themselves. Even Challis might have been trusted to reply, "Oh! but you must answer the Astronomer Royal's question: you may think it stupid, but you had better answer it politely, and show him that you know what you are about." It is unprofitable to pursue speculation further; this did *not* happen, and something else did. But I have always felt that my old University made a scapegoat of the wrong man in venting its fury upon Airy, when the real culprit was among themselves, and was the man they had themselves chosen to represent astronomy. He was presumably the best they had; but if they had no one better than this, they should not have been surprised,

He finds too late that he had observed the planet.

Sensation caused by the discovery.

Not all *national* jealousy.

The position of Cambridge in the matter.

Challis the weakest point.

and must not complain, if things went wrong. If a University is ambitious of doing great things, it must take care to see that there are men of ability and initiative in the right places. This is a most difficult task in any case, and we require all possible incentives towards it. To blink the facts when a weak spot is mercilessly exposed by the loss of a great opportunity is to lose one kind of incentive, and perhaps not the least valuable.

Curious difference between actual and supposed planet.

Let us now turn to some curious circumstances attending this remarkable discovery of a planet by mathematical investigation, of which there are several. The first is, that although Neptune was found so near the place where it was predicted, its orbit, after discovery, proved to be very different from that which Adams and Le Verrier had supposed. You will remember that both calculators assumed the distance from the sun, in accordance with Bode's Law, to be nearly twice that of Uranus. The actual planet was found to have a mean distance less than this by 25 per cent., an enormous quantity in such a case. For instance, if the supposed planet and the real were started round the sun together, the real planet would soon be a long way ahead of the other, and the ultimate disturbing effect of the two on Uranus would be very different. To explain the difference, we must first recall a curious property of such disturbances. When two planets are revolving, so that one takes just twice or three times, or any exact number of times, as long to revolve round the sun as the other, the usual mathematical expressions for the disturbing action of one planet on the other would assign an *infinite* disturbance, which, translated into ordinary language, means that we must start with a fresh assumption, for this state of things cannot persist. If the period of one were a little *longer* than this critical value, some of the mathematical expressions would be of contrary sign from those corresponding to a period a little *shorter*. Now it is curious that the supposed planet and the real had orbits on opposite sides of a critical value of this kind, namely, that which would assign a period of revolution for Neptune exactly half that of Uranus; and it was pointed out in America by Professor Peirce that the effect of the planet imagined by Adams and Le Verrier was thus totally different from that of Neptune. He therefore declared that the mathematical work had not really led to the discovery at all; but that it had resulted from mere coincidence, and this opinion—somewhat paradoxical though it was—found considerable support. It was not replied to by Adams until some thirty years later, when a short reply was printed in *Liouville's Journal*. The explanation is this: the expressions considered by Professor Peirce are those representing the action of the planet throughout an indefinite past, and did not enter into the problem, which would have been precisely the same if Neptune had been suddenly created in 1690; while, on the other hand, if Neptune had existed up till 1690 (the time when Uranus was first observed, although unknowingly), and then had been destroyed, there would have been no means of tracing its previous existence. In past ages it had no doubt been perturbing the orbit of Uranus, and had effected large changes in it; but if it had then been suddenly destroyed, we should have had no means of identifying these changes. There might have been instead of Neptune another planet, such as that supposed by Adams and Le Verrier; and its action in all past time would have been very different from that of Neptune, as is properly represented in the mathematical expressions which Professor Peirce considered. In consequence the orbit of Uranus in 1690 would have been very different from the orbit as it was actually found; but in either case the mathematicians Adams and Le Verrier would have had to take it as they found it; and the disturbing action which they considered in their calculations was the comparatively small disturbance which began in 1690 and ended in 1846. During this limited number of years the disturbance of the planet they imagined, although not precisely the same as that of Neptune, was sufficiently like it to give them the approximate place of the planet.

Professor Peirce's contention that the discovery was a mere accident.

The explanation.

Still it is somewhat bewildering to look at the mathematical expressions for the disturbances as used by Adams and Le Verrier, when we can now compare with them the actual expressions to which they ought to correspond; and one may say frankly that there seems to be no sort of resemblance. Recently a memorial of Adams' work has been published by the Royal Astronomical Society; they have reproduced in their Memoirs a facsimile of Adams' MS. containing the "first solution," which he made in 1843 in the Long Vacation after he had taken his degree, and which would have given the place of Neptune at that time with an error of 15°. In an introduction describing the whole of the MSS., written by Professor R. A. Sampson of Durham, it is shown how different the actual expressions for Neptune's influence are from those used by Adams, and it is one of the curiosities of this remarkable piece of history that some of them seem to be actually *in the wrong direction*; and others are so little alike that it is only by fixing our attention resolutely on the considerations above mentioned that we can realise that the analytical work did indeed lead to the discovery of the planet.

Suggested elementary method for finding Neptune illusory.

A second curiosity is that a mistaken idea should have been held by at least one eminent man (Sir J. Herschel), to the effect that it would have been possible to find the place of the planet by a much simpler mathematical calculation than that actually employed by Adams or Le Verrier. In his famous "Outlines of Astronomy" Sir John Herschel describes a simple graphical method, which he declares would have indicated the place of the planet without much trouble. Concerning it I will here merely quote Professor Sampson's words:—

"The conclusion is drawn that *Uranus* arrived at a conjunction with the disturbing planet about 1822; and this was the case. Plausible as this argument may seem, it is entirely baseless. For the maximum of perturbations depending on the eccentricities has no relation to conjunction, and the others

which depend upon the differences of the mean motions alone are of the nature of forced oscillations, and conjunction is not their maximum or stationary position, but their position of most rapid change."

Professor Sampson goes on to show that a more elaborate discussion seems quite as unpromising; and he concludes that the refinements employed were not superfluous, although it seems *now* clear that a different mode of procedure might have led more certainly to the required conclusion.

For the third curious point is that both calculators should have adhered so closely to Bode's Law. If they had not had this guiding principle it seems almost certain that they would have made a better approximation to the place of the planet, for instead of helping them it really led them astray. We have already remarked that if two planets are at different distances from the sun, however slight, and if they are started in their revolution together, they must inevitably separate in course of time, and the amount of separation will ultimately become serious. Thus by assuming a distance for the planet which was in error, however slight, the calculators immediately rendered it impossible for themselves to obtain a place for the planet which should be correct for more than a very brief period. Professor Sampson has given the following interesting lists of the dates at which Adams' six solutions gave the true place of the planet and the intervals during which the error was within 5° either way.

	I.	II.	III.	IV.	V.	VI.
Correct	1820	1835	1872	1830	1861	1856
Within ±5° {	1812	1827	1865	1813	1815	1826
	1827	1842	1877	1866	1871	1868

Now the date at which it was most important to obtain the correct place was 1845 or thereabouts when it was proposed to look for the planet; but no special precaution seems to have been taken by either investigator to secure any advantage for this particular date. Criticising the procedure after the event (and of course this is a very unsatisfactory method of criticism), we should say that it would have been better to make several assumptions as regards the distance instead of relying upon Bode's Law; but no one, so far as I know, has ever taken the trouble to write out a satisfactory solution of the problem as it might have been conducted. Such a solution would be full of interest, though it could only have a small weight in forming our estimation of the skill with which the problem was solved in the first instance.

Fourthly, we may notice a very curious point. Le Verrier went to some trouble not only to point out the most likely place for the planet, but to indicate limits outside which it was not necessary to look. This part of his work is specially commented upon with enthusiasm by Airy, and I will reproduce what he says. It is rather technical perhaps, but those who cannot follow the mathematics will be able to appreciate the tone of admiration.

"M. Le Verrier then enters into a most ingenious computation of the limits between which the planet must be sought. The principle is this: assuming a time of revolution, all the other unknown quantities may be varied in such a manner that though the observations will not be so well represented as before, yet the errors of observation will be tolerable. At last, on continuing the variation of elements, one error of observation will be intolerably great. Then, by varying the elements in another way, we may at length make another error of observation intolerably great; and so on. If we compute, for all these different varieties of elements, the place of the planet for 1847, its *locus* will evidently be a discontinuous curve or curvilinear polygon. If we do the same thing with different periodic times, we shall get different polygons; and the extreme periodic times that can be allowed will be indicated by the polygons becoming points. These extreme periodic times are 207 and 233 years. If now we draw one grand curve, circumscribing all the polygons, it is certain that the planet must be within that curve. In one direction, M. Le Verrier found no difficulty in assigning a limit; in the other he was obliged to restrict it, by assuming a limit to the eccentricity. Thus he found that the longitude of the planet was certainly not less than 321°, and not greater than 335° or 345°, according as we limit the eccentricity to 0.125 or 0.2. And if we adopt 0.125 as the limit, then the mass will be included between the limits 0.00007 and 0.00021; either of which exceeds that of *Uranus*. From this circumstance,
combined with a probable hypothesis as to the density, M. Le Verrier concluded that the planet would have a visible disk, and sufficient light to make it conspicuous in ordinary telescopes.

"M. Le Verrier then remarks, as one of the strong proofs of the correctness of the general theory, that the error of radius vector is explained as accurately as the error of longitude. And finally, he gives his opinion that the latitude of the disturbing planet must be small.

"My analysis of this paper has necessarily been exceedingly imperfect, as regards the astronomical and mathematical parts of it; but I am sensible that, in regard to another part, it fails totally. I cannot attempt to convey to you the

impression which was made on me by the author's undoubting confidence in the general truth of his theory, by the calmness and clearness with which he limited the field of observation, and by the firmness with which he proclaimed to observing astronomers, 'Look in the place which I have indicated, and you will see the planet well.' Since Copernicus declared that, when means should be discovered for improving the vision, it would be found that *Venus* had phases like the moon, nothing (in my opinion) so bold, and so justifiably bold, has been uttered in astronomical prediction. It is here, if I mistake not, that we see a character far superior to that of the able, or enterprising, or industrious mathematician; it is here that we see the philosopher."

Peirce's views of the limits. But now this process of limitation was faulty and actually misleading. Let us compare what is said about it by Professor Peirce a little later. [Pg 80]

"Guided by this principle, well established, and legitimate, if confined within proper limits, M. Le Verrier narrowed with consummate skill the field of research, and arrived at two fundamental propositions, namely:—

"1st. That the mean distance of the planet cannot be less than 35 or more than 37.9. The corresponding limits of the time of sidereal revolution are about 207 and 233 years.

"2nd. 'That there is only one region in which the disturbing planet can be placed in order to account for the motions of Uranus; that the mean longitude of this planet must have been, on January 1, 1800, between 243° and 252°.'

"'Neither of these propositions is of itself necessarily opposed to the observations which have been made upon Neptune, but the two combined are decidedly inconsistent with observation. It is impossible to find an orbit, which, satisfying the observed distance and motion, is subject to them. If, for instance, a mean longitude and time of revolution are adopted according with the first, the corresponding mean longitude in 1800 must have been at least 40° distant from the limits of the second proposition. And again, if the planet is assumed to have had in 1800 a mean longitude near the limits of the second proposition, the corresponding time of revolution with which its motions satisfy the present observations cannot exceed 170 years, and must therefore be about 40 years less than the limits of the first proposition.' [Pg 81]

"Neptune cannot, then, be the planet of M. Le Verrier's theory, and cannot account for the observed perturbations of Uranus under the form of the inequalities involved in his analysis"—(*Proc. Amer. Acad. I.*, 1846-1848, *p.* 66).

Newcomb's criticism. At the time when Professor Peirce wrote, the orbit of Neptune was not sufficiently well determined to decide whether one of the two limitations might not be correct, though he could see that they could not both be right, and we now know that they are *both wrong*. The mean distance of Neptune is 30, which does *not* lie between 35 and 37.9; and the longitude in 1800 was 225°, which does *not* lie between 243° and 252°. The ingenious process which Airy admired and which Peirce himself calls "consummately skilful" was wrong in principle. As Professor Newcomb has said, "the error was the elementary one that, instead of considering all the elements simultaneously variable, Le Verrier took them one at a time, considering the others as fixed, and determining the limits between which each could be contained on this hypothesis. No solver of least square equations at the present day ought to make such a blunder. Of course one trouble in Le Verrier's demonstration, had he attempted a rigorous one, would have been the impossibility of forming the simultaneous equations expressive of possible variations of all the elements." [Pg 82]

Element of good fortune. The map used by Galle. The account of Le Verrier's limits by Professor Peirce, though it exhibits the error with special clearness, is a little unfair to Le Verrier in one point. If, instead of taking the limits for the date 1800, we take them for 1846 (when the search for Neptune was actually made), we shall find that they do include the actual place of the planet, as Airy found. The erroneous mean motion of Le Verrier's planet allowed of his being right at one time and wrong at another; and Airy examined the limits under favourable conditions, which explains his enthusiasm. But we can scarcely wonder that Professor Peirce came to the conclusion that the planet discovered was not the one pointed out by Le Verrier, and had been found by mere accident. And all these circumstances inevitably contribute to a general impression that the calculators had a large element of good fortune to thank for their success. Nor need we hesitate to make this admission, for there is an element of good fortune in all discoveries. To look no further than this—if a man had not been doing a particular thing at a particular time, as he might easily not have been, most discoveries would never have been made. If Sir William Herschel had not been looking at certain small stars for a totally different purpose he would never have found Uranus; and no one need hesitate to admit the element of chance in the finding of Neptune. It is well illustrated by a glance at the map which, as has been remarked, Galle used to compare with the sky on the night when he made the actual discovery. The planet was found down near the bottom corner of the map, and since the limits assigned for its place might easily have varied a few degrees one way or the other, it might easily have been off the map; in which case, it is probable that the search would not have been successful, or at any rate that success would have been delayed. [Pg 83]
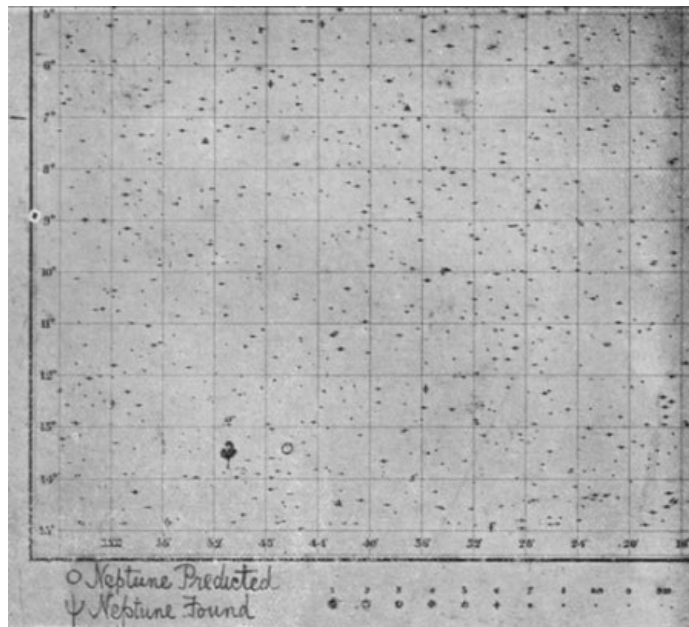
V.—Corner of the Berlin Map, by the use of which Galle found Neptune.

Every one made mistakes.

Thus, it is a most remarkable feature of the discovery of Neptune that mistakes were made by almost every one concerned, however eminent. Airy made a mistake in regarding the question of the Radius Vector as of fundamental importance; Sir J. Herschel was wrong in describing an elementary method which he considered might have found the planet; Professor Peirce was wrong in supposing that the actual and the supposed planet were essentially different in their action on Uranus; Le Verrier was wrong in assigning limits outside which it was not necessary to look when the actual planet was outside them; Adams was more or less wrong in thinking that the eccentricity of the new planet could be found from the material already at disposal of man. Both Adams and Le Verrier gave far too much importance to Bode's Law.

To review a piece of history of this kind and note the mistakes of such men is certainly comforting, and need not in any way lessen our admiration. In the case of the investigators themselves, much may be set down to excitement in the presence of a possible discovery. Professor Sampson has provided us with a small but typical instance of this fact. When Adams had carried through all his computations for finding Neptune, and was approaching the actual place of the planet, he, "who could carry through fabulous computations without error," for the first time wrote down a wrong figure. The mistake was corrected upon the MS., "probably as soon as made," but no doubt betrays the excitement which the great worker could not repress at this critical moment. There is a tradition that, similarly, when the mighty Newton was approaching the completion of his calculations to verify the Law of Gravitation, his excitement was so great that he was compelled to assign to a friend the task of finishing them.

Finally, we may remark how the history of the discovery of Neptune again illustrates the difficulty of formulating any general principles for guiding scientific work. Sometimes it is well to follow the slightest clue, however imperfectly understood; at other times we shall do better to refuse such guidance. Bode's Law pointed to the existence of minor planets, and might conceivably have helped in finding Uranus: but by trusting to it in the case of Neptune, the investigators were perilously near going astray. Sometimes it is better to follow resolutely the work in hand whatever it may be, shutting one's ears to other calls; but Airy and Challis lost their opportunities by just this course of action. The history of science is full of such contradictory experiences; and the only safe conclusion seems to be that there are no general rules of conduct for discovery.

CHAPTER III

BRADLEY'S DISCOVERIES OF THE ABERRATION OF LIGHT AND OF THE NUTATION OF THE EARTH'S AXIS

Biographical

In examining different types of astronomical discovery, we shall find certain advantages in

varying to some extent the method of presentation. In the two previous chapters our opportunities for learning anything of the life and character of those who made the discoveries have been slight; but I propose to adopt a more directly biographical method in dealing with Bradley's discoveries, which are so bound up with the simple earnestness of his character that we could scarcely appreciate their essential features properly without some biographical study. But the record of his life apart from his astronomical work is not in any way sensational; indeed it is singularly devoid of incident. He had not even a scientific quarrel. There was scarcely a man of science of that period who had not at least one violent quarrel with some one, save only Bradley, whose gentle nature seems to have kept him clear of them all. Judged by ordinary standards his life was uneventful: and yet it may be doubted whether, to him who lived it, that life contained one dull moment. Incident came for him in his scientific work: in the preparation of apparatus, the making of observations, above all in the hard-thinking which he did to get at the clue which would explain them; and after reviewing his biography,[2] I think we shall be inclined to admit that if ever there was a happy life, albeit one of unremitting toil, it was that of James Bradley.

He was born at Sherbourn, in Gloucestershire, in 1693. We know little of his boyhood except that he went to the Grammar School at Northleach, and that the memory of this fact was preserved at the school in 1832 when Rigaud was writing his memoir. [The school is at present shut up for want of funds to carry it on; and all inquiries I have made have failed to elicit any trace of this memory.] Similarly we know little of his undergraduate days at Oxford, except that he entered as a commoner at Balliol in 1710, took his B.A. in the regular course in 1714, and his M.A. in 1717. As a career he chose the Church, being ordained in 1719, and presented to the vicarage of Bridstow in Monmouthshire; but he only discharged the duties of vicar for a couple of years, for in 1721 he returned to Oxford as Professor of Astronomy, an appointment which involved the resignation of his livings; and so slight was this interruption to his career as an astronomer that we may almost disregard it, and consider him as an astronomer from the first. But to guard against a possible misconception,

let me say that Bradley entered on a clerical career in a thoroughly earnest spirit; to do otherwise would have been quite foreign to his nature. When vicar of Bridstow he discharged his duties faithfully towards that tiny parish, and moreover was so active in his uncle's parish of Wansted that he left the reputation of having been curate there, although he held no actual appointment. And thirty years later, when he was Astronomer Royal and resident at Greenwich, and when the valuable vicarage of Greenwich was offered to him by the Chancellor of the Exchequer, he honourably refused the preferment, "because the duty of a pastor was incompatible with his other studies and necessary engagements."

But now let us turn to Bradley's astronomical education. I must admit, with deep regret, that we cannot allow any of the credit of it to Oxford. There was a great astronomer in Oxford when Bradley was an undergraduate, for Edmund Halley had been appointed Savilian Professor of Geometry in 1703, and had immediately set to work to compute the orbits of comets, which led to his immortal discovery that some of these bodies return to us again and again, especially the one which bears his name—Halley's Comet—and returns every seventy-five years, being next expected about 1910. But there is no record that Bradley came under Halley's teaching or influence as an undergraduate. In later years the two men knew each other well, and it was Halley's one desire towards the close of his life that Bradley should succeed him as Astronomer Royal at Greenwich; a desire which was fulfilled in rather melancholy fashion, for Halley died without any assurance that his wish would be gratified.

But Bradley got no astronomical teaching at Oxford either from Halley or others. The art of astronomical observation he learnt from his maternal uncle, the Rev. James Pound, Rector of Wansted, in Essex. He is the man to whom we owe Bradley's training and the great discoveries which came out of it. He was, I am glad to say, an Oxford man too; very much an Oxford man; for he seems to have spent some thirteen years there migrating from one Hall to another. His record indeed was such as good tutors of colleges frown upon; for it was seven years before he managed to take a degree at all; and he could not settle to anything. After ten years at Oxford he thought he would try medicine; after three years more he gave it up and went out in 1700 as chaplain to the East Indies. But he seems to have been a thoroughly lovable man, for news was brought of him four years later that he had a mind to come home, but was dissuaded by the Governor saying that "if Dr. Pound goes, I and the rest of the Company will not stay behind." Soon afterwards the settlement was attacked in an insurrection, and Pound was one of the few who escaped with his life, losing however all the property he had gradually acquired. He returned to England in 1706, and was presented to the living of Wansted; married twice, and ended his days in peace and fair prosperity in 1724. Such are briefly the facts about Bradley's uncle, James Pound; but the most important of all remains to be told—that somehow or other he had learnt to make first-rate astronomical observations, how or when is not recorded; but in 1719 he was already so skilled that Sir Isaac Newton made him a present of fifty guineas for some observations; and repeated the gift in the following year; and even three years before this we find Halley writing to ask for certain observations from Mr. Pound.

With this excellent man Bradley used frequently to stay. To his nephew he seems to have been more like a father than an uncle. When his nephew had smallpox in 1717, he nursed him through it; and he supplemented from his own pocket the scanty allowance which was all that Bradley's own father could afford. But what concerns us most is that he fostered, if he did not actually implant, a love of astronomical observation in his nephew. The two worked together, entering their observations one after the other on the same paper; and it

with him. was to the pair of them together, rather than to the uncle alone, that Newton made his princely presents, and Halley wrote for help in his observations. There seems to be no doubt that the uncle and nephew were about this time the best astronomical observers in the world. There was no rivalry between them, and therefore there is no need to discuss whether the partnership was one of equal merit on both sides; but it is interesting to note that it probably was. The ability of Pound was undoubted; many were keenly desirous that he, and not his nephew, should be elected to the Oxford Chair in 1721, but he felt unequal to the duties at his advanced age. On the other hand, when Bradley lost his uncle's help, there was no trace of faltering in his steps to betray previous dependence on a supporting or guiding hand. He walked erect and firm, and trod paths where even his uncle might not have been able to follow.

The work done by Pound and Bradley. A few instances will suffice to show the kind of observations made by this notable firm of Pound and Bradley. They observed the positions of the fixed stars and nebulæ: these being generally the results required by Halley and Newton. They also observed the places of the planets among the stars, and especially the planet Mars, and determined its distance from the Earth by the method of parallax, thus anticipating the modern standard method of finding the Sun's distance; and though with their imperfect instruments they did not obtain a greater accuracy than 1 in 10, still this was a great advance on what had been done before, and excited the wonder and admiration of Halley. They also paid some attention to double stars, and did a great deal of work on Jupiter's satellites. We might profitably linger over the records of these early years, which are full of interest, but we must press on to the time of the great discoveries, and we will dismiss them with brief illustrations of three points: Bradley's assiduity, his skill in calculation, and his wonderful skill in the management of instruments. Of his assiduity an example is afforded by his calculations of the orbits of two comets which are still extant. One of them fills thirty-two pages of foolscap, and the other sixty; and it must be remembered that the calculations themselves were quite novel at that time. Of his *skill* in calculation, apart from his assiduity, we have a proof in a paper communicated to the Royal Society rather later (1726), where he determines the longitudes of Lisbon and New York from the eclipses of Jupiter's satellites, using observations which were not simultaneous, and had therefore to be corrected by an ingenious process which

Use of very long telescopes. Bradley devised expressly for this purpose. And finally, his skill in the management of instruments is shown by his measuring the diameter of the planet Venus with a telescope actually 212¼ feet in length. It is difficult for us to realise in these days what this means; even the longest telescope of modern times does not exceed 100 feet in length, and it is mounted so conveniently with all the resources of modern engineering, in the shape of rising floors, &c., that the management of it is no more difficult than that of a 10-foot telescope. But Bradley had no engineering appliances beyond a pole to hold up one end of the telescope and his own clever fingers to work the other; and he managed to point the unwieldy weapon accurately to the planet, and measure the diameter with an exactness

Reason for great length. which would do credit to modern times. A few words of explanation may be given why such long telescopes were used at all. The reason lay in the difficulty of getting rid of coloured images, due to the composite character of white light. Whenever we use a *single* lens to form an image, coloured fringes appear. Nowadays we know that by making two lenses of different kinds of glass and putting them together, we can practically get rid of these coloured fringes; but this discovery had not been made in Bradley's time. The only known ways of dealing with the evil then were to use a reflecting telescope like Newton and Gregory, or if a lens was used, to make one of very great focal length; and hence the primary necessity for these very long telescopes. They had another advantage in producing a large image, or they would probably have given way to the reflector. This advantage is gradually bringing them back into use, and perhaps in the eclipse of 1905 we may use a telescope as long as Bradley's; but we shall not use it as he did in any case. It will be laid comfortably flat on the ground, and the rays of light reflected into it by a coelostat.

Bradley appointed at Oxford, In 1721 Bradley was appointed to the Savilian Professorship of Astronomy at Oxford, vacant by the death of Dr. John Keill. Once it became clear that there was no chance of securing his uncle for this position, Bradley himself was supported enthusiastically by all those whose support was worth having, especially by the Earl of Macclesfield, who was then Lord Chancellor; by Martin Foulkes, who was afterwards the President of the Royal Society; and by Sir Isaac Newton himself. He was accordingly elected on October 31, 1721, and forthwith resigned his livings. His resignation of the livings was necessitated by a definite statute of the University relating to the Professorship, and not by the existence of any very onerous duties attaching to it; indeed such duties seem to have been conspicuously absent, and after but continues to work at Wansted. Bradley's election he passed more time than ever with his uncle in Wansted, making the astronomical observations which both loved; for there was not the vestige of an observatory in Oxford. His uncle's death in 1724 interrupted the continuity of these joint observations, and by an odd accident prepared the way for Bradley's great discovery. He was fain to seek elsewhere that companionship in his work which had become so essential to him, and his new friend gave a new bent to his observations.

Samuel Molyneux. Samuel Molyneux was a gentleman of fortune much attached to science, and particularly to astronomy, who was living about this time at Kew. He was one of the few, moreover, who are not content merely to amuse themselves with a telescope, but had the ambition to do some real earnest work, and the courage to choose a problem which had baffled the human race for more than a century. The theory of Copernicus, that the earth moved round the sun, necessitated a corresponding apparent change in the places of the stars, one relatively to

another; and it was a standing difficulty in the way of accepting this theory that no such change could be detected. In the old days before the telescope it was perhaps easy to understand that the change might be too small to be noticed, but the telescope had made it possible to measure changes of position at least a hundred times as small as before, and still no "parallax," as the astronomical term goes, could be found for the stars. The observations of Galileo, and the measures of Tycho Brahé, as reduced to systematic laws by Kepler, and finally by the great Newton, made it clear that the Copernican theory was *true*: but no one had succeeded in proving its truth in this particular way. Samuel Molyneux must have been a man of great courage to set himself to try to crack this hard nut; and we can understand the attraction which his enterprise must have had for Bradley, who had just lost the beloved colleague of many courageous astronomical undertakings. His co-operation seems to have been welcomed from the first; his help was invited and freely given in setting up the instrument, and he fortunately had the leisure to spend considerable time at Kew making the observations with Molyneux, just as he had been wont to observe with his uncle.

I must now briefly explain what these observations were. There is a bright star γ Draconis, which passes almost directly overhead in the latitude of London. Its position is slowly changing owing to the precession of the equinoxes, but for two centuries it has been, and is still, under constant observation by London astronomers owing to this circumstance, that it passes directly overhead, and so its position is practically undisturbed by the refraction of our atmosphere.

It was therefore thought at the time that, there being no disturbance from refraction, the disturbance from precession being accurately known, and there being nothing else to disturb the position but "parallax" (the apparent shift due to the earth's motion which it was desirable to find), this star ought to be a specially favourable object for the determination of parallax. Indeed it had been announced many years before by Hooke that its parallax had been found; but his observations were not altogether satisfactory, and it was with a view of either confirming them or seeing what was wrong with them that Molyneux and Bradley started their search. They set up a much more delicate piece of apparatus than Hooke had employed. It was a telescope 24 feet long pointed upwards to the star, and firmly attached to a large stack of brick chimneys within the house. The telescope was not absolutely fixed, for the lower end could be moved by a screw so as to make it point accurately to the star, and a plumb-line showed how far it was from the vertical when so pointing. Hence if the star changed its position, however slightly, the reading of this screw would show the change. Now, before setting out on the observations, the observers knew what to expect if the star had a real parallax; that is to say, they knew that the star would seem to be farthest south in December, farthest north in June, and at intermediate positions in March and September; though they did not know *how much* farther south it would appear in December than in June —this was exactly the point to be decided.
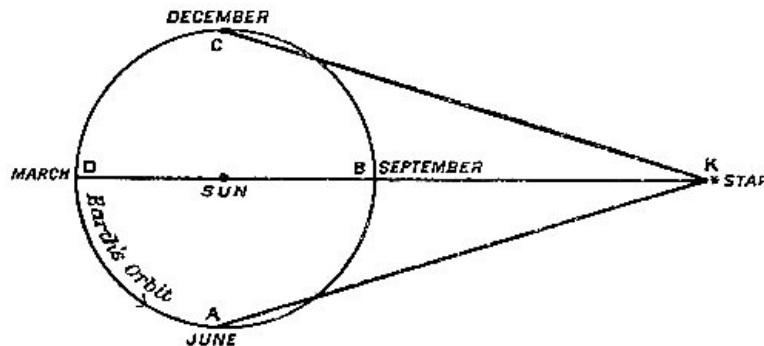
F<small>IG</small>. 2.

The reason of this will be clear from Fig. 2. [Remark, however, that this figure and the corresponding figure 4 do not represent the case of Bradley's star, γ Draconis: another star has been chosen which simplifies the diagram, though the principle is essentially the same.] Let A B C D represent the earth's orbit, the earth being at A in June, at B in September, and so on, and let K represent the position of the star on the line D B. Then in March and September it will be seen from the earth in the same direction, namely, D B K; but the directions in which it is seen in June and December, viz. A K and C K, are inclined in opposite ways to this line. The farther away the star is, the less will this inclination or "parallax" be; and the star is actually so far away that the inclination can only be detected with the utmost difficulty: the lines C K and A K are sensibly parallel to D B K. But Bradley did not know this; it was just this point which he was to examine, and he expected the greatest inclination in one direction to be in December. Accordingly when a few observations had been made on December 3, 5, 11, and 12 it was thought that the star had been caught at its most southerly apparent position, and might be expected thereafter to move northwards, if at all. But when Bradley repeated the observation on December 17, he found to his great surprise that the star was still moving southwards. Here was something quite new and unexpected, and such a keen observer as Bradley was at once on the alert. He

soon found that the changes in the position of the star were of a totally unexpected character. Instead of the extreme positions being occupied in June and December, they were occupied in March and September, just midway between these. And the range in position was quite large, about 40″—not a quantity which could have been detected in the days before telescopes, but one which was unmistakable with an instrument of the most moderate measuring capacity.

What, then, was the cause of this quite unforeseen behaviour on the part of the star? The first thought of the observers was that something might be wrong with their instrument, and it was carefully examined, but without result. The next was that the apparent movement was in the plumb-line, the line of reference. If the whole earth, instead of carrying its axis round the sun in a constant direction, were to be executing an oscillation, then all our plumb-lines would oscillate, and when the direction of a star like γ Draconis was compared with that of the plumb-line it would seem to vary, owing actually to the variation in the plumb-line. The earth might have a motion of this kind in two ways, which it will be necessary for us to distinguish, and the adopted names for them are "nutation of the axis" and "variation of latitude" respectively. In the case of nutation the North Pole remains in the same geographical position, but points to a different part of the heavens. The "variation of latitude," on the other hand, means that the North Pole wanders about on the earth itself. We shall refer to the second phenomenon more particularly in the sixth chapter.

But it was the first kind of change, the nutation, which Bradley suspected; and very early in the series of observations he had already begun to test this hypothesis. If it was not the star, but the earth and the plumb-line, which were in motion, then other stars ought to be affected. The telescope had been deliberately restricted in its position to suit γ Draconis; but since the stars circle round the Pole, if we draw a narrow belt in the heavens with the Pole as centre, and including γ Draconis, the other stars included would make the same circuit, preceding or following γ Draconis by a constant interval. Most of them would be too faint for observation with Bradley's telescope; but there was one bright enough to be observed, which also came within its limited range, and it was promptly put under *surveillance* when a nutation of the earth's axis was suspected. Careful watching showed that it was not affected in the same way as γ Draconis, and hence the movement could not be in the plumb-line. Was there, then, after all, some effect of the earth's atmosphere which had been overlooked? We have already remarked that since the star passes directly overhead there should be practically no refraction; and this assumption was made by Molyneux and Bradley in choosing this particular star for observation. It follows at once, if we assume that the atmosphere surrounds the earth in spherical layers. But perhaps this was not so? Perhaps, on the contrary, the atmosphere was deformed by the motion of the earth, streaming out behind her like the smoke of a moving engine? No possibility must be overlooked if the explanation of this puzzling fact was to be got at.
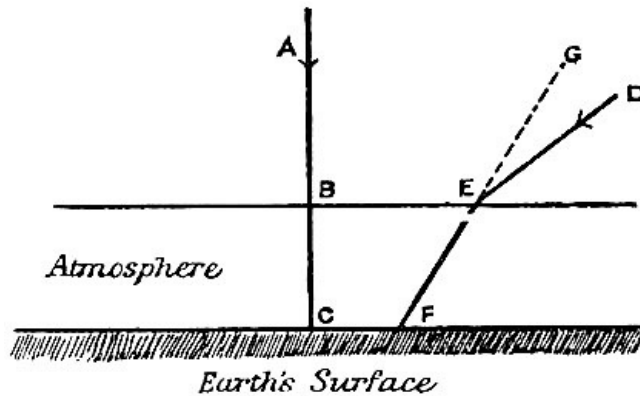
F<small>IG</small>. 3.

The way in which a deformation of the atmosphere might explain the phenomenon is best seen by a diagram. First, it must be remarked that rays of light are only bent by the earth's atmosphere, or "refracted," if they enter it obliquely.

If the atmosphere were of the same density throughout, like a piece of glass, then a vertical ray of light, A B (see Fig. 3), entering the atmosphere at B would suffer no bending or refraction, and a star shining from the direction A B would be seen truly in that direction from C. But an oblique ray, D E, would be bent on entering the atmosphere at E along the path EF, and a star shining along D E would appear from F to be shining along the dotted line G E F. The atmosphere is not of the same density throughout, but thins out as we go upwards from the earth; and in consequence there is no clear-cut surface, B E, and no sudden bending of the rays as at E: they are gradually bent at an infinite succession of imaginary surfaces. But it still remains true that there is no bending at all for vertical rays; and of oblique rays those most oblique are most bent.
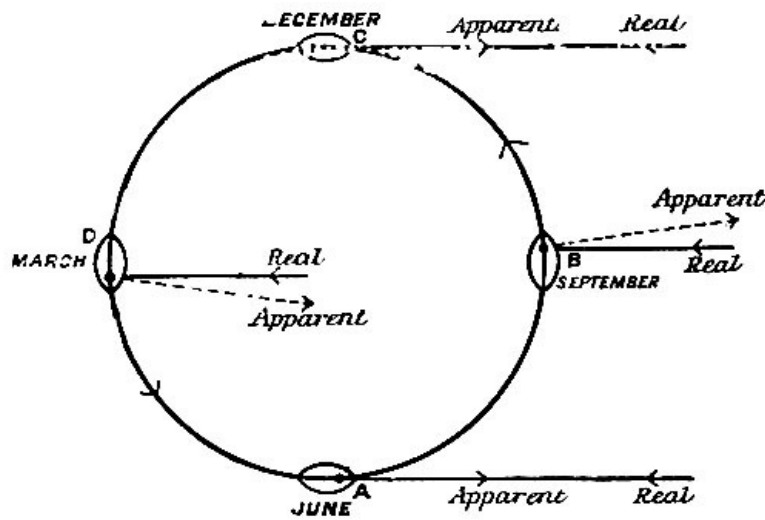
F<small>IG</small>. 4.

Now, suppose the atmosphere of the earth took up, owing to its revolution round the sun, an elongated shape like that indicated in diagram 4, and suppose the star to be at a great distance away to the right of the diagram. When the earth is in the position labelled "June," the light would fall vertically on the nose of the atmosphere at A, and there would be no refraction. Similarly in "December" the light would fall at C on the stern, also vertically, and there would be no refraction. [The rays from the distant star in December are to be taken as sensibly parallel to those received in June, notwithstanding that the earth is on the opposite side of the sun, as was remarked on p. 98.] But in March and September the rays would strike obliquely on the sides of the supposed figure, and thus be bent in opposite directions, as indicated by the dotted lines; and the extreme positions would thus occur in March and September, as had been observed. The explanation thus far seems satisfactory enough.

But we have assumed the star to lie in the plane of the earth's orbit; and the stars under observation by Bradley did not lie in this plane, nor did they lie in directions equally inclined to it. Making the proper allowance for their directions, it was found impossible to fit in the facts with this hypothesis, which had ultimately to be abandoned.

Delay in finding real explanation.

It is remarkable to find that two or three years went by before the real explanation of this new phenomenon occurred to Bradley, and during this time he must have done some hard thinking. We have all had experience of the *kind* of thinking if only in the guessing of conundrums. We know the apparent hopelessness of the quest at the outset: the racking of our brains for a clue, the too frequent despair and "giving it up," and the simplicity of the answer when once it is declared. But with scientific conundrums the expedient of "giving it up" is not available. We must find the answer for ourselves or remain in ignorance; and though we may feel sure that the answer when found will be as simple as that to the best conundrum, this expected simplicity does not seem to aid us in the search. Bradley was not content with sitting down to think: he set to work to accumulate more facts. Molyneux's instrument only allowed of the observation of two stars, γ Draconis and the small star above mentioned. Bradley determined to have an instrument of his own which should command a wider range of stars; and by this time he was able to return to his uncle's house at Wansted for this purpose. His uncle had been dead for two or three years, and the memory of the loss was becoming mellowed with time. His uncle's widow was only too glad to welcome back her nephew, though no longer to the old rectory, and she allowed him to set up a long telescope, even though he cut holes in her floor to pass it through. The object-glass end was out on the roof and the eye end down in the coal cellar; and accordingly in this coal cellar Bradley made the observations which led to his immortal discovery. He had a list of seventy stars to observe, fifty of which he observed pretty regularly. It may seem odd that he did not set up this new instrument at Oxford, but we find from an old memorandum that his professorship was not bringing him in quite £140 a year, and probably he was glad to accept his aunt's hospitality for reasons of economy. By watching these different stars he gradually got a clear conception of the laws of aberration. The real solution of the problem, according to a well-authenticated account, occurred to him almost accidentally. We all know the story of the apple falling and setting Newton to think about the causes of gravitation. It was a similarly trivial circumstance which suggested to Bradley the explanation which he had been seeking for two or three years in vain. In his own words, "at last, when he despaired of being able to account for the phenomena which he had observed, a satisfactory explanation of them occurred to him all at once when he was not in search of it." He accompanied a pleasure party in a sail upon the river Thames. The boat in which they were was provided with a mast which had a vane at the top of it. It blew a moderate wind, and the party sailed up and down the river for a considerable time. Dr. Bradley remarked that every time the boat put about the vane at the top of the boat's mast shifted a little, as if there had been a slight change in the direction of the wind. He observed this three or four times without speaking; at last he mentioned it to the sailors, and expressed his surprise that the wind

Bradley sets up another instrument at Wansted.

Finds the right clue.

A wind-vane on a boat.

should shift so regularly every time they put about. The sailors told him that the wind had not shifted, but that the apparent change was owing to the change in the direction of the boat, and assured him that the same thing invariably happened in all cases. This accidental observation led him to conclude that the phenomenon which had puzzled him so much was owing to the combined motion of light and of the earth. To explain exactly what is meant we must again have recourse to a diagram; and we may also make use of an illustration which has become classical.
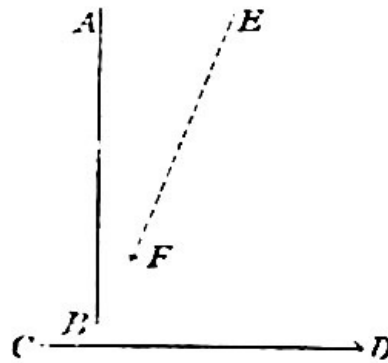
FIG. 5.

Analogy of rain. If rain is falling vertically, as represented by the direction A B; and if a pedestrian is walking horizontally in the direction C D, the rain will appear to him to be coming in an inclined direction, E F, and he will find it better to tilt his umbrella forwards. The quicker his pace the more he will find it advisable to tilt the umbrella. This analogy was stated by Lalande before the days of umbrellas in the following words: "Je suppose que, dans un temps calme, la pluie tombe perpendiculairement, et qu'on soit dans une voiture ouverte sur le devant; si la voiture est en repos, on ne reçoit pas la moindre goutte de pluie; si la voiture avance avec rapidité, la pluie entre sensiblement, comme si elle avoit pris une direction oblique." Lalande's example, modified to suit modern conditions, has been generally adopted by teachers, and in examinations candidates produce graphic pictures of the stationary, the moderate-paced, and the flying, possessors of umbrellas.

Aberration. Applying it to the phenomenon which it is intended to illustrate, if light is being received from a star by an earth, travelling across the direction of the ray, the telescope (which in this case represents the umbrella) must be tilted forward to catch the light. Now on reference to Fig. 4 it will be seen that the earth is travelling across the direction of rays from the star in March and September; and in opposite directions in the two cases. Hence the telescope must be tilted a little, in opposite directions, to catch the light; or, in other words, the star will appear to be farthest south in March, farthest north in September. And so at last the puzzle was solved, and the solution was found, as so often happens, to be of the simplest kind; so simple when once we know, and so terribly hard to imagine when we don't! It may comfort us in our struggles with minor problems to reflect that Bradley manfully stuck to his problem for two or three years. It was probably never out of his thoughts, waking or sleeping; when at work it was the chief object of his labours, and when on a pleasure party he was ready to catch at the slightest clue, in the motion of a wind-vane on a boat, which might help him to the solution.

Results of discovery. The discovery of aberration made Bradley famous at a bound. Oxford might well be proud of her two Savilian Professors at this time, for they had both made world-famous discoveries— Halley that of the periodicity of comets, and Bradley of the aberration of light. How different their tastes were and how difficult it would have been for either to do the work of the other! Bradley was no great mathematician, and though he was quite able to calculate the orbit of a comet, and carried on such work when Halley left it, it was probably not congenial to him. Halley, on the other hand, almost despised accurate observations as finicking. "Be sure you are correct to a minute," he was wont to say, "and the fractions do not so much matter." With such a precept Bradley would never have made his discoveries. No quantity was too small in his eyes, and no sooner was the explanation of aberration satisfactorily established than he perceived that though it would account for the main facts, it would not explain all. There was something left. This is often the case in the history of science. A few years ago it was thought that we knew the constitution of our air completely—oxygen, nitrogen, water vapour, and carbonic acid gas; but a great physicist, Lord Rayleigh, found that after extracting all the water and carbonic acid gas, all the oxygen and all the nitrogen, there was something left—a very minute residuum, which a careless experimenter would have overlooked or neglected, but which a true investigator like Lord Rayleigh saw the immense importance of. He kept his eye on that something left, and presently discovered a new gas which we now know as argon. Had he repeated the process, extracting all the argon after the nitrogen, he might have found by a scrutiny much more accurate still yet another gas, helium, which we now know to exist in extremely minute quantities in the air. But meantime this discovery was made in another way.

Still something to be explained.

Probably nutation.

His nineteen years' campaign.

When Bradley had extracted all the aberration from his observations he found that there was something left, another problem to be solved and some more thinking to be done to solve it. But he was now able to profit by his previous labours, and the second step was made more easily than the first. The residuum was not the parallax of which he had originally been in search, for it did not complete a cycle within the year; it was rather a progressive change from year to year. But there was an important clue of another kind. He saw that the apparent movements of all stars were in this case the same; and he knew that a movement of this kind can be referred, not to the stars themselves, but to the plumb-line from which their directions are measured. He had thought out the possible causes of such a movement of the plumb-line or of the earth itself, and had realised that there might be a *nutation* which would go through a cycle in about nineteen years, the period in which the moon's nodes revolve. He was not mathematician enough to work out the cause completely, but he saw clearly that to trace the whole effect he must continue the observations for nineteen years; and accordingly he entered on this long campaign without any hesitation. His instrument was still that in his aunt's house at Wansted, where he continued to live and make the observations for a few years, but in 1732 he removed to Oxford, as we shall see, and he must have made many journeys between Wansted and Oxford in the course of the remaining fifteen years during which he continued to trace out the effects of nutation. His aunt too left Wansted to accompany Bradley to Oxford, and the house passed into other hands. It is to the lasting credit of the new occupant, Mrs. Elizabeth Williams, that the great astronomer was allowed to go on and complete the valuable series of observations which he had commenced. Bradley was not lodged in her house; he stayed with a friend close by on his visits to Wansted, but came freely in and out of his aunt's old home to make his observations. How many of us are there who would cheerfully allow an astronomer to enter our house at any hour of the night to make observations in the coal-cellar! It says much, not only for Bradley's fame, but for his personal attractiveness, that he should have secured this permission, and that there should be no record of any friction during these fifteen years. At the end of the whole series of nineteen years his conclusions were abundantly verified, and his second great discovery of nutation was established. Honours were showered upon him, and no doubt the gentle heart of Mrs. Elizabeth Williams was uplifted at the glorious outcome of her long forbearance.

Residence at Oxford.

But we may now turn for a few moments from Bradley's scientific work to his daily life. We have said that in 1732, after holding his professorship for eleven years, he first went definitely to reside in Oxford. He actually had not been able to afford it previously. His income was only £140 a year, and the statutes prevented him from holding a living: so that he was fain to accept Mrs. Pound's hospitable shelter. But in 1729 an opportunity of adding to his income presented itself, by giving lectures in "experimental philosophy." The observations on nutation were not like those on aberration: he was not occupied day and night trying to find the solution: he had practically made up his mind about the solution, and the actual observations were to go on in a quiet methodical manner for nineteen years, so that he now had leisure to look about him for other employment. Dr. Keill, who had been Professor of Astronomy before Bradley, had attracted large classes to lectures, not on astronomy, but on experimental philosophy: but had sold his apparatus and goodwill to Mr. Whiteside, of Christ Church, one of the candidates who were disappointed by Bradley's election. In 1729 Bradley purchased the apparatus from Whiteside, and began to give lectures in experimental philosophy. His discovery of aberration had made him famous, so that his classes were large from the first, and paid him considerable fees. Suddenly therefore he changed his poverty for a comfortable income, and he was able to live in Oxford in one of two red brick houses in New College Lane, which were in those days assigned to the Savilian Professors (now inhabited by New College undergraduates). His aunt, Mrs. Pound, to whom he was devotedly attached, came with him, and two of her nephews. In his time of prosperity Bradley was thus able to return the hospitality which had been so generously afforded him in times of stress.

Astronomer Royal at Greenwich.

Letter from Earl of Macclesfield.

Before he completed his observations for nutation another great change in his fortunes took place. In 1742 he was elected to succeed Halley as Astronomer Royal. It was Halley's dying wish that Bradley should succeed him, and it is said that he was even willing to resign in his favour, for his right hand had been attacked by paralysis, and the disease was gradually spreading. But he died without any positive assurance that his wish would be fulfilled. The chief difficulty in securing the appointment of Bradley seems to have been that he was the obvious man for the post in universal opinion. "It is not only my friendship for Mr. Bradley that makes me so ardently wish to see him possessed of the position," wrote the Earl of Macclesfield to the Lord Chancellor; "it is my real concern for the honour of the nation with regard to science. For as our credit and reputation have hitherto not been inconsiderable amongst the astronomical part of the world, I should be extremely sorry we should forfeit it all at once by bestowing upon a man of inferior skill and abilities the most honourable, though not the most lucrative, post in the profession (a post so well filled by Dr. Halley and his predecessor), when at the same time we have amongst us a man known by all the foreign, as well as our own astronomers, not to be inferior to either of them, and one whom Sir Isaac Newton was pleased to call the best astronomer in Europe." And again, "As Mr. Bradley's abilities in astronomical learning are allowed and confessed by all, so his character in every respect is so well established, and so unblemished, that I may defy the worst of his enemies (if so good and worthy a man have any) to make even the lowest or most trifling objection to it."

"After all," the letter goes on, "it may be said if Mr. Bradley's skill is so universally acknowledged, and his character so established, there is little danger of opposition, since no competitor can entertain the least hope of success against him. But, my lord, we live in an age when most men how little soever their merit may be, seem to think themselves fit for whatever they can get, and often meet with some people, who by their recommendations of them appear to entertain the same opinion of them, and it is for this reason that I am so pressing with your lordship not to lose any time."

Such recommendations had, however, their effect: the dreaded possibility of a miscarriage of justice was averted, and Bradley became the third Astronomer Royal, though he did not resign his professorship at Oxford. Halley, Bradley, and Bliss, who were Astronomers Royal in succession, all held the appointment along with one of the Savilian professorships at Oxford; but since the death of Bliss in 1761, the appointment has always gone to a Cambridge man.

Instruments very defective. When Bradley went to Greenwich, in June 1742, he was at first unable to do much from the wretched state in which he found the instruments. Halley was not a good observer: his heart was not in the work, and he had not taken the trouble to set the instruments right when they went wrong. The counterpoises of that instrument which ought to have been the best in the world at the time rubbed against the roof so that the telescope could scarcely be moved in some positions: and some of the screws were broken. There was no proper means of illuminating the cross-wires, and so on. With care and patience Bradley set all this right, and began observations. He had the good fortune to secure the help of his nephew, John Bradley, as assistant, and the companionship seems to have been as happy as that previous one of James Bradley and his uncle Pound. John Bradley was able to carry on the observations when his uncle was absent in Oxford, and the work the two got through together in the first year is (in the words of Bradley's biographer Rigaud) "scarcely to be credited." The transit observations occupy 177 folio pages, and no less than 255 observations were taken on one night. And at the same time, it must be remembered, Bradley was still carrying on his nutation observations at Wansted, still lecturing at Oxford, and not content with all this, began a course of experiments on the length of the seconds' pendulum. Truly a giant for hard work!

But, in spite of his care in setting them right, the instruments in the Observatory were found to be hopelessly defective. The history of the instruments at the Royal Observatory is a curious one. When Flamsteed was appointed the first Astronomer Royal he was given the magnificent salary of £100 a year, and no instruments to observe with. He purchased some instruments with his own money, and at his death they were claimed by his executors. Hence Halley, the second Astronomer Royal, found the Observatory totally unprovided in this respect. He managed to persuade the nation to furnish the funds for an equipment; but Halley, though a man of great ability in other ways, did not know a good instrument from a bad one; so that Bradley's first few years at the Observatory were wasted owing to the imperfection of the equipment. When this was fully realised he asked for funds to buy new **New instruments.** instruments, and such was the confidence felt in him that he got what he asked for without much difficulty. More than £1000, a large sum for those days, was spent under his direction, the principal purchases being two quadrants for observation of the position of the stars, one to the north and the other to the south. With these quadrants, which represented the perfection of such apparatus at that time, Bradley made that long and wonderful series of observations which is the starting-point of our knowledge of the movements of the stars. The instruments are still in the Royal Observatory, the more important of the two in its original position as Bradley mounted it and left it.

Work at Greenwich. It seems needless to mention his work as Astronomer Royal, but I will give quite briefly a summary of what he accomplished, and then recall a particular incident, which shows how far ahead of his generation his genius for observation placed him. The summary may be given as follows. We owe to Bradley—

1. A better knowledge of the movements of Jupiter's satellites.

2. The orbits of several comets calculated directly from his own observations, when such work was new and difficult.

3. Experiments on the length of the pendulum.

4. The foundation of our knowledge of the refraction of our atmosphere.

5. Considerable improvements in the tables of the moon, and the promotion of the method for finding the longitude by lunar distances.

6. The proper equipment of our national Observatory with instruments, and the use of these to form the basis of our present knowledge of the positions and motions of the stars.

Many men would consider any one of these six achievements by itself a sufficient title to fame. Bradley accomplished them all in addition to his great discoveries of aberration and nutation.

Might have found variation of And with a little more opportunity he might have added another great discovery which has shed lustre on the work of the last decade. We said earlier in this chapter that the axis of the earth may move in one or two ways. Either it may point to a different star, remaining fixed

relatively to the earth, as in the nutation which Bradley discovered; or it may actually change its position in the earth. This second kind of movement was believed until twenty years ago not to exist appreciably; but the work of Küstner and Chandler led to the discovery that it did exist, and its complexities have been unravelled, and will be considered in the sixth chapter. Now a century and a half ago Bradley was on the track of this "variation of latitude." His careful observations actually showed the motion of the pole, as Mr. Chandler has recently demonstrated; and, moreover, Bradley himself noticed that there was something unexplained. Once again there was a *residuum* after (first) aberration and (next) nutation had been extracted from the observations; and with longer life he might have explained this residuum, and added a third great discovery to the previous two. Or another coming after him might have found it; but after the giant came men who could not tread in his footsteps, and the world waited 150 years before the discrepancy was explained.

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The attitude of our leading universities towards science and scientific men is of sufficient importance to justify another glance at the relations between Bradley and Oxford. We have seen that Oxford's treatment of Bradley was not altogether satisfactory. She left him to learn astronomy as he best could, and he owes no teaching to her. She made him Professor of Astronomy, but gave him no observatory and a beggarly income which he had to supplement by giving lectures on a different subject. But when he had disregarded these discouragements and made a name for himself, Oxford took her share in recognition. He was created D.D. by diploma in 1742; and when his discovery of nutation was announced in 1748, and produced distinctions and honours of all kinds from over the world, we are told that "amidst all these distinctions, wide as the range of modern science, and permanent as its history, there was one which probably came nearer his heart, and was still more gratifying to his feeling than all. Lowth (afterwards Bishop of London), a popular man, an elegant scholar, and possessed of considerable eloquence, had in 1751 to make his last speech in the Sheldonian Theatre at Oxford as Professor of Poetry. In recording the benefits for which the University was indebted to its benefactors, he mentioned the names of those whom Sir Henry Savile's foundation had established there: 'What men of learning! what mathematicians! we owe to Savile, Briggs, Wallis, Halley; to Savile we owe Greaves, Ward, Wren, Gregory, Keill, and one whom I will not name, for posterity will ever have his name on its lips.' Bradley was himself present; there was no one in the crowded assembly on whom the allusion was lost, or who did not feel the truth and justice of it; all eyes were turned to him, while the walls rung with shouts of heartfelt affection and admiration; it was like the triumph of Themistocles at the Olympic games."

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The study of "residual phenomena."

These words of Rigaud indicate the fame deservedly acquired by an earnest and simple-minded devotion to science: but can we learn anything from the study of Bradley's work to guide us in further research? The chief lessons would seem to be that if we make a series of careful observations, we shall probably find some deviation from expectation: that we must follow up this clue until we have found some explanation which fits the facts, not being discouraged if we cannot hit upon the explanation at once, since Bradley himself was puzzled for several years: that after finding one *vera causa*, and allowing for the effect of it, the observations may show traces of another which must again be patiently hunted, even though we spend nineteen years in the chase: and that again we may have to leave the complete rectification of the observations to posterity. But though we may admit the general helpfulness of these directions, and that this patient dealing with residual phenomena seems to be a method capable of frequent application, we cannot deduce any universal principle of procedure from them: witness the difficulty of dealing with meteorological observations, for instance. It is not always possible to find any orderly arrangement of the residuals which will give us a clue to start with. When such an arrangement is manifested, we must certainly follow up the clue; it would almost seem that no expense should be prohibitive, since it is impossible to foresee the importance of the result.

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CHAPTER IV

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ACCIDENTAL DISCOVERIES

In reviewing various types of astronomical discovery I have laid some stress upon the fact that they are, generally speaking, far from being accidental in character. A new planet does not "swim into our ken," at any rate not usually, but is found only after diligent search, and then only by an investigator of acute vision, or other special qualifications. But this is, of course, not always the case. Some discoveries are made by the merest accident, as we have had occasion to remark incidentally in the case of the minor planets; and for the sake of completeness it is desirable to include among our types at least one case of such accidental discovery. As, however, the selection is a little invidious, I may perhaps be pardoned for taking the instance from my own experience, which happens to include a case where one of those remarkable objects called "new stars" walked deliberately into a net spread for totally different objects. There is the further reason for choosing this instance: that it will afford me

The Oxford

new star found during work on Astrographic Chart.

the opportunity of saying something about the special research in which we were actually engaged, the work of mapping out the heavens by photography, or, as it has been called, the Astrographic Chart—a great scheme of international co-operation by which it is hoped to leave as a legacy for future centuries a record of the state of the sky in our age. Such a record cannot be complete; for however faint the stars included, we know that there are fainter stars which might have been included had we given longer exposures to the plates. Nor can it be in other ways final or perfect; however large the scale, for instance, on which the map is made, we can imagine the scale doubled or increased many-fold. But the map will be a great advance on anything that has hitherto been made, and some account of it will therefore no doubt be of interest.

Origin of the chart.

We may perhaps begin with a brief historical account of the enterprise. Photographs of the stars were taken many years ago, but only by a few enthusiasts, and with no serious hope of competing with eye observations of the sky. The old wet-plate photography was, in fact, somewhat unsuited to astronomical purposes; to photograph faint objects a long exposure is necessary, and the wet plate may dry up before the exposure is concluded—nay, even before it is commenced, if the observer has to wait for passing clouds—and therefore it may be said that the successful application of photography to astronomy dates from the time when the dry plate was invented; when it became possible to expose a plate in the telescope for hours, or by accumulation even for days. The dry plate remains sensitive for a long period, and if it is desired to extend an exposure beyond the limits of one night, it is quite easy to close up the telescope and return to the operations again on the next fine night; and so on, if not perhaps indefinitely, at any rate so long as to transcend the limits of human patience up to the present.

VII.—GREAT COMET OF NOV. 7TH, 1882
(*From a photograph taken at the Royal Observatory, Cape of Good Hope.*)

Comet of 1882.

But to consider our particular project. We may assign, perhaps, the date 1882 as that in which it first began to take shape. In that year there was a magnificent bright comet, the last really large comet which we, in the Northern Hemisphere, have had the good fortune to see. Some of us, of course, were not born at that time, and perhaps others who were alive may nevertheless not have seen that comet; for it kept somewhat uncomfortably early morning hours, and I can well remember myself feeling rather more resentment than gratitude to the man who waked me up about four o'clock to see it. Many observations were of course made of this interesting visitor, and what specially concerns us is that at the Cape of Good Hope some enterprising photographers tried to photograph it. They tried in the first instance with ordinary cameras, and soon found—what any astronomer could have told them —that the movement of the earth, causing an apparent movement of the comet and the stars in the opposite direction, frustrated their efforts. The difficulties of obtaining pictures of moving objects are familiar to all photographers. A "snap-shot" might have met the difficulty, but the comet was scarcely bright enough to affect the plate with a short exposure. Ultimately Dr. David Gill, the astronomer at the Cape Observatory, invited one of the photographers to strap his camera to one of the telescopes at the Observatory, a telescope which could be carried round by clockwork in the usual way, so as to counteract the earth's motion, and in effect to keep the comet steadily in view, as though it were at rest. As a consequence, some very beautiful and successful pictures of the comet were obtained, and on them a large number of stars were also shown. They were, as I have said, not by any means the first pictures of stars obtained by photography, but they represented

Stars shown on the pictures.

in facility and in success so great an advance upon what had been formerly obtained that they attracted considerable attention. They were sent to Europe and stimulated various workers to further experiments.

The late Dr. Common in England, an amateur astronomer, began that magnificent pioneer work in astronomical photography which soon brought him the Gold Medal of the Royal Astronomical Society for his photographs of nebulæ. But the most important result for our purpose was produced in France. There had been started many years before by the French astronomer Chacornac a series of star maps round the Zodiac similar in intention to the Berlin maps which figured in the history of the discovery of Neptune. Chacornac died before his enterprise was very far advanced, and the work was taken up by two brothers, Paul and Prosper Henry, who followed Chacornac in adopting for the work the laborious method of eye observation of each individual star. They proceeded patiently with the work on these lines; but when they came to the region where the Zodiac is crossed by the Milky Way, and the number of stars in a given area increases enormously, they found the labour so great as to be practically prohibitive, and were in doubt how to deal with the difficulty. It was at this critical moment that these comet photographs, showing the stars so beautifully, suggested the alternative of mapping the stars photographically. They immediately set to work with a trial lens, and obtained such encouraging results that they proceeded themselves to make a larger lens of the same type; this again was satisfactory, and the idea naturally arose of extending to the whole heavens the scheme which they had hitherto intended only for the Zodiac, a mere belt of the heavens. But this rendered the enterprise too large for a single observatory. It became necessary to obtain the co-operation of other observatories, and with this end in view an International Conference was summoned to meet in Paris in 1887 to consider the whole project. There were delegates from, if not all nations, at any rate a considerable number:—

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The brothers Henry begin work.

Conference of 1887.

France	20	U.S. America	3	Spain	1
British Empire	8	Austria	2	Switzerland	1
Germany	6	Sweden	2	Portugal	1
Russia	3	Denmark	2	Brazil	1
Holland	3	Belgium	1	Argentine Republic	1
		Italy	1		

The Conference had a number of very important questions to discuss, for knowledge of the photographic method and its possibilities was at that time in its infancy. There was, for instance, the question whether all the instruments need be of the same pattern, and if so what that pattern should be. The first of these questions was settled in the affirmative, as we might expect; in the interests of uniformity it was desirable that the maps should be as nearly similar as possible. The second question was not so easy; there were at least three different types of instruments which might be used. First of all, there was the photographic lens, such as is familiar to all who have used an ordinary camera, consisting of two lenses with a space between; though since each of these lenses is itself made up of two, we should more correctly say four lenses in all. It was with a lens of this kind that the comet pictures had been taken at the Cape of Good Hope, and it might seem the safest plan to adopt what had been shown to be capable of such good work. But there was this difficulty; the pictures of the comet were on a very small scale, and taken with a small lens; a much larger lens was required for the scheme now under contemplation, and when there are four separate lenses to be made, each with two surfaces to polish, and each requiring a perfectly sound clear piece of glass, it will be obvious that the difficulties of making a large compound lens of this kind are much greater, and the expense much more serious than in the case of a single lens, or even a pair. It was this question of expense which had led the brothers Henry to experiment with a different kind of instrument, in which only one pair of lenses was used instead of two. Their instrument was, in fact, very similar to the ordinary telescope, excepting that they were bound to make their lenses somewhat different in shape in order to bring to focus the rays of light suitable for photography, which are not the same as those suitable for eye observation with the ordinary telescope. Dr. Common, again, had used a third kind of instrument, mainly with the view of reducing the necessary expense still further, or, perhaps, of increasing the size of the instrument for the same expense. His telescope had no lens at all, but a curved mirror instead, the mirror being made of glass silvered on the face (not on the back as in the ordinary looking-glass). In this case there is only one surface to polish instead of four, as in the Henrys' telescope, or eight, as in the case of the photographic doublet; and, moreover, since the rays of light are reflected from the surface of the glass, and do not pass *through* it at all, the internal structure of the glass is not so strictly important as in the other cases. Hence the reflector is a very cheap instrument, and it is, moreover, quite free from some difficulties attached to the other instruments. No correction for rays of light of different colours is required, since all rays of whatever colour come to the same focus automatically. These advantages of the reflector were so considerable as to almost outweigh one well-known disadvantage, which is, however, not very easily expressed in words. The reflector might be described as an instrument with a temper; sometimes it gives excellent results, but at others *something* seems to be wrong, though the worried observer does not exactly know what. Long experience and patience are requisite to humour the instrument and get the best results from it, and it was felt that this uncertainty was sufficient to disqualify the instrument for the serious piece of routine work contemplated in mapping the heavens. Accordingly the

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Choice of instrument.

Expense of "doublet."

Advantages of reflector.

Refractor

handier and more amiable instrument with which the brothers Henry had done such good work was selected as the pattern to be adopted.

It is curious that at the Conference of 1887 nothing at all was said about the type of instrument first mentioned (the "doublet lens"), although a letter was written in its favour by Professor Pickering of Harvard College Observatory. Since that time we have learnt much of its advantages, and it is probable that if the Conference were to meet now they might arrive at a different decision; but at that time they were, to put it briefly, somewhat afraid of an instrument which seemed to promise, if anything, too well, especially in one respect. With the reflector and the refractor it had been found that the field of good images was strictly limited. The Henrys' telescope would not photograph an area of the sky greater in extent than 2° in diameter at any one time, and the reflector was more limited still; within this area the images of the stars were good, and it had been found that their places were accurately represented. Now the "doublet" seemed to be able to show much larger areas than this with accuracy, but no one had been able to test the accuracy to see whether it was sufficient for astronomical purposes; and although no such feeling was openly expressed or is on record, I think there is no doubt that a feeling existed of general mistrust of an instrument which seemed to offer such specious promises. Whatever the reason, its claims were passed over in silence at the Conference, and the safer line (as it was then thought) of adopting as the type the Henrys' instrument, was taken.

This was perhaps the most important question settled at the Conference, and the answers to many of the others naturally followed. The size of the plates, for instance, was settled automatically. The question down to what degree of faintness should stars be included, resolved itself into the equivalent question, What should be the length of time during which the plates were exposed? Then, again, the question, What observatories should take part in the work? became simply this: What observatories could afford to acquire the instruments of this new pattern and get other funds for carrying out the work specified? It was ultimately found that eighteen observatories were able to obtain the apparatus and funds, though unfortunately three of the eighteen have since found it impossible to proceed. The following is the original list, and in brackets are added the names of three other observatories which in 1900 undertook to fill the places of the defaulters.

OBSERVATORIES CO-OPERATING FOR THE ASTROGRAPHIC CHART.

Observatory.	Zones of Declination.	Number of Plates.
Greenwich	+90° to +65°	1149
Rome	+64° " +55°	1140
Catania	+54° " +47°	1008
Helsingfors	+46° " +40°	1008
Potsdam	+39° " +32°	1232
Oxford	+31° " +25°	1180
Paris	+24° " +18°	1260
Bordeaux	+17° " +11°	1260
Toulouse	+10° " + 5°	1080
Algiers	+ 4° " - 2°	1260
San Fernando	- 3° " - 9°	1260
Tacubaya	-10° " -16°	1260
Santiago (Monte Video)	-17° " -23°	1260
La Plata (Cordoba)	-24° " -31°	1360
Rio (Perth, Australia)	-32° " -40°	1376
Cape of Good Hope	-41° " -51°	1512
Sydney	-52° " -64°	1400
Melbourne	-65° " -90°	1149

In the list is also shown the total number of plates that were to be taken by each observatory. When once the size of the plates had been settled, it was a straightforward matter to divide up the sky into the proper number of regions necessary to cover it completely, not only without gaps between the plates, but with actually a small overlap of contiguous plates. And more than this, it was decided that the whole sky should be completely covered *twice over*. It was conceivable that a question might arise whether an apparent star image on a plate was, on the one hand, a dust speck, or, on the other hand, a planet, or perhaps a variable or new star. By taking two different plates at slightly different times, questions of this kind could be settled; and to make the check more independent it was decided that the plates should not be exactly repeated on the same portion of sky, but that in the second series the centre of a plate should occupy the point assigned to the corner of a plate in the first series.

Then there came the important question of time of exposure, which involved a long debate between those who desired the most modest programme possible consistent with efficiency, and those enthusiasts who were anxious to strain the programme to the utmost limits attainable. Ultimately it was resolved to take two series of plates; one series of long exposure which was set in the first instance at 10 minutes, then became 15, then 30, then 40, and has by some enterprising observers been extended to 1½ hours; the other a series of

short exposures which have been generally fixed at 6 minutes. Thus instead of covering the sky twice, it was decided to cover it in all four times, and the number of plates assigned to each observatory in the above list must be regarded as doubled by this new decision. And further still, on the series of short-exposure plates it was decided to add to the exposure of six minutes another one of three minutes, having slightly shifted the telescope between the two so that they should not be superimposed; and later still, a third exposure of twenty seconds was added to these. It would take too long to explain here the reasons for these details, but it will be clear that the general result of the discussion was to extend the original programme considerably, and render the work even more laborious than it had appeared at the outset.

Measurement of plates.

When all these plates have been taken, the work is by no means finished; indeed, it is only just commencing. There remains the task of measuring accurately on each of the short-exposure plates the positions of the stars which it represents, numbering on the average some 300 or 400; so that for instance at Oxford the total number of stars measured on the twelve hundred plates is nearly half a million. These are not all separate stars; for the sky is represented twice over, and there is also the slight overlap of contiguous plates; but the number of actual separate stars measured at this one observatory is not far short of a quarter of a million, and it has taken nearly ten years to make the measurements, with the help of three or four measurers trained for the purpose. To render the measures easy, a

The réseau.

network or réseau of cross lines is photographed on each plate by artificial light after it has been exposed to the stars, so that on development these cross lines and the stars both appear. We can see at a glance the approximate position of a star by counting the number of the space from left to right and from top to bottom in which it occurs; and we can also estimate the fraction of a space in addition to the whole number; but it is necessary for astronomical purposes to estimate this fraction with the greatest exactness. The whole numbers are already given with great exactness by the careful ruling of the cross lines, which can be spaced with extraordinary perfection. To measure the fraction, we place the

The microscope.

plate under a microscope in the eye-piece of which there is a finally divided cross scale; the centre of the cross is placed over a star image, and then it is noted where the lines of the réseau cut the cross scale. In this way the position of the image of a star is read off with accuracy, and after a little practice with considerable rapidity. It has been found at Oxford that under favourable conditions the places of nearly 200 stars per hour can be recorded in this way by a single measurer, if he has some one to write down for him the numbers he calls out. This is only one form of measuring apparatus; there are others in which, instead of a scale in the eye-piece, micrometer screws are used to measure the fractions; but the general principle in all these instruments is much the same, and the rate of work is not very different; while to the minor advantages and disadvantages of the different types there

Reversal of plates.

seems no need here to refer. One particular point, however, is worth noting. After a plate has been measured, it is turned round completely, so that left is now right, and top is now bottom, and the measurements are repeated. This repetition has the advantage first of all of checking any mistakes. When a long piece of measuring or numerical work of any kind is undertaken there are invariably moments when the attention seems to wander, and some small error is the result. But there are also certain errors of a systematic character similar to those denoted by the term "personal equation," which has found its way into other walks

Personal equation.

of life. In the operation of placing a cross exactly over the image of a star, different observers would show slight differences of habit; one might place it a little more to the right than another. But when the plate is turned round the effect of this habit on the measure is exactly reversed, and hence if we take the mean of the two measures any personal habit of this kind is eliminated. It has been found by experience that such personal habits are much smaller for measures of this kind than for those to which we have long been accustomed in observations made by eye on the stars themselves. The troubles from "personal equation" have been much diminished by the photographic method, and certain peculiarities of the former method have been clearly exhibited by the comparison. For instance, it has gradually become clear that with eye observations personal equation is not a constant quantity, but is different for stars of different brightness. When observing the transit of a bright star the observer apparently records an instant definitely earlier than in recording the transit of a faint one; and this peculiarity seems to be common to the large majority of observers, which is perhaps the reason why it was not noticed earlier. But when positions of the stars determined in this way are compared with their positions measured on the photographic plates, the peculiarity is made clearly manifest. For example, at Oxford, our first business after making measurements is to compare them with visual observations on a limited number of the brighter stars made at Cambridge about twenty years ago. (About 14,000 stars were observed at Cambridge, and we are dealing with ten times that number.) The comparison shows that the Cambridge observations are affected with the following systematic errors:—

If stars of magnitude 10 are observed correctly,
then   "   "   9   "   0.10 secs. too early
     "   "   8   "   0.16   "
     "   "   7   "   0.19   "
     "   "   6   "   0.21   "
     "   "   5   "   0.23   "

This may serve as an illustration of various incidental results which are already flowing from the enormous and laborious piece of work which, as far as the University Observatory at Oxford is concerned, we have just completed, though some of the other colleagues are not so far advanced. But the main results will not appear just yet. The work must be repeated, and the positions of the stars just obtained must be compared with those which they will be found to occupy at some future date, in order to see what kind of changes are going on in the heavens. Whether this future date shall be one hundred years hence, or fifty, or ten, or whether we should begin immediately to repeat what has been done, is a matter not yet decided, and one which requires some little consideration.

Main object of the work.

I have said perhaps enough to give you a general idea of the work on which we have been engaged at Oxford for the last ten years. Ten years ago it seemed to stretch out in front of us rather hopelessly; the pace we were able to make seemed so slow in view of the distance to be covered. We felt rather like the schoolboy who has just returned to school and sees the next holidays as a very remote prospect, and we solaced ourselves much in the same way as he does, by making a diagram representing the total number of plates to be dealt with and crossing off each one as it was finished, just as he sometimes crosses off the days still remaining between him and the prospective holidays. It was pleasant to watch the growth of the number of crosses on this diagram, and by the end of the year 1902 we had the satisfaction of seeing very little blank space remaining. Now, up to this point it had not much mattered whether any particular plate was secured in any particular year, or in a subsequent year, so long as there were always sufficient plates to keep us occupied in measuring them. But it then became a matter of importance to secure each plate at the proper time of year; for the sun, as we know, travels round the Zodiac among the stars, obliterating by his radiance a large section of the sky for a period of some months, and in this way a particular region of the heavens is apt to "run into daylight," as the observatory phrase goes, and ceases to be available for photography during several months, until the sun is again far enough away to allow of the particular region being seen at night.

The concluding year.

Roughly speaking then, if a plate which should be taken in February is not secured in this month owing to bad weather, the proper time for taking it will not occur again until the following February; and when there was a fair prospect of finishing our work in 1903, it became important to secure each plate at the proper time in that year. Hence we were making special efforts to utilise to the full any fine night that Providence sent in our way, and on such occasions it is clearly an economy, if not exactly to "make hay while the sun shines," at any rate to take plates vigorously while the sun is *not* shining and the night is fine; leaving the development of them until the daytime. There is, of course, the risk that the whole night's work may in this way be lost owing to some fault in the plates, which might have been detected if some of them were immediately developed. Perhaps in the early days of our work it would have been reckless or foolish to neglect this little precaution; but we had for years been accustomed to rely upon the excellence of the plates without finding our trust betrayed; and the sensitiveness of the plates had increased rather than diminished as time went on. Hence it will be readily understood that when one fatal morning we developed a series of some thirty plates, and found that owing to some unexplained lack of sensitiveness they were all unsuitable for our purpose, it came as a most unwelcome and startling surprise. It was, of course, necessary to make certain that there was no oversight, that the developer was not at fault, and that the weather had not been treacherous. All such possibilities were carefully considered before communication with the makers of the plates, but it ultimately became clear that there had been some unfortunate failure in sensitiveness, and that it would be necessary to repeat the work with opportunities restricted by the intervening lapse of time. However, disappointments from this or similar causes are not unknown in astronomical work; and we set about this repetition with as little loss of time and cheerfulness as was possible. Under the circumstances, however, it seemed desirable to examine carefully whether anything could be saved from the wreck—whether any of the plates could be admitted as *just* coming up to the minimum requirements. And I devoted a morning to this inquiry. In the course of it I came across one plate which certainly seemed worth an inclusion among our series from the point of view of the number of stars shown upon it. It seemed quite rich in stars, perhaps even a little richer than might have been expected. On inquiry I was told that this was not one of the originally condemned plates, but one which had been taken since the failure in sensitiveness of the plates had been detected; was from a new and specially sensitive batch with which the courteous makers had supplied us; but though there were certainly a sufficient number of stars upon the plate, owing to some unexplained cause the telescope had been erroneously pointed, and the region taken did not correspond to the region required. To investigate the cause of the discrepancy I thereupon took down from our store of plates the other one of the same region which had been rejected for insufficiency of stars, and on comparing the two it was at once evident that there was a strange object on the plate taken later of the two, a bright star or other heavenly body, which was not on the former plate. I have explained that by repeating the exposure more than once, it is easily possible to recognise whether a mark upon the plate is really a celestial body or is an accidental blot or dust speck, and there was no doubt that this was the image of some strange celestial body. It might, of course, be a new planet, or even an old one which had wandered into the region; but a few measures soon showed that it was not in movement. The measures consisted in comparing the separation of the three exposures with the separation of the corresponding exposures of obvious stars, for the exposures were not, of course, simultaneous, and if the body were a planet and had moved

A disappointment.

A curious plate.

A strange object.

in the interval between them, this would be made manifest on measuring the separations. No such movements could be detected; and the possibilities were thus restricted to two. So far as we knew the object was a star, but might be either a star of the class known as *variable* or of that known as *new*. In the former case it would become bright and faint at more or less regular intervals, and might possibly have been already catalogued; for the number of these bodies already known amounts to some hundreds. Search being made in the catalogues, no entry of it was found, though it still might be one of this class which had hitherto escaped detection. Or it might be a "new star," one of those curious bodies which blaze up quite suddenly to brightness and then die away gradually until they become practically invisible. The most famous perhaps of these is the star which appeared in 1572, and was so carefully observed by Tycho Brahé; but such apparitions are rare, and altogether we have not records as yet of a score altogether; so that in this latter case the discovery would be of much greater interest than in the former. In either event it was desirable to inform other observers as soon as possible of the existence of a strange body; already some time had elapsed since the plate had been taken, March 16th, for the examination of which I have spoken was not made until March 24th. Accordingly, a telegram was at once despatched to the Central Office at Kiel, which undertakes to distribute such information all over the world, and a few post-cards were sent to observers close at hand who might be able to observe the star the same night. Certain observations with the spectroscope soon made it clear that the object was really a "new star."

This, therefore, is the discovery which we made at Oxford: as you will see, in an entirely accidental manner, during the course of a piece of work in which it was certainly never contemplated. Its purely accidental nature is sufficiently illustrated by the fact that if the plates originally supplied by the makers had been of the proper quality, the plate which led to the discovery would never have been taken. If the plates exposed in February had been satisfactory, we should have been content, and should not have repeated the exposure on March 16th. Again I can testify personally how purely accidental it was that the examination was made on March 24th to see whether anything could be saved, as I have said, from the wreck. The idea came casually into my mind as I was walking through the room and saw the neat pile of rejected plates; and one may fairly call it an accidental impulse. This new star is not, however, the first of such objects to have been discovered "accidentally"; many of the others were found just as much by chance, though a notable exception must be made of those discovered at the Harvard Observatory, which are the result of a deliberate search for such bodies by the careful examination of photographic plates. Mrs. Fleming, who spends her life in such work, has had the good fortune to detect no less than six of these wonderful objects as the reward of her laborious scrutiny; and she is the *only* person who has thus found new stars by photography until this accidental discovery at Oxford. The following is a complete list of new stars discovered to date:—

List of New Stars.

Ref. No.	Constellation.	Year.	Discoverer.
1	Cassiopeia	1572	Tycho Brahé.
2	Cygnus	1600	Janson.
3	Ophiuchus	1604	Kepler.
4	Vulpecula	1670	Anthelm.
5	Ophiuchus	1848	Hind.
6	Scorpio	1860	Auwers.
7	Corona Borealis	1866	Birmingham.
8	Cygnus	1876	Schmidt.
9	Andromeda	1885	Hartwig.
10	Perseus	1887	Fleming.
11	Auriga	1891	Anderson.
12	Norma	1893	Fleming.
13	Carina	1895	Fleming.
14	Centaurus	1895	Fleming.
15	Sagittarius	1898	Fleming.
16	Aquila	1899	Fleming.
17	Perseus	1901	Anderson.
18	Gemini	1903	At Oxford.
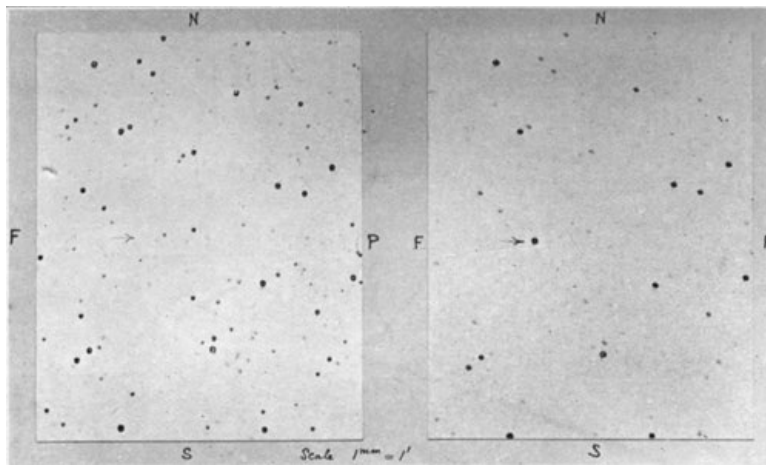
MARCH 1, 1903   MARCH 14, 1903

VIII.—The Oxford New Star.

A PAIR OF PHOTOGRAPHS TAKEN AT THE HARVARD COLLEGE OBSERVATORY BEFORE AND AFTER ITS APPEARANCE

(*The arrow indicates the place of the new star. It will be seen that the left-hand picture though it shews fainter stars than the other, has not a trace of the new star.*)

Dr. Anderson.

Generally these stars have been noted by eye observation, as in the case of the two found by Dr. Anderson of Edinburgh. In these cases also we may say that deliberate search was rewarded; for Dr. Anderson is probably the most assiduous "watcher of the skies" living, though he seldom uses a telescope; sometimes he uses an opera-glass, but usually the naked eye. He describes himself as an "Astrophil" rather than as an astronomer. "I love the stars," he says; "and whenever they are shining, I must be looking." And so on every fine night he stands or sits at his open study window gazing at the heavens. I believe he was just about to leave them for his bed, near 3 A.M. on the night of February 21, 1901, when, throwing a last **Nova Persei.** glance upward, he suddenly saw a brilliant star in the constellation Perseus. His first feeling was actually one of disappointment, for he felt sure that this object must have been there for some time past without his knowing of it, and he grudged the time lost when he might have been regarding it. More in a spirit of complaint than of inquiry, he made his way to the Royal Observatory at Edinburgh next day to hear what they had to say about it, though he found it difficult to approach the subject. He first talked about the weather, and the crops, and similar topics of general interest; and only after some time dared he venture a casual reference to the "new portent in the heavens." Seeing his interlocutor look somewhat blank, he ventured a little farther, and made a direct reference to the new star in Perseus; and then found to his astonishment, as also to his great delight, that he was the first to bring news of it. The news was soon communicated to other observers; all the telescopes of the world were soon trained upon it; and this wonderful "new star of the new century" has taught us more of the nature of these extraordinary bodies than all we knew before.

Records previous to discovery.

Perhaps I may add a few remarks on one or two features of these bodies. Firstly, let us note that Professor Pickering of Harvard is now able to make a most important contribution to the *former* history of these objects—that is to say, their history preceding their actual detection. We remember that, after Uranus had been discovered, it was found that several observers had long before recorded its place unknowingly; and similarly Professor Pickering and his staff have usually photographed other new objects unknowingly. There are on the shelves at Harvard vast stores of photographs, so many that they are unable to examine them when they have been taken; but once any object of interest has been discovered, it is easy to turn over the store and examine the particular plates which may possibly show it at an earlier date. In this way it was found that Dr. Anderson's new star had been visible only for a few days before its discovery, there being no trace of it on earlier plates. Similarly, in the case of the new star found at Oxford, plates taken on March 1st and 6th, fifteen days and ten days respectively before the discovery-plate of March 16th, showed the star. But, in this particular instance, greater interest attaches to two still earlier plates taken elsewhere, and with exposures much longer than any available at Harvard. One had been obtained at Heidelberg by Dr. Max Wolf, and another at the Yerkes Observatory of Chicago University, by Mr. Parkhurst; and on both there appeared to be a faint star of about the fourteenth or fifteenth magnitude, in the place subsequently occupied by the Nova; and the question **Was Nova Geminorum previously shining faintly?** naturally arose, Was this the object which ultimately blazed up and became the new star? To settle this point, it was necessary to measure its position, with reference to neighbouring stars, with extreme precision; and here it was unfortunate that the photographs did not help us as much as they might, for they were scarcely capable of being measured with the requisite precision. The point was an important one, because if the identity of the Nova with this faint star could be established, it would be the second instance of the kind; but so far as they went, measurements of the photographs were distinctly against the identity. Such was the conclusion of Mr. Parkhurst from his photograph alone; and it was confirmed by measures made at Oxford on copies of both plates, which were kindly sent there for the purpose. The conclusion seemed to be that there was a faint star *very near*, but *not at*, the

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place of the new star; and it was therefore probable that, although this faint star was temporarily invisible from the brightness of the adjacent Nova, as the latter became fainter (in the way with which we have become familiar in the case of new stars), it might be possible to see the two stars alongside each other. This critical observation was ultimately made by the sharp eyes of Professor Barnard, aided by the giant telescope of the Yerkes Observatory; and it seems clear therefore that the object which blazed up to become the Nova of 1903 could not have previously been so bright as a faint star of the fourteenth magnitude. Although this is merely a negative conclusion, it is an important one in the history of these bodies.

The second point to which I will draw your attention is from the history of the other Nova just mentioned—Dr. Anderson's New Star of 1901. In this instance it is not the history previous to discovery, but what followed many months after discovery, that was of engrossing interest; and again Yerkes Observatory, with its magnificent equipment, played an important part in the drama. When, with its giant reflecting telescope, photographs were taken of the region of Nova Persei after it had become comparatively faint, it was found that there was an extraordinarily faint nebulosity surrounding the star. Repeating the photographs at intervals, it was found that this nebulosity was rapidly changing in shape. "Rapidly" is, of course, a relative term, and a casual inspection of two of the photographs might not convey any impression of rapidity; it is only when we come to consider the enormous distance at which the movements, or apparent movements, of the nebulæ must be taking place that it becomes clear how rapid the changes must be. It was not possible to determine this distance with any exactness, but limits to it could be set, and it seemed probable that the velocity of the movement was comparable with that of light. The conclusion suggested itself that the velocity might actually be identical with that of light, in which case what we saw was not the movement of actual matter, but merely that of illumination, travelling from point to point of matter already existing.

SEPT. 20, 1901

The suspicion negatived.

Nebula round Nova Persei.

Its changes.

Due to travelling illumination.

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NOV. 13, 1901

IX—Nebulosity round Nova Persei
(*From photographs taken at the Yerkes Observatory by G. W. Ritchey.*)

An analogy from the familiar case of sound may make clearer what is meant. If a loud noise is made in a large hall, we hear echoes from the walls. The sound travels with a velocity of about 1100 feet per second, reaches the walls, is reflected back from them, and returns to us with the same velocity. From the interval occupied in going and returning we could calculate the distance of the walls. The velocity of light is so enormous compared with that of sound that we are usually quite unable to observe any similar phenomenon in the case of light. If we strike a match in the largest hall, all parts of it are illuminated so immediately that we cannot possibly realise that there was really an interval between the striking of the match, the travelling of the light to the walls, and its return to our eyes. The scale of our terrestrial phenomenon is far too small to render this interval perceptible. But those who accept the theory above mentioned regarding the appearances round Nova Persei (although there are some who discredit it) believe that we have in this case an illustration of just this phenomenon of light echoes, on a scale large enough to be easily visible. They think that, surrounding the central star which blazed up so brightly in February 1901, there was a vast dark nebula, of which we had no previous knowledge, because it was not shining with any light of its own. When the star blazed up, the illumination travelled from point to point of this dark nebula and lighted it up; but the size of the nebula was so vast that, although the light was travelling with the enormous velocity of 200,000 miles per second, it was not until months afterwards that it reached different portions of this nebula; and we accordingly got news of the existence of this nebula some months after the news reached us of the central **When did it** conflagration, whatever it was. Remark that all we can say is that the news of the nebula **all happen?** reached us *some months later* than that of the outburst. The actual date when either of the actual things happened, we have as yet no means of knowing; it may have been hundreds or even thousands of years ago that the conflagration actually occurred of which we got news in February 1901, the light having taken all that time to reach us from that distant part of space; and the light reflected from the nebula was following it on its way to us all these years at that same interval of a few months.

An Now, let me refer before leaving this point to the chief objection which has been urged **objection.** against this theory. It has been maintained that the illumination would necessarily appear to travel outwards from the centre with an approach to uniformity, whereas the observed rate of travel is not uniform, and has been even towards the centre instead of away from it; which would seem as though portions of the nebula more distant from the centre were lighted up sooner than those closer to it. By a simple illustration from our solar system, we shall see that these curious anomalies may easily be explained. Let us consider for simplicity two planets only, say the Earth and Saturn. We know that Saturn travels round the sun in an orbit which is ten times larger than the orbit of the earth. Suppose now that the sun were suddenly to be extinguished; light takes about eight minutes to travel from the sun to the earth, and consequently we should not get news of the extinction for some eight minutes; the sun would appear to us to still go on shining for eight minutes after he had really been extinguished. Saturn being about ten times as far away from the sun, the news would take eighty minutes to reach Saturn; and from the earth we should see Saturn shining more[3] than eighty minutes after the sun had been extinguished, although we ourselves should have lost the sun's light after eight minutes. I think we already begin to see possibilities of curious anomalies; but they can be made clearer than this. Instead of imagining an observer

on the earth, let us suppose him removed to a great distance away in the plane of the two orbits; and let us suppose that the sun is now lighted up again as suddenly as the new star blazed up in February 1901. Then such an observer would first see this blaze in the centre; eight minutes afterwards the illumination would reach the earth, a little speck of light near the sun would be illuminated, just as we saw a portion of the dark nebula round Nova Persei illuminated; eighty minutes later another speck, namely, Saturn, would begin to shine. But now, would Saturn necessarily appear to the distant observer to be farther away from the sun than the earth was? Looking at the diagram, we can see that if Saturn were at S_1 then it would present this natural appearance of being farther away from the sun than the earth; but it might be at S_2 or S_3, in which case it would seem to be nearer the sun, and the illumination would seem to travel inwards towards the central body instead of outwards. Without considering other cases in detail, it will be tolerably clear that almost any anomalous appearance might be explained by choosing a suitable arrangement of the nebulous matter which we suppose lighted up by the explosion of Nova Persei. Another objection urged against the theory I have sketched is that the light reflected from such a nebula would be so feeble that it would not affect our photographic plates. This depends upon various assumptions which we have no time to notice here; but I think we may say that there is certainly room for the acceptance of the theory.

FIG. 6.

Did the nebula cause the outburst?

Now, if this dark nebula was previously existing in this way all round the star which blazed up, the question naturally arises whether the nebula had anything to do with the conflagration. Was there previously a star, either so cold or so distant as not to be shining with appreciable light, which, travelling through space, encountered this vast nebula, and by the friction of the encounter was suddenly rendered so luminous as to outshine a star of the first magnitude? The case of meteoric stones striking our own atmosphere seems to suggest such a possibility. These little stones are previously quite cold and invisible, and are travelling in some way through space, many of them probably circling round our sun. If they happen in their journey to encounter our earth, even the extremely tenuous atmosphere, so thin that it will scarcely bend the rays of light appreciably, even this is sufficient by its friction to raise the stones to a white heat, so that they blaze up into the falling stars with which we are familiar. This analogy is suggested, but we must be cautious in accepting it; for we know so very little of the nature of nebulæ such as that of which we have been speaking. But in any case, a totally new series of phenomena have been laid open to our study by those wonderful photographs taken at the Yerkes Observatory and the Lick Observatory in the few years which the present century has as yet run.

Importance of new stars

One thing is quite certain: we must lose no opportunity of studying such stars as may appear, and no diligence spent in discovering them at the earliest possible moment is thrown away. We have only known up to the present, as already stated, less than a score of them, and of these many have told us but little; partly because they were only discovered too late (after they had become faint), and partly because the earlier ones could not be observed with the spectroscope, which had not then been invented. It seems clear that in the future we must not allow accident to play so large a part in the discovery of these objects; more must be done in the way of deliberate search. Although we know beforehand that this will involve a vast amount of apparently useless labour, that months and years must be spent in comparing photographic plates, or portions of the sky itself, with one another without detecting anything remarkable, it will not be the first time that years have been cheerfully spent in such searches without result. We need only recall Hencke's fifteen years of fruitless search, before finding a minor planet, to realise this fact.

Superposition of plates.

One thing of importance may be done; we may improve our methods of making the search, so as to economise labour, and several successful attempts have already been made in this direction. The simplest plan is to superpose two photographs taken at different dates, so that the stars on one lie very close to those on the other; then if an image is seen to be

unpaired we *may* have found a new star, though of course the object may be merely a planet or a variable. The superposition of the plates may be either actual or virtual. A beautiful instrument has been devised on the principle of the stereoscope for examining two plates placed side by side, one with each eye. We know that in this way two photographs of the same object from different points of view will appear to coalesce, and at the same time to give an appearance of solidity to the object or landscape, portions of which will seem to stand out in front of the background. Applying this principle to two photographs of stars, what happens is this: if the stars have all remained in the same positions exactly, the two pictures will seem to us to coalesce, and the images all to lie on a flat background; but if in the interval between the exposures of the two plates one of the stars has appreciably moved or disappeared, it will seem, when looked at with this instrument, to stand out in front of this background, and is accordingly detected with comparatively little trouble. This new instrument, to which the name Stereo-comparator has been given, promises to be of immense value in dredging the sky for strange bodies in the future. I am glad to say that a generous friend has kindly presented the University Observatory at Oxford with one of these beautiful instruments, which have been constructed by Messrs. Zeiss of Jena after the skilful designs of Dr. Pulfrich. Whether we shall be able to repeat by deliberate search the success which mere accident threw in our way remains to be seen.

The stereo-comparator.

CHAPTER V

SCHWABE AND THE SUN-SPOT PERIOD

Discoveries contrary to expectation.

In preceding chapters we have reviewed discoveries, some of which have been made as a result of a deliberate search, and others accidentally in the course of work directed to a totally different end; but so far we have not considered a case in which the discoverer entered upon an enterprise from which he was positively dissuaded.

In the next chapter we shall come across a very striking instance of this type; but even in the discovery that there was a periodicity in the solar spots, with which I propose to deal now, Herr Schwabe began his work in the face of deterrent opinions from eminent men. His definite announcement was first made in 1843, though he had himself been convinced some years earlier. In 1857 the Royal Astronomical Society awarded him their gold medal for the discovery; and in the address delivered on the occasion the President commenced by drawing attention to this very fact, that astronomers who had expressed any opinions on the subject had been uniformly and decidedly against the likelihood of there being anything profitable in the study of the solar spots. I will quote the exact words of the President, Mr. Manuel Johnson, then Radcliffe Observer at Oxford.

Nothing expected from spots.

"It was in 1826 that Heinrich Schwabe, a gentleman resident in Dessau, entered upon those researches which are now to engage our attention. I am not aware of the motive that induced him—whether any particular views had suggested themselves to his own mind—or whether it was a general desire of investigating, more thoroughly than his predecessors had done, the laws of a remarkable phenomenon, which it had long been the fashion to neglect. He could hardly have anticipated the kind of result at which he has arrived; at the same time we cannot imagine a course of proceeding better calculated for its detection, even if his mind had been prepared for it, than that which he has pursued from the very commencement of his career. Assuredly if he entertained such an idea, it was not borrowed from the authorities of the last century, to whom the solar spots were objects of more attention than they have been of late years.

"'Nulla constanti temporum lege apparent aut evanescunt,' says Keill in 1739.—*Introduct. ad Physic. Astronom.*, p. 253.

"'Il est manifest par ce que nous venons de rapporter qu'il n'y a point de règle certaine de leur formation, ni de leur nombre et de leur figure,' says Cassini II. in 1740.—*Elém d'Astron.*, vol. i. p. 82.

"'Il semble qu'elles ne suivent aucune loi dans leur apparitions,' says Le Monnier in 1746.—*Instit. Astron.*, p. 83.

"'Solar spots observe no regularity in their shape, magnitude, number, or in the time of their appearance or continuance,' says Long in 1764.—*Astron.*, vol. ii. p. 472.

"'Les apparitions des tâches du soleil n'ont rien de regulier,' says Lalande in 1771.—*Astron.*, vol. iii. § 3131, 2nd edit.

"And Delambre's opinion may be inferred from a well-known passage in the

third volume of his 'Astronomy' (p. 20), published in 1814, where treating of the solar spots he says, 'Il est vrai qu'elles sont plus curieuses que vraiment utiles.'"[4]

It will thus be evident that Herr Schwabe had the courage to enter upon a line of investigation which others had practically condemned as likely to lead nowhere, and that his discovery was quite contrary to expectation. It is a lesson to us that not even the most unlikely line of work is to be despised; for the outcome of Schwabe's work was the first step in the whole series of discoveries which have gradually built up the modern science of Solar Physics, which occupies so deservedly large a part of the energies of, for instance, the great observatory attached to the University of Chicago.

Schwabe's announcement. It has been our practice to recall the actual words in which the discoverer himself stated his discovery, and I will give the original modest announcement of Schwabe, though for convenience of those who do not read German I will attempt a rough translation. He had communicated year by year the results of his daily counting of the solar spots to the *Astronomische Nachrichten*, and after he had given ten years' results in this way he collected them together, but he made no remark on the curious sequence which they undoubtedly showed at that time. Waiting patiently six years for further material, in 1843 he ventured to make his definite announcement as follows:—"From my earlier observations, which I have communicated annually to this journal, there was manifest already a certain periodicity of sun-spots; and the probability of this being really the case is confirmed by this year's results. Although I gave in volume 15 the total numbers of groups for the years 1826-1837, nevertheless I will repeat here a complete series of all my observations of sun-spots, giving not only the number of groups, but also the number of days of observation, and further the days when the sun was free from spots. The number of groups alone will not in itself give sufficient accuracy for determination of a period, since I have convinced myself that when there are a large number of sun-spots the number will be reckoned somewhat too small, and when few sun-spots, the number somewhat too large; in the first case several groups are often counted together in one, and in the second it is easy to divide a group made up of two component parts into two separate groups. This must be my excuse for repeating the early catalogue, as follows:— [Pg 159]

Year.	Number of Groups.	Days free from Spots.	Days of Observation.
1826	118	22	277
1827	161	2	273
1828	225	0	282
1829	199	0	244
1830	190	1	217
1831	149	3	239
1832	84	49	270
1833	33	139	267
1834	51	120	273
1835	173	18	244
1836	272	0	200
1837	33	0	168
1838	282	0	202
1839	162	0	205
1840	152	3	263
1841	102	15	283
1842	68	64	307
1843	34	149	324
(1844)	(52)	(111)	(320)

"If we now compare together the number of groups, and the days free from spots, we find that the sun-spots have a period of about ten years, and that for about five years they are so numerous that during this period few days, if any, are free from spots. The sequel must show whether this period is constant, whether the minimum activity of the sun in producing spots lasts for one or two years, and whether this activity increases more quickly than it decreases." [Pg 160]
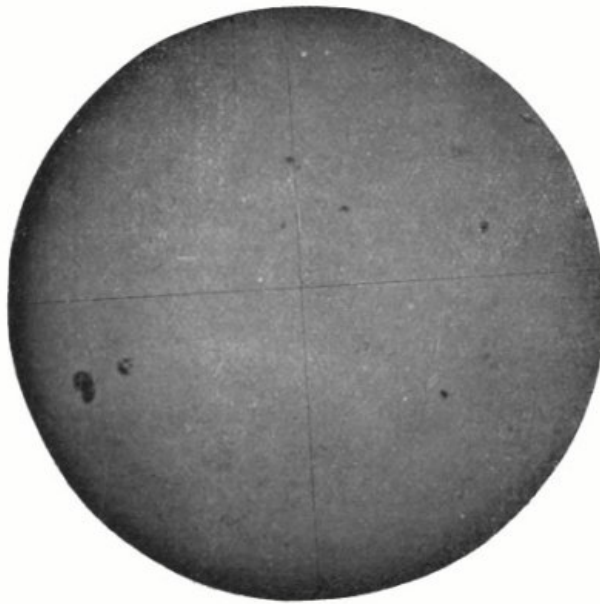
FEB. 18, 1894.
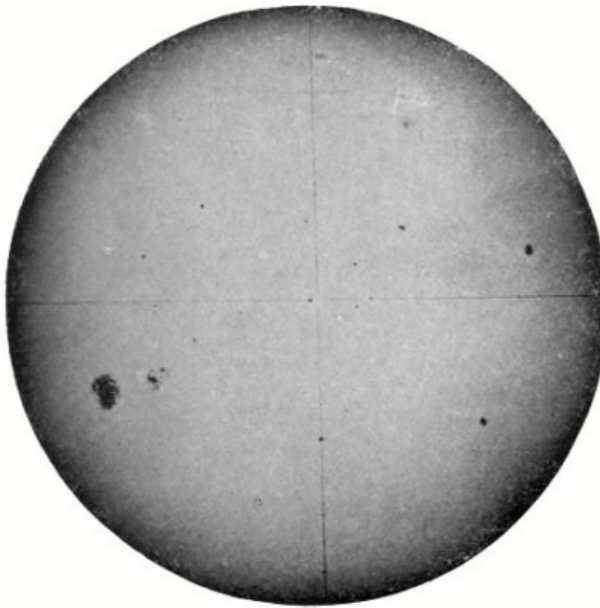
FEB. 19, 1894.

X.—Photographs of the Sun taken at the Royal Observatory, Greenwich, Shewing Sunspots.

Attracted little attention, — This brief announcement is all that the discoverer says upon the subject; and it is perhaps not remarkable that it attracted very little attention, especially when we remember that it related to a matter which the astronomical world had agreed to put aside as unprofitable and not worth attention. Next year, in giving his usual paper on the spots for 1844 he recurs to the subject in the following sentence: "The periodicity of spots of about ten years which was indicated in my summary published last year, is confirmed by this year's observations." I have added in brackets to the table above reproduced the numbers for 1844 subsequently given, and it will be seen how nearly they might have been predicted.

Still the subject attracted little attention. Turning over the leaves of the journal at random, I came across the annual report of the Astronomer Royal of England, printed at length. But in it there is no reference to this discovery, which opened up a line of work now strongly represented in the annual programme of the Royal Observatory at Greenwich. Mr. Johnson remarks that the only person who had taken it up was Julius Schmidt, who then resided near until eight years later. — Hamburg. But Schwabe went on patiently accumulating facts; and in 1851 the great Von Humboldt in the third volume of his *Cosmos*, drew attention to the discovery, which was [Pg 161] accordingly for the first time brought into general notice. It will be seen that there are not many facts of general interest relating to the actual discovery beyond the courage with which the work was commenced in a totally unpromising direction, and the scant attention it received after being made for us. We may admit that interest centres chiefly in the tremendous consequences which flowed from it. We now recognise that many other Other phenomena sympathetic — phenomena are bound up with this waxing and waning of the solar spots. We might be prepared for a sympathy in phenomena obviously connected with the sun itself; but it was an unexpected and startling discovery that magnetic phenomena on the earth had also a

sympathetic relation with the changes in sun-spots, and it is perhaps not surprising that when once this connection of solar and terrestrial phenomena was realised, various attempts have been made to extend it into regions where we cannot as yet allow that it has earned a legitimate right of entry. We have heard of the weather and of Indian famines occurring in cycles identical with the sun-spot cycle; and it is obvious how tremendously important it would be for us if this were found to be actually the case. For we might in this way predict years of possible famine and guard against them; or if we could even partially foretell the kind of weather likely to occur some years hence, we might take agricultural measures accordingly. The importance of the connection, if only it could be established, is no doubt the reason which has misled investigators into laying undue stress on evidence which will not bear close scrutiny. For the present we must say decidedly that no case has been made out for paying serious attention to the influence of sun-spots on weather. Nevertheless, putting all this aside, there is quite enough of first-rate importance in the sequel to Schwabe's discovery.

Let us review the facts in order. Most of us, though we may not have had the advantage of seeing an actual sun-spot through a telescope, have seen drawings or photographs of spots. There is a famous drawing made by James Nasmyth (of steam-hammer fame), in July, 1864, which is of particular interest, because at that time Nasmyth was convinced—and he convinced many others with him—that the solar surface was made up of a miscellaneous heap of solid bodies in shape like willow leaves, or grains of rice, thrown together almost at random, and the drawing was made by him to illustrate this idea. Comparing a modern photograph with it, we see that there is something to be said for Nasmyth's view, which attracted much attention at the time and occasioned a somewhat heated controversy. But since the invention of the spectroscope it has become quite obsolete; it probably does not correspond in any way to the real facts. But instead of looking at pictures which have been enlarged to show the detailed structure in and near a spot, we will look at a series of pictures of the whole sun taken on successive days at Greenwich in which the spots are necessarily much smaller, but which show the behaviour of the spots from day to day. (See Plates X. and XI.) From the date at the foot of each it will be seen that they gradually cross the disc of the sun (a fact first discovered by Galileo in 1610), showing that the sun rotates on an axis once in about every twenty-five days. There are many interesting facts connected with this rotation; especially that the sun does not rotate as a solid body, the parts near the (Sun's) Equator flowing quicker than those nearer the Poles; but for the present we cannot stop to dwell upon them. What interests us particularly is the history, not from day to day, but from year to year, as Schwabe has already given it for a series of years.
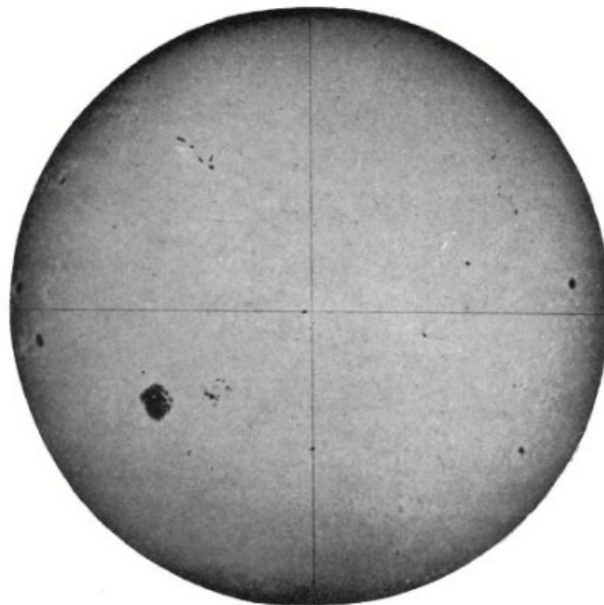
FEB. 20, 1894.
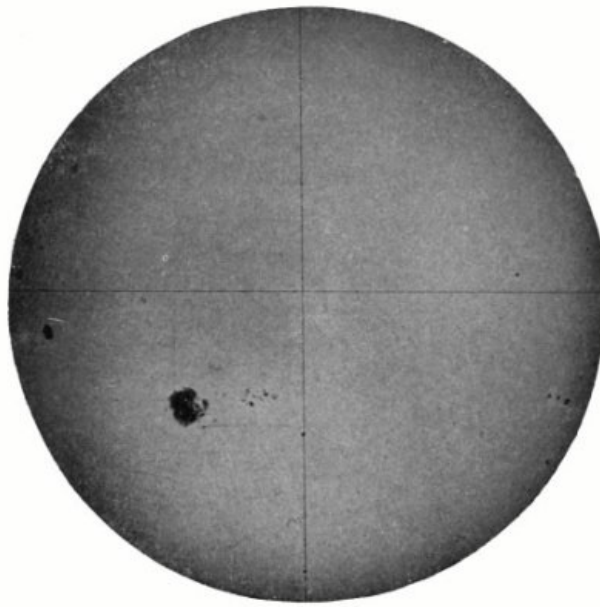
FEB. 21, 1894.

XI.—Photographs of the Sun taken at the Royal Observatory, Greenwich, shewing Sunspots

When it became generally established that this periodicity existed, Rudolf Wolf of Zurich collected the facts about sun-spots from the earliest possible date, and represented this history by a series of numbers which are still called Wolf's Sun-Spot Numbers. You will see from the diagram the obvious rise and fall for eleven years,—not ten years, as Schwabe thought, but just a little over eleven years. The facts are, however, given more completely by the work done at the Royal Observatory at Greenwich. It is part of the regular daily work of that Observatory to photograph the sun at least twice. Many days are of course cloudy or wet, so that photographs cannot be obtained; but there are available photographs similarly taken in India or in Mauritius, where the weather is more favourable, and from these the gaps are so well filled up that very few days, if any, during the whole year are left without some photograph of the sun's surface. On these photographs the positions and the areas of the spots are carefully measured under a microscope, and the results when submitted to certain necessary calculations are published year by year. It is clearly a more accurate estimate of the spottedness of the sun to take the total *area* of all the spots rather than their mere *number*, for in the latter case a large spot and a small one count equally. Hence the Greenwich records will perhaps give us an even better idea of the periodicity than Wolf's numbers. Now, at the same observatory magnetic observations are also made continuously. If a magnet be suspended freely we are accustomed to say that it will point to the North Pole; but this is only very roughly true. In the first place, it is probably well known to you that there is a considerable deviation from due north owing to the fact that the magnetic North Pole is not the same as the geographical North Pole; but this for the present need not concern us. What does concern us is, that if the needle is hung up and left long enough to come to rest, it does not then remain steadily at rest, but executes slow and small oscillations backwards and forwards, up and down, throughout the day; repeating nearly the same oscillations on the following day, but at the same time gradually changing its behaviour so as to oscillate differently in summer and winter. These changes are very small, and would pass unnoticed by the naked eye; but when carefully watched through a telescope, or better still, when photographed by some apparatus which will at the same time magnify them, they can be rendered easily visible. When the history of these changes is traced it is seen at once that there is a manifest connection with the cycle of sun-spot changes; for instance, if we measure the range of swing backwards and forwards during the day and take the average for all the days in the year, and then compare this with the average number of sun-spots, we shall see that the averages rise and fall together. Similarly we may take the up and down swing, find the average amount of it throughout the year, and again we shall find that this corresponds very closely with the average number of sun-spots.

NUMBER OF SUNSPOTS (Wolf) Compared with
DAILY RANGE of MAGNETIC DECLINATION
& DAILY RANGE of MAGNETIC HORZL. FORCE
(as observed at Greenwich.)
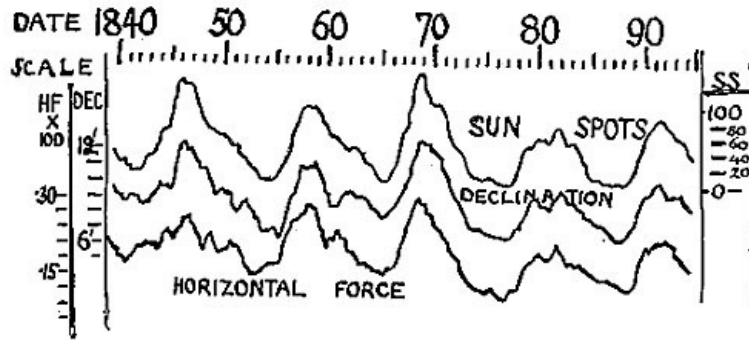
PLATE XII.

But perhaps the most striking way to exhibit the sympathy is to combine different variations of the needle into one picture. And first we must remark that there is another important variation of the earth's magnetic action which we have not yet considered. We have mentioned the swing of the needle to and fro, and the swing up and down, and these correspond to changes in the *direction* of the force of attraction on the needle. But there may be also changes in *intensity* of this action; the pull may be a little stronger or a little weaker than before, and these variations are not represented by any actual movement of the needle, though they can be measured by proper experiments. We can, however, imagine them represented by a movement of the end of the needle if we suppose it made of elastic material, so that it would lengthen when the force was greater and contract slightly when the force was less. If a pencil were attached to the end of such an elastic needle so as to make a mark on a sheet of paper, and if for a moment we exclude the up and down movements, the pencil would describe during the day a curve on the paper, as the end of the needle swung backwards and forwards with the change in direction, and moved across the direction of swing with the change in intensity. Now when curves of this kind are described for a day in each month of the year, there is a striking difference between the forms of them. During the summer months they are, generally speaking, comparatively large and open, and during the winter months they are small and close. This change in form is seen by a glance at Plate XIII., which gives the curves throughout the whole of one year. Let us now, instead of forming a curve of this kind for each month, form a single average curve for the whole year; and let us further do this for a series of years. (Plate XIV.) We see that the curves change from year to year in a manner very similar to that in which they change from month to month in any particular year, and the law of change is such that in years when there are many sun-spots we get a large open curve similar to those found in the summer, while for years when there are few sun-spots we get small close curves very like those in the winter. Hence we have two definite conclusions suggested: firstly, that the changes of force are sympathetic with the changes in the sun-spots; and secondly, that times of maximum sun-spots correspond to summer, and times of minimum to winter. And here I must admit that this is about as far as we have got at present in the investigation of this relationship. *Why* the needle behaves in this way we have as yet only the very vaguest ideas; suggestions of different kinds have certainly been put forward, but none of them as yet can be said to have much evidence in favour of its being the true one. For our present purpose, however, it is sufficient to note that there is this very real connection, and that consequently Schwabe's sun-spot period may have a very real importance with regard to changes in our earth itself.

Daily curves.

Difference between summer and winter, and between sun-spot maximum and minimum.

Cause unknown.

[Pg 166]

[Pg 167]

GREENWICH MAGNETIC CURVES
1859-60

P<small>LATE</small> XIII.

GREENWICH MAGNETIC CURVES FOR APRIL 1841-1860
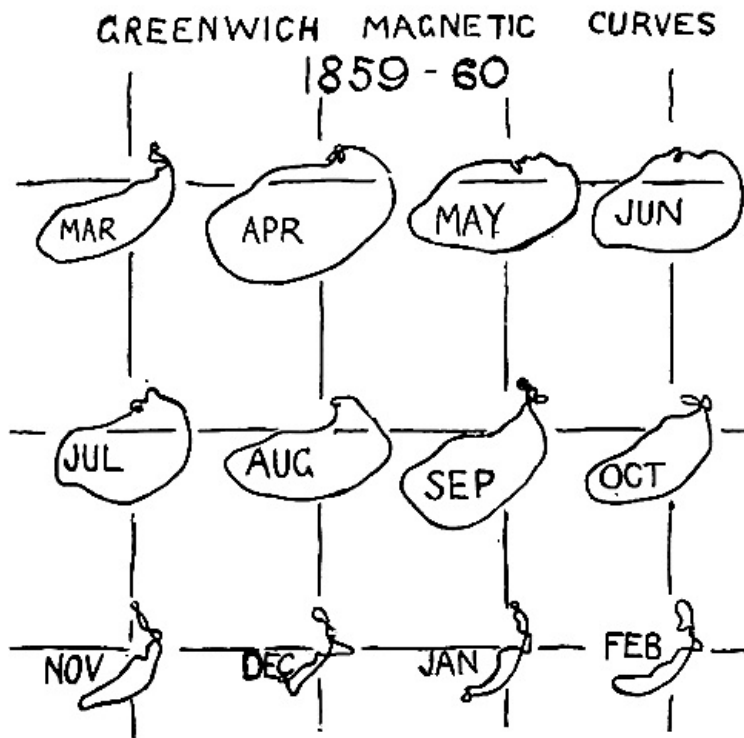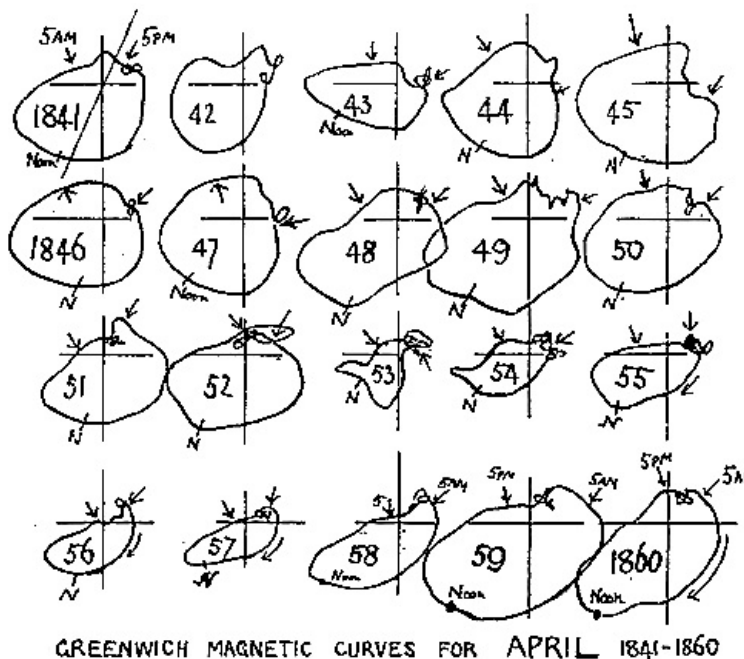
P<small>LATE</small> XIV.

But I may perhaps repeat the word of caution already uttered against extending without sufficient evidence this notion of the influence of sun-spots to other phenomena, such as weather. A simple illustration will perhaps serve better than a long argument to show both the way in which mistakes have been made and the way in which they can be seen to be mistakes. There is at the Royal Observatory at Greenwich an instrument for noting the direction of the wind, the essential part being an ordinary wind-vane, the movements of which are automatically recorded on a sheet of paper. As the wind shifts from north to east the pencil moves in one direction, and when it shifts back again towards the north the pencil moves in the reverse way. But sometimes the wind shifts continuously from north to east, south, west, and back to north again, the vane making a complete revolution; and this causes the pencil to move continuously in one direction, until when the vane has come to north again, the pencil is far away from the convenient place of record; on such occasions it is often necessary to replace it by hand. Then again, the vane may turn in the opposite direction, sending the pencil inconveniently to the other side of the record. During the year it is easy to count the number of complete changes of wind in either direction, and subtracting one number from the other, we get the excess of complete revolutions of the vane in one direction over that in the other. Now if these rather arbitrary numbers are set down year by year, or plotted in the shape of a diagram, we get a curve which may be

Illustration of spurious connection.

compared with the sun-spot curve, and during a period of no less than sixteen years—from 1858 to 1874—there was a remarkable similarity between the two diagrams. From this evidence *alone* it might fairly be inferred that the sun-spots had some curious effect upon the weather at Greenwich, traceable in this extraordinary way in the changes of the wind. But the particular way in which these changes are recorded is so arbitrary that we should naturally feel surprise if there was a real connection between the two phenomena; and fortunately there were other records preceding these years and following them which enabled us to test the connection further, and it was found, as we might naturally expect, that it was not confirmed. On looking at diagrams (Plate XV.) for the periods before and after, no similarity can be traced between the sun-spot curve and the wind-vane curve, and we infer that the similarity during the period first mentioned was entirely accidental. This shows that we must be cautious in accepting, from a limited amount of evidence, a connection between two phenomena as real and established; for it may be purely fortuitous. We may particularly remark that it is desirable to have repetitions through several complete periods instead of one alone. It is possible to reduce to mathematical laws the rules for caution in this matter; and much useful work has already been done in this direction by Professor Schuster of Manchester and others, though as yet too little attention has been paid to their rules by investigators naturally eager to discover some hitherto unthought-of connection between phenomena.

[Pg 169]

Faculæ follow spots

With this example of the need for caution, we may return to phenomena of which we can certainly say that they vary sympathetically with the sun-spots. Roughly speaking, the whole history of the sun seems to be bound up with them. Besides these dark patches which we call spots (which, by the way, are not really dark but only less bright than the surrounding part of the disc), there are patches brighter than the rest which have been called faculæ. With ordinary telescopes, either visual or photographic, these can generally only be detected near the edge of the sun's disc; but even with this limitation it can easily be established that the faculæ vary in number and size from year to year much in the same way as the spots, and this conclusion is amply confirmed by the beautiful method of observing the faculæ with the new instrument designed by Professor Hale of the Yerkes Observatory. With this instrument, called a spectroheliograph, it is possible to photograph the faculæ in all parts of the sun's disc, and thus to obtain a much more complete history of them, and there is no

[Pg 170]

and the chromosphere.

doubt whatever of their variation sympathetically with the spots. Nor is there any doubt about similar variations in other parts of the sun which we cannot see *at all* with ordinary telescopes, except on the occasions when the sun is totally eclipsed. Roughly speaking, these outlying portions of the sun consist of two kinds, the chromosphere and the corona, the former looking like an irregular close coating of the ordinary sun, and the latter like a pearly halo of light extending to many diameters of the sun's disc, but not with any very regular form.

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Sᴍᴏᴏᴛʜᴇᴅ Sᴜɴsᴘᴏᴛ Cᴜʀᴠᴇ (Wᴏʟꜰ) ᴄᴏᴍᴘᴀʀᴇᴅ ᴡɪᴛʜ ᴛʜᴇ ɴᴜᴍʙᴇʀ ᴏꜰ ᴛᴜʀɴs ᴍᴀᴅᴇ ɪɴ ᴇᴀᴄʜ ʏᴇᴀʀ ʙʏ ᴛʜᴇ Osʟᴇʀ Aɴᴇᴍᴏᴍᴇᴛᴇʀ Vᴀɴᴇ ᴏꜰ ᴛʜᴇ Rᴏʏᴀʟ Oʙsᴇʀᴠᴀᴛᴏʀʏ, Gʀᴇᴇɴᴡɪᴄʜ (ᴛʜᴇ ᴇxᴄᴇss ᴏꜰ ᴛʜᴇ ᴅɪʀᴇᴄᴛ ᴛᴜʀɴs (D) ᴏᴠᴇʀ ᴛʜᴇ ʀᴇᴛʀᴏɢʀᴀᴅᴇ ᴛᴜʀɴs (R) ᴏʀ *ᴠɪᴄᴇ ᴠᴇʀsᴀ*.)

Larger Image
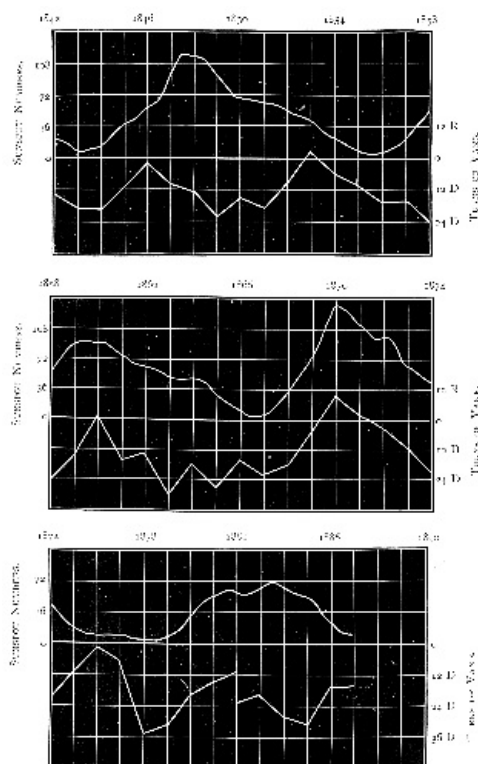
The upper curve is in each case the Sunspot Curve, the lower the Vane Curve. The break in 1882 in the Vane Curve is due to the omission of evidently accidental turns from that date.

Plate XV.

The chromosphere, from which shoot out the prominences or "red flames," can now be observed without an eclipse if we employ the beautiful instrument above-mentioned, the spectroheliograph; and Professor Hale has succeeded in photographing spots, faculæ, and prominences all on the same plate. But although many have made the attempt (and Professor Hale, perhaps, a more determined attempt than any man living), no one has yet succeeded in obtaining any picture or evidence of the existence of the corona excepting on the occasion of a total solar eclipse.

Eclipses of sun.

Now these occasions are very rare. There are two or three eclipses of the sun every year, but they are generally of the kind known as partial; when the moon does indeed come between us and the sun to some extent, but only cuts off a portion of his light—a clean-cut black disc is seen to encroach more or less on the surface of the sun. Most of us have had an opportunity of seeing a partial eclipse, probably more than once; but few have seen a total eclipse. For this the moon must come with great exactness centrally between us and the sun; and the spot where this condition is fulfilled completely only covers a few hundred miles of the earth's surface at one moment. As the earth turns round, and as the moon revolves in its orbit, this patch from which the sun is totally eclipsed travels over the earth's surface, marking out a track some thousands of miles in length possibly, but still not more than 200 miles wide; and in order to see the sun totally eclipsed even on the rare occasions when it is possible at all (for, as already remarked, in the majority of cases the eclipse is only partial), we must occupy some station in this narrow belt or track, which often tantalisingly passes over either the ocean or some regions not easily accessible to civilised man. Moreover, if we travel to such favoured spots the whole time during which the sun is totally eclipsed cannot exceed a few minutes, and hence observations are made under rather hurried and trying conditions. In these modern days of photography it is easier to take advantage of these precious moments than it used to be when there was only the eye and memory of an excited observer to rely upon. It is perhaps not surprising that some of the evidence collected on these earlier occasions was conflicting; but nowadays the observers, generally speaking, direct their energies in the first place to mounting accurately in position photographic apparatus of different kinds, each item of it specially designed to settle some particular problem in the most feasible way; secondly, to rehearsing very carefully the exact programme of exposures necessary during the critical few minutes; and finally, to securing these photographs with as few mistakes as possible when the precious moments actually arrive. Even then the whole of their efforts are quite likely to be rendered unavailing by a passing cloud; and bitter is the disappointment when, after travelling thousands of miles, and spending months in preparation, the whole enterprise ends in nothing owing to some caprice of the weather.

Total eclipses rare.

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Hence it will easily be imagined that our knowledge of the corona, the part of the sun which we can still only study on occasions of a total solar eclipse, advances but slowly. During the last twenty years there has been altogether scarcely half-an-hour available for this research, though it may fairly be said that the very best possible use has been made of that half-hour. And, what is of importance for our immediate purpose, it has gradually been established by comparing the photographs of one eclipse with those of another, that the corona itself undergoes distinct changes in form in the same period which governs the changes of sun-spots. When there are many sun-spots the corona spreads out in all directions from the edge of the sun's disc; when there are few sun-spots the corona extends very much further in the direction of the sun's equator, so that at sun-spot minimum there is an appearance of two huge wings. Although the evidence is necessarily collected in a scrappy manner, by this time there is sufficient to remove this relationship out of the region of mere suspicion, and to give it a well-established place in our knowledge of the sun's surroundings.

Corona follows spots.

Now the corona of the sun may be compared to some rare animal which we only see by paying a visit to some distant land, and may consider ourselves even then fortunate to get a glimpse of; and it might be thought that the habits of such an animal are not likely to be of any great importance in our everyday life. But so far from this being the case in regard to the corona, it is more than possible that the knowledge of its changes may be of vital interest to us. I have already said that, as yet, we have no satisfactory account of the reason why changes in sun-spots seem to influence changes in our magnets on the earth; but one of the theories put forward in explanation, and one by no means the least plausible, is that this influence may come, not from the sun-spots themselves, but from some other solar phenomenon which varies in sympathy with them; and in particular that it may come from the corona. These wings which reach out at sun-spot minimum can be seen to extend a considerable distance, and there is no reason to suppose that they actually cease at the point where they become too faint for us to detect them further; they may extend quite as far as the earth itself and even beyond; and they may be of such a nature as to influence our magnets. As the earth revolves round the sun it may sometime plunge into them, to emerge later and pass above or below them; as again the wings spread themselves at sun-spot minimum and seem to shrink at maximum, so our magnets may respond by sympathetic though very small vibrations. Hence it is quite possible that the corona is directly

Corona may influence magnets.

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influencing the magnetic changes on the earth.

Possible importance of corona.

But it may be urged that these changes are so slight as to be merely of scientific interest. That may be true to-day, but who will be bold enough to say that it will be true to-morrow? If we are thinking of practical utility alone, we may remember that two great forces of Nature which we have chained into the service of man, steam and electricity, put forth originally the most feeble manifestations, which might readily have been despised as valueless; but by careful attention to proper conditions results of overwhelming practical importance have been obtained from these forces, which might have been, and for many centuries were, neglected as too trivial to be worth attention. Recently the world has been startled by the discovery of new elements, such as radium, whose very existence was only detected by a triumph of scientific acuteness in investigation, and yet which promise to yield influences on our lives which may overwhelm in importance all that has gone before. And similarly it may be that these magnetic changes, when properly interpreted or developed, may become of an importance in the future out of all proportion to the attention which they have hitherto attracted. Hence, although perhaps sufficient has already been established to show the immense consequences which flow from Schwabe's remarkable discovery of the periodicity in solar spots, we may be as yet only on the threshold of its real value.

From what little causes great events spring! How little can Schwabe have realised, when he began to point his modest little telescope at the sun, and to count the number of spots—the despised spots which he had been assured were of no interest and exhibited no laws, and were generally unprofitable—that he was taking the first step in the invention of the great science of Solar Physics!—a science which is, I am glad to say, occupying at the present moment so much of the attention, not only of the great Yerkes Observatory, but of many other observatories scattered over the globe.

CHAPTER VI

THE VARIATION OF LATITUDE

If we should desire to classify discoveries in order of merit, we must undoubtedly give a high place to those which are made under direct discouragements. In the last chapter we saw that Schwabe entered upon his work under conditions of this kind, it being the opinion of experienced astronomers who had looked at the facts that there was nothing of interest to be got by watching sun-spots. In the present chapter I propose to deal with a discovery made in the very teeth of the unanimous opinion of the astronomical world by an American amateur, Mr. S. C. Chandler of Cambridge (Massachusetts). It is my purpose to allow him to himself explain the steps of this discovery by giving extracts from the magnificent series of papers which he contributed to the *Astronomical Journal* on the subject in the years 1891-94, but it may help in the understanding of these extracts if I give a brief summary of the facts. And I will first explain what is meant by the "Variation of Latitude."

Latitude.

We are all familiar with the existence of a certain star in the heavens called the Pole Star, and we know that at any particular place it is seen constantly in the north at a definite height above the horizon, which is the latitude of the place. When watched carefully with a telescope it is found to be not absolutely stationary, but to describe a small circle in the heavens day by day, or rather night by night. These simple facts are bound up with the phenomenon of the earth's rotation in this way: the axis about which it is rotating points to the centre of that little circle, and any change in the position of the axis can therefore be determined by observing these motions of the Pole Star. Such changes may be of two kinds: firstly, we might find that the size of the circle increased or diminished, and this would mean that the earth's axis was pointing farther away from the Pole Star or nearer to it—pointing, that is to say, in a different direction in space. This actually happens (as has been known for some thousands of years) owing to the phenomenon called "precession"; the circle described by our Pole Star is at present getting a little smaller, but it will ultimately increase in size, and after thousands of years become so large that the Pole Star will entirely lose its character as a steady guide to the North.

Precession.

Change of latitude.

Secondly (and this is what more immediately concerns us), the centre of the circle may alter its position and be no longer at the same height above the horizon of any given place. This would mean that the earth's axis was shifting *in the earth itself*—that the North Pole which our explorers go to seek is not remaining in the same place. That it does not change appreciably in position we know from familiar experience; our climates, for instance, would suffer considerably if there were any large changes. But astronomers are concerned with minute changes which would not have any appreciable effect on climate, and the question has long been before them whether, putting aside large movements, there were any minute variations in position of the North Pole. Twenty years ago the answer to this question would have been given decidedly in the negative; it was considered as certain that the North Pole did not move at all within the limits of our most refined astronomical observations. Accepted

Twenty years ago disbelieved.

theory seemed to indicate that any movements must in any case recur after a period of ten months, and careful discussion of the observations showed that there was no oscillation in such a period. Now we know that the theory itself was wrong, or rather was founded upon a mistaken assumption; and that the facts when properly examined show clearly a distinct movement of the North Pole, not a very large one, for all its movements take place within the area occupied by a moderate-sized room, but still a movement easily measurable by astronomical observations, and Mr. Chandler was the first to point out the law of these movements, and very possibly the first to suspect them.

Chandler's papers.
With these few words of explanation I will let Mr. Chandler tell his own story. His first paper appeared in the *Astronomical Journal* in November 1891, and is courageously headed, "On the Variation of Latitude"—I say courageously, because at that time it was believed that the latitude did *not* vary, and Mr. Chandler himself was only in possession of a small portion of the facts. They unravelled themselves as he went forward; but he felt that he had firm hold of the end of the thread, and he faced the world confidently in that belief. He begins thus:—

First signs of change.
"In the determination of the latitude of Cambridge[5] with the Almucantar, about six years and a half ago, it was shown that the observed values, arranged according to nights of observation, exhibited a decided and curious progression throughout the series, the earlier values being small, the later ones large, and the range from November 1884 to April 1885 being about four-tenths of a second. There was no known or imaginable instrumental or personal cause for this phenomenon, yet the only alternative seemed to be an inference that the latitude had actually changed. This seemed at the time too bold an inference to place upon record, and I therefore left the results to speak for themselves. The subsequent continuation of the series of observations to the end of June 1885 gave a maximum about May 1, while the discussion of the previous observations from May to November 1884 gave a minimum about September 1, indicating a range of 0″.7 within a half-period of about seven months."

Mr. Chandler then gives some figures in support of these statements, presenting them with the clearness which is so well marked a feature of the whole series of papers, and concludes this introductory paper as follows:—

"It thus appears that the apparent change in the latitude of Cambridge is verified by this discussion of more abundant material. The presumption that it is real, on this determination alone, would justify further inquiry.

Confirmed in Europe.
"Curiously enough Dr. Küstner, in his determination of the aberration from a series of observations coincident in time with those of the Almucantar, came upon similar anomalies, and his results, published in 1888, furnish a counterpart to those which I had pointed out in 1885. The verification afforded by the recent parallel determinations at Berlin, Prague, Potsdam, and Pulkowa, which show a most surprising and satisfactory accordance, as to the character of the change, in range and periodicity, with the Almucantar results, has led me to make further investigations on the subject. They seem to establish the nature of the law of those changes, and I will proceed to present them in due order."

The second paper appeared on November 23, and opens with the following brief statement of his general results at that time:—

427 days' period.
"Before entering upon the details of the investigations spoken of in the preceding number, it is convenient to say that the general result of a preliminary discussion is to show a revolution of the earth's pole in a period of 427 days, from west to east, with a radius of thirty feet, measured at the earth's surface. Assuming provisionally, for the purpose of statement, that this is a motion of the north pole of the principal axis of inertia about that of the axis of rotation, the direction of the former from the latter lay towards the Greenwich meridian about the beginning of the year 1890. This, with the period of 427 days, will serve to fix approximately the relative positions of these axes at any other time, for any given meridian. It is not possible at this stage of the investigation to be more precise, as there are facts which appear to show that the rotation is not a perfectly uniform one, but is subject to secular change, and perhaps irregularities within brief spaces of time."

Contrary to received views.
It is almost impossible, now that we have become familiar with the ideas conveyed in this paragraph, to understand, or even fully to remember, the impression produced by them at the time; the sensation caused in some quarters, and the ridicule excited in others. They were in flat contradiction to all accepted views; and it was believed that these views were not only theoretically sound, but had been matured by a thorough examination of observational evidence. The only period in which the earth's pole could revolve was believed to be ten mouths; and here was Mr. Chandler proclaiming, apparently without any idea that he was contradicting the laws of dynamics, that it was revolving in fourteen months! The radius of its path had been found to be insensible by careful discussion of observations, and now he proclaimed a sensible radius o£ thirty feet. Finally, he had the audacity to announce a *variable* period, to which there was nothing at all corresponding in the mathematical

possibilities. This was the bitterest pill of all. Even after Professor Newcomb had shown us how to swallow the other two, he could not recommend any attempt at the third, as we shall presently see; and Mr. Chandler was fain ultimately to gild it a little before it could be gulped.

But this is anticipating, and it is our intention to follow patiently the evidence adduced in support of the above statements, made with such splendid confidence to a totally disbelieving world. Mr. Chandler first examines the observations of Dr. Küstner of Berlin, quoted at the end of his last paper, and shows how well they are suited by the existence of a variation in the latitude of 427 days; and that this new fact is added—when the Cambridge (U.S.A.) latitudes were the smallest those of Berlin were the largest, and *vice versâ*, as would clearly be the case if the phenomenon was due to a motion of the earth's pole; for if it moved nearer America it must move further from Europe. He then examines a long series of observations made in the years 1864-1873 at Pulkowa, near St. Petersburg, and again finds satisfactory confirmation of his law of variation. Now it had long been known that there was something curious about these observations, but no one could tell what it was. The key offered by Mr. Chandler fitted the lock exactly, and the anomalies which had been a puzzle were removed. This was in itself a great triumph; but there was another to come, which we may let Mr. Chandler describe in his own words:—

Pulkowa puzzle solved,

also Washington.

"In 1862 Professor Hubbard began a series of observations of α Lyræ at the Washington Observatory with the prime vertical transit instrument, for the purpose of determining the constants of aberration and nutation and the parallax of the star. The methods of observation and reduction were conformed to those used with such success by W. Struve. After Hubbard's death the series was continued by Professors Newcomb, Hall, and Harkness until the beginning of 1867. Professor Hall describes these observations as the most accurate determinations of declination ever made at the Naval Observatory. The probable error of a declination from a single transit was $\pm0''.141$, and judging from the accidental errors, the series ought to give trustworthy results. Upon reducing them, however, it was found that some abnormal source of error existed, which resulted in anomalous values of the aberration-constant in the different years, and a negative parallax in all. A careful verification of the processes of reduction failed to discover the cause of the trouble, and Professor Hall says that the results must stand as printed, and that probably some annual disturbance in the observations or the instrument occurred, which will never be explained, and which renders all deductions from them uncertain. The trouble could not be connected with personal equation, the anomalies remaining when the observations of the four observers who took part were separately treated. Nor, as Professor Hall points out, will the theoretical ten-month period in the latitude furnish the explanation.

"It is manifest, however, that if the 427-day period exists, its effect ought to appear distinctly in declination-measurements of such high degree of excellence as these presumably were, and, as I hope satisfactorily to show, actually are. When this variation is taken into account the observations will unquestionably vindicate the high expectations entertained with regard to them by the accomplished and skilful astronomers who designed and carried them out."

From this general account I am excluding technical details and figures, and unfortunately a great deal is thereby lost. We lose the sense of conviction which the long rows of accordant figures force upon us, and we lose the opportunities of admiring both the astonishing amount of work done and the beautiful way in which the material is handled by a master. But I am tempted to give one very small illustration of the numerical results from near the end of the paper. After discussing the Washington results, and amply fulfilling the promise made in the preceding extract, Mr. Chandler compares them with the Pulkowa results, and shows that the Earth's Pole must be revolving from west to east, and not from east to west. And then he writes down a simple formula representing this motion, and compares his formula with the observations. He gives the results in seconds of arc, but for the benefit of those not familiar with astronomical measurements we may readily convert these into feet; and in the following tables are shown the distances of the Earth's Pole *in feet* from its average position,[6] as observed at Washington and at Pulkowa, and the same distances calculated according to the formula which Mr. Chandler was able to write down at this early stage. The signs + and - of course indicate opposite directions of displacement:—

Direction of revolution of Pole.

Example of results.

WASHINGTON.
Deviation of Pole.

Date.	Observed.	Formula.
1864, Dec. 28	- 28 feet	- 23 feet
1865, Mar. 19	- 1 "	- 12 "
" June 1	+15 "	+12 "
" Aug. 11	+22 "	+23 "
" Oct. 9	+11 "	+15 "

" Dec. 13	- 17 "	- 6 "

PULKOWA.
Deviation of Pole.

Date.	Observed.	Formula.
1865, July 25	- 18 feet	- 12 feet
" Sept. 9	+ 3 "	+ 3 "
" Nov. 22	+26 "	+22 "
1866, Feb. 22	+18 "	+13 "
" June 4	- 11 "	- 18 "
" July 17	- 16 "	- 23 "

Of course the figures are not exact in every case, but they are never many feet wrong; and it may well be imagined that it is a difficult thing to deduce, even from the most refined observations, the position of the earth's pole to within a foot. The difficulty is exactly the same as that of measuring the length of an object 300 miles away to within an inch!

Mr. Chandler winds up his second paper thus:—

> "We thus find that the comparison of the simultaneous series at Pulkowa and Washington, 1863-1867, leads to the same conclusion as that already drawn from the simultaneous series at Berlin and Cambridge, 1884-1885. The direction of the polar motion may therefore be looked upon as established with a large degree of probability.

> "In the next paper I will present the results derived from PETERS, STRUVE, BRADLEY, and various other series of observations, after which the results of all will be brought to bear upon the determination of the best numerical values of the constants involved."

Bradley's observations. The results were not, however, presented in this order. In the next paper, which appeared on December 23, 1891, Mr. Chandler begins, with the work of Bradley, the very series of observations at Kew and Wansted which led to the discoveries of aberration and nutation, and which we considered in the third chapter. He first shows that, notwithstanding the obvious accuracy of the observations, there is some unexplained discordance. The very constant of aberration which Bradley discovered from them differs by half-a-second of arc from our best modern determinations. Attempts have been made to ascribe the discordance to changes in the instrument, but Mr. Chandler shows that such changes, setting aside the fact that Bradley would almost certainly have discovered them, will not fit in with the facts. **Latitude varied in twelve months then.** The facts, when analysed with the skill to which we have become accustomed, are that there is a periodic swing in the results *with a period of about a year*, and not fourteen months, as before, "a result so curious," as he admits, that "if we found no further support, it might lead us to distrust the above reasoning, and throw us back to the possibility that, after all, BRADLEY's observations may have been vitiated by some kind of annual instrumental error. But it will abundantly appear, when I have had the opportunity to print the deductions from all the other series of observations down to the present time, that the inference of an increase in the period of polar revolution is firmly established by their concurrent testimony." We shall presently return to this curious result, which might well have dismayed a less determined researcher than Mr. Chandler, but which only led him on to renewed exertions.

The results obtained from Bradley's observations may be put in the form of a diagram thus:—

VARIATION OF LATITUDE
Bradley's Observations.
(A.J. Nº 251, p. 85.)

+1.0   +1.0

0.0   0.0

-1.0 APRIL,   APRIL,   APRIL,   -1.0
     1728.   1729.   1730.
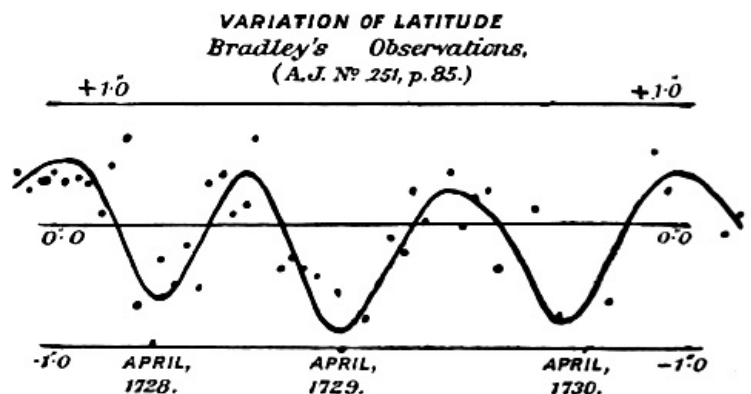
FIG. 7.

It will be seen that the maxima and minima fall in the spring and autumn, and this fact alone

seemed to show that the effect could not be due to temperature, for we should expect the greatest effect in that case in winter and summer. It could not be due to the parallax of the stars for which Bradley began his search, for stars in different quarters of the heavens would then be differently affected, and this was not the case. "There remains," concluded Mr. Chandler after full discussion, "the only natural conclusion of an actual displacement of the zenith, in other words, a change of latitude." And he concludes this paper with the following fine passage:—

> "So far, then, as the results of this incomparable series of observations at Kew and Wansted, considered by themselves alone, can now be stated, the period of the polar rotation at that epoch appears to have been probably somewhat over a year, and certainly shorter by about two months than it is at the present time. The range of the variation was apparently in the neighbourhood of a second of arc, or considerably larger than that shown by the best modern observations.

> "Before taking leave of these observations for the present I cannot forbear to speak of the profound impression which a study of them leaves upon the mind, and the satisfaction which all astronomers must feel in recognising that, besides its first fruits of the phenomena of aberration and nutation, we now owe also our first knowledge of the polar motion to this same immortal work of Bradley. Its excellence, highly appreciated as it has been, has still been hitherto obscured by the presence of this unsuspected phenomenon. When divested of its effects, the wonderful accuracy of this work must appear in a finer light, and our admiration must be raised to higher pitch. Going back to it after one hundred and sixty years seems indeed like advancing into an era of practical astronomy more refined than that from which we pass. And this leads to a suggestion worthy of serious practical consideration—whether we can do better in the future study of the polar rotation, than again to avail ourselves of Bradley's method, without endangering its elegant simplicity and effectiveness by attempts at improvement, other than supplying certain means of instrumental control which would without doubt commend themselves to his sagacious mind.

> "In the next article Bradley's later observations at Greenwich, the results of which are not so distinct, will be discussed; and also those of Brinkley at Dublin, 1808-13 and 1818-22. This will bring again to the surface one of the most interesting episodes in astronomical history, the spirited and almost acrimonious dispute between Brinkley and Pond with regard to stellar parallaxes. I hope to show that the hitherto unsolved enigma of Brinkley's singular results finds its easy solution in the fact of the polar motion. The period of his epoch appears to have been about a year, and its range more than a second. Afterwards will follow various discussions already more or less advanced towards completion. These include Bessel's observations at Königsberg, 1820-24, with the Reichenbach circle, and in 1842-44 with the Repsold circle; the latitudes derived from the polar-point determinations of Struve and Mädler with the Dorpat circle, 1822-38; Struve's observations for the determination of the aberration; Peters' observations of *Polaris*, 1841-43, with the vertical-circle; the results obtained from the reflex zenith-tube at Greenwich, 1837-75, whose singular anomalies can be referred in large part to our present phenomenon, complicated with instrumental error, to which until now they have been exclusively attributed; the Greenwich transit-circle results, 1851-65, in which case, however, a similar complication and the large accidental errors of observation seem to frustrate efforts to get any pertinent results; the Berlin prime-vertical observations of Weyer and Brünnow, 1845-46, in which I hope to show that the parallax of β *Draconis* derived from them is simply a record of the change of latitude; the conflicting latitude determinations at Cambridge, England; the Washington observation of *Polaris* and other close Polars, 1866-87, with the transit-circle; also those at Melbourne, 1863-84, a portion of which have already been drawn upon in the last number of the *Journal*, and some others. While the list is a considerable one, I shall be able to compress the statement of results for many of the series into a short space.

> "In connection with this synopsis of the scope of the investigations, one or two particulars may be of interest, which at the present writing seem to foreshadow the probable outcome. I beg, however, that the statement will be regarded merely as a provisional one. First, while the period is manifestly subject to change, as has already once or twice been intimated, I have hitherto failed in tracing the variations to any regular law, expressible in a numerical formula. Indeed, the general impression produced by a study of these changes in the length of the period is that the cause which produces them operates capriciously to a certain degree, although the average effect for a century has been to diminish the velocity of the revolution of the pole. How far this impression is due to the uncertainty of the observations, and to the complication of the phenomenon with other periodical changes of a purely

instrumental kind, I cannot say. Almost all of the series of any extent which have been examined, have the peculiarity that they manifest the periodicity quite uniformly and distinctly for a number of years, then for a while obscurely. In some cases, however, what at first appears to be an objective irregularity proves not to be so by comparison with overlapping series at other observatories.

"Another characteristic which has struck my attention, although somewhat vaguely, is that the variations in the length of the period seem to go hand in hand with simultaneous alterations in the amplitude of the rotation; the shorter periods being apparently associated with the larger coefficients for the latter. The verification of these surmises awaits a closer comparative scrutiny, the opportunity for which will come when the computations are in a more forward state. If confirmed, these observations will afford a valuable touchstone, in seeking for the cause of a phenomenon which now seems to be at variance with the accepted laws of terrestrial rotation."

Reception of discovery.

Let us now for a few moments turn aside from the actual research to see how the [Pg 195] announcement was received. It would be ungracious to reprint here any of the early statements of incredulity which found their way into print, especially in Germany. But the first note of welcome came from Simon Newcomb, in the same number of the *Astronomical Journal* as the paper just dealt with, and the following extract will indicate both the difficulties felt in receiving Mr. Chandler's results and the way in which Newcomb struck at the root of them.

Newcomb's explanation.

"Mr. Chandler's remarkable discovery, that the apparent variations in terrestrial latitudes may be accounted for by supposing a revolution of the axis of rotation of the earth around that of figure, in a period of 427 days, is in such disaccord with the received theory of the earth's rotation that at first I was disposed to doubt its possibility. But I am now able to point out a *vera causa* which affords a complete explanation of this period. Up to the present time the treatment of this subject has been this: The ratio of the moment of inertia of the earth around its principal axis to the mean of the other two principal moments, admits of very accurate determination from the amount of precession and nutation. This ratio involves what we might call, in a general way, the solid ellipticity of the earth, or the ellipticity of a homogeneous [Pg 196] spheroid having the same moments of inertia as the earth.

"When the differential equations of the earth's rotation are integrated, there appear two arbitrary constants, representing the position of any assigned epoch of the axis of rotation relative to that of figure. Theory then shows that the axis of rotation will revolve round that of figure, in a period of 306 days, and in a direction from west toward east. The attempts to determine the value of these constants have seemed to show that both are zero, or that the axes of rotation and figure are coincident. Several years since, Sir William Thomson published the result of a brief computation from the Washington Prime-Vertical observations of α Lyrae which I made at his request and which showed a coefficient 0″.05. This coefficient did not exceed the possible error of the result; I therefore regarded it as unreal.

The forgotten assumption.

"The question now arises whether Mr. Chandler's result can be reconciled with dynamic theory. I answer that it can, because the theory which assigns 306 days as the time of revolution is based on the hypothesis that the earth is an absolutely rigid body. But, as a matter of fact, the fluidity of the ocean plays an important part in the phenomenon, as does also the elasticity of the earth. The combined effect of this fluidity and elasticity is that if the axis of rotation is displaced by a certain amount, the axis of figure will, by the changed action [Pg 197] of the centrifugal force, be moved toward coincidence with the new axis of rotation. The result is, that the motion of the latter will be diminished in a corresponding ratio, and thus the time of revolution will be lengthened. An exact computation of the effect is not possible without a knowledge of the earth's modulus of elasticity. But I think the result of investigation will be that the rigidity derived from Mr. Chandler's period is as great as that claimed by Sir William Thomson from the phenomena of the tides."

This was very satisfactory. Professor Newcomb put his finger on the assumption which had been made so long ago that it had been forgotten: and the lesson is well worth taking to heart, for it is not the first time that mistaken confidence in a supposed fact has been traced to some forgotten preliminary assumption: and we must be ever ready to cast our eyes backward over all our assumptions, when some new fact seems to challenge our conclusions. It might further be expected that this discovery of the way in which theory had been defective would as a secondary consequence inspire confidence in the other conclusions which Mr. Chandler had arrived at in apparent contradiction to theory; or at least suggest the suspension of judgment. But Professor Newcomb did not feel that this was possible in respect of the *change* of period, from about twelve months in Bradley's time to [Pg 198] fourteen months in ours. We have seen that Mr. Chandler himself regarded this as a "curious result" requiring confirmation: but since the confirmation was forthcoming, he

But Chandler's work still mistrusted.

stated it with full confidence, and drew the following remarks from Professor Newcomb in July 22, 1892:—

> "The fact of a periodic variation of terrestrial latitudes, and the general law of that variation, have been established beyond reasonable doubt by the observations collected by Mr. Chandler. But two of his minor conclusions, as enumerated in No. 3 of this volume, do not seem to me well founded. They are—

> "1. That the period of the inequality is a variable quantity.

> "2. That the amplitude of the inequality has remained constant for the last half century."

Professor Newcomb proceeds to give his reasons for scepticism, which are too technical in character to reproduce here. But I will quote the following further sentence from his paper:—

> "The question now arises how far we are entitled to assume that the period must be invariable. I reply that, perturbations aside, any variation of the period is in such direct conflict with the laws of dynamics that we are entitled to pronounce it impossible. But we know that there are perturbations, and I do not see how one can doubt that they have so acted as to increase the amplitude of the variation since 1840." [Pg 199]

In other words, while recognising that there may be a way of reconciling one of the "minor" conclusions with theory, Professor Newcomb considers that in this case the other must go. Mr. Chandler's answer will speak for itself. It was delayed a little in order that he might present an immense mass of evidence in support of his conclusions, and was ultimately printed on August 23, 1892.

> "The material utilised in the foregoing forty-five series aggregates more than thirty-three thousand observations. Of these more than one-third were made in the southern hemisphere, a fact which we owe principally to Cordoba. It comprises the work of seventeen observatories (four of them in the southern hemisphere) with twenty-one different instruments, and by nine distinct methods of observation. Only three of the series (XXI., XXV., and XXXV.), and these among the least precise intrinsically, give results contradictory of the general law developed in No. 267. This degree of general harmony is indeed surprising when the evanescent character of the phenomenon under investigation is considered.

> "The reader has now before him the means for independent scrutiny of the material on which the conclusions already drawn, and those which are to follow, are based. The space taken in the printing may seem unconscionable, [Pg 200] but I hope this will be charged to the extent of the evidence collected, and not to diffuseness or the presentation of needless detail; for I have studiously sought to compress the form of statement without omitting anything essential for searching criticism. That it was important to do this is manifest, since the conclusions, if established, overthrow the existing theory of the earth's rotation, as I have pointed out on p. 21. I am neither surprised nor disconcerted, therefore, that Professor Newcomb should hesitate to accept some of these conclusions on the ground (*A. J.*, No. 271) that they are in such conflict with the laws of dynamics that we are entitled to pronounce them impossible. He has been so considerate and courteous in his treatment of my work thus far, that I am sure he will not deem presumptuous the following argument in rebuttal.

> "It should be said, first, that in beginning these investigations last year, I deliberately put aside all teachings of theory, because it seemed to me high time that the facts should be examined by a purely inductive process; that the nugatory results of all attempts to detect the existence of the Eulerian period probably arose from a defect of the theory itself; and that the entangled condition of the whole subject required that it should be examined afresh by processes unfettered by any preconceived notions whatever. The problem [Pg 201] which I therefore proposed to myself was to see whether it would not be possible to lay the numerous ghosts—in the shape of numerous discordant residual phenomena pertaining to determinations of aberration, parallaxes, latitudes, and the like—which had heretofore flitted elusively about the astronomy of precision during the century; or to reduce them to tangible form by some simple consistent hypothesis. It was thought that if this could be done, a study of the nature of the forces, as thus indicated, by which the earth's rotation is influenced, might lead to a physical explanation of them.

> "Naturally, then, I am not much dismayed by the argument of conflict with dynamic laws, since all that such a phrase means must refer merely to the existent state of the theory at any given time. When the 427-day period was propounded, it was as inconsistent with known dynamic law as the variation of

it now appears to be. Professor Newcomb's own happy explanation has already set aside the first difficulty, as it would appear, and advanced the theory by an important step. Are we so sure yet of a complete knowledge of all the forces at work as to exclude the chance of a *vera causa* for the second?"

There is a splendid ring of resolution about these words. Let us compare them with a notable utterance of Faraday:—

"The philosopher should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biassed by appearances; have no favourite hypothesis; be of no school; and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of Nature."

Tested by this severe standard, Mr. Chandler fails in no particular, least of all in that of industry. The amount of work he got through about this time was enormous, for besides the main line of investigation, of which we have only had after all a mere glimpse, he had been able to turn aside to discuss a subsidiary question with Professor Comstock; he had examined with great care some puzzling characteristics in the variability of stars; he computed some comet ephemerides; and he was preparing a new catalogue of variable stars —a piece of work involving the collection and arrangement of great masses of miscellaneous material. Yet within a few months after replying as above to Professor Newcomb's criticism,

he was able to announce that he had found the key to the new puzzle, and that "theory and observation were again brought into complete accord." We will as before listen to the account of this new step in his own words, but a slight preliminary explanation may help

those unaccustomed to the terminology. The polar motion was found to be compounded of *two* independent motions, both periodic, but having different periods. Now, the general results of such a composition are well known in several different branches of physics,

especially in the theory of sound. If two notes of nearly the same pitch be struck at the same time, we hear the resultant sound alternately swell and die away, because the vibrations caused by the two notes are sometimes going in the same direction, and after an interval are going exactly in opposite directions. Diagrammatically we should represent the vibrations by two waves, as below; the upper wave goes through its period seven and a half times between A and D, the lower only six times; and it is easily seen that at A and C the waves are sympathetic, at B and D antipathetic. At A and C the compound vibration would be doubled; at B and D reduced to insensibility. The point is so important that perhaps a numerical illustration of it will not be superfluous. The waves are now represented by rows of figures

as below. The first series recurs after every 6, the second after every 7.
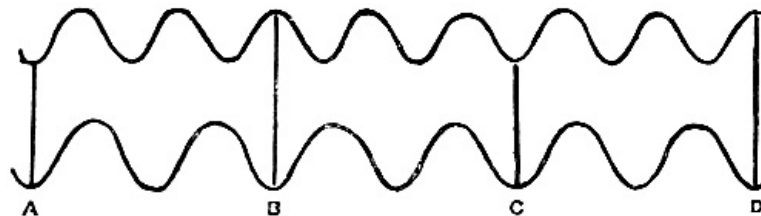
F<small>IG.</small> 8.

First Wave	1 2 3 4 3 2 1 2 3 4 3 2 1 2 3 4 3 2 1 2 3 4 3 2 1 2 3 4 3 2 1
Second Wave	1 2 3 4 4 3 2 1 2 3 4 4 3 2 1 2 3 4 4 3 2 1 2 3 4 4 3 2 1 2 3
Combined Effect	2 4 6 8 7 5 3 3 5 7 7 6 4 4 4 6 6 6 5 5 5 5 5 5 5 6 6 6 4 4 4
	Great disturbance. Calm.

First Wave	2 3 4 3 2 1 2 3 4 3 2 1 2 3 4 3 2 1 2 3 4 3 2 1 2 3 4 3 2 1 2
Second Wave	4 4 3 2 1 2 3 4 4 3 2 1 2 3 4 4 3 2 1 2 3 4 4 3 2 1 2 3 4 4 3
Combined Effect	6 7 7 5 3 3 5 7 8 6 4 2 4 6 8 7 5 3 3 5 7 7 6 4 4 4 6 6 6 5 5
	Great disturbance.

Adding the two rows together, the oscillations at first reinforce one another and we get numbers ranging from 2 to 8 instead of from 1 to 4; but one wave gains on the other, until it is rising when the other is falling, and the numbers add up to a steady series of 5's. It will be seen that there are no less than seven consecutive 5's, and all the variation seems to have disappeared. But presently the waves separate again, and the period of great disturbance recurs; it will be seen that in the "combined effect" the numbers repeat exactly after the 42nd term. Now those unfamiliar with the subject may not be prepared for the addition of

one physical wave to another, as though they were numbers, but the analogy is perfect. Travellers by some of the fast twin-screw steamers have had unpleasant occasion to notice

this phenomenon, when the engineer does not run the two screws precisely at the same speed; there come times when the ship vibrates violently, separated by periods of comparative stillness. Instances from other walks of life may recur to the memory when once attention is called to the general facts; but enough has been said to explain the point numbered (2) in the subjoined statement. To understand the rest, we must remember that if the two waves are not equal in "amplitude," *i.e.* if the backward and forward motion is not the same in both, they cannot annul one another, but the greater will always predominate. Those interested in following the matter further should have no difficulty in constructing simple examples to illustrate such points. We will proceed to give Mr. Chandler's statements:—

Chandler's final formulæ.

"We now come upon a new line of investigation. Heretofore, as has been seen, the method has been to condense the results of each series of observations into the interval comprised by a single period, then to determine the mean epoch of minimum and the mean range for each series, and, finally, by a discussion of these quantities, to establish the general character of the law of the rotation of the pole. It is now requisite to analyse the observations in a different way, and discover whether the deviations from the general provisional law, in the last column of Table II., are real, and also in what manner the variation of the period is brought about. The outcome of this discussion, which is to be presented in the present paper, is extremely satisfactory. The real nature of the phenomenon is most distinctly revealed, and may be described as follows:—

"1. The observed variation of the latitude is the resultant curve arising from two periodic fluctuations superposed upon each other. The first of these, and in general the more considerable, has a period of about 427 days, and a semi-amplitude of about 0″.12. The second has an annual period with a range variable between 0″.04 and 0″.20 during the last half-century. During the middle portion of this interval, roughly characterised as between 1860 and 1880, the value represented by the lower limit has prevailed, but before and after those dates, the higher one. The minimum and maximum of this annual component of the variation occur at the meridian of Greenwich, about ten days before the vernal and autumnal equinoxes respectively, and it becomes zero just before the solstices.

"2. As the resultant of these two motions, the effective variation of the latitude is subject to a systematic alternation in a cycle of seven years' duration, resulting from the commensurability of the two terms. According as they conspire or interfere, the total range varies between two-thirds of a second as a maximum, to but a few hundredths of a second, generally speaking, as a minimum.

"3. In consequence of the variability of the coefficient of the annual term above mentioned, the apparent average period between 1840 and 1855 approximated to 380 or 390 days; widely fluctuated from 1855 to 1865; from 1865 to about 1885 was very nearly 427 days, with minor fluctuations; afterwards increased to near 440 days, and very recently fell to somewhat below 400 days. The general course of these fluctuations is quite faithfully represented by the law of eq. (3), (No. 267), and accurately, even down to the minor oscillations of individual periods, by the law of eq. (15), hereafter given, and verbally interpreted above. This law also gives a similarly accurate account of the corresponding oscillations in the amplitude. The closeness of the accordance between observation and the numerical theory, in both particulars, places the reality of the law beyond reasonable doubt."

Those who cannot follow the details of the above statement will nevertheless catch the general purport—that the difficulties felt by Professor Newcomb have been surmounted; and this is made clearer by a later extract:—

"A very important conclusion necessarily follows from the agreement of the values of the 427-day term, deduced from the intervals between the consecutive values of T in Table XII., namely, that there has been no discontinuity in the revolution, such as Professor Newcomb regarded as so probable that he doubted the possibility of drawing any conclusions from the comparison of observations before and after 1860 (*A. J.*, 271, p. 50).

Theory must go, if it will not fit observation.

"The present investigation demonstrates that the way out of the apparently irreconcilable contradiction of theory and observation in this matter does not lie in the direction of discrediting the observations, as he is inclined to do. On the contrary, the result is a beautiful vindication of the trustworthiness of the latter, and, at the same time, of the theory that demands an invariable rate of motion; providing a perfectly fitting key to the riddle by showing that another cause has intervened to produce the variability of the period. I feel confident that Professor Newcomb will agree with the reality of the explanation here set forth, and will reconsider his view that the perturbations in the position of the Pole must be of the nature of chance accumulations of motion, a view which he

then considered necessary to the maintenance of the constancy in the period of latitude-variation."

The paper from which these words are taken appeared on November 4, 1892. The next paper on the main theme did not appear till a year later, though much work was being done in the meantime on the constant of aberration and other matters arising immediately after the discovery. On November 14, 1893, Mr. Chandler winds up the series of eight papers "On the Variation of Latitude," which he had commenced just two years before. His work was by no means done; rather was it only beginning, for the torch he had lit illuminated many dark corners. But he rightly regarded his discovery as now so firmly established that the series of papers dealing with it as still under consideration might be terminated. In this final paper he first devotes the most careful attention to one point of detail. He had shown earlier in the series that the North Pole must be revolving from West to East, and not from East to West; but this was when the motion was supposed to be simple and not complex, and it was necessary to re-examine the question of direction for each of the components. After establishing conclusively that the original direction holds for each of the components, he almost apologises for the trouble he has taken, thus:— [Pg 209]

> "It is therefore proved beyond reasonable doubt that the directions of the rotations is from West to East in both elements; whence the general form of the equation for the variation of latitude adopted in *A. J.*, 284, p. 154, eq. (19). It may be thought that too much pains have been here bestowed upon a point which might be trusted to theory to decide. I cannot think so. One of the most salient results of these articles has been the proof of the fact that theory has been a blind guide with regard to the velocity of the Polar rotation, obscuring truth and misleading investigators for a half a century. And even if we were certain, which we are not, that the fourteen months' term is the Eulerian period in a modified form. It would still be necessary to settle by observation the direction of the annual motion, with regard to which theory is powerless to inform us. To save repetition of argument, I must refer to the statement in *A. J.*, 273, pp. 68, 70, of the principles adopted in beginning these inquiries in 1891." [Pg 210]

Finally, he answers one of the few objectors of eminence who still lingered, the great French physicist Cornu:—

> "The ground is now cleared for examination of the only topic remaining to be covered, to establish, upon the foundation of fact, every point in the present theory of these remarkable movements of the earth's axis. This is the question of the possibility that these movements are not real, but merely misinterpretations of the observed phenomena; being in whole or in part an illusory effect of instrumental error due to the influence of temperature. Such a possibility has been a nightmare in practical astronomy from the first, frightening us in every series of unexplained residuals, brought to light continually in nearly all attempts at delicate instrumental research. A source of danger so subtle could not fail to be ever present in the mind of every astronomer and physicist who has given even a superficial attention to the question of the latitude variations, and there is no doubt that some are even now thus deterred from accepting these variations as proved facts. Perhaps the most explicit and forcible statement of the doubts that may arise on this subject has been given very recently by Mr. Cornu. The views of so distinguished a physicist, and of others who are inclined to agree with him, call for careful attention, and cannot be neglected in the present closing argument upon the theory presented in these articles. It is unnecessary, for the purpose of disposing of objections of the sort raised by Cornu, to insist that it is not sufficient to show that the observed variations, attributed to the unsteadiness of the Earth's Pole, are near the limit of precision attainable in linear differential measures, and in the indication of the direction of gravity by means of the air bubble of the level; or to show that there are known variations in divided circles and in levels, dependent on temperature and seasons. Nor need we require of objectors the difficult, although essential, task—which they have not distinctly attempted—of showing that these errors are not eliminated, as they appear to be, by the modes in which astronomers use their instruments. Neither need we even urge the fact that a large portion of the data which have been utilised in the present researches on the latitude were derived by methods which dispense with levels, or with circles, a part of them indeed with both, and yet that the results of all are harmonious. On the contrary, let us admit, although merely for argument's sake, that all the known means of determining the direction of gravity—including the plumb-line, the level, and a fluid at rest, whether used for a reflecting surface or as a support for a floating instrument—are subject to a common law of periodical error which vitiates the result of astronomical observation, obtained by whatever methods, and in precisely the same manner. Now, the observed law of latitude variation includes two terms, with periods of fourteen and twelve months respectively. Since the phases of the first term are repeated at intervals of two months in successive years, and hence in a series of years come into all [Pg 211] [Pg 212]

possible relations to conditions of temperature dependent on season, the argument against the reality of this term, on this ground, absolutely fails, and needs no further notice. As to the second, or annual term, while the phases, as observed in any given longitude, are indeed synchronical with the seasons, they are not so as regards different longitudes. If, therefore, the times of any given phase, as observed in the same latitude, but in successively increasing longitudes, occurred at the same date in all of them, there would be a fatal presumption against the existence of an annual period in the polar motion. If, on the contrary, they occur at times successively corresponding to the differences of longitude, the presumption is equally fatal to the hypothesis that they can possibly be due to temperature variation as affecting instrumental measurement. But the facts given in the foregoing section correspond most distinctly to the latter condition. Therefore, unless additional facts can be brought to disprove successively these observed results, we may dismiss for ever the bugbear which has undoubtedly led many to distrust the reality of the annual component of the latitude-variation, while they admit the existence of the 427-day term.”

At this point we must leave the fascinating account of the manner in which this great discovery was established, in the teeth of opposition such as might have dismayed and dissuaded a less clear-sighted or courageous man. It is my purpose to lay more stress upon the method of making the discovery than upon its results; but we may afford a brief glance at some of the consequences which have already begun to flow from this step in advance. Some of them have indeed already come before us, especially that large class represented by the explanation of anomalies in series of observations which had been put aside as inexplicable. We have seen how the observations made in Russia, or in Washington, or at Greenwich, in all of which there was some puzzling error, were immediately straightened out when Chandler applied his new rule to them. We in England have special cause to be grateful to Chandler; not only has he demonstrated more clearly than ever the greatness of Bradley, but he has rehabilitated Pond, the Astronomer Royal of the beginning of the nineteenth century; showing that his observations, which had been condemned as in some way erroneous, were really far more accurate than might have been expected; and further he has shown that the beautiful instrument designed by Airy, and called the Reflex Zenith Tube, which seemed to have unaccountably failed in the purpose for which it was designed, was really all the time accumulating observations of this new phenomenon, the Variation of Latitude. Instead of Airy having failed in his design, he had in Chandler's words “builded better than he knew.”

Consequences of the discovery.

Suspected observers acquitted.

Secondly, there is the modifying influence of this new phenomenon on other phenomena already known, such, for instance, as that of “aberration.” We saw in the third chapter how Bradley discovered this effect of the velocity of light, and how the measure of it is obtained by comparing the velocity of light with that of the earth. This comparison can be effected in a variety of ways, and we should expect all the results to agree within certain limits; but this agreement was not obtained, and Chandler has been able to show one reason why, and to remove some of the more troublesome differences. It is impossible to give here an idea of the far-reaching consequences which such work as this may have; so long as there are differences of this kind we cannot trust any part of the chain of evidence, and there is in prospect the enormous labour of examining each separate link until the error is found. The velocity of light, for instance, may be measured by a terrestrial experiment; was there anything wrong in the apparatus? The velocity of the earth in its journey round the sun depends directly upon the distance of the sun: have we measured this distance wrongly, and if so what was the error in the observations made? These are some of the questions which may arise so long as the values for the *Constant of Aberration* are still conflicting; but it requires considerable knowledge of astronomy to appreciate them fully.

Constant of Aberration improved.

Another example will, perhaps, be of more general interest. If the axis of the earth is executing small oscillations of this kind, there should be an effect upon the tides; the liquid ocean should feel the wobble of the earth's axis in some way; and an examination of tidal registers showed that there was in fact a distinct effect. It may cause some amusement when I say that the rise and fall are only a few inches in any case; but they are unmistakable evidences that the earth is not spinning smoothly, but has this kind of unbalanced vibration, which I have compared to the vibrations felt by passengers on an imperfectly engineered twin-screw steamer. A more sensational effect is that apparently earthquakes are more numerous at the time when the vibration is greatest. We remarked that the vibration waxes and wanes, much as that of the steamer waxes and wanes if the twin-screws are not running quite together. Now the passengers on the steamer would be prepared to find that breakages would be more numerous during the times of vigorous oscillation; and it seems probable that in a similar way the little cracks of the earth's skin which we call great earthquakes are more numerous when these unbalanced vibrations are at their maximum; that is to say, about once every seven years. This result is scarcely yet worthy of complete confidence, for our observations of earthquakes have only very recently been reduced to proper order; but if it should turn out to be true, it is scarcely necessary to add any words of mine to demonstrate the importance of this rather unexpected result of the Latitude Variation.

Latitude Variation Tide.

Earthquakes.

Finally I will mention another phenomenon which seems to be at present more of a curiosity

than anything else, but which may lead to some future great discovery. It is the outcome of observations which have been recently made to watch these motions of the Pole; for although there seems good reason to accept Mr. Chandler's laws of variation as accurate, it is necessary to establish their accuracy and complete the details by making observations for some time yet to come; and there could be no better proof of this necessity than the discovery recently made by Mr. Kimura, one of those engaged in this watch of the Pole in Japan. Perhaps I can give the best idea of it by mentioning one possible explanation, which, however, I must caution you may not be by any means the right one. We are accustomed to think of this great earth as being sufficiently constant in shape; if asked, for instance, whether its centre of gravity remains constantly in the same place inside it, we should almost certainly answer in the affirmative, just as only twenty years ago we thought that the North Pole remained in the same place. But it seems possible that the centre of gravity moves a few feet backwards and forwards each year—this would at any rate explain certain curious features in the observations to which Mr. Kimura has drawn attention. Whatever the explanation of them may be, or to settle whether this explanation is correct, we want more observations, especially observations in the Southern Hemisphere; and it is a project under consideration by astronomers at the present moment whether three stations can be established in the Southern Hemisphere for the further observation of this curious phenomenon. The question resolves itself chiefly into a question of money; indeed, most astronomical projects do ultimately resolve themselves into questions of money; and I fear the world looks upon scientific men as insatiable in this respect. One can only hope that on the whole the money is expended so as to give a satisfactory return. In this instance I have no hesitation in saying that an immediate return of value for a comparatively modest expenditure is practically certain, if only in some way we can get the means of making the observations.

It would be natural, at the conclusion of this brief review of some types of astronomical discovery, to summarise the lessons indicated: but there is the important difficulty that there appear to be none. It has been pointed out as we proceeded that what seemed to be a safe deduction from one piece of history has been flatly contradicted by another; no sooner have we learnt that important results may be obtained by pursuing steadily a line of work in spite of the fact that it seems to have become tedious and unprofitable (as in the search for minor planets) than we are confronted with the possibility that by such simple devotion to the day's work we may be losing a great opportunity, as Challis did. We can scarcely go wrong in following up the study of residual phenomena in the wake of Bradley; but there is the important difficulty that we may be wholly unable to find a clue for the arrangement of our residuals, as is at present largely the case in meteorology. And, in general, human expectations are likely to be quite misleading, as has been shown in the last two chapters; the discoveries we desire may lie in the direction precisely opposite to that indicated by the best opinion at present available. There is no royal road to discovery, and though this statement may meet with such ready acceptance that it seems scarcely worth making, it is hoped that there may be sufficient of interest in the illustrations of its truth.

The one positive conclusion which we may derive from the examples studied is that discoveries are seldom made without both hard work and conspicuous ability. A new planet, even as large as Uranus, does not reveal itself to a passive observer: thirteen times it may appear to such a one without fear of detection, until at last it encounters an alert Herschel, who suspects, tests, and verifies, and even then announces a comet—so little did he realise the whole truth. Fifteen years of unrequited labour before Astræa was found, nineteen years of observation before the discovery of nutation could be announced: how seldom do these years of toil present themselves to our imaginations when we glibly say that "Bradley discovered nutation," or "Hencke discovered Astræa"! That the necessary labour is so often forgotten must be my excuse for recalling attention to it somewhat persistently in these examples.

But beyond the fact that he must work hard, it would seem as though there were little of value to tell the would-be discoverer. The situation has been well summarised by Jevons in his chapter on Induction in the "Principles of Science;" and his words will form a fitting conclusion to these chapters:—

> "It would seem as if the mind of the great discoverer must combine contradictory attributes. He must be fertile in theories and hypotheses, and yet full of facts and precise results of experience. He must entertain the feeblest analogies, and the merest guesses at truth, and yet he must hold them as worthless till they are verified in experiment. When there are any grounds of probability he must hold tenaciously to an old opinion, and yet he must be prepared at any moment to relinquish it when a clearly contradictory fact is encountered."

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[3] Since the light must travel from the sun to Saturn *and back again to the earth*, the interval would be more nearly 150 minutes.

[4] Monthly Notices of the Royal Astronomical Society, vol. xvii. p. 126.

[5] This should be Cambridge, *Mass.*

[6] The distances do not represent the *total* displacement, but only the displacement towards Washington in one case and towards Pulkowa in the other.

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