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CONDUCTED BY WM. W. PAYNE.

Director of Carleton College Observatory.

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PHOTOGRAPHERS versus OLD FASHIONED ASTRONOMERS.

ORMOND STONE.*

Professor Holden's article in the *Overland Monthly* for November, entitled, "Photography the Servant of Astronomy," will not simply interest an intelligent public, but is likely to prove of real service to practical astronomers by again calling their attention to the great use which photography promises to offer to astronomy, and that too in the immediate future, and by so doing stimulate them to still greater activity in this important branch of celestial physics.

Celestial photography is making rapid strides, but the subject is yet in its infancy, and is likely to accomplish vastly more than most of us have realized. Professor Holden's resumé is interesting, and gives a comprehensive view of what photography is doing and is likely to do in aid of astronomy.†

I desire, however, to call attention to the impression which must naturally be made upon the ordinary reader by much of what is now written by those interested in celestial photog-

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†One minor subject he does not touch upon, namely, the investigation of the characteristics of optical glass, which, while not strictly a part of astronomy, is a subject so important to every astronomer as to be fitly classed with the other services mentioned by Professor Holden. The characteristics of an object glass can be readily studied by photographing the spectra of stars, and studying the widths of the different portions of these spectra when observed at various positions on each side of the focus of the objective under consideration.

The use of photography for the investigation of the indices of refraction of the ultra violet rays, I have not seen suggested anywhere, though I do not doubt it has been proposed as it might easily have escaped my notice. All that is necessary apparently, is to substitute a camera for the eye in the use of the spectrometer, photographing, at the same time, a plumb line attached to the viewing telescope. The details of the operation will easily suggest themselves to anyone familiar with the use of the goniometer and the camera.

raphy. One would infer, I think, that there is no longer any use for that venerated, but now to-pass-away, class of individuals hereafter to be known as "old fashioned astronomers," and that hereafter observers skilled in the use of the meridian circle and the micrometer, will bear about the same relation to astronomical photographers as did the astronomers who lived before the invention of the telescope to the Herschels and the Struves of later date.

While welcoming photography as ushering in a new era in the history of the science, it may well be questioned whether the older methods will be altogether discarded. While, no doubt, photography will accomplish much, will it accomplish everything? In fact, cannot many observations be made better without the aid of the camera than with, and will this not be the case for some time to come?

The most important work relating to the structure of the heavens, as perhaps all will agree, is the determination of the accurate positions of the stars. However interesting such experiments may be, it is questionable whether the introduction of a photographic plate instead of the eye will not complicate rather than improve observations made with the meridian circle. In the case of close objects, such as clusters, while the positions of a larger number of objects observed in the field of an equatorial can probably be obtained with a given accuracy in a given time, still it remains to be seen whether the *highest* accuracy is to be obtained by the micrometer with or without the aid of the camera; for after all, it is not a question of doing away with the micrometer, but of making the observations directly at night upon the celestial objects themselves, or afterwards measuring their photographic images, still using the micrometer, though under a different name; nor am I so sanguine that by photography micrometrical measurements will be completely freed from the effects of atmospheric displacement. In the case of double stars, further evidence is needed to show that with a given accuracy a larger number of objects can be observed in the same time with the camera than without, assuming that it requires nearly as much time and trouble to make a good photograph of a double star as it does to make one of a cluster. As to the new methods not requiring highly skilled observers; if skill is required to properly bisect a double star as seen in the heavens, why will not the same skill be required to bisect its images as observed in the measuring engine?

In regard to the Nebula of Orion, the beautiful photograph of Mr. Common does, without doubt, give the form with an ease and an accuracy which are unattainable by any other known means;* still I am inclined to agree with Professor Holden, "that the figure of the Nebula of Orion has remained the same from 1758 to now; * * * but that in the brightness of its parts undoubted variations have taken place, and that such changes are even now going on;" and it may be taken as an axiom, that any changes in form will be at least accompanied, if not preceded, by changes in brightness. If this be true it is not to photography but to photometry, that we must look for the earliest positive evidence of changes in this the most remarkable nebula in the heavens. The observations of this nebula which were made at this observatory in 1885 were merely tentative in their character and were made without a knowledge of the existence of Mr. Common's photograph. The winter of 1885-86 was a very cloudy one and but few opportunities were obtained for observing it. During the past September and October, however, the nebula was repeatedly examined, not for the purpose of determining the form of the nebula but of determining the relative brightness of the various condensations of which it is composed. The region *A* preceding the trapezium was especially observed, and the brightness of its condensations compared with one another and with other portions of the Huyghenian region. The differences of brightness were esti-

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*In some parts of the nebula I am unable to see with our great refractor some of the details there given; in other parts I have recognized details not shown in the photograph, and this remark is not limited to portions concealed by the enlarged images of the brighter stars. In this connection it is interesting to note that the portion of the region *A* just following the star B 589=H 57 is perhaps the *brightest* portion of A, as shown in Mr. Common's photograph; while, as seen here this year, it is one of the *faintest*.

mated in "steps," as was done by Argelander in his observations of variable stars, each condensation being usually compared on the same night both with brighter and with fainter condensations. Such estimates probably give more reliable results than can be obtained with the aid of the camera.

Other cases where the older methods are the better will probably suggest themselves.

The object of this note is, however, not to decry the use of photography for astronomical purposes, but rather to suggest that photography will not necessarily displace every other method of observation, but that much still remains which can be accomplished by an old fashioned astronomer. After all, I take it, Prof. Holden and others who are so forcibly impressed with the utility of photography, do not mean to "ring out the old," but simply to "ring in the new." Then hail to photography, one of astronomy's most useful, but not her only servant.

TEN YEARS' PROGRESS IN ASTROMOMY (1876-1886).*

PROFESSOR C. A. YOUNG.

The Earth.

In what may be called the astronomy of the earth, there is no very great discovery, nothing extremely new and brilliant to record during the past decade; but there has been considerable and steady progress.

As regards the earth's form and dimensions, it has become quite certain that Bessel's ellipticity ($\frac{1}{299}$) is too small. Clarke's value of $\frac{1}{293}$ is now admitted and employed on the U. S. Coast Survey with a decided improvement of accordance. A slightly larger value even is suggested by the most recent pendulum observations, and $\frac{1}{292}$ is now adopted in Europe.

One of the most important steps in this branch of investigation is the discovery, by Mr. Peirce (of our own Coast Survey), of the large correction required in many former pendu-

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*Address before the New York Academy of Sciences, delivered May 17, 1886.

lum determinations, on account of the yielding of the stand from which the pendulum is suspended.

During the past ten or fifteen years, a great amount of material has been collected towards a complete gravitational survey of the earth, by the work of Lieut.-Col. Herschel in India, and of the officers of the Coast Survey in this country and elsewhere, and a very important part of it has consisted in connecting the older work with the new, by Peirce's operations in Europe, and those of Herschel in this country.

At the same time it has become increasingly evident that very little is now to be gained by endeavoring to find a spheroid fitting the earth's actual form more closely. It will be best simply to adopt some standard (say that of Clarke, but it makes very little difference what), and to investigate hereafter the local deviations from it. These deviations seem to be larger and more extensive than used to be supposed, the station errors in latitude and longitude being at least quantities of the same order as the variations of elevation.

We mention, in passing, the investigations of Fergola, based on observations at Pulkowa and Greenwich, and leading to a suspicion that the axis of the earth is slightly changing its position and shifting the place of the poles on the earth's surface. Operations have been organized to determine the question, by co-operation between different observatories in nearly the same latitude, but widely differing in longitude.

Nor ought we to pass unnoticed an elaborate paper by Kapteyn, of Groningen, on the determination of latitude by a method depending upon time-observation of stars, at equal altitudes, though in widely different parts of the sky; the stars being so selected that all errors of star-places, instrument, and clock, are almost perfectly eliminated. In the same connection we ought to mention also the new equal-altitude instrument, the Almucantar, invented by Chandler, of Cambridge, and his development of the method of determining time by its use. It may possibly supersede the transit instrument for this purpose, as he seems to expect, though we think it hardly likely.

Rapid progress has been made in determining the difference of longitude between all the principal parts of the earth. There now remain very few stations of much importance, which have not their longitude from Greenwich telegraphically settled within a small fraction of a second. In Europe, Albrecht has combined into a consistent whole all the different data for more than one hundred points. Our American system has been similarly worked out by Schott, and is connected with the European by no less than four different and independent cable-determinations. South America is connected with the United States by the recent operations of our naval officers in the West Indies and along the eastern and western coasts of the continent; and with Europe by a cable connection between Lisbon and Pernambuco, also effected by them. It is worth noting that two large errors in European longitudes owe their detection to American astronomers. The difference of longitude between Greenwich and Paris was corrected by our Coast Survey in 1872 to the extent of nearly half a second of time, and our naval officers in 1878 showed that the then received longitude of Lisbon was 8.54*s* too small! It is a less surprising fact that an error of 35*s* was found in the longitude of Rio.

Our navy has also determined an important series of telegraphic longitudes along the eastern coast of Asia and through the East Indies. The French have been doing similar work in the same regions, especially in connection with the transits of Venus; and the English have determined a large number of longitudes in India. These Asiatic longitudes have been recently connected with Australia and New Zeland by English astronomers, and a telegraphic longitude connection has been effected down the eastern coast of Africa from Aden to the Cape; so that now it is perfectly practicable, if it is desirable, to have one standard of time in all the civilized world.

A word perhaps is here in place as to this question of standard time, and the beginning of the day. The adoption by our railroads of the system of standards differing from Greenwich time only by entire hours has, I think, been admittedly a great step in advance, as regards public convenience and safety in traveling. At a few points, where standard and local time happen to differ by nearly the maximum possible amount of half an hour, some annoyance is felt and there is still some opposition; but it seems quite clear that, in this country at least, all resistance will soon die out.

As regards the more purely astronomical question of making the astronomical day coincide with the civil day, by beginning at midnight, instead of noon, as it does at present, there is more difference of opinion. For my own part I am frankly in favor of the change, because I see no use in perpetuating an anomaly which is sometimes annoying and confusing. At the same time the change would, of course, involve some inconvenience to computers and night observers, and it must be admitted that at present a large number, and possibly a majority of the most eminent astronomers, in other countries as well as in this, are opposed to it. Those of us whose work falls about as much in the day as in the night, and those, I think, who take a long look ahead, are in favor of the reform; but those whose work is mainly nocturnal, or is based on observations made chiefly at or near the "witching hour," dread the inconvenience of a change of date in the midst of the record, and the risk of confusion in the interpretation of old observations.

The question, however, seems to me not a very important one. I notice that the visitors of the Royal Observatory have just recommended that the change be introduced into the British Nautical Almanac for 1891.

Before passing to the moon, a word should be added as to the outcome of the most recent investigations regarding the steadiness of the earth's rotation. Some irregularities in the lunar motions have appeared to justify a suspicion, at least, that they might be caused by irregularities in the length of the day. The researches of Newcomb upon ancient eclipses and occultations of stars give results not necessarily inconsistent with this hypothesis, perhaps even slightly in its favor; but his careful examination of the past transits of Mercury contra-indicates it pretty decidedly.

The Moon.

During the past ten years there has been no work upon the lunar theory quite on a level with that of Hansen, Delaunay, Plantamour, and Adams in the years preceding; but the labors of Neison, Hill, and Newcomb well deserve mention. The former especially has carried his approximations to a considerably higher point than any of his predecessors, though not without making a few numerical mistakes, which have been detected and corrected by Hill. The investigation of ancient and mediæval observations of the moon by Newcomb is also a very important work, as showing clearly that the lunar theory is still incomplete, and that it is impossible by any tables yet made to represent accurately the whole series of observations. A value of the secular acceleration, which suits the observations of the last two hundred years, will not fit the Arabian observations made one thousand years ago, nor will it satisfy the eclipse observations of still more ancient date; unless at least the received interpretation of those ancient eclipses be admitted to be wrong, as Prof. Newcomb seems to consider rather probable. From his discussion he derives for the secular acceleration a value of 8.4″ as against the value of 12.1″ deduced by Hansen.

It will be remembered probably by every one present that the *theoretical* value of this quantity is about 6″, and that Ferrel, Adams, Delaunay and others attributed its apparent increase to 12″ to the action of the tides in retarding the earth's rotation and so lengthening the day; if Newcomb's value is correct, this retardation is cut down from 6″ to about to 2.5″.

The study of the moon's surface has been carried on with assiduity, but I do not know that any remarkable results have been reached, though Klein's observation, in 1877, of what he supposed to be a newly-formed crater (Hyginus N.) excited a good deal of interest and discussion for a number of years; and the most eminent selenographers are still divided in opinion on the question.

The publication by the German government of Schmidt's great map of the moon, in 1878, unquestionably marks an epoch in selenography; and the photographic work of Pritchard, and the heliometric determination of the moon's physical libration by Hartwig, must not pass unnoticed.

Probably, however, the lunar work which has drawn to itself most attention and interest is the investigation of the moon's heat by Lord Rosse and Professor Langley.

The earliest observations of the kind date back now forty years, when Melloni, in 1846, first detected the moon's heat by means of the then newly invented thermopile. But the first really scientific *measurements* are only about fifteen years old, due to Lord Rosse, at Parsonstown, and to Marie Davy, at Paris; and they seemed to show that at the time of full moon we receive from our satellite, not merely *reflected* heat, but warmth *radiated* from the moon's surface; as if this surface were raised to a considerable temperature by the long insolation to which it has been exposed during the preceding fortnight. Lord Rosse estimated the probable temperature of this heated rock to be as high as from 300° to 500° F.

But within the past four or five years this conclusion has been called in question. Observations at Parsonstown, of the rapid diminution of radiation during a lunar eclipse, seem to favor the newer view, that the moon's surface, like that of a lofty mountain-top on the earth, never gets very hot, since the absence of air enables the solar heat to escape nearly as fast as it is received.

Professor Langley's recent and still progressing work upon this subject far excels in delicacy and elaborateness anything done before. At first it seemed to show that the temperature of freezing water was never reached even at the hottest parts of the lunar surface; but the latter observations throw some doubt on the legitimacy of this inference. It is found that the radiation from the moon unquestionably contains a considerable percentage of rays which have a wave-length *longer* than any of the heat rays from melting ice; and this fact has been supposed to make it probable that the moon's surface was colder than the ice. But then, within a few weeks, Professor Langley has found the long-waved rays in the radiation from an *electric arc!* So the question still hangs debatable.

The Sun's Parallax.

I think we may say that, during the past ten years, substantial progress has been made with the problem of the solar parallax. The transit of Venus, in 1882, adds whatever value its results may have, to those obtained eight years before; but, on the whole, so far as can be judged from the reductions thus far completed and published, it would seem likely that the outcome of the transit observations will be simply to confirm the results obtained by other methods. It may be that the data obtained from the German heliometer measurements will prove more accordant and decisive than those derived from photographs and from the contact observations; there are flying rumors that they will, but it will be necessary to await the official publication for certain knowledge on this point. If they do not, we shall be obliged, hereafter, to relegate transit observations to a secondary rank, as a means of determining the sun's distance. From the various observations of the two transits, different computers have deduced values of the parallax all the way from 8″.6 to 8″.95, corresponding to a distance ranging from 95,000,000 to 91,500,000 miles.

The case is quite different with the heliometer observations of the opposition of Mars, in 1877, made by Mr. Gill, at Ascension Island. These give in a most definite and apparently authoritative manner a value of 8″.783, and are apparently irreconcilable with any value much greater than 8″.81, or less than 8″.75. So far as can be judged from the number, nature and accordance of the observations, I believe we must accept this as the most trustworthy of the geometrical methods yet employed; though the weight of the result would certainly be increased if it did not depend to such an extent upon the work of a single individual.

The confidence of astronomers in the correctness of this value is greatly fortified by the fact that the most recent and reliable determinations of the velocity of light, made by Michelson and Newcomb, in 1877, 1880, 1881 and 1882, when combined with the Pulkowa constant of aberration determined by Nyren from all the data available up to 1882, give a solar parallax accordant with the preceding, almost to the hundredth of a second—8″.794 as against 8″.783. It is true there are possible theoretical objections to the method ; as for instance, that the result may be slightly affected by the motion of the solar system through space. Enough is not known certainly about the constitution of the medium that transmits light through space, to decide all such questions *a priori* and authoritatively ; but it is unquestionable that any correction needed on account of such possible causes of error must be very minute.

We believe, therefore, that it is safe to assume pretty confidently, that the solar parallax is about 8″.8 (though probably a trifle less), which makes the sun's mean distance 93,000,000 miles, with an error not likely much to exceed 150,000 miles. A larger value of the parallax (about 8″.85) still holds its ground in the nautical almanacs, and undeniably is nearer the *average* of the results given by *all* the known methods. But none of the other methods seem to us to compete at all in precision with the two whose authority we accept.

The Sun and Meteorology.

The study of the solar surface has been carried on very persistently by Spoerer, in Germany, as well as by others, and a great amount of material has been collected bearing upon the theory and nature of sun-spots, and their periodicity. The extensive series of photographs obtained at Kew, and at Dehra Doon, in India, constitutes almost a continuous record of the solar surface for several years. The relation between this periodicity and terrestrial conditions has been assiduously examined, but on the whole the outcome seems to me to leave this connection as doubtful as it ever was, in most cases at least. While in some parts of the earth it looks as if there were a slight but marked increase of storm and rainfall at the time of sun-spot maximum, the reverse seems to be true in other countries. In South America, Dr. Gould thinks that he has demonstrated a very perceptible effect of the condition of the sun's surface in modifying the strength and direction of the winds ; but thus far similar investigations elsewhere show no such result. It will evidently be necessary to wait for a longer and more widely extended collection of statistics to settle the question. We do not even know as yet whether we get more or less than the average heat from the sun during the sun-spot maximum.

But I think it may be set down as certain that the condition of the sun's surface exerts, if perhaps a real, yet only a very slight effect upon our earthly meteorology. With terrestrial magnetism the case is markedly and singularly different, and one of the most interesting problems now pressing for solution is the nature of the connection between solar disturbances and magnetic storms.

Solar Heat.

A great deal of labor has been expended upon the study of the sun's heat during the last decade. The investigations that strike me on the whole as most worthy of mention are those of our own Langley and of the Italian Rosetti, whose early death a few months ago is a great loss to science. Secchi and Ericsson, on the one side, had contended for a solar temperature of some millions of degrees, basing their results on Newton's law of cooling ; while on the other, Crova and Violle, from their measures of the solar radiation, reduced according to the so-called law of Dulong and Petit, maintained that the temperature does not much exceed that of many terrestrial furnaces, somewhere from 1,500° to 2,500° C. Rosetti's experiments upon the radiation of the electric arc and other sources of intense heat, showed pretty clearly the inapplicability of Dulong and Petit's law to high temperatures, and indicate a solar temperature not far from 10,000° C., or 18,000° F. But they also make it clear that the limits of uncertainty are still very great.

Professor Langley, by his invention of the bolometer, has been able to investigate separately the amount of energy transmitted to the earth in the solar rays of every possible wave-length, and to determine the effect of our atmosphere in absorbing each kind of ray. He has shown that the older method of investigating this solar radiation, *in a lump* so to speak, gives fallacious results on account of atmospheric absorption ; and that the necessary correction compels us to increase our estimate of the sun's energy at least twenty per cent. In my own little book upon the sun, published in 1881, I had set the so-called solar constant at twenty-five calorics per square metre per minute. It is now certain that it must be put at least as high as thirty. Professor Langley's investigations seem also to show another remarkable fact—that we do not receive from the sun any at all of the low pitched, slowly pulsing waves, such as we get from surfaces at or below the temperature of boiling water. The solar spectrum appears to be cut off abruptly at the lower end ; and this cutting off we know cannot have been effected in the earth's atmosphere, because we receive from the moon in considerable quantity just this very sort of low-pitched rays. Langley finds them also abundant in the radiation of the electric arc, so that we can hardly suppose them to be *originally* wanting in the solar heat. It now looks as if we must admit that they have been suppressed either in the atmosphere of the sun itself, or in interplanetary space. Another striking conclusion first clearly pointed out by Langley is that, if the sun's atmosphere were removed, its light would be strongly blue.

The Solar Surface and Spots.

As regards the general make-up of the solar surface, I do not think there has been any new fact of extreme importance brought out within ten years. Janssen has, however, carried solar photography to higher excellence than ever attained before, and has obtained plates that show the "granules" and their grouping on a scale previously unknown. He thinks that his plates prove a peculiar constitution of the solar surface, consisting in collections of clearly defined and rounded granules, separated by regions or streaks where they are ill defined and elongated; and he calls the phenomenon the "reseau photospherique," or photospheric network. According to him the "net" remains approximately constant for some minutes at a time, as shown by plates taken in quick succession, but is subject to rapid and enormous changes in periods exceeding a quarter of an hour or so. I find some skepticism among high authorities as to the trustworthiness of his conclusions. There are suggestions that the appearances presented may be due to currents of air in the telescope tube and at the surface of the sensitive plate; but I am disposed to think he is right, for, on several occasions when the seeing has been exceptionally fine, I have observed with my own eyes something quite analogous in our large telescope at Princeton.

The spots have been carefully studied by several observers, by Spoerer especially, in a statistical way, and by Vogel, Lohse, Tacchini, and others, as to structure and detail. Spoerer has brought out very clearly the connection between the number and average latitude of the spots. It appears that, speaking broadly, the disturbance which produces the sun-spots begins in two belts on each side of the sun's equator in a latitude of over 30°; these belts or spot-zones then gradually move in towards the equator, the sun-spot maximum occurring when their latitude is about 16°; while the disturbance gradually and finally disappears at a latitude of 8° or 10°, some twelve or fourteen years after its first appearance. But two or three years before this disappearance, a new zone of disturbance shows itself in the same latitude as its predecessor, so that for a while, about the time of sun-spot minimum, there are two well-marked zones of spots on each side of the sun's equator; one pair near the equator, due to the expiring disturbance which began some ten or twelve years ago; the other far from the equator, and due to the newly arising out-burst, which will reach its maximum in three or four years, and then pass away like the former.

There can be no doubt that the phenomenon is a very significant one, but its explanation, like that of the periodicity itself, is still to be found.

Nor is the problem of the spots themselves yet fully solved. Not that there is any reasonable question that they are *hollows* in the solar photosphere ; but how they originate, how deep they are, and what are the causes of their darkness, and the condition and temperature of the darkening substance ; these are questions to which only uncertain answers can now be given. A long and important series of observations upon the widening of the lines of certain elements in the sun-spot spectra has been made by Mr. Lockyer, and establishes clearly the fact that those lines, of *iron* for instance, which are conspicuously black and wide in the sun-spots, are often just those which do *not* show themselves conspicuously in the prominences; and moreover both in spots and prominences the iron lines that do show themselves are most frequently those which closely coincide with lines in the spectra of other substances. Singularly, also, and so far quite without explanation, it appears according to his observations that at the sun-spot maximum those *iron* lines, which at other times are conspicuous in spot spectra, entirely disappear.

Perhaps I may be allowed to mention here a *recent* observation of my own upon these spot spectra : with a high dispersion the darkest part of the spot spectrum is found to be not continuous, but made up of fine lines overlapping or almost touching each other, with here and there a clear space left, like a fine bright line. It means, I think, that the absorbing vapors which darken the interior of the spot are wholly gaseous, and tend to disprove the idea that they are mostly of the nature of smoke or steam. We mention, also, in passing, another thing which has been shown by our large instrument at Princeton—that the apparently bulbous, finger-tip-like terminations of the penumbral filaments are often, under the best circumstances of vision, resolved into five bright, sharp-pointed hooks which look like the tips of curling flames.

The Solar Spectrum.

In 1877, Dr. Henry Draper, of New York, by a series of most laborious, time-consuming and expensive researches, discovered the presence of oxygen in the sun, evidenced in his photographs, not by fine dark lines, as in the case of elements previously recognized, but by bright, hazy bands. It is difficult to assign any reason why this gas should behave so peculiarly and so differently from others, and for this reason many high authorities are indisposed to accept the discovery. But the evidence of the photographs seems fairly to outweigh any such purely negative theoretical objections.

Other advances have been made in the study of the spectrum, due mainly to the great improvements in spectroscopic apparatus. Until recently it has not been easy to decide with certainty as to some lines in the spectrum whether they were of solar or telluric origin; the great bands known as A and B for instance. It was only in 1883 that the Russian Egoroff succeeded in proving that these are produced by the oxygen in the earth's atmosphere. In his experiments on a scale previously unknown, the light was transmitted through tubes more than sixty feet in length, closed at the end with transparent plates, and filled with condensed gas.

It was quite early pointed out that the sun's rotation ought to produce a shift in the position of lines in the spectrum according as the light is derived from the advancing or receding edge of the solar disc, and Zollner thought he could perceive it. The earliest *measures*, however, were, I believe, those obtained independently by Vogel and the writer in 1876. In the great bisulphide of carbon spectroscope of Thollon, the displacement becomes easy of observation; and very recently Cornu, by taking advantage of it, and by an extremely ingenious arrangement for making a small image of the sun to oscillate across the spectroscope slit two or three times a second, has been able to discriminate at a glance between the telluric and solar lines; the former stand firm and fast, while the latter seem to wave back and forth.

In this connection also should be mentioned the great map of the solar spectrum, for which Thollon received the Lalande prize of the French Academy of Sciences last January, and the still more accurate and important map photographed by

Professor Rowland, by means of his wonderful diffraction gratings, and now in course of publication. Nor would it be just either to omit the earlier and less accurate maps of Fievez and Vogel, which, when published, were as far in advance of anything before them as they are behind the new ones; nor the maps just made by Professor Smyth, of Edinburgh.

It was in connection with the construction of such a map by Mr. Lockyer, that he was led to his theory of the compound nature of the so-called chemical elements, partly as a result of his comparisons of the spectra of different substances with the solar spectrum, and partly in consequence of considerations drawn from certain phenomena observed in the solar and stellar spectra themselves. His first paper on the subject was read late in 1878. This "working hypothesis," as its author calls it, has met with much discussion, favorable and unfavorable. It unquestionably removes many difficulties, and explains many puzzling phenomena; at the same time there are very serious objections to it, and some of the arguments upon which Mr. Lockyer originally laid much stress have turned out unsound. For instance, he made a great point of the fact that, after all precautions are taken to remove impurities, several elementary substances show in their spectra common lines—"basic lines" he called them—indicating, as he thought, a common component. He found in the solar spectrum about seventy of these "basic lines." Now, under the high dispersion of our newer spectroscopes, these lines, which were single to his instruments, almost without exception dissolve into pairs and triplets, and withdraw their support from his theory.

I suppose that at present the weight of scientific opinion is against him; but for one I do not believe his battle is lost. In view of the law of Dulong and Petit, which establishes a relation between the atomic weight and specific heat of bodies, it seems to be pretty certain that *hydrogen* cannot be the elementary "urstoff" out of which all other elements are made by building up, as he at first seemed disposed to maintain; this element stands apparently on no different footing from the rest. But I see no reason why the elements, as we know them, may not constitute one *class* of bodies by themselves, all built up out of some as yet more elemental substance or substances. The "periodic law" of Mendeljeff suggests such a relation. And our received theories so stumble, hesitate, and falter in their account of many of the simplest phenomena of the solar and stellar atmospheres, that a strong presumption still remains in favor of the new hypothesis. I am not prepared to accept it yet; but certainly not to reject it.

The Chromosphere.

The study of the chromosphere and prominences has been kept up, very systematically and statistically, by Tacchini in Italy, and with less continuity, but still assiduously, by several other observers. I do not know, however, that any new results of much importance have been arrived at. The list of bright lines visible in their spectra has been a good deal enlarged: and Trouvelot thinks he has observed *dark* prominences—objective forms that show, black but active, upon the background of bright scarlet hydrogen in the surrounding chromospheric clouds. It may be that he is right; but so far as I can learn, no other observer of the solar atmosphere has seen anything similar. I certainly have not myself. And I think some of his published observations of velocities of two or three thousand miles a second in the motions of the prominences, as evidenced by the displacement of lines in the spectrum, are still more questionable.

In two or three cases, prominences have been observed since 1876 considerably higher than any known previously. In October, 1878, I myself observed one which attained an elevation of nearly 400,000 miles (13½′).

Eclipses and the Corona.

The sun's corona has been perhaps more earnestly studied than anything else about the central luminary, especially during the four elipses which have occurred since 1876. At the eclipse of 1878, in the midst of an epoch of sun-spot quiescence, the corona was found less brilliant than ordinary, and especially deficient in the unknown gas that produces the so-called 1474 line—the line which characterizes the spectrum of the corona, and first demonstrated conclusively its solar origin in 1869. But while the corona at this time was less brilliant than it had been formerly, it was far more extensive. At least it seemed so; for at Pike's Peak and Creston, Langley and Newcomb were able to follow its streamers to a distance of 6° from the sun. It is possible, however, that this extension was only due to the superior transparency of the mountain air.

The Egyptian eclipse of 1882 gave us some interesting results respecting the spectrum of the prominences and the corona. It appears that the light of the corona is especially rich in the ultra-violet, and in the photographs of the spectrum a number of bands are found which have been interpreted, with questionable correctness, I think, as indicating the presence of carbon. The eclipse of 1883 was observed in the Pacific Ocean by French and American parties, but, I think, added very little real information. Professor Hastings made an observation which he believed to establish a peculiar theory proposed by himself, viz., that the corona is merely a diffraction effect produced by the moon's limb, and depending on the non-continuity of phase in long stretches of light vibrations. With a peculiar apparatus prepared expressly for the purpose, he found that at any moment the 1474 line was visible to a much greater distance from the sun, on the side least deeply covered by the moon, than on the other: as unquestionably would happen if his theory were correct. But the same thing would result from the mere diffusion of light by the air; and, notwithstanding his protests, the French observers who were at the same place, and nearly all others who have discussed the observations, think that this was the true explanation of what he saw. So far as I know, the discussion of the subject which has resulted from his publication has only strengthened the older view—that the corona is a true solar appendage; an intensely luminous but excessively attenuated cloud of mingled gas and fog and dust surrounding the sun, formed and shaped by solar forces.

The diffraction theory has one advantage—that it relieves us from stretching our conceptions as to the possible attenuation of matter to the extent necessary in order to account for the fact that a comet, itself mostly a mere airy nothing, experiences no perceptible retardation in passing through the coronal regions. There can be no question that this has happened several times: the last instance having been the great comet of 1882. But on careful consideration it will be found, I think, that our conceptions will bear the stretching without involving the least absurdity; a single molecule to the cubic foot would answer every necessary condition of the luminous phenomenon observed. And all the rifts and streamers, and all the radiating structure and curved details of form, cry out against the diffraction hypothesis. The observations of the eclipse of 1885 (observed only by a few amateurs in New Zealand) have not proved important.

At present the most interesting debate upon the subject centres around the attempt of Mr. Huggins (first in 1883) to obtain photographs of the corona in full sunlight. He succeeded in getting a number of plates showing around the sun certain faint and elusive halo forms which certainly look very coronal. Plans were made and have been carried out, for using a similar apparatus on the Riffelberg, in Switzerland, and at the Cape of Good Hope. But so far nothing has been obtained much in advance of Mr. Huggins' own first results. Since September, 1883, until very recently, the air has been full, as every one knows, of a fine haze, probably dust and vapor from Krakatoa, which has greatly interfered with all such operations. It is now fast clearing away, and I for one am somewhat sanguine that a much greater success will be reached next winter at the Cape, and perhaps even in England during the coming summer.

Just about the same time that Huggins was photographing in England, Professor Wright was experimenting in New Haven in a different way: isolating the blue and ultra-violet rays by the use of colored media, stopping out the sun's disc and receiving the image of the coronal regions on a fluorescent screen. He also had obtained what he believed, and still believes, to be a real image of the corona, when the aerial haze intervened to put an end to all such operations; for of course it is evident that whether one operates by this method or by photography, success is possible only under conditions of unusual atmospheric transparency and purity.

I suppose at present the predominant feeling among astronomers is that the case is hopeless, and that Huggins and Wright are mistaken. It may be so. But my own impression is that they are probably correct; although, of course, the matter is still in doubt.

Inferior Planets.

Leaving now the sun, and passing to the planetary system, we come first to the subject of intra-Mercurial planets.

The general opinion among astronomers (in which I fully concur) is that the question has been now fairly decided in the negative, *i. e.*, it is practically certain that within the orbit of Mercury there is no planet of a diameter as large as five hundred miles, probably not one hundred. If such a one existed, it could not have failed to be discovered by the wide-angled photographs taken at the eclipses of 1882 and 1883, to say nothing of the visual observations. Of course, it is well known that at the eclipse of 1878 Professor Watson supposed he had discovered two such bodies, and his extensive experience and his high authority, led, for a time, to a pretty general acceptance of his conclusion. I notice that Dr. Ball, even very lately, in his "Study of the Heavens," is still disposed to credit the discovery. But Dr. Peters, by a masterly discussion of the circumstances of the observations themselves, and a comparison with the star maps, has shown that it is almost certain that Watson really saw only the two stars Theta and Zeta Cancri. In the same paper also, Peters examined all the observations of small, dark spots crossing the sun's disc which, up to that date (1879), had been made by Leverrier and others the ground for their belief in "Vulcan;" and he shows that they really afford no sufficient ground for the conclusion. As to Mr. Swift's supposed observation of two objects with large discs "both pointing to the sun," they certainly were not the two seen by Watson, while they were in the region covered by Watson and several other observers. What the precise nature of the mistake or illusion may have been it is perhaps not now possible to discover, but I think no one, unless perhaps Mr. Swift himself, now considers the observation important.

While, however, the question of a "Vulcan" is now pretty definitely settled, it is not at all impossible, or even improbable, that there may be intra-Mercurial asteroids, and that some of them may be picked up as little stars of the sixth magnitude or smaller, by the photographers at the eclipse of next August, or in 1887. The sensitiveness of our present photographic plate is now many times greater than it was even in 1882.

As to the planet Mercury, there is very little to report. It "transited" the sun in May, 1878, and again in November, 1881, and during the transits numerous measures were made of its diameter, giving results substantially in accord with the older values. I have already alluded, in connection with the earth's rotation, to Newcomb's investigation of former transits of this planet as establishing the sensible uniformity of the earth's rotation.

The planet Venus, by her transit in 1882, has attracted much attention, and much interest is felt as to the final outcome of the whole enormous mass of data, photographic and visual. Just how long we shall have to wait for the publication, seems still uncertain. I have already said, however, that probably these transits will never again be considered as important as hitherto.

The most important physical observations upon the planet during the decade seem to be those of Langley, who, during the transit of 1882, observed a peculiar, and so far unexplained, illumination of one point on the edge of the planet's disc, and those of Trouvelot and Denning, who have observed and figured certain surface markings of the planet. I think I may fairly mention also our Princeton observation of the spectrum of the planet's atmosphere during the transit, and our confirmation of Gruithuisen's old observation of a white cap (likely enough an ice-cap), at the edge of the planet's disc—probably marking the planet's pole, and showing that the planet's equator has no such anomalous inclination of 50° or 60°, as stated in some of the current text-books. This cap has also been observed by Trouvelot and Denning. But this lovely planet is most refractory and unsatisfactory as a telescopic object, apparently enveloped in dense clouds which mostly hide the real surface of the globe, and mock us with a meaningless glare.

We mention in passing, but without indorsement, the speculations of Houzeau, who has attempted to account for some of the older observations of a satellite to Venus, by supposing another smaller sister planet, "Neith," circling around the sun in an orbit a little larger than that of Venus, and from time to time coming into conjunction with it. But the theory is certainly untenable ; a planet large enough to show phases, as the hypothetical satellite is said to have done, in the feeble telescopes with which many of the observations were made one hundred years ago or more, would be easily visible to the *naked eye even.* There can be little doubt that all the Venus satellites so far observed are simply *ghosts* due to reflections between the lenses of the telescope, or between the cornea of the eye and all eye lens.

Mars.

But while Venus has gained no moons during the past ten years, Mars has acquired two, and they are both native Americans. There is no need to recount the faithful work of Professor Hall with the then new great telescope at Washington and its brilliant result; brilliant in a scientific sense, that is, for regarded as luminaries, it must be admitted that the Martial satellites, in spite of their formidable names of Phobos and Deimos, do not amount to much. Under the best of circumstances, they are too faint to be seen by any but keen eyes at the end of great telescopes. Small as they are, however, the little creatures punctually pursue the orbits which Hall has computed for them, and, when the planet came to its opposition a few weeks ago, they were found just in their predicted places. They are interesting, too, from the light they throw upon the genesis and evolution of the planetary system, almost compelling the belief that they have come *gradually* into their present relation to the planet. The inner one, Phobos, revolves around the primary in *7h* 39*m*, which is less than one-third of the planet's day. The theory of "tidal evolution," proposed by Professor G. H. Darwin in 1878-80, as the result of his investigations upon the necessary mechanical consequence of the tidal reactions between the earth, sun and moon will account for Phobos, and I know nothing else that will, though, of course, it would be rash to assert that no other account can ever be given.

Much attention has also been paid to the study of the planet's surface. In 1876 we were already in possession of three elaborate maps, by Proctor, Kaiser and Terby, agreeing in the main as to all the characteristic formations. In 1877, Schiaparelli, of Milan, detected, or thought he did, on the planet's surface a numerous system of "canals"—long, straight channels, some of them more than a thousand miles in length, with a pretty uniform width of fifty or sixty miles; and from his observations he constructed a new map, differing from the older ones somewhat seriously, though still accordant in the most essential features. His nomenclature of the seas and continents derived from ancient geography is certainly a great improvement on that of his predecessors, who had affixed to them the names of their friends and acquaintances among living astronomers. There has been some skepticism as to the reality of these "canals"; but in 1879 and 1881 they were all recovered by Schiaparelli, and several other observers, notably Burton, also made them out. Moreover, Terby finds from drawings in his possession that they had before been seen, though not understood or clearly recognized, by Dawes, Secchi, and other observers. At present, the balance of evidence is certainly in their favor, especially as the observers at Nice report seeing them last spring. I do not think the same can be said in respect to another observation of Schiaparelli's on the same object made in 1881. He then found nearly all of these canals—more than twenty of them—to be *double*, *i. e.*, in place of a single canal there were two—parallel and two or three hundred miles apart. No one else so far has confirmed this "gemination" of the canals; but the planet does not come to a really favorable opposition again until 1890 and 1892, when probably the question can be settled.

The time of rotation has during the past year been determined with great accuracy by Bakhuyzen, who has corrected some errors of Kaiser and Proctor, and finds it 24*h* 37*m* 22.66*s*. In 1876, there still remained some question as to the amount by which the planet is flattened at the poles. The majority of observers had found a difference between equatorial and polar diameters amounting to between $\frac{1}{100}$ and $\frac{1}{30}$, while, on the other hand, a few of the best observers had found it insensible. The writer in 1879 made a very careful determination, and found it $\frac{1}{219}$, a quantity closely agreeing with the theoretical value deduced by Adams as probable from the motion of the newly-discovered satellites.

The Asteroids.

On May 1st, 1876, the number of known asteroids was 163. To-day it stands at 258, 95 of these little bodies having been discovered within the decade, 45 of them by one man, Palisa, of Vienna, while our own Peters is responsible for 20.

None of the new ones are especially remarkable, *i. e.*, some of the older ones are always more so; the most inclined and most eccentric orbits, the longest and the shortest periods, none of them belonging to any of the late discoveries. One point is noteworthy, that the more recently discovered bodies are much smaller than the earlier ones. The first 25, discovered between May, 1876, and October, 1878, have an average opposition magnitude of 11.2, while the last 25, discovered since April, 1883, average only 12.2; *i. e.*, the first 25 average about 2½ times as bright as the last. Out of the whole 95, two are of the 9th magnitude (one of them, No. 234, was discovered as recently as August, 1883,) 14 are of the 10th, 33 of the 11th, 33 of the 12th, and 13 of the 13th. Of these last 13, 10 have been found within the past two years; and of the 12 others found within the same time, 6 are of the 11th magnitude, and 6 of the 12th.

It is clear that there can remain very few to be discovered as large as the 10th magnitude; but there may be an indefinite number of the smaller sizes.

The Major Planets.

As regards the planet Jupiter, the one interesting feature for the past ten years has been "the great red spot." This is an oval spot, some 30,000 miles in length by 6,000 or 7,000 in width, which first attracted attention in 1878. At first, and for three years, it was very conspicuous, but in 1882 it became rather faint, though still remaining otherwise pretty much unchanged. In 1885 it was partly covered with a central whitish cloud, which threatened to obscure in entirely; but this season the veiling cloud has diminished, and the marking is again as plain as it was in 1882 or 1883. How long it will continue no one can say; nor is there any general and authoritative agreement among astronomers as to its nature and cause.

In connection with observations upon this object, several new determinations have been made of the planet's rotation period, and they all show that, as in the case of the sun, the equatorial markings complete the circuit more rapidly than those in higher latitudes; a white spot near the equator gives 9*h* 50*m* .06*s*, as against 9*h* 55*m* 36*s*, for the red spot, which is approximately in latitude 30°.

We must not omit to mention Professor Pickering's new photometric method of observing the eclipses of this planet's satellites. Instead of contenting himself with observing merely the moments of their disappearance and reappearance—an observation not susceptible of much accuracy—he makes a series of rapid comparisons between the brightness of the waning or waxing point of light during the two or three minutes of its change, using as the standard one of the neighboring uneclipsed satellites. From these comparisons he determines the moment when the satellite under eclipse has just half its normal brightness ; and this with a probable error hardly exceeding a single second, while the old-fashioned method gave results doubtful by not less than a quarter of a minute. Cornu and Obrecht have independently introduced the same method at Paris. When we have a complete twelve years' series of such observations, they will give an exceedingly precise determination of the time required by light to traverse the earth's orbit, and so, indirectly, of the solar parallax.

As regards Saturn, there is nothing to report so startling as Jupiter's red spot. A white spot, which appeared in 1877, enabled Hall to make a new determination of the rotation period which came out 10*h* 14*m* 14*s*. This is in substantial accord with an earlier determination of W. Herschel's (10*h* 16*m* 07*s*), but involves a serious correction of the value 10*h* 29*m* 17*s* given in most of the text-books. The error probably came from a servile copying of a slip of pen made by some book compiler, fifty years ago or more, in accidently writing Herschel's value of the rotation of the inner ring, instead of that of the planet.

Much time has been spent in observations of the rings, and Trouvelot has reported a number of remarkable phenomena, most of which, however, he alone has seen as yet. The most recent micrometric measures have failed to confirm Struve's suspicion that the rings are contracting on the planet. Extensive series of observations have been made upon the satellites by H. Struve, Meyer, and others in Europe, and by Hall in this country. Hall's observations are especially valuable, and the series is now so nearly completed that we may soon hope to have most accurate tables. In the case of Hyperion, there is found a singular instance of a *retrograde* motion of the line of apsides of the orbit, produced by the action of an *outside* body, the effect being due to the near commensurability of the periods of Hyperion and Titan. This most peculiar and paradoxical disturbance first showed itself as an observed fact in Hall's observations ; and, soon after, Newcomb gave the mathematical explanation and development. He finds the mass of Titan to be about $\frac{1}{12800}$ that of Saturn. It may be noted, too, that Hall's observations of the motions of Mimas and Enceladus indicate for the rings a mass less than $\frac{1}{10}$ that deduced by Bessel; instead of being $\frac{1}{100}$ as large as the planet, they cannot be more than $\frac{1}{1000}$, and are probably less than $\frac{1}{10000}$.

The satellites of Uranus have also been assiduously observed at Washington, so that at present the Uranian system is probably as accurately determined as the Jovian, perhaps more so. The form of the planet has been shown to be decidedly elliptical (about 1-14) by observations of Schiaparelli and at Princeton; and the same observers have detected faint belts upon the disc, which have also been seen at Nice, and by the Henrys in Paris. Many of the observations appear to indicate a very paradoxical fact—that the belts, and consequently the planet's equator, are inclined to the orbits of the satellites at a considerable angle. The mathematical investigations of Tisserand appear to demonstrate that, in the case of a planet perceptibly flattened at the poles, satellites near enough to be free from much solar disturbance must revolve nearly in the same plane of the equator; while those more remote, and disturbed more by the sun than by the protuberant equator of the planet, must revolve nearly in the plane of the planet's orbit. Thus the two satellites of Mars, the four satellites of Jupiter, and the seven inner satellites of Saturn, all move nearly in the equatorial plane, while our moon and Japetus move in ecliptical orbits. It is very difficult to believe that the satellites of Uranus, which are certainly not ecliptical and are very near the planet, do not move equatorially. And yet it is unquestionable that most of the observations with sufficently powerful telescopes (my own among them) do seem to indicate pretty decidedly that the planet's equator is inclined as much as 15° or 20° to the orbit plane of the satellites.

As to Neptune, there is nothing new. One or two old observations of the planet have turned up in the revision of old star catalogues, and Hall, of Washington, has made a careful and accurate determination of the orbit of its one satellite, and of the planet's mass; while Maxwell Hall, of Jamaica, has deduced a very doubtful value of the planet's rotation from certain photometric observations of its brightness.

There has been some hope that a planet beyond Neptune might be found. Guided by certain slight indications of systematic disturbances in the motion of Neptune, Todd made an extended search for it in 1877–8, using the Washington telescope, and hoping to detect it by its disc, but without results. If such a planet exists, it is likely to appear as a star between the 11th or 13th magnitude, and may be picked up any time by the asteroid-hunters. But its slow motion and the fact that our present charts give but few stars below the 11½ magnitude, will render the recognition difficult.

The indications I have spoken of, and certain others first noted in 1880 by Prof. G. Forbes, and depending upon the behavior of certain periodic comets, furnish pretty strong reasons for believing in its existence, though as yet they fall far short of making it certain.

Comets.

During the past ten years we have been favored with an extraordinary number of comets, and while perhaps no single great step has been made, yet it is certain, I think, that our knowledge of these myterious objects has gained a real and considerable advance.

In 1876, curiously enough, not a single comet appeared; but in 1877 there were 6; in 1878,.3; in 1879, 5; in 1880, 5; in 1881, 8; in 1882, 3; in 1883, 2; in 1884, 3; and in 1885, 6; and so far this year, 3. Forty-four comets in all have been observed during the ten years, six of which were conspicuous objects to the naked eye, and two of them, the great comet of 1881, and the still greater one of 1882, were very remarkable ones.

The first of these will always be memorable as the first comet ever photographed. Dr. Henry Draper photographed both the comet itself and its spectrum; Janssen obtained a picture of the comet, and Huggins of its spectrum.

A number of excellent photographs were obtained of the great comet of 1882, especially by Gill, at the Cape. And it is worth mentioning that in May, 1882, a little comet (not included in the preceding list, because no observations were obtained of it) was caught upon the photographs of the Egyptian eclipse.

Two of the bright comets, Wells' comet of 1881 and the great comet of 1882, approached very close to the sun, and their spectra, as a consequence, became very complex and interesting. A great number of bright lines made their appearance. Sodium was readily and certainly recognized; iron and calcium probably, but not so surely. The evidence as to the nature of the sun's corona, derived from the swift passage of the 1881 comet through the coronal regions, has already been alluded to.

The Pons-Brooks comet of 1883–4 is extremely interesting presenting the first instance (excepting Halley's comet, of course) of one of the Neptunian family of comets returning to perihelion. There are six of these bodies with periods ranging from sixty-eight to seventy years. Halley's comet, the only large one of the group, has made many returns, and is due in 1910. Pons' comet, first observed in 1812, has now returned; Olbers' comet of 1815 is due in 1889, and the three others, all of them small, in 1919–'20 and '22.

I have spoken of them as Neptunian comets, *i. e.*, their presence in our system is known to be due in some way to this planet. The now generally received theory is that they have had their orbits changed from parabolas into their present state by the disturbing action of Neptune. Mr. Proctor has pointed out certain unquestionable, though, I think, inconclusive, objections to this view, and he proposes, as an alternative, the startling and apparently improbable hypothesis, that they have been *ejected* from the planet at some past time by something like volcanic action.

On the whole, however, the most important work relating to comets has been that of the Russian astronomer, Bredichin. He has brought the mechanical and mathematical portion of the theory of comets' tails to a high degree of perfection; following out the lines laid down by Bessel, but improving and correcting Bessel's formulæ, and determining their constants by a most thorough discussion of all the accurate observations available.

It is hardly possible to doubt any longer that all the facts can be represented on the hypothesis that the tails are composed of minute particles of matter, first driven off by the comet and then repelled by the sun. Bredichin's most interesting result, arrived at in 1878, is that the tails appear to be of three, and only three, distinct types—the long straight streamers which are due to a repulsive acceleration about twelve times as great as the sun's attraction ; the second and most ordinary class of broad-curved tails for which the repulsive force ranges between one and two and a half times that of attraction ; and finally, the short, stubby brushes which are found in a few cases, and correspond to a repulsive force not more than one-fourth the sun's attraction. Supposing as he does, that the *real* repulsion is the same for each atom, the *apparent* repulsion, or repulsive *acceleratiou*, would be greater for the lighter atoms and nearly inversely proportional to their molecular weights ; and so he concludes that probably tails of the first type are composed of hydrogen, those of the second type of hydro-carbons, like coal gas, and those of the third, of iron and its kindred metals. As to the second type, the spectroscope speaks distinctly in confirmation. Tails of the first and third types are not common, and are usually faint, and since Bredichin's result was announced, there has been no opportunity for spectroscopic verification in their case.

I said his investigations had given a mathematical and mechanical explanation of comets' tails ; but the *physical* question as to the nature of the force which causes the observed repulsion, remains unsettled, though I think there is no doubt that general opinion is crystallizing into a settled belief that it is electrical ; that the sun is not at the same electric potential as surrounding space, and that, in consequence, semi-conducting masses of pulverulent matter, such as comets seem to be, are subject to powerful electric forces as they approach and recede from the central body. At the same time there are those—Mr. Ranyard, for instance—who forcibly urge that the direct action of the solar *heat* might produce a similar repulsive effect by causing rapid evaporation from the front surface of minute particles, charged with gases and vapors, *frozen* by the cold of outer space.

I ought not to dismiss the subject of comets without at least alluding to the numerous unprecedented and interesting phenomena presented by the great comet of 1882: First, its unquestionable relation to, but distinctness from, its predecessors of 1880, 1843, and 1668, the three belonging to one brotherhood, of common origin, and all following nearly the same path around the sun. I call special attention to this point because Miss Clerke, in her new and admirable "History of Astronomy in the Ninteenth Century" (which I hope every one interested in astronomy will read as soon as may be) has, I think, made a mistake regarding it, assigning to the difference between the computed periods of these comets much too great an importance.

The strange elongation of the nucleus of this comet into a string of luminous pearls; the faint, straight-edged beam of light that enveloped and accompanied the comet for some time; and the several detached wisps of attendant nebulosity that were seen by several observers, are all important and novel items of commetary history.

Meteors.

Time will not allow any full discussion of the progress of meteoric astronomy. It must suffice to say that the whole course of things has been to give increased certainty to our newly acquired knowledge of the connection between meteor-swarms and comets, and to make it more than probable that a meteoric-swarm is the result of the disintegration and breaking up of a comet. This seems to be the special lesson of the Bielids, the reappearance of which as a brilliant star shower last November attracted so much attention. In an important paper read before the National Academy of Sciences, last April, Professor Newton pointed out how all the facts connected with the division into two of Biela's comet forty years ago, its subsequent movements and disappearance, and the meteoric showers of 1872, and 1885, and especially the peculiar features of this last shower, all conspire to enforce this doctrine.

I mention, doubtfully, in this same connection the recent supposed discovery by Denning of what are generally alluded to as "long radiants:" systems of meteors, *i. e.*, which for weeks, and even months together, seem nightly to emanate from the same point in the sky. One of these radiants, for instance, the first of half a dozen described by Mr. Denning, is about 1½° north of β Trianguli, and the shower appears to last from July to November, at the rate of perhaps one or two an hour.

If the fact is *real*, it follows inevitably that, disseminated through all the space in which the earth is moving, and has been moving for several years—not less than 1,000,000,000 miles—there are countless meteoroids moving in parallel lines, and with a velocity so great that the earth's orbital motion of nineteen miles a second is absolutely insignificant as compared with theirs. Their speed must be many hundreds of miles per second. This may be true, but I own I am not ready to accept it yet. The observations indicate directly no extraordinary swiftness. Mr. Proctor, whose mind appears at present to be chiefly occupied by the idea that suns and planets are continually bombarding their neighbors (or at least do so at some stage of their existence), ascribes such meteors to the projectile energies of some of the "great" stars. But there is not time to discuss his notion, and it is hardly necessary until it has begun to receive somewhat more extensive acceptance. I am not aware that so far he has any converts to his theory of comets and meteors.

Stars.

Want of time will also prevent any adequate treatment of the recent progress of Stellar astronomy.

Two great works in the determination of star places must, however, be mentioned. One is the nearly completed catalogue of all the northern stars, down to the ninth magnitude, begun almost twenty years ago, under the auspices of the Astronomische Gesellschaft, by the co-operation of some fifteen different observatories. The observations are now nearly finished, and several of the observatories have already reduced and published their work. A very few years more ought to bring the undertaking to a successful end.

Another similar work, almost, though not quite, as extensive, is the great catalogue of southern stars, made at the observatory of Cordova by our own Dr. Gould and his assistants. He himself, with his own eyes, observed every star of the whole number—nearly 80,000—his assistants reading the circle and making the records: and the whole has been reduced, printed, and published within the space of twelve years—a veritable labor of Hercules, for which, most justly, our National Academy has awarded him the Watson medal. He had already, some years ago, received the gold medal of the English Royal Astronomical Society, for the *Uranometria Argentina*, an enumeration of all the naked-eye stars of the southern hemisphere, with their approximate positions and estimated magnitudes. This, however, was only a sort of preliminary by-play, to pass the time while waiting for the completion of his observatory and meridian circle.

We must mention also the remarkable star-charts made by Dr. Peters, of Hamilton College, of which he has already published and distributed at his own expense about twenty, and more are soon to follow.

But the old-fashioned way of cataloguing and charting the stars is obviously inadequate to the present needs of astronomy, and a new era has begun. While, hereafter, as hitherto, the principal stars, several hundred of them, will be observed even more assiduously and carefully than ever before, with the meridian circle or similar instruments, the photographic plate will supersede the eye for all the rest. It is now easily possible to photograph stars down to the thirteenth or fourteenth magnitude, and to cover a space of 2½° square on a single plate. The remarkable thirteen and one-half inch instrument constructed by the Henry Brothers, for the Paris observatory, and first brought into use last August, does this perfectly. Instruments very similar, but smaller, lately set up at Harvard College, at the Cape of Good Hope, and at Liverpool, while they do not reach so faint stars, cover more ground at a time.

Negotiations are already under way to secure the co-operation of a number of observatories for a photographic survey of the heavens; and it is probable that, after some preliminary consultation and before very long, it will be actually in progress. According to Struve's estimates, it could be accomplished in about ten or twelve years, even on the Paris scale, by the combined efforts of fourteen or fifteen establishments. Orders have already been given to the Henry Brothers, by Dom Pedro, of Brazil, and Mr. Common, of England, for instruments precisely like the one at Paris. Americans, and New Yorkers especially, may well take a peculiar interest in astronomical photography, since it was at Cambridge, in 1861, that the first star-photographs were ever made, and here in New York, Rutherfurd and Draper were among the earliest and most successful workers; in the observatory above us is now mounted the very instrument with which Rutherfurd made his unrivalled pictures of the moon and his plates of the Pleiades, more than twenty years ago.

During the past ten years, stellar photometry has become almost a new science. Its foundations, indeed, were laid by J. Herschel, Seidel, Wolff, and Zollner, before 1870, and the magnitudes of some two hundred stars had been measured, and the law of atmospheric absorption determined. But the great work of Pickering, at Harvard, in the invention and perfecting of new instruments, and his Harvard photometry, which gives us a careful measurement of the brightness of all the naked-eye stars of the northern hemisphere, marks an epoch. And he is pushing on, and has already well under way the measurement of the 300,000 stars of Argelander's Durchmusterung. Nor must we omit to mention Pritchard, of England, whose name has just been joined with Pickering's by the Royal Astronomical Society, in the bestowal of their gold medal for his wedge-photometer and the photometric work done with it. The Harvard photometry, and the Uranometria Oxoniensis together will carry down to all time the record of the present brightness of the stars. They will be especially valuable as data for determining changes in stellar brilliancy.

During the past ten years the number of variable stars has risen from about 100 to nearly 150; and our knowledge of their periods and light-curves has been greatly improved. In America, Chandler and Sawyer, of Boston, and Parkhurst, of this city, have done especially faithful work. During the ten years we have had two remarkable "temporary stars," as they are called—first the one which, in November, 1876, in the constellation of Cygnus, blazed up from the ninth magnitude to the second and then slowly faded back to its former brightness, but to a *nebulous* condition, as shown by its spectrum. Then also the one which, last autumn, appeared in the heart of the nebula of Andromeda as of the sixth magnitude (where no star had ever been seen before), slowly dwindled away, and is now beyond the reach of any existing telescope. Perhaps, too, we ought to mention another little ninth magnitude star in Orion's club, which last December rose to the sixth magnitude, and is now fading; it seems likely, however, from its spectrum, that this is only a new variable of long period.

As to star-spectra, a good deal of work has been done in their investigation with the ordinary stellar spectroscopes by the Greenwich Observatories, by Vogel at Potsdam, and by a number of other observers,—work well deserving extended notice if time permitted. But the application of photography to their study, first by Henry Draper in this city, and by Huggins in England, is the important new step. By the liberality of Mrs. Draper, and as a memorial of her husband, his work is to be carried on with the new photographic instrument and method just introduced by Prof. Pickering at Cambridge. He is able to obtain on a single plate the spectra of all the stars down to the eighth magnitude in the group of the Hyades, each spectrum showing under the microscope the characteristic lines quite sufficiently for classification. A different instrument is also to be built with the Draper fund, which will give single star-spectra on a much larger scale and in fuller detail.

During the decade, the stellar parallax has been worked at by a number of observers. Old results have been confirmed or corrected, and the number of stars whose parallax is determined has been more than doubled. The work of Brunnow and Ball in Ireland, of Gill and Elkins at the Cape of Good Hope, and of Hall at Washington, deserves especial mention. A new heliometer of seven inches aperture has been ordered for the Cape observatory, and when it is received, a vigorous attack is planned by co-operation between that observatory and that of Yale College, which possesses the only heliometer in America.

During the ten years, our knowledge of double stars has been greatly extended; several observers, and most eminent among them Burnham, of Chicago, have spent much time as hunters of these objects, and have bagged between one and two thousand of them. Several others, especially Dobereck in England, and Flammarion in France, have devoted attention to the calculation of the orbits of the binaries, so that we have now probably about seventy-five fairly well defined.

In the study of the nebulæ, less has been done. Stephan at Marseilles and Swift at Rochester have discovered many new ones, mostly faint, and Dreyer, of Dublin, has published a supplementary catalogue, which brings Sir J. Herschel's invaluable catalogue pretty well down to date. The studies of Holden upon the great Orion nebula and the so-called "trifid nebula" deserve special mention, as securely establishing the fact that these objects are by no means changeless, even for so short a time as twenty or thirty years; also the discovery of a new nebula in the Pleiades by means of photography.

Observatories.

During the ten years, a considerable number of new observatories have been founded. Abroad we mention as most important the observatories for astronomical physics at Potsdam, in Prussia, and at Meudon, in France, also the Bischoffsheim observatory at Nice and its succursal in Algiers. The great observatory at Strasburg can hardly be said to have been founded within the period indicated, but the new buildings and new instruments and new efficiency date since 1880. We ought not to pass unnoticed the smaller observatory at Natal, in South Africa, and the private establishments of von Konkoly at O-Gyalla, of Gothard at Hereny (both in Hungary), and of the unpronounceable gentleman Jedrzejewicz at Plonsk, in Poland, and the observatory at Mount Ætna, from which, however, we have no results as yet.

In the United States we have the public observatories at Madison, Wis., at Rochester, N. Y., and at the University of Virginia, and the, as yet, unfinished Lick Observatory in California; also a host of minor observatories connected with institutions of learning, and mainly designed for purposes of instruction; such establishments have been founded within ten years at Princeton, at Northfield, Minn., at South Hadley, Ms., at Beloit, at Marietta, at Depauw, at Nashville, and at St. Louis, also at Franklin and Marshall College, and at Doane College, in Nebraska; at Columbia College, Ann Arbor and Madison, Wis., and at one or two other institutions which escape me for the moment. Several others are also at this moment in process of erection. Every one of them has a telescope from six to thirteen inches aperture, with accessory apparatus sufficient, in the hands of an astronomer, for useful scientific work.

Instruments.

A large number of new instruments of great power have been constructed. We mention the great thirty-inch refractor of Pulkowa, the twenty-six-inch of Charlottesville, and the twenty-three-inch at Princeton, for all which the lenses were made by our own Clark. We add the great Vienna twenty-seven-inch telescope by Grubb, and the twenty-nine-inch object glass by the Henrys, made for the Nice observatory but not yet mounted; also the nineteen-inch telescope at Strasburg by Merz. Grubb has also at present a twenty-eight-inch object glass under way for the Greenwich observatory, and Clark has nearly completed the monstrous thirty-six-inch lens for the Lick observatory. There never was a decade before when such an advance in optical power has been made. Great *reflectors* have been scarce, the only ones of much importance constructed during the time being the twenty-inch instrument at Algiers, and Mr. Common's exquisite three-foot telescope, which he has lately sold to Mr. Crossly in order to make way for one of five feet diameter now, I believe, under construction. The old three-feet and six-feet instruments of Lord Rosse have been improved in various ways, and are still in use—especially in work upon lunar heat. Among newly *invented* instruments we mention the meridian photometer of Pickering, the wedge photometer of Pritchard, the almucantar of Chandler, the concave diffraction grating of Rowland, and the bolometer of Langley—all but one American. Repsold's improvements in the micrometer, in the heliometer, and in the mounting of equatorials should also be mentioned here.

As to new astronomical methods, enough has been already said about photometry and astronomical photography. It is plain that we are entering upon a new era.

Literature.

Astronomical literature has flourished. Among the books of the past ten years, important in one way or another, I mention in the first rank the great work of Oppolzer upon orbit calculation, Gylden's "Astronomy," and the papers of Tisserand, Neison, Darwin, Adams, Hall and Newcomb, on numerous subjects. Among the popular books on general astronomy we have Newcomb's "Popular Astronomy," Ball's "Story of the Heavens," Kaiser's "Sterrenhemel," Fayes' "Origine de la Monde," and Miss Clerke's admirable "History of Astronomy in the Nineteenth Century." More special popular treatises are Nasmyth's and Neison's books upon the moon, Lockyer's "Solar Physics," and my own little book upon the sun, Ledger,s "Sun, Moon and Planets," Gledhill's and Flammarion's books on double and binary stars, and Terby's "Areographie." Of course, it is possible to mention only a few, and I name those which, in one way or another, have attracted for some reason my special attention, leaving doubtless many others just as valuable unreferred to.

A few new astronomical periodicals have sprung up. In England, *The Observatory* was founded in 1877, and has become an established and very valuable publication.

Copernicus was a still more important and elevated journal, but did not appeal to so large a circle of readers, and, I am sorry to say died only three years old.

In France, the *Bulletin de l'Astronomie*, recently established, is extremely valuable, and I trust will be able to maintain itself. Less importance attaches to Flammarion's *l'Astronomie* which, however, I presume, has many more readers. *Ciel et Terre* is a new astronomical magazine published at Brussels.

In the United States, we have but one distinctively astronomical journal, the SIDEREAL MESSENGER, published by the energetic young director of the Carleton College Observatory in Minnesota. It is interesting and, in many ways, excellent, but in some respects not yet quite up to the standard of American astronomy. There is room and need among us for an astronomical journal of high mathematical character ; but its financial success would be questionable.

Necrology.

The ten years have stricken from the roll of astronomers a few illustrious names, and many of honorable rank. Leverrier, the greatest of them all, died in 1877, and Secchi in 1878 ; Lamont and Maclear in 1879 ; Peters, the vetern editor of the *Astronomische Nachrichten*, Lassel, Dembowski, Pierce and Watson were taken in 1880. Bruhns died in 1881. In 1882 we lost Zoellner, Plantamour, Challis, and Henry Draper. Villarceau died in 1883; Klinkerfues and Schmidt in 1884, and last year Webb, the author of that *vade mecum* of all amateur astronomers, the "Celestial Objects for Common Telescopes."

It would probably be invidious and unwise to attempt to designate precisely those of our younger astronomers who are to succeed to the eminence of those we have lost. It will be easier to prophesy after the fact. But one cannot go wrong in saying that among the astronomical names which have either first appeared, or have first become conspicuous, during the past decade, we ought to mention, in our own country, Pickering, Holden, Langley, Stone, Burnham, Boss, Chandler, Pritchett, Todd, Paul, Payne, and Elkin. In Europe, we have Gill, Darwin, Common, Gledhill, Tisserand, Vogel, Palisa, Hasselberg, H. and L. Struve, Hartwig, Valentine, and Von Konkoly. And there are many others, both here and abroad, hardly, if at all, their inferiors.

In this rapid, though, I fear, tedious review, I have tried to put before you a just and fairly proportioned sketch of the progress that has actually been made. While no great discovery like that of gravitation appears upon the record, yet I am inclined to think that, with one or two exceptions (during the life of Galileo and Newton), no other decade in all the history of our unselfish science can make a better showing.

As an American, too, I have been surprised and delighted to find how honorable a place our American astronomers hold in the record. Take out of the ten years' story the works of Hall and Newcomb and Gould, of Draper, Langley and Pickering, of Burnham, and Holden, and Stone, and the loss would indeed be grievous.

EDITORIAL NOTES.

Our Happy New Year greeting to all readers, old and new! Though much is in mind, we will be pardoned if we say little this time, in order that we may give as a whole, at once, Professor C. A. Young's excellent review of the progress of astronomy during the last ten years.

This journal begins its sixth volume with more encouragement and a brighter hope for the future than ever before; and still its management is pleased to ask all who think it has merit, to call the attention of their friends interested in astronomy to it. Its size and value shall be enhanced as its list of subscribers is increased.

An article of unusual interest, entitled *The Motion of the Lunar Apsides*, by E. Colbert, of Chicago, is already in type for our our next issue. Some of its analytical features will attract the attention of mathematical astronomers.

President Holden, of the University of California and Director of the Lick Observatory, has recently visited Messrs. Warner & Swasey, of Cleveland, Ohio, who have the contract to mount the great 36-inch refractor for the observatory and to build its mammoth dome.

Comet of Finlay.—This comet seems to trouble the mathematicians more than it does the observers. In A. N., No. 2,754, Professor Brunnow expresses the opinion that it is probably identical with that of de Vico, 1844, I, the orbit of which he himself computed at that time, making its period about 5.47 years. If the two comets are identical, he thinks it probable that the above-named period was wrongly obtained in consequence of great changes going on in the form of the comet during the time of observation. With a period of 5.5 years there could be no planetary disturbance outside of the year 1885, and the present early perihelion passage would seem to make even that period impossible. If a period of 5.26 years were assumed the disturbance of Jupiter in 1873 would be taken into account, but a less period than this would be necessary to account for its appearance in 1844, on the supposition of eight revolutions in the meantime. When Professor Brunnow communicated his views to A. N., Professor Boss had published neither of his two orbits of this comet which have since appeared. The inclination and period of the first were respectively 2° 50′ and 4.32 years; of the second, 3° 1′ and 6.5 years. Computation by ordinary methods, with ordinary data, where the inclination of the orbit is small, makes the period uncertain. Elements by Dr. Krueger in *Observatory* are nearly the same as those given last above. Rev. G. M. Searle has also computed an orbit on the supposition of a period of 5.278 years, the average of eight revolutions since 1844, without planetary disturbance, and finds elements fairly in accord with others. Professor Boss thinks the period cannot be less than six years, and that probably the comet has made seven revolutions instead of eight since 1844. It was in the constellation of *Pisces*, December 31, and one degree south of the equator, moving northeast. Though slowly growing faint, it will be visible in the telescope possibly until February.

List of Nebulæ from the McCormick Observatory.—In our catalogue of nebulæ, published in the Astronomical Journal, No. 146, it was found, after our copy had gone to press, that No. 6 was identical with Swift's nebula No. 2, catalogue No. 4. No. 146 was also found to be G. C. 998. Its right ascension in the General Catalogue is probably 2*m* too small, that given in the note as brought up from C. H. being right. Swift No. 83, catalogue No. 4, appears to be identical with G. C. 6040.

University of Virginia, Nov. 30, 1886. ORMOND STONE.

Tracing the Fixed Stars.—Professor Elias Colbert, of Chicago, former Superintendent of Dearborn Observatory, has done real service for teachers and students of elementary astronomy, in preparing a little book entitled "The Fixed Stars; Maps for Out-Door Study," published by George Sherwood, of Chicago. This little book is an easy introduction to the study of the heavens, in showing only the most prominent stars in their relative positions, arranged in groups according to the common divisions of the sky called constellations. Its twelve full page maps give all the stars, down to the fifth magnitude, and also a few stars belonging to the fifth magnitude.

The stars of the several groups are connected by right lines so as to distinguish groups, and no attempt is made to reproduce the ordinary figures of the star map. The stars given are properly designated by letters and symbols as they appear in standard star-charts.

We do not know of a more condensed and convenient little hand-book of the stars than this for the student, teacher or pracat the larger telescopes. It was certainly three seconds of time from the time of the star being at the border and the time of disappearance. In the small telescope G. F. D. lost the star when it touched the moon's limb.

The following are the observed local sidereal times of the observations:

Immersion at the bright limb of the moon,—

Observers, G. D.=23*h* 22*m* 50.73*s*.
C. B. H.=23*h* 22*m* 50.70*s*.

Emersion at the dark limb of the moon,—

G. D. =00*h* 18*m* 59.10*s*.
C. B. H.=00*h* 18*m* 59.27*s*.

The moon being one day past the full there was a narrow, dark edge of the moon where the star reappeared,which it did with the suddenness and brightness indicating no gradual emergence.

At the Chabot Observatory, at Oakland, Mr. Charles Burchhalter, using the eight inch Clark equatorial observed the immersion and emersion at the following local sidereal times:

Immersion,—

C. B.=23*h* 23*m* 28.8*s*.

Emersion,—

C. B.=00*h* 19*m* 45.5*s*.

The conditions attending his observation of the immersion were nearly the same as those already mentioned, but the star apparently did not advance so far on the moon's disc, and in his telescope some of the rays of the star projected outside the moon's limb.

Prof. Davidson then explained a similar circumstance that exhibited itself when he observed the occultation of α Scorpii at the bright limb of the moon in daylight, as far back as 1848 when Prof. Bache failed to elicit from him any explanation of the phenomenon. A second occultation of α Scorpii was referred to a having been made at the bright limb during his work in Puget Sound, with almost identical phenomena. But subsequent experience in observing occultations, eclipses, etc., and in continuous work in goedetic observations convinced him that the peculiarity mentioned, the Baily's beads of total solar eclipses, the ligament and the black drop of the transits of Venus and Mercury, and similar phenomena, depended wholly and solely upon the unsteadiness of our atmosphere arising from the irregularities of refraction through strata of abnormal temperatures causing objects to exhibit spurious discs. With these convictions and his experience at great elevations he has for many years urged the occupation of high mountain peaks for astrophysical research and observation.

The following are the geograghical positions of the observation stations:

Davidson Observatory, Latitude 37° 47′ 24.75″ north.
Longitude 122° 25′ 40.5″ west.
Chabot Observatory, Latitude 37° 48′ 05.0″ north.
Longitude 122° 16′ 34.4″ west.

The observers, G. D.=George Davidson, C. B. H.=Chas. B. Hill, G. F. D.=G. Fauntleroy Davidson. The immersion was observed with two three inch Iraunhofers, power 105; emersion by Hill, same; by Davidson 6.4 inch Equatorial, Clark, power 150.

GEORGE DAVIDSON.

BOOK NOTICES.

Johnson, J. B.: The Theory and Practice of Surveying, pp. 683, 9½x6½. New York, John Wiley & Sons.

Prof. Johnson is the teacher of Civil Engineering at Washington University, St. Louis, and has been employed on both the U. S. Lake and Mississippi River surveys. The book is intended to give a modern and scientific treatment of the subjects of most value to American surveyors, special emphasis being put on directions for field-work. Book I containing 171 pages is devoted to the description of over twenty different instruments, with directions for their adjustment, and exercises in their use. Book II treats of the ordinary topics of Land, Railroad and Mining Surveying, and also contains valuable chapters on Topographical, Hydrographic, City and Geodetic Surveying. Railroad Surveying is very inadequately treated in a dozen pages. The chapter on Geodetic Surveying is necessarily very elementary, and is confined chiefly to field operations. Some of the tables at the end of the book are well printed, but the table of four-place logarithms of natural numbers, and the logarithmic traverse table are to be read with a magnifying glass; the tables of trigonometric functions are trying to the eye, and should have occupied twice as much space. A teacher of surveying, who is not a civil engineer, will find in this book much useful and interesting information.

Gore, J. H.: Elements of Geodesy, pp. 282, 9½x6½. New York, John Wiley & Sons.

This work is not intended as a handbook for the professional geodesist. Its aim is to give teachers and advanced students a clear idea of the observations and measurements made in a geodetic triangulation and their reduction by the method of Least Squares. In addition there is a historic sketch of geodetic operations, and a short discussion of the figure of the earth, which is prefaced by a chapter on the development of the formulas for latitudes, longitudes, and azimuths. At the end of each chapter is given a list of books bearing on the subject of the chapter. Some may think that the author has been excessively simple in his derivation of formulæ, but the busy reader can forgive that. It is the opinion of the reviewer that both author and publisher have done their work exceptionally well, and that the book fills its niche admirably.

Elements of Analytic Geometry. By G. A. Wentworth, A. M., Professor of Mathematics in Phillips Execter Academy. Boston: Messrs. Ginn & Company, publishers, 1886.

Like other books in the Wentworth series of Mathematics, this is written by a teacher who evidently knows the needs of a beginner in analytic geometry. The author has not tried to produce either a treatise or a reference book in this most interesting theme of the pure mathematics, but he has presented in a plain, direct and logical way the essentials for elemental study for a judicious start in independent thinking without the aid of the calculus. He has had a definite object in mind, and he has accomplished it well. The usual topics are presented with a variety of exercises under each aptly chosen to illustrate the principles and fix them in the mind. That the student, on first sight, may recognize the important formulæ, they appear in different type. This new book sustains the well-earned credit of the series in which the publisher's handsome work forms no small part.

The Sidereal Messenger,

CONDUCTED BY WM. W. PAYNE,

Director of Carlton College Observatory, Northfield, Minnesota.

VOL. 6, NO. 2. FEBRUARY, 1887. WHOLE NO. 52

MOTION OF THE LUNAR APSIDES.

E. COLBERT.*

The motion of the lunar orbit has long been a vexed question with the mathematicians. The following method of reconciling the theory with observation is novel, and perhaps will be accepted as conclusive:

It may be mentioned, incidentally, that the motion of the lunar apsides has for the last two hundred years been a stumbling block. NEWTON tried to account for it on the gravitation theory, but left it with the remark, "*Apsis lunæ est duplo velocior circiter*" (the motion is about twice as great as this). CLAIRAUT showed, in 1750, how to account theoretically for the other half, but the attempt to reduce the equations to a numerical form still left a residual, and when LAPLACE attacked the problem he was only able to make the theory responsible for 444 parts out of 445. It has been attempted to bridge over the difficulty by adding $2 \sin^2 \frac{1}{2}\gamma \,.\, d\Omega \div dt$ to the motion of the perihelion; which in the case of the moon is practically equal to $4 \sin^4 \frac{1}{2}\gamma$, because $d\Omega \div d☽ \;=\; \frac{1}{2} \sin^2 \gamma; \;\; =.\; 2 \sin^2 \frac{1}{2}\gamma$, nearly. It will be observed that this quantity is not needed in that shape, neither is the existence of a second moon required to account for the perigeal motion. I may not be familiar with *all* the literature of the subject, but believe that the outstanding residual has not hitherto been eliminated by any investigator; and note a recent remark by G. W. HILL to the effect that it is not probable the perigeal motion will ever be accounted for by theory so closely as it can be obtained by a comparison of observations.

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* Formerly Superintendent Dearborn Observatory.

If $1 - c$ represent the perigeal motion divided by that of the moon, then c^2 and $(1 - c^2)$ are the squares of two sides of the right-angled triangle the hypothenuse of which is unity; and $\sqrt{1 - c^2}$ is the perturbation of the radius vector. (This is not new.)

The quantity $1 - c^2$ comprises a radial, which involves r^3: a tangential, depending on the square of the velocity in the orbit, involving r^4; and one that originates in the displacement, being really a perturbation of the perturbation. The last is usually treated as a single quantity, namely as a function of r^4. It is more philosophical to regard it as furnishing a multiple for each of the other two instead of being a quantity simply additive. Also, for obtaining the mean motion of the apsides, it is sufficient to derive the constant portion of each function considered, being what we shall here call the "Average Value" of the quantity:

With g the mean anomaly, e the eccentricity, r the radius vector, and a the semi-axis major, we have the following extension of a well-known equation:

$$
\begin{aligned}
\frac{r}{a} = 1 + \frac{e^2}{2} \\
&\left(-e + \frac{3}{8}e^3 - \frac{5}{3 \cdot 8^2}e^5 + \frac{7}{18 \cdot 8^3}e^7\right)\cos g \\
&\left(-\frac{3}{8}e^3 + \frac{5 \cdot 3^2}{2 \cdot 8^2}e^5 - \frac{7 \cdot 3^4}{10 \cdot 8^3}e^7\right)\cos 3g \\
&\left(-\frac{5^3}{6 \cdot 8^2}e^5 + \frac{7 \,.\, 5^4}{18 \cdot 8^3}e^7\right)\cos 5g \\
&\left(-\frac{7^5}{90 \cdot 8^3}e^7\right)\cos 7g \\
&\left(-\frac{e^2}{2} + \frac{e^4}{3} - \frac{e^6}{16} + \frac{e^8}{180}\right)\cos 2g \\
&\left(-\frac{e^4}{3} + \frac{2}{5}e^6 - \frac{56}{315}e^8\right)\cos 4g \\
&\left(-\frac{27}{80}e^6 + \frac{81}{140}e^8\right)\cos 6g \\
&-\frac{128}{315}e^8 \cos 8g
\end{aligned}
$$

And an inversion of this series gives the following, which it is not necessary to carry out beyond the sixth power:

$$\begin{aligned} \frac{a}{r} = 1 &+ \left(+ e - \frac{e^3}{8} + \frac{e^5}{192}\right) \cos g \\ &\left(+ e^2 - \frac{e^4}{3} + \frac{3}{8} e^6\right) \cos 2g \\ &\left(+ \frac{9}{8} e^3 - \frac{81}{128} e^5\right) \cos 3g \\ &\left(+ \frac{4}{3} e^4 - \frac{16}{15} e^6\right) \cos 4g \\ &+ \frac{625\ e^5}{16 \cdot 24} \cos 5g \\ &+ \frac{203\ e^6}{120} \cos 6g \end{aligned}$$

Raising each of these expressions to the required powers, and omitting all that is periodical, we have as "average values":—

For $\frac{r}{a}$: $1 + \frac{e^2}{2}$

" $\frac{r^2}{a^2}$: $1 + \frac{3}{2} e^2$

" $\frac{r^3}{a^3}$: $1 + 3 e^2 + \frac{3}{8} e^4 + \frac{1}{8^2} e^6$, etc.

" $\frac{r^4}{a^4}$: $1 + 5 e^2 + \frac{15}{8} e^4 + \frac{14}{9 \cdot 8^2} e^6$, etc.

" $\frac{a}{r}$: 1

" $\frac{a^2}{r^2}$: $1 + \frac{e^2}{2} + \frac{3}{8} e^4 + \frac{15}{48} e^6$

" $\frac{a^3}{r^3}$: $1 + \frac{3}{2} e^2 + \frac{15}{8} e^4 + \frac{7}{4} e^6$

" $\frac{a^4}{r^4}$: $1 + 3 e^2 + \frac{45}{8} e^4 + \frac{35}{4} e^6$.

For the solution of the problem we take the following as the most probable values of the quantities named. They are deduced from the figures given by **NEWCOMB**, in 1879, in his paper on the recurrence of solar eclipses. The epoch chosen is A.D. 1800. The processes of the subsequent computation are given, as they may be of use in verification; and all of the logarithms have been computed closely enough to secure accuracy in the last figure of the result as here presented. The mark ' following some of the numbers or logarithms indicates that the next succeeding figure would be nearly 5. The same mark inverted, thus, ‚ shows that the given value is too great by half a unit, or nearly so, in the right hand place:

		Logarithms.
⊙'s daily motion,	3548″.1927904	3·55000 72091
Sidereal year, days,	365 ·2563647	2·56259 77924
☽'s synodical rev. days,	29 ·53058844	1·47027 21009'
Sidereal rev. days,	27 ·32166120	1·43650 71016
☽'s daily motion,	47434″.890233	4·67609 78999'
☽'s — ⊙'s daily motion,	43886″.697443	4·64233 29006
☽'s π, daily motion,	400″.9187565	2·60305 63747
Half square ratio sidereal periods;		
	= (1 ÷ 357·447).	97·44678 86227
	358·447 ÷ 357·447	0·00121 32943
Nominal perturbation;	$\frac{m}{2}$ = 0·00278 98145	97·44557 53284

Then for $e_,$, the eccentricity of the earth's orbit, with $e_, = 0·01679228$, we have by a preceding formula:

$(a_, \div r_,)^3$ = 1·00042 31202 0·00018 37199

and taking an approximate value for ☽'s e, with γ = about 5° 8′ 40″.6 we obtain E, the perturbation of the perigeal motion due to the earth's elliptical figure, as follows:

Constant of precession (JULIAN) =	54″.94625		1·73993 805
Obliquity, (1800) = 28° 27′ 54″.8		cos	9·96251 23'
	50″.40230		1·70245 04
Daily soli-lunar	0″.1379940		9·13986 02
☽'s $m \div a^3$ × ⊙'s $a_,^3 \div m_,$			0·33673 10
☽'s $(a \div r)^3$	1·00453 806		0·00196 64
$1 - (3 \div 2) \sin^2 \gamma$			9·99473 02‚
	2·154902		0·33342 75
⊙'s $(a_, \div r_,)^3$	1·000423		
Sum of ☽ and ⊙ =	3·155325		0·49904 41
(3 ÷ 2) seconds sidereal arc in solar day,			6·28988 37
(Solar days in sidereal year)^2		a. c.	94·87480 44
Obliquity of ecliptic,		cos	99·96251 23'
Daily soli-lunar precession,	0″.13799 40	a. c.	0·86013 98
	306·468		2·48638 44

Twice do × Moon's mass,	=	7·51744	a. c.	99·12395 63
Lunar precession,		0″.09424 18		98·97424 36
E on perigee,		0″.01253 718		98·09819 99
	=	*d* ☽ (0·00000 02643 03)		93·42210 20
$2E \div (1 - c^2)$	=	1·00003 14038 5 =		0·00001 36383

Our value of *E* is slightly larger than the one given by Laplace. The precession here used is greater than that observed; the difference being due to a planetary perturbation which causes the equinox to move forward a little more than 17″ in a century. The number 306.468 is the earth's moment of inertia divided by the momentum of the ring of matter that forms our equatorial protuberance.

The value of *e* is, however, a direct function of the perturbation. We obtain it as follows:

$(1 - c^2) \div 6$			97·44799 80098
Syn. ÷ sid. period of ☽			0·03376 49993'
= $(e^2 \div p^2)$			97·48176 30091'
Whence e	=	0·05489 97758	98·73957 057
e^2	=	·00301 39854	97·47914 11416
p	=	0·99698 60146	9·99868 90662

Then, for the averages on radius vector we have:

$(r \div a)^3$	=	1·00904 53630 5'	0·00391 06910 0'
$(r \div a)^4$	=	1·01508 69601 8	0·00650 44721 7

Also for the inclination we have:

$8\,e^2 \div 3$	=	0·00803729430	; γ = 5° 8′ 39″815 + 0″804
$3\,e^4\,p \div 8$	=	+ 339627	
$\sin^2\ \gamma$	=	0·00804 069057	97·90529 335

When n is an even power, the average value of $\sin^n\gamma$ is

$$\frac{n\,(n-1)\,.\,(n-2)\,\ldots\ldots\,(\tfrac{1}{2}n+1)}{2^n\,(1\,.\,2\,.\,3\,.\,4\,\ldots\ldots\,\tfrac{1}{2}n)}$$

Giving $(1 \div 2)$ for $\sin^2$; $(3 \div 8)$ for $\sin^4$;
$(5 \div 16)$ for $\sin^6$; $(35 \div 128)$ for $\sin^8$; etc.

Cos² ☽'s latitude = $1 - \sin^2\gamma\ \sin^2$ longitude. Hence we get the following values, not for the latitude at any particular point but the *average* cos, cos², etc., of the ☽'s latitude:

$$\cos\ \text{lat.} = 1 - \frac{1}{4}\sin^2\gamma - \frac{3}{64}\sin^4\gamma - \frac{5}{256}\sin^6\gamma - \frac{175}{16384}\sin^8\gamma.$$

$$\cos^2\text{lat.} = 1 - \tfrac{1}{2}\sin^2\gamma.$$

$$\cos^3 \text{lat.} = 1 - \frac{3}{4}\sin^2\gamma + \frac{9}{64}\sin^4\gamma + \frac{5}{256}\sin^6\gamma + \frac{105}{16384}\sin^8\gamma.$$

$$\cos^4 \text{lat.} = 1 - \sin^2\gamma + \tfrac{3}{8}\sin^4\gamma.$$

These relations give us:

$\cos^3$ lat. = 0·99397 85840 : 9·99737 70272

$\cos^4$ lat. = 0·99198 35542 : 9·99650 44722

And these multiplied into the average values of r^3 and r^4 give the average third and fourth powers of the projection of r on the plane of the ecliptic:

It is important to note that the sum of the cube cosines for an inclination of 5° 8′ 40″.619 is equal to that for a medial value of 0″.804 less; so that our computation gives us 5° 8′ 39″.815 ± 5′. This corresponds precisely to the **Hansenian** value of 5° 8′ 39″.96 corrected by the — 0″.15 by **Newcomb** deduced from a discussion of the Greenwich and Washington observations from 1862 to 1874.

If ξ be the ☽'s distance divided by that of ⊙, and taking the parallaxes as equal to 3422″.75 and 8″.794, we have ξ^2 = 0·000006601803; and the value of $3m \div 2$ must be multiplied into $(1 + \frac{9}{8}\xi^2$, etc.) and $(1 + \frac{15}{8}\xi^2$, etc.) for the perturbative series in the direction of r and perpendicular thereto.

For the effect due to the "variation," let $1 + x$ and $1 - x$ represent the semi-axes of the ellipse, the longer axis being in quadratures and the other in the syzigies. Let w be the *mean* angular distance from the direction of the minor-axis of this ellipse. Then if r_0 denote the distance from the centre to any point in the circumference, we have, by comparison of the ellipse with its circumscribing circle:

$$\begin{aligned} r_c^2 &= \sin^2(w + dw) \, . \, (1 + x)^2 + \cos^2(w + dw) \, . \, (1 - x)^2; \\ &= 1 + x^2 - 2x \cos 2w + 4\text{Q}\, x^2 \sin 2w; \end{aligned}$$

if $\text{Q}x$ denote the maximum perturbation in longitude in the average orbit:—that which gives unequal areas in equal times.

Now, $1 + x^2 = a_0^2$, if a_0 be the radius of the circle of equal area that would have been described in the absence of compression; because $(1 + x) \, . \, (1 - x) = 1 - x^2$. Hence

$$r_0^2 = a_0^2 - 2x \cos 2w + 4\text{Q}x^2 \sin^2 2w,$$

if there were no change of area; and becomes

$$\left(1 + \frac{x^2}{3}\right) . \left(1 - 2x \cos 2w + 4\text{Q}x^2 \sin^2 2w\right)$$

on account of solar perturbation on $a_\circ$. From this we have:

$$r_\circ^3 = \left(1 + \frac{x^2}{2}\right) . \left(1 + \frac{3}{4}x^2 + 3\,\mathrm{Q}x^2 + \frac{9}{4}\,\mathrm{Q}^2x^4\right)$$

$$r_\circ^4 = \sqrt[3]{\left(1 + \frac{x^2}{2}\right)^4} . \left(1 + 2\,x^2 + 4\,\mathrm{Q}\,x^2 + 6\,\mathrm{Q}^2x^4\right).$$

The numerical values are as follows:

$3\,(t \div t_{,})^2$		a. c.	1·77506 01269'
e^2			97·47914 11416
(syn ÷ sid)^2 − 1	=	0·16823 44223	9·22591 48612
= $4\,x \div (1 + x)^2$			98·48011 6130

which is the square of the average eccentricity in the hypothetical orbit described by the moon once in each synodical lunation.

x	=	·00766 81608	97·88469 12105
$1 + x$			0·00331 75364
$1 - x$			9·99665 69260
x^2		·00005 88006 9	95·76938 24210
$(1 + x) \div (1 - x)$	=	tan (45° + 1581″.633) =	0·00666 06104
$1 + (x^2 \div 2)$			0·00001 27682

$(1 - c^2) \div 2$			97·92511 926
☽'s daily motion ÷ p average $\cos^3 \gamma$			4·68003 181
	402″.857	=	2·60515 107
add	1581″.633		
	1984″.490		3·29764 88
	Syn ÷ sid ☽		0·03376 50
	2144″.934; = Variation;	=	3·33141 39
And Q	=	(log)	9·40603 472'

Taking the logarithms, we have:

	For r^3	For r^4
Function of e	0·00391 06910	0·00650 32488
" γ	9·99737 70273	9·99650 44722
" x	0·00005 14323	0·00009 41089'
" ξ	0·00000 32255	53758
(logs.)	0·00134 23761	0·00310 72057'

and the numbers are	1·00309 57170
	1·00718 02610
	2·01027 59780

The logarithm of the sum =		0·30325 56830'
$(3 \div 2)\, m$		97·92269 65831'
Solar; $(a_{,} \div r_{,})^3$,		0·00018 37199
Earth perturbation,		0·00001 36383
Planetary perturbation,		9·99999 96358
$1 - c^2$	·01683 25245 7	98·22614 92602
$1 - c$	·00845 19803 3	97·92695 84777
☽		4·67609 78999'
$(1 - c)$ =	400″.91875 926‚	2·60305 63777‚

(The planetary perturbation is that adopted by Hill in his tables of Venus. It is what Laplace terms the "indirect" perturbation; being that due to the enlargement of the earth's radius vector by planetary action, which lessens the solar disturbing force. The direct planetary perturbation is neglected, being infinitessimal as between the earth and moon.)

This result is identical with the value of the perigeal motion which Newcomb has obtained from a discussion of the eclipses of 2500 years preceding the present century. The difference between the two is less than one part in 100,000,000. Hence the problem is completely solved.

The following is the resulting value of the daily motion of g, the mean anomaly: 47033″.97147 4‚

Newcomb,	47033″.97147
Hansen; (Tables ☽),	47033″.97227

If any one should object to our deduction of the values of e and γ from that of the quantity sought he is respectfully referred to the top of page 174 of Loomis' Practical Astronomy, with the fact that a comparison of the rates of change in the values of the quantities shows this to be a parallel case with that given by Loomis on page 173. It is not necessary to our result to carry out the logarithm of e^2 to ten places; but I think there needs be no doubt in the future in regard to the precise values of e, γ, or x in the lunar orbit. Of course the numerical values of these quantities are slightly reduced since the beginning of the century by the decreasing eccentricity of the earth's orbit. There is still room for a possible very small correction to the assumed values of sidereal motion of the sun and moon.

Chicago, December 12, 1886.

ASTRONOMY AND THE ICE AGE.

W. H. S. MONCK, Dublin, Ireland.

For the Messenger.

It appears to me that the real difficulty in Dr. Croll's theory of the Ice Age has been but very partially met by that author and is altogether overlooked by Sir Robert Ball, the distinguished Astronomer Royal of Ireland, in the article which appears in the SIDEREAL MESSENGER for December, 1886. Briefly stated that difficulty is this: In the Glacial Period lands on which snow and ice now lie only in small quantities and for a comparatively short time were covered with a snow-cap or ice-cap to the depth of several hundred feet. It is plain that such a snow-cap could not be produced in one winter and melted off during the following summer. It must have gone on increasing in depth year after year for a considerable period during which the surface of the soil could never have been free from snow and ice. Now the difficulty is to show how this effect could be produced by an increased cold in winter *accompanied by a corresponding increase of heat during the ensuing summer.* It is admitted that the total amount of solar heat received by the earth in the course of the year at the period of greatest eccentricity is not less, but even greater, than that which it receives at any other period. Why then should not the summer-heat suffice to melt the winter snow and ice at all places where it is now capable of doing so? Indeed I do not see why this should not take place even if the extremes of heat instead of being 1.38 and 0.68, as in Sir Robert Ball's computation, were 2.00 and 0.00. As a matter of fact many places within the Arctic Circle are cut off for several days from all direct solar heat and yet they do not wear a perpetual snow-cap. Inequalities of temperature too appear, in some instances at least, to produce a tropical vegetation rather than a perpetual covering of snow. The mean temperature of Astrakhan is not higher than that of London but at the former place the finest grapes ripen in the open air and plants flower which in England are to be found only in greenhouses:

to which I may add that though the increase of solar heat at the period in question is small, (it has been estimated at $\frac{1}{800}$ of the total amount) the effect of exposure to this increased heat for centuries could hardly fail to be the reverse of that supposed by Dr. Croll. It would probably suffice to melt one foot of ice in three years, or 1,000 feet in 3,000 years, during which latter period there would be no perceptible change of eccentricity.

If we take a place at which the temperature remains steady throughout the year at the temperature of 31° F, it is plain that if not cut off from the sources of snow or rain it will wear a perpetual snow-cap. But suppose that I were allowed to divide the quantity of heat which this place receives in the year as unequally as I chose, could I not succeed in clearing away the snow for a time and perhaps even in bringing some kind of crops to perfection? Something of the kind in fact takes place in most localities where the range of temperature is very great.

The Glacial Period is, I think, still an unsolved mystery. It seems to have been preceded by a very long period of declining temperature as if the sun was being slowly cooled by radiation. This of course is in entire agreement with theoretic Astronomy. But how did the sun recover his heat at the close of the Glacial Epoch? This I suspect will be the ultimate form of the question, and if so, Astronomy alone can supply the answer.

RECENT STELLAR PHOTOGRAPHY.*

E. E. BARNARD.

Celestial photography began forty-six years ago; though as a means of accurate and delicate research its history is confined to the past three or four years.

As only late photographic work is to be considered at this time, we can only mention that done by the elder Draper, De

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* Abstract of article in the *Daily American*, Nashville, Tenn., Jan. 2, 1887.

la Rue, Bond, Rutherford (whose photographs of the moon in 1864 are still perhaps the best made), Dr. B. A. Gould (photographs of star-groups and clusters in the southern skies), Professor Pickering (study of the light of stars) and Dr. Huggins (in attempting to photograph the corona without an eclipse).

Since the death of Dr. Henry Draper in 1882, celestial photography has received more attention in France and England than elsewhere. In France the refracting telescope is used altogether in photography, while in England preference is given to the reflecting telescope. These two instruments differ from each other chiefly in the way light rays are used as indicated by the meaning of the words refracting and reflecting. In the first, the lenses are specially corrected for the photographic or actinic rays ; in the second, no such correction is required, since the actinic and visual focuses coincide. A few years ago Mr. A. A. Common, of Ealing, England, procured a large reflecting telescope of thirty-six inches aperture. With this instrument he has closely pursued the work of celestial photography, and in 1883 he succeeded in making the finest photograph of that wonderful object, the Great Nebula of Orion, that has yet been made. This remarkable picture shows the nebula in all its singular ramifications. All the details that are seen in ordinary instruments are faithfully depicted, and many more that are beyond the reach of any but the most powerful instruments. But we must give the first honor to an American for obtaining the earliest accurate photograph of this magnificent nebula. This was made by Dr. Henry Draper, of New York. His photograph, made with an eighteen-inch reflector in 1882, created great interest at the time. It required an exposure of one hour and thirty-seven minutes and showed stars as faint as the fifteenth magnitude, which were scarcely visible to the eye in the same instrument. Mr. Common's picture is superior to Dr. Draper's. It shows much more detail, and the definition of the entire object is far better ; it brings out clearly the wonderful bat-like form of the Great Nebula.

Mr. Common is now having constructed a yet larger instrument, the speculum of which will be five feet in diameter. The prosecution of photography with such an instrument will result in a still better knowledge of the far distant wonders of the sky.

But by far the most important work has been done at Paris. The skill and tireless energy of two brothers, the Messrs. Paul and Prosper Henry, have overcome, to a wonderful extent, the optical, mechanical and chemical difficulties that have been such a barrier to success in celestial photography. The instruments with which their remarkable success has been achieved, were constructed by themselves. Their first experiments were made with a telescope of nine inches aperture. The surprising results obtained with this led them to construct a larger instrument of 13.4 inches.

A few of the most important results cannot fail to be of popular interest, though it would require some familiarity with the subject to fully appreciate their wonderful pictures.

The Pleiades have been carefully studied, and accurate charts of the brighter stars made by such astronomers as Bessel, Wolf and Elkin.

Wolf's chart of the Pleiades, made at the Paris Observatory, required three years of assiduous work upon the part of a clever observer (1873, 1874, 1875) and also much work in 1878. Such a task may well be considered as the principal life work of an astronomer.

This chart contained 671 stars down to the thirteenth magnitude, and its author, after having thoroughly studied the entire group with the great telescope of forty-seven and one-fourth inches aperture, and also with the refractor of twelve inches, thought that he had attained the limit of the visible universe in that region of the sky. This same cluster was photographed at the same observatory in one hour's time. The old chart and the photograph were on the same scale, and could, therefore, easily be compared. The photograph showed in the same space 1,421 stars down to the sixteenth magnitude, or over twice as many as the eye had seen with a far greater instrument.

Doubts had already arisen as to the accuracy of some of the details of Wolf's chart, both in the position of the fainter stars and their magnitude. The photograph is perfectly accurate, and will stand as an authentic picture of the present appearance of the Pleiades. Beyond this extremely important fact the picture revealed many wonderful things that were unknown before. When the plate was developed, a small stain was seen close to, and apparently originating in, the star Maia, "very intense and effecting a very characteristic spiral form." Was it a stain? Another exposure showed the same object; this proved it to be real and to belong to the Pleiades; it must then be a new nebula, for no such object was known at that point. A close search for it with the great telescope of the observatory—five or six times as large as the one that photographed it—failed to show any trace of the object. A third photograph showed it as clearly as the previous two. Then the observers had the courage to announce the discovery. But though a diligent search was made for it with all the great telescopes of the world, only two—and these with difficulty—were able to see it. These were the great thirty-inch refractor at Pulkowa and the large telescope at Nice. Yet the Pulkowa telescope was five times as great as the one that photographed the nebula.

But this was not all the wonderful Paris photograph revealed. In 1859, Temple at Florence, Italy, discovered a large nebula about, and extending southwesterly for a half degree from, the star Merope of this group. Since that date observers have been equally divided as to its existence or non-existence; some seeing it readily, others not being able to even "glimpse it."

But that Paris photograph showed that it did exist, and not only that, instead of one there were four nebulæ at that point. But it must be admitted that the full extension of the nebula that is common to observers was not shown in the picture, the more diffused nebulosity seemingly having escaped the sensitive plate or lost in developing.

Among the discoveries in the Pleiades were a number of minute companions close to several of the brighter stars. Some of these can be seen with no telescope, for the eye is dazzled by the light of the bright star. This in some part accounts for the difficulty in seeing the Maia nebula ; the sensitive plate, however, is not so affected, being able to photograph a faint star close to a bright one as readily as if it were isolated on the dark sky. One of these little stars that had been carefully observed and measured by Wolf, the photograph showed to be two little stars close together.

Excellent photographs of the planet Saturn were obtained directly enlarged eleven times. The black separation of the two bright rings was clearly shown ; the belts, the polar caps and the semi-transparent ring were also perfectly pictured.

Good photographs of Neptune were secured. The satellite of that planet was photographed in all parts of its orbit. This was all the more interesting, because photography is actually the only means of observing this satellite at Paris, the regular observing instrument not being powerful enough to deal with it. Photographs of double and multiple stars were obtained ; also fine views of groups and clusters, including those of Hercules and Perseus.

The celebrated Ring Nebula, of Lyra, was photographed as a ring of light with a perfectly black center. A photograph of Epsilon Lyræ showed, after two hours exposure, stars fainter than the debilissima of Herschel, and of less than the sixteenth magnitude, while an exposure near Vega showed stars still fainter, some of which, it is believed, will never be revealed to mortal eye save through the aid of photography. When one of these pictures was being made, a small asteroid happened to be among the stars that were photographed, and as it was in motion it left its path on the sensitive plate as a trail of light.

In the meanwhile work has been pushed forward in England, and some of the very latest advances have been made there.

At the November meeting of the Royal Astronomical Society in London, Mr. Isaac Roberts exhibited negatives taken with his twenty-inch reflector which received the highest praise. Some of these were parts of Cygnus, and covered the same ground that the Paris pictures showed. As the French and English negatives were on the same scale and covered identically the same regions, they were strictly comparable. It was found that Mr. Roberts' negatives showed on the average ninety-one stars to the square inch, while the Paris picture had only fifty-five stars in the same area.

Mr. Roberts also exhibited a negative of the Pleiades, which not only showed all the details photographed at Paris, but much more. This was made with an exposure of three hours, and showed that not only were the stars Alcyone, Maia, Electra and Merope surrounded by nebula, but that the nebulosity extends in streamers and in fleecy masses till it seems almost to fill up the spaces between the stars, and to go far beyond them. It suggests the probability that these principal stars of the Pleiades, together with many of the stars around them, are either directly involved or else in alignment with one vast nebula. This plate showed the Merope nebula in all its extension, just as it is seen and drawn by observers, which was not depicted in full upon the Paris photograph.

But all these results are not secured without the astro-photographer's brain being taxed. He cannot simply place his sensitive plate in the telescope and then sit down and leisurely wait till the propitious stars have kindly registered themselves. There must be a most delicate motive power applied to the telescope. So faint are some of the objects that the celestial photographer grapples with, that all the sensitiveness of his wonderful dry plates is brought into action, as we have seen, only after hours of exposure. Unless the telescope throughout this long interval is driven with unerring precision, the result is worthless. The most ingenious mechanism has been applied to the telescope, so that the great instrument follows the star slowly—almost imperceptibly, but surely.

The length of exposure varies with the brightness of the object. It has been calculated that with a thirteen-inch telescope the stars Vega and Sirius can be photographed in the $\frac{5}{1000}$ of a second; for a star just visible to the naked eye one-half second, while for the faintest stars visible in the telescope, one hour and twenty minutes are required.

The image of one of these faint stars on the photographic plate is $\frac{1}{1000}$ of an inch in diameter. What does this signify? It signifies that for a period of eighty minutes the telescope must move in a direction contrary to the earth's rotation, with a velocity so uniform that the image of that stellar point must not vary from its original position on that plate by less than $\frac{1}{1000}$ of an inch, or the star would not be photographed at all.

So great has the importance of celestial photography become through the work of the Henry brothers that a congress of astronomers from all parts of the world will meet at Paris in April next to discuss plans for a grand survey of the sky. It is proposed to photograph the entire heavens from pole to pole, thus accumulating for our own use, and especially for that of coming generations of astronomers, the most accurate charts of the millions of stars, including those that are seen only in the largest telescopes. It is expected that the heavens will be divided up into zones and these distributed among different observatories, and thus the work can be rapidly pushed forward and completed in the next ten years.

The subjects that are proposed for discussion at this congress are various and thoroughly cover all the ground of celestial photography, among which are: The size and prices of the instruments; the method of pointing, whether with an additional telescope, as now, or by watching the image through the sensitive plate as it is being photographed; the size of the charts and the minimum limit of the stars they wish to secure; the best method to avoid confusion of accidental spots on the negative with small stars, the present method being to make three successive exposures on the same plate, which is shifted at each exposure, every star then is represented by a small triangle of three points, accidental spots being thus easily distinguishable. The best possible method of preserving the plates against deterioration by time will be one of the most important questions discussed. Several observatories have already ordered instruments from Paris, but these will not be constructed until after the April meeting, as many improvements are expected to be brought forward then.

THE METEORITES, THE METEORS AND THE SHOOTING STARS.*

PROFESSOR H. A. NEWTON.

You are kindly giving to me an hour to-night in which I may speak to you. I do not have enough confidence in myself to justify me in speaking to such an audience as this upon one of those broad subjects that belong equally to all sections of the Association. The progress, the encouragements and the difficulties in each field are best known to the workers in the field, and I should do you little good by trying to sum up and recount them. Let me rather err then, if it all, by going to the opposite extreme.

Two years ago your distinguished President instructed and delighted us all by speaking of the Pending Problems of Astronomy, what they are and what hopes we have of solving them. To one subject in this one science, a subject so subordinate that he very properly gave it only brief notice, I ask your attention. I propose to state some propositions which we may believe to be probably true about the meteorites, the meteors, and the shooting stars.

In trying to interest you in this subject so remote from your ordinary studies I rely upon your sense of the unity of all science, and at the same time upon the strong hold which these weird bodies have ever had upon the imaginations of men. In ancient times temples were built over the meteorite images that fell down from Jupiter and divine worship was paid them, and in these later days a meteorite stone that fell last year in India became the object of daily anointings and other ceremonial worship. In the fearful imagery of the Apocalypse the terrors are deepened by there falling "from heaven a great star burning as a torch," and by the stars of heaven falling "unto the earth as a fig tree casteth her unripe figs when she is shaken of a great wind." The "great red dragon having seven

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*An address by the retiring President before the American Association for the Advancement of Science, at Buffalo, August, 1886.

heads and ten horns and upon his heads seven diadems" is presented in the form of a huge fireball. "His tail draweth the third part of the stars of heaven, and did cast them to the earth." Records of these feared visitors under the name of flying dragons are found all through the pages of the monkish chroniclers of the Middle Ages. The Chinese appointed officers to record the passage of meteors and comets for they were thought to have somewhat to say to the weal or woe of rulers and people.

By gaining in these latter days a sure place in science, these bodies have lost their terrors, but so much of our knowledge about them is fragmentary, and there is still so much that is mysterious, that men have loved to speculate about their origin, their functions, and their relations to other bodies in the solar system. It has been easy, and quite common too, to make these bodies the cause of all kind of things for which other causes could not be found.

They came from the moon; they came from the earth's volcanoes; they came from the sun; they came from Jupiter and the other planets; they came from some destroyed planet; they came from comets; they came from the nebulous mass from which the solar system has grown; they came from the fixed stars; they came from the depths of space.

They supply the sun with his radiant energy; they give the moon her accelerated motion; they break in pieces heavenly bodies; they threw up the mountains on the moon; they made large gifts to our geologic strata; they cause the auroras; they give regular and irregular changes to our weather.

A comparative geology has been built up from the relations of the earth's rocks to the meteorites; a large list of new animal forms has been named from their concretions; and the possible introduction of life to our planet has been credited to them.

They are satellites of the earth; they travel in streams, and in groups, and in isolated orbits about the sun; they travel in groups and singly, through stellar spaces; it is they that reflect the zodiacal light; they constitute the tails of comets; the solar corona is due to them; the long coronal rays are meteor streams seen edgewise.

Nearly all of these ideas have been urged by men deservedly of the highest repute for good, personal work in adding to human knowledge. In presence of this host of speculations, it will not, I hope, be a useless waste of your time to inquire what we may reasonably believe to be probably true. And if I shall have no new hypothesis to give to you, I offer as my excuse that nearly all possible ones have been already put forth. This Association exists, it is true, for the advancement of science, but science may be advanced by rejecting bad hypotheses as as well as by forming good ones.

I begin with a few propositions about which there is now practical unanimity among men of science. Such propositions need only be stated. The numbers that are to be given express quantities that are open to revision and moderate changes.

1. The luminous meteor tracks are in the upper part of the earth's atmosphere. Few meteors, if any, appear at a height greater than one hundred miles, and few are seen below a height of thirty miles from the earth's surface, except in rare cases, when stones and irons fall to the ground. All these meteor tracks are caused by bodies which come into the air from without.

2. The velocities of the meteors in the air are comparable with that of the earth in its orbit about the sun. It is not easy to determine the exact values of those velocities, yet they may be roughly stated as from fifty to two hundred and fifty times the velocity of sounds in the air, or of a connon ball.

3. It is a necessary consequence to these velocities that the meteors move about the sun and not about the earth as the controlling body.

4. There are four comets related to four periodic star-showers that have occurred on the dates April 20th, August 10th, November 14th and November 27th. The meteoroids which have given us any one of these star-showers constitute a group, each individual of which moves in a path which is like

that of the corresponding comet. The bodies are, however, now too far from one another to influence appreciably each other's motions.

5. The ordinary shooting stars in their appearance and phenomena do not differ essentially from the individuals in star-showers.

6. The meteorites of different falls differ from one another in their chemical composition, in their mineral forms and in their tenacity. Yet through all these differences they have peculiar common properties which distinguish them entirely from all terrestrial rocks.

7. The most delicate researches have failed to detect any trace of organic life in meteorites.

These propositions have practically universal acceptance among scientific men. We go on to consider others which have been received with hesitation, or in some cases have been denied.

With a very great degree of confidence we may believe that shooting stars are solid bodies. As we see them they are discrete bodies, separated even in prolific star-showers by large distances one from another. We see them penetrate the air many miles, that is, many hundred times their own diameters at the very least. They are sometimes seen to break in two. They are sometimes seen to glance in the air. There is good reason to believe that they glance before they become visible.

Now these are not the phenomena which may be reasonably expected from a mass of gas. In the first place, a spherical mass of matter at the earth's distance from the sun, under no constraint and having no expansive or cohesive power of its own, must exceed in density air at one-sixth of a millimeter pressure (a density often obtained in the ordinary air pumps) or else the sun by his unequal attraction for its parts will scatter it. Can we conceive that a small mass of gas, with no external constraint to resist its elastic force, can maintain so great a density?

But suppose that such a mass does exist, and that its largest and smallest dimensions are not greatly unequal; and suppose further that it impinges upon the air with a planetary velocity; could we possibly have as the visible result a shooting star? When a solid meteorite comes into the air with a like velocity its surface is burned or melted away. Iron masses and many of the stones have had burned into them those wonderful pittings or cupules which are well imitated, as M. Daubrée has shown, by the erosion of the interior of steel cannon by the continuous use of powder under high pressure. They are imitated also by the action of dynamite upon masses of steel near which the dynamite explodes. Such tremendous resistance that mass of gas would have to meet. The first effect would be to flatten the mass for it is elastic; the next to scatter it for there is no cohesion. We ought to see a flash instead of a long burning streak of light. The mass that causes the shooting star can hardly be conceived of except as a solid body.

Again, we may reasonably believe that the bodies that cause the shooting stars, the large fireballs and the stone producing meteor, all belong to one class. They differ in kind of material, in density, in size. But from the faintest shooting star to the largest stone-meteor we pass by such small gradations that no clear dividing lines can separate them into classes.

See wherein they are alike.

1. Each appears as a ball of fire traversing the apparent heavens just as a single solid but glowing or burning mass would do.

2. Each is seen in the same part of the atmosphere, and moves through its upper portion. The stones come to the ground, it is true, but the brightly luminous portion of their paths generally ends high up in the air.

3. Each has a velocity which implies an orbit about the sun.

4. The members of each class have apparent motions which imply common relations to the horizon, to the ecliptic, and to the line of the earth's motion.

5. A cloudy train is sometimes left along the track both of the stone-meteor and of the shooting star.

6. They have like varieties of colors, though in the small me-

teors the colors are naturally less intense and are not so variously combined as in the large ones.

In short, if the bodies that produce the various kinds of fireballs had just the differences in size and material which we find in meteorites, all the differences in the appearances would be explained; while, on the other hand, a part of the likenesses that characterize the flights point to something common in the astronomical relations of the bodies that produce them.

This likeness of the several grades of luminous meteors has not been admitted by all scientific men. Especially it was not accepted by your late president, Professor J. Lawrence Smith, who by his studies added so much to our knowledge of the meteorites.

The only objection, however, so far as I know, that has been urged against the relationship of the meteorites and the star-shower meteors, and the only objection which I have been able to conceive that has apparent force, is the fact that no meteorites have been secured that are known to have come from the star-showers. This objection is plausible and has been urged both by mineralogists and astronomers as a perfect reply to the argument for a common nature to all the meteors.

But what is its real strength? There have been in the last one hundred years five or six star-showers of considerable intensity. The objection assumes that if the bodies then seen were like other meteors, we should have reason to expect that among so many hundreds of millions of individual flights, a large number of stones would have come to the ground and have been picked up.

Let us see how many such stones we ought to expect. A reasonable estimate of the total number of meteors in all of these five or six star-showers combined makes it about equal to the number of ordinary meteors which come into the air in six or eight months. Inasmuch as we can only guess at the numbers seen in some of the showers let us suppose that the total number for all the star-showers was equal to one year's supply of ordinary meteors. Now the average annual number of stone-meteors of known date, from which we have secured specimens, has during this hundred years been about two and a half.

Let us assume then that the luminous meteors are all of like origin and astronomical nature ; and further assume that the proportion of large ones, and of those fitted to come entirely through the air without destruction, is the same among the star-shower meteors as among the other meteors. With these two assumptions a hundred years of experience would then lead us to expect two, or perhaps three, stone falls from which we secure specimens during all the half-dozen star-showers put together. To ask for more than two or three is to demand of star-shower meteors more than other meteors give us. The failure to get these two or three may have resulted from chance, or from some peculiarity in the nature of the rocks of Biela's and Tempel's comets. It is very slender ground upon which to rest a denial of the common nature of objects that are so similar in appearance and behavior as the large and small meteors.

It may be assumed then as reasonable that the shooting stars and stone-meteors, together with all the intermediate forms of fireballs, are like phenomena. What we know about the one may with due caution be used to teach facts about the other. From the mineral and physical nature of the different meteorites we may reason to the shooting stars, and from facts established about the shooting stars we may infer something about the origin and history of the meteorites. Thus it is reasonable to suppose that the shooting stars are made up of such matter and such varieties of matter as are found in meteorites. On the other hand, since star-showers are surely related to comets, it is reasonable to look for some relation of the meteorites to the astronomical bodies and systems of which the comets form a part.

This common nature of the stone-meteor and the shooting stars enables us to get some idea, indefinite, but yet of great value, about the masses of the shooting stars. Few meteoric stones weigh more than one hundred pounds. The most productive stone falls have furnished only a few hundred pounds each, though the irons are larger. Allowing for fragments not found, and for portions scattered in the air, such meteors may be regarded as weighing a ton, or it may be several tons, on entering the air. The explosion of such a meteor is heard a hundred miles around shaking the air and the houses over the whole region like an earthquake. The size and brilliancy of the flame of the ordinary shooting star are so much less than that of the stone-meteor that it is reasonable to regard the ordinary meteoroid as weighing pounds or even ounces, rather than tons.

Determinations of mass have been made by measuring the light and computing the energy needed to produce the light. These are to be regarded as lower limits of size, because a large part of the energy of the meteors is changed into heat and motion of the air. The smaller meteors visible to the naked eye may be thought of without serious error as being of the size of gravel stones, allowing, however, not a little latitude to the meaning of the indefinite word gravel.

These facts about the masses of shooting stars have important consequences.

The meteors, in the first place, are not the fuel of the sun. We can measure and compute within certain limits of error the energy emitted by the sun. The meteoroids, large enough to give shooting stars visible to the naked eye, are scattered very irregularly through the space which the earth traverses, but in the mean each is distant two or three hundred miles from its near neighbors. If these meteoroids supply the sun's radiant energy a simple compuatation shows that the average shooting star ought to have a mass enormously greater than is obtained from the most prolific stone fall.

Moreover, if these meteoroids are the source of the solar heat their direct effect upon the earth's heat by their impact upon our atmosphere ought also to be very great: whereas the November star-showers in some of which a month's supply of meteoroids was received in a few hours do not appear to have been followed by noticeable increase of heat in the air.

[TO BE CONTINUED.]

TELESCOPIC ILLUMINATION.

ORMOND STONE.*

For the Messenger.

Miss Byrd, in her pleasing address on "Popular Fallacies about Observatories," says, "I raise no objection to poetry or velvet gowns, but until some one invents a way of lighting telescopic fields so that the observer is not obliged constantly to handle greasy lamps, the question is not open to discussion; the bans between poetry and practical astronomy are positively forbidden." And adds that "the modern observer is mindful of sulphuric acid and sperm oil and dons an old coat, or a shabby dress, as the case may be."

In electricity a means of illumination has been found which is practically all that could be desired. The reading circles of the great equatorial of the Leander McCormick Observatory have been illuminated by electricity from the beginning, although some difficulty was experienced at the very first in obtaining a battery which would do the work properly and with little care. Edison incandescent lamps of one-candle power are employed, run by a somewhat superior form of the ordinary bichromate battery, known as the Orne Motor Battery.

Since the time required to make a single reading of either circle is short, the intervening time is ample to allow the battery to recuperate and the illumination is practically constant. In making observations of double stars, however, this form of battery runs down so quickly as to be practically useless. After some experimenting it was found that a single candle-power lamp run by two Edco-batteries gave results in every way satisfactory. The Orne battery has small circular cups, while the Edco cells are large, each battery being supplied with two cells, each cell having a 6×8 inch carbon plate and two zinc plates of the same size.

When the Edco is first put in order, a single battery of two cells arranged for intensity is ample. After the electro-motive force has diminished, the "quantity" is increased by the addition of the other battery. The directions are that the porous

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* Director of the Leander McCormick Observatory, University of Virginia.

cups be replenished each day; this, however, is not done in practice; in fact, no care is taken of the batteries further than to lift the plates out of the fluid when the night's work is done. At the end of a couple of months, a short time is spent in cleaning and replenishing the cells. When in use, the cover should never be allowed to rest on the cell, but should be separated from it. This can be done by laying a small pine strip between the cover and the cell. If the air is excluded there is danger of burning the plates.

The opening of a carboy of acid is so dangerous a process that if the acid be purchased in quantity it is better to do so through a neighboring druggist and let him open the carboy.

An old suit of clothes should be worn when cleaning the batteries or mixing the fluids. The cleaning of the batteries occurs, however, so seldom as to be really a very trifling matter. The Orne battery requires about the same care, but is fed by the refuse fluid from the Edco. Five or six Orne cells are usually employed, arranged for intensity.

The same battery is used for both reading circles, being turned on to either circle by a button switch conveniently located near the eye-end of the telescope tube. The Edco battery also runs two lamps, one being used at a time. The switch for these lamps is placed at the rear of the observing chair. One of the lamps is used for illuminating the micrometer wires, the other the observer can carry in his hand, and with it reads the position circle of the micrometer, and makes his record in his observing book; in short, uses it for any purpose he may desire.

A large cork is so cut that the lamp nestles in one end, and is thus protected from breaking. The wires run through, but are fastened to the cork, so that any pull on the wires does not produce a pull on the lamps themselves. This is important, as the connections to such small lamps are necessarily so delicate as to be easily broken. The cork fits into a tube at one end of the micrometer box, or may be used as a handle for carrying in the hand. These lamps are used all night without sensible diminution of intensity. Oil lamps are wholly discarded, and the astronomer's work at night is as cleanly as his office work in the day.

In observing the Nebula of Orion, one of the lamps hangs by its wires from the eye-end of the telescope, at a convenient height for reading the observing book, the cork acting as a shade. The room is thus kept dark, except at the instant the record is being made, and even then the eye is shaded.

The unsteadiness of the ordinary lamps employed with the Zollner photometer is one of the principal objections to its use. No such objection could exist if illumination by electricity were employed.

The success of the experiment here has resulted in the use of electricity, at least for circle illumination, at West Point, Yale, and other observatories. At the Cape of Good Hope, as was afterwards learned, storage batteries had been previously employed, but it seems to me preferable to work from the battery directly. Of course, where steam or other power is at hand, as at Princeton, a dynamo is naturally employed.

THEMES IN FOREIGN ASTRONOMICAL JOURNALS.

Astronomische Gesellschaft. In the fourth part of this German publication for 1886 is found, among other valuable articles, a review by J. Luroth, of Professor Newcomb's article which was published in No. 4, Vol. VIII, of the *American Journal of Mathematics*, and entitled *A generalized theory of the combinations of observations so as to obtain the best results.*

The reviewer points out with care the differences between the theory suggested by Professor Newcomb and that commonly known as the method of Least Squares, and gives the principal mathematical expressions of the new theory, and completes the review in the following language:

"It is clear from the foregoing process, that good observations enter into the result with less weight, and the bad with greater weight, than is the case in the common theory, though one does not know at all whether an observation with a small error may not accidentally belong to a group of bad observations.

"Besides difficult computations which are necessarily performed by methods of approximation, it is a disadvantage to the foregoing theory that one knows neither the p [probability] nor the h [the modulus of precision] beforehand. Newcomb proposes, first of all, to seek the unknown quantity in the usual way, then to form the residuals for separate observations, and next by trial to ascertain what modulus of precision to employ and how many observations one must assign to each class in order that the actual distribution of the residuals may be nearly represented by the theory. What is arbitrary in this may not have any great influence on the result. On the other hand the result will not differ very much from that which the method of least squares gives, except in computation slightly, at least if one accepts (according to Bessel) that elementary errors have equal weights. But whether an empirical law of probability applied to residuals is an adequate expression of the probability of error, also in the case of different large elementary errors, may not be certain, at least, according to Bessel's researches without something further. As one can not come at once without assumption to a rule of computation for the combination of observations, so might the arbitrary acceptance of the theory of combinations with its simple deductions always fulfill the purpose in a most elegant manner to give a possible process for the discretion of the individual computer."

Ciel et Terre for December, 1886, contains a long article *On the staying of the sun during the battle of Muhlberg*, by C. Lagrange. His summary is as follows:

"1. The most recent account of a staying of the sun, that of the battle of Muhlberg, originated from an unusual state of the atmosphere and an extraordinary aspect of the sun. What eye-witnesses tell of this last is probable. No argument can be drawn from this recent tradition against the reality of the facts to which the more ancient traditions allude.

"2. The tradition of a staying of the sun is found among a great number of peoples far removed from one another.

"3. The account of the simultaneous staying of the sun and the moon by Joshua ought for historic reasons to be regarded as concerning a real fact, which he places on the morning of the battle of Gideon and not the evening. That staying appears to be connected, in the narrative, with a retardation of the course of the sun. The durations are not fixed with precision.

"I have mentioned, in passing, the hypotheses compatible with the actual ideas of science which might serve to explain such a fact, in its immediate causes, either as a local phenomenon, or as a phenomenon embracing the whole surface of the earth."

Notice, in conclusion, a striking example of the reserve is to be imposed upon too absolute conclusions as to the facts which we refuse to admit because they go beyond the habitual order of our knowledge or common experience. We know that in the verses of the tenth chapter of Joshua that precede the account of the staying of the sun, there is mention of a shower of stones which destroyed in part the army of the Canaanitish kings. Voltaire, in his criticism of the Bible, treats as they merit (to him) that shower of stones and that staying of the sun. Less than fifty years afterwards the Academy of Sciences established the possibility of the fact reported in verse 11; how will it be with the fact mentioned in verses 12 and 13?

NOTE ON THE ORIGIN OF COMETS.

DANIEL KIRKWOOD.

Have comets originated in the solar system, or do they enter it from without? This question has been considered by Laplace, Proctor, H. A. Newton, and others. The last named presents arguments of no little weight in favor of their origin in inter-stellar space. To these arguments I shall attempt no reply. On the contraty, I have been disposed to accept them as, in the main, valid. For certain comets of short period, however, various facts seem to indicate an origin within the system.

(1.) According to M. Lehmann-Filhés the eccentricity of the third comet of 1884, before its last close approach to Jupiter, was only 0·2787.* This is exceeded by that of twelve known minor planets. Its period before this great perturbation was about 3619 days, and its mean distance 4·611. It was then an asteroid, too remote to be seen, even in perihelion. Its period was very nearly commensurable with that of Jupiter; six of the one being very nearly equal to five of the other. According to Hind and Krueger the great transformation of its orbit by Jupiter's influence occurred in May, 1875. Its present period is about 6½ years. It was discovered by M. Wolf at Heidelberg, September 17, 1884. Its history indicates an origin in the zone of asteroids.

(2.) *The second comet of 1867.*—This body was discovered by M. Tempel on the third of April. Its perihelion distance is 2·073; its aphelion 4·8973; so that its entire path, like those of the asteroids, is included between the orbits of Mars and Jupiter. The eccentricity of this comet at its successive returns has been as follows:

Date of Return.	Eccentricity.
1867	0·5092
1873	0·4625
1879	0·4624
1885	0·4051

The last is nearly identical with the eccentricity of Æthra, the 132d asteroid (0.38). The period, inclination, and longitude of the ascending node are approximately the same with those of Sylvia, the 87th minor planet.

This comet may be regarded as an asteroid whose elements have been considerably modified by perturbation.

Other comets furnish suggestive facts which bear upon the same question; but their discussion must await the development of additional data.—*Am. Journal of Sc. and Arts.*

R. R. Beard, Vice-President of National Bank, Pella, Ia., has a 6¼-inch Clark glass and is interesting himself in solar studies.

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*Annuaire, 1886.

EDITORIAL NOTES.

Although not long, it is regretted that want of space makes it necessary to divide Professor Newton's very instructive address on meteorites, meteors and shooting stars.

The authorities of the Northwestern University at Evanston, Ill., have decided to build an astronomical observatory. The financial interest back of the enterprise is promising. Chicago must look out for its laurels.

Miss Mary E. Byrd's address at the laying of the corner stone of the new astronomical observatory of Carleton College, October 2, 1886, entitled, "Popular Fallacies about Observatories," has received general and very favorable notice at home and abroad. It was reprinted in full in the *Observatory*, and the following are the words of a Fellow of the Royal Astronomical Society, as taken from the *English Mechanic :*

"The mention of the *Observatory* suggests to me to recommend the perusal, in the current number of that serial, of a report of an address delivered at the laying of the corner stone of the new astronomical observatory at Carleton College, Northfield, U. S., by Miss Mary E. Byrd, the assistant in mathematics and astronomy. I would especially advise all those to read it who picture to themselves astronomers lying luxuriously on their backs, on handsomely upholstered sofas of the last degree of mechanical refinement and ingenuity, and gazing on lunar details, the wonders of the Saturnian system, or the clusters in Perseus, Cygnus and Lyra in a warm, comfortable and convenient apartment. A better or more truthful description of the mere 'grind' of ordinary observatory work it will be difficult to find anywhere."

U. S. Naval Observatory.—The report of the superintendent, Allan D. Brown, for the year ending June 30, 1886, has been received. In it we notice that the 26-inch refractor has been used in observing the faint satellites of the outer planets and difficult double stars, and that "observations of stellar parallax have also been made." How Professor Hall made "observations of stellar parallax," we can not imagine. Of course he was successful, and therefore Supt. Brown ought to direct the great Washington equatorial at once to the observation of the solar parallax and speedily have done with that knotty little puzzle that has been in the way of everybody since the times of Tycho.

As usual, the reductions of the meridian circle observations are three years behind. The observatory has suffered for years in this way because it has not been supplied with a sufficient computing force to do the work. Without additional help, the proposed Transit Circle Catalogue, which is also greatly needed, will be a thing of the distant future.

In Professor Harkness' report of the work of the Transit-of-Venus Commission, made to the superintendent last October, it is stated that the computations for deducing the position angle and distance of Venus relatively to the sun's center, from the measures of the photographs of eleven different stations, had all been revised and checked. The work has since been advancing and is now near completion, if we are rightly informed.

*Lalande 4219.**—R. A. 2*h* 10*m*; Decl. —18° 45′ (1890); Magnitudes 8.0—8.8. This double star was observed here in 1886, and upon reduction was found to differ widely in P. A. from Prof. Hall's measures in 1879. Upon searching I could find no other observations until Prof. Hall kindly sent me some unpublished ones he had made in 1885. The measures are as follows:

Time.	P. A.	Δ	No. of Obs.	Observer.
1879.92	311.8	2.22	2	H.
1885.04	333.4	2.29	3	H.
1886.84	336.8	2.12	2	L.

So rapid a change in P. A. led to the suspicion that the motion was due to proper motion of the principal star. By request, Dr. Wilson collected the observations of position and

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* Mr. Burnham tells me this star was discovered to be double by Dr. Hastings.

found that a proper motion of $-0.002s$ in α and $-0.24''$ in δ made the observations which extended over a period of about eighty years, agree pretty well.

Upon applying this motion to Prof. Hall's first observation in order to reduce it to the time of the other two, the P. A. was found to be 333° and the Δ $3.3''$. The difference in Δ $1.1''$ thus found is too great to be accounted for by errors of observation; so unless the companion has proper motion, or there is some considerable error in the value of the proper motion in δ, the motion must be binary in its nature.

A relative proper motion $\alpha - 0.008s$, $\delta - 0.08''$ will satisfy the measures approximately. F. P. LEAVENWORTH.

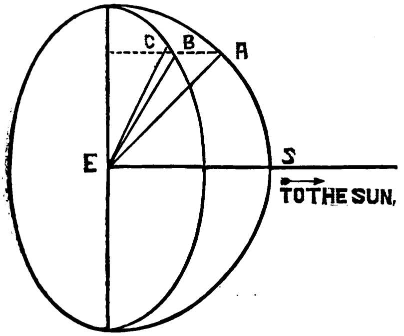
Leander McCormick Observatory, Dec. 28, 1886.

Yarnall's Catalogue.—We notice from the last report of the superintendent of the Naval Observatory, that Professor Frisby is charged with the duty of preparing a third edition of Yarnall's Catalogue. This revision will involve very considerable labor, and go forward mainly in the following manner: The lists of errors published from time to time by different astronomers have been carefully analyzed by reference to the annual volumes in which the observations are printed. If necessary, recourse is then had to the original record in the observing-books, extending over a period of more than thirty years; in some cases it has been necessary to refer to the chronograph sheets. If any errors are found the observations are reduced *de novo;* if no errors are found the stars are examined with the 9.6-inch equatorial and compared with those in well-known catalogues. Every catalogue in the library has been examined for names of stars noted as anonymous by Yarnall. Up to the present time a large number of errors have been discovered, many of which are, however, of but slight importance. It is hoped to have the revision completed during the next year (1887).

The Dearborn Observatory.—We learn from a circular issued by Professor E. Colbert, Jan. 8, that the Dearborn Observatory, which is the property of the Chicago Astronomical Society, is situated on ground leased to it by the now extinct University of Chicago. The Society is at present a mere tenant at will, and may be asked to vacate any time on sixty days' notice. It is, however, in receipt of an offer to transfer its telescopes, library and other property to an institution of learning outside of Chicago. To retain this noble observatory the city will need to act promptly. Some influential members of the Society are leading in appropriate endeavor to change its site in the city and remount its instruments. It would be the great and sad mistake of the greatest inland city of this continent to allow its chief scientific attraction to be banished in poverty.

Motion of the Lunar Apsides.—A few words explanatory and the accompanying figure may increase the student's interest in the leading article of this number.

Moon's Orbit.



If $AES = 45°$

$BEA = 1581''633$

$CEB = 402''857$

Sum, $CEA \times \frac{syn}{sid} = 2144''934$

Average "Variation" = $2144''934. \sin 2w$,

EC being the true radius vector for mean angle AES.

In a private letter Mr. Colbert says:

"The fact that the lunar orbit is compressed in the direction of the line joining the earth and sun, instead of the contrary, has been noted by several of the men who compile text-books on astronomy, but I do not know of one that helps the reader to a precise comprehension of this very interesting phenomenon of motion under the influence of attraction—doubly interesting because it is paradoxical in the ordinary sense of the word, and apparently so in the proper acceptation of the term. Doubtless you have noticed that the 2145″ (nearly) given on page 55 is the co-efficient of a perturbation in longitude, but is derived from that of the radius vector. The latter corresponds to a value of 26.25″ cos 2 *w*. For + values of cos *w* it is less, and for — values greater, by a little less than 1″ cos *w*. Those who may wish to compare these results with those used by Hansen in his tables for longitude and table XVI for parallax, or the modified values given by Newcomb, will do well to note that their $g - g' + (w - w')$ is the same quantity that I have denoted by *w;* being the mean longitude of the moon minus that of the sun."

In this connection we call attention to a misprint on page 55, line eighteen, $\cos^3 \gamma$ should be $\cos^3$ lat. The quantity referred to is that given in line four of page 54, used with the semi-parameter as a divisor.

The Smithsonian review of astronomy for 1886 is to be prepared by Mr. W. C. Winlock of the Naval Observatory, Washington, Professor Holden being unable to continue the review on account of pressing official duties. Mr. Winlock would be glad to obtain from amateur astronomers a brief account of any work undertaken or completed during the year,—with a description of observatories and instruments—for insertion in the report.

Dr. Swift's Fifth Catalogue of New Nebulæ.—Several of the nebulæ in "Catalogue No. 5 of Nebulæ discovered at the Warner Observatory," *Astronomische Nachrichten, No. 2763*, have previously been published in other places. A list of these is given below; the first column contains the number as given in the *Nachrichten*, the third column the previous place of publication, and the fifth column the name of the discoverer. Only as much of the description is given as appears interesting for comparison. Herschel's abbreviations are used; in the descriptions of nebulæ discovered at this observatory numerical magnitudes are used to indicate brightness, a nebula of about the sixteenth magnitude being the faintest visible with the 66 in. refractor; in these descriptions also the sizes are given in minutes of arc. In some cases the identity is doubtful either on account of lack of agreement between the places or the descriptions; in these cases, however, had two nebulæ existed some note would probably have been made of the fact. In the case of No. 57, although the descriptions do not agree an unpublished sketch shows the equilateral triangle mentioned in the description. The description and place of the nebula near No. 54 has not been published previous to this.

1	pB	G. C. 5092	vF	Secchi.
13	eF pL	A. N. 2502, No. 14	eeF, vS, r	Stephan.
18	*a*, 1*h* 53.1*m*; δ, —0° 2.8′	A. N. 2746, No. 208	*a*, 1*h* 52.8*m*, δ, —0° 1.4′, No. 18?	Swift.
21	eeeF, pL	A. J. 146, No. 49	15.5 eS	Stone.
28	*a*, 2*h* 20.3*m*, δ, —0° 51.1′	G. C. 5236: A. N. 2212, No. 9	*a*, 2*h* 20.2*m*; δ, —0° 53.9,′ No. 28?	Tempel.
30	eeF, pS	G. C. 5262	eeF	Stephan.
31	eeeF, vS	G. C. 5263	fainter than G. C. 5262, vS	Stephan.
33	eeeF, eee diff	A. J. 146, No. 61	15.0, neb?	Stone.
49	F, iR, 1st of 2	A. J. 146, No. 94	14.0, E 60°, stell N, 1st of 3	Stone.
		A. J. 146, No. 95	15.5, dif, 2nd of 3	Stone.
50	eF, vS, 2nd of 2	A. J. 146, No. 96	14.5, stell, 3rd of 3	Stone.
54	vF	Royal Ast. Soc. Vol. 44	not vF	Burnham.
		McCormick Obs.	15.0, 0.4′, P. A. 70°, Δ 2.4′ with No. 54	Muller.
56	eF, pS, R	A. J. 146, No. 113 A. J. 146, No. 114	16.0, 0.8,′ stell N 16.0, 0.8, stell N } P. A. 310°, Δ 0.4′, No. 56?	[Stone.
57	R, equilateral triangle 2st	A. J. 146, No. 116	pE 45°,	Leavenworth.
59	vF	A. J. 146, No. 122	14.0	Stone.
66	F, pS, eE	A. J. 146, No. 141 A. J. 146, No. 142	12.0, vS, spMN 12.0, vS } P. A. 110°, Δ 10″	Leavenw'th.

Leander McCormick Observatory, Jan. 12, 1887.

FRANK MULLER.

The Swift-Watson Intra-Mercurial Observations.—I have been much interested in the perusal of Professor Young's able paper in the MESSENGER for January, but think it a duty to record my dissent from his conclusion that the two objects seen by Swift, during the solar eclipse of July, 1878, were "certainly not the two seen by Watson." I do not wish to be understood as considering "the observation important" or otherwise; but think it is about time that the matter were set right. I presume it will be remembered that I was the chief of the party of which Swift was a member on the occasion referred to, and therefore ought to be able to speak with some authority in regard to it.

The fact is that Swift had no appliances to his telescope for measuring position or angular distance ; and made the unfortunate mistake of trying to locate the objects which he saw (or believed he saw) with a precision not warranted by the circumstances of the case. He was all right at the time of the eclipse, but did wrong in trying to make his observation agree with that of Watson. His first idea was that he saw a fixed star, and an intra-mercurial planet near it. On a star chart which I had prepared previous to the eclipse he marked down the position of the supposed stranger as being near a small star that was much farther away from the sun than was *Theta Cancri.* Only after I had called his attention to the fact that the star selected by him could not possibly have been seen through his telescope at the time, and that it must have been *Theta*, if any, that was seen near his intra-mercurial planet, did he arrive at the conclusion that *Theta* was probably one of the objects seen by him. His subsequent statements of position, and efforts to make his observation agree with that of Watson, were, as I think, unwarrantable.

I can, and do hereby testify, that Swift announced his supposed discovery before the news of Watson's observations reached me. I can also testify that no importance ought to be attached to his statements of position, beyond the fact that his objects were some distance to the right and below the place of the sun. Professor Hough, who was also a member of the party, endorses this statement of the case. I may add that I laid the facts of the matter before Watson the following winter, and discussed it with him at some length verbally. I have reason to believe that Watson would have made a full statement of the case in print but for his untimely death.

Permit me to add that I think Young ought to have mentioned Hough's work in his notice of Jupiter. E. COLBERT.

New Variable in Orion.—Gore's new star in Orion passed its maximum about Dec. 8. It appears to be diminishing in brightness more slowly than last year.

New York, Jan. 13, 1887. HENRY M. PARKHURST.

Lick Observatory.—A late California paper states that President Holden has contracted with Fauth & Company, Washington, D. C., for a micrometer for the 36-inch equatorial, and with Brashear & Company, Pittsburgh, Pa., for a spectroscope which shall be the most powerful yet made; both instruments to be completed by June 1. The great lenses are now in the observatory at Mt. Hamilton, and Warner & Swasey, Cleveland, Ohio, have work on the mounting of the telescope well under way, and it is expected that the transfer of the observatory, its instruments and funds, from the Lick trustees so-called, to the University of California, will take place in July next. The bequest of $700,000, made eleven years ago, has been expended in equipping the observatory so far, except $190,000 which is to be set apart for current expenses. It is thought that this amount will yield an annual income of $10,000 for this purpose.

President Holden is reported to say that the observing force, at first, will consist of two astronomers besides himself. The persons that he will nominate to the Board of Regents are S. W. Burnham of Chicago, and J. E. Keeler, formerly of Allegheny Observatory and now at Mt. Hamilton. The choice of Mr. Burnham is eminently wise. His work with the micrometer is unsurpassed and duly recognized everywhere. The revision of his double star catalogue will soon be possible if this programme is carried out. Mr. Keeler is a graduate of Johns Hopkins and Berlin Universities, and for some time past has been assistant to Professor Langley. He is already a specialist of reputation in astronomical physics and is devoting himself to the spectroscope. We notice also that President Holden has been invited by the Minister of France, resident at Washington, to be present in Paris, April 16, 1887, to attend a convention of leading astronomers of the world to discuss the best methods of using photography in astronomical work.

Brooks' Comet No. 3, of 1886, (discovered May 22, 1886,) proves to be an interesting object, moving in an elliptical orbit of short period. Soon after discovery Dr. Oppenheim of

Berlin computed its elements and gave it a period of about nine years. Now Hind, on more complete data, finds that it is moving in an elliptical orbit, with the short period of six years and three months.

Lick Telescope Object-Glass.—As eleswhere stated, the lenses for the great equatorial of the Lick Observatory have been shipped from Cambribgeport, Mass. by express to San Jose, California; thence they were taken to the Observatory building on Mt. Hamilton. The manner in which they were cased at the shops of the Clarks in Cambridgeport for this long journey may interest some of our readers.

They were wrapped separately in fifteen or twenty thicknesses of soft, clean, cotton cloth. Next came a thick layer of cotton and then a layer of paper. The glasses were then put into boxes of wood and lined with felt. No nails were used near the glasses, and the boxes were made the shape of the glasses. The boxes were inclosed in two others of steel, each about the shape of a cube, being packed tightly with curled hair. Each steel box was inclosed in another steel box, the inner sides of which were covered with spiral springs. Both steel boxes were made air tight and water proof, and the outer chests packed with asbestos to render them fire-proof. Each was then suspended by pivots in strong wooden frames, with contrivances for turning each chest one quarter around every day during the journey to California. This is to prevent any molecular disarrangement in the glasses and to avoid the danger of polarization, it being feared that the jarring of the train would disturb the present arrangements of the molecules unless the position of the glass should be changed and all lines of disturbance thus broken up.

C. H. Rockwell's Almucantar sustains its good record in latitude work admirably. Recently he says: "For July 24, I had eight pairs of stars. Five of these gave a correction of less than one-tenth of a second to my assumed latitude, 41° 04′ 13.72″. The other three pairs gave corrections less than two-tenths of a second. The largest was 0.192″; the smallest, 0.001″.

Sun Spots.—From my observing book I make the annexed showing of sun spots for 1886, which may interest some of the readers of the SIDEREAL MESSENGER.

1886.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Spots..........	15	16	22	24	19	19	23	24	24	20	1	6
No Spots......	2	1	0	1	3	3	4	4	4	3	22	5
Cloudy, no Observ'n ..	14	11	9	5	9	8	4	3	2	8	7	9

From the 30th of October to the 16th of November the sun was more free from spots for a longer continuous period than has occurred since 1879. G. G.

THE PLANETS.

Mercury will be near the sun Feb. 1, and far to the south.

Venus is an evening planet and Feb 1 sets 6*h* 16*m*.

Mars is near the sun and in the remote part of his orbit, and consequently of little interest to observers at present.

Jupiter rises Feb. 1, 14 minutes after midnight, and is in conjunction with the moon Feb. 13 at 7 o'clock A. M.

Saturn is an evening planet and rises at 2*h* 51*m* P. M. on the 5th day of the month. It is more than 22° north declination.

Uranus rises at 10 o'clock and 14 minutes in the evening Feb. 1.

Neptune rises 21 minutes before midnight on same date before mentioned. The above figures give the approximate places for the purpose of identifying objects only.

Ten Years' Progress in Astronomy.—Professor Young notices some errors in his paper in our last issue. On page 35, date of photography at Cambridge, by slip of pen, was written 1861; it should have been 1851. The others following he justly charges to our proof-reader. Absence of the editor during part of the printing, is the explanation. He says:

"Let me congratulate you on *improved* proof-reading. But you are not perfect yet. I notice,

"On page 31, about middle of page, 'acceleratio*u*.'

"On page 37, about middle of page, Dober*e*ck for Doberck.

"On page 39, near top, Crossly for Crossley.

"On page 44, near bottom, Mich*a*elson for Michelson (twice).

"On page 47, near top, 'geogra*g*hical.'

"On page 47, near middle, 'Iraunhofers' !

"On page 48, two-thirds down, Exe*c*ter for Exeter."

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CONDUCTED BY WM. W. PAYNE,

Director of Carleton College Observatory, Northfield, Minnesota

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OPTICAL APPEARANCES OF COMETS.

S. V. CLEVENGER.

For the Messenger.

The olden, though still current, conjectures of astronomers regarding the causes of cometary and meteoric phenomena, in their intricacy, are paralleled by the Ptolemaic explanations of planetary movements.

Having recently looked over some notes which I made about fifteen years ago, additional consideration enables me to submit a view which seems to reconcile the apparent discordances and bring the astonishing metamorphoses of these "visitants from the star-depths" into accord with simple optical laws and reduce their anomalies to ordinary astronomical explanations.

Comets are aggregations, usually spherical or spheroidal, of relatively tenuous and variable densities, and their nuclei and tails are optical appearances only.

The substances composing comets may be heterogeneous or homogeneous; one may consist of dense solids such as meteoric iron particles of various sizes, with liquids and gases, and another contain gases alone; but the entire body, of whatsoever composed, must be globular or approximately so.

Granting these premises the first or telescopic globular appearance of a comet is the real one, because at great distances a nebular mass would appear compact, "luminous mists without trains, sometimes without central condensation."

The coma of the telescopic or just visible comet includes the entire body, and the subsequent appearances are due to the light reflected, in varying ways, as the mass approaches the sun and earth.

The nucleus or most luminous part is usually absent in telescopic comets and is no less transparent than the coma, for Herschel II saw 16 and 17 mag. stars through that of Biela in 1832, and Miss Mitchell records a central transit in 1847 over a 5 mag. star "which shone through it unchanged." This nucleus lies as Arago pointed out toward the margin of the coma nearest the sun. I consider this apparition to be due to the simple reflection of light from the convex surface of the coma, the circular, oval or radiated nuclei caused by surface or density variations in the cometary aggregation and by the sun's position.

The coma irregularity, as it nears the sun and the observer, is owing to the nearer view of the cloudy mass causing such portions to disappear as are not reflecting light to the observer, and the still nearer advance of the cometary globe by apparent dispersion of the nearest particles compared with the relative compactness of the farthest particles, change the reflection from those afforded by a convex to such as are characteristic of a concave surface. As the mass approaches the observer the nearest molecules or small solids are, relatively to him, more widely dispersed, while the farthest particles are relatively close, causing the globe to act as a concave reflector. In other words, perspective would afford an apparently compact reflecting surface, necessarily concavely cut by the range of vision in all directions; proximity to the near surface relatively dispersing both substance and reflected light.

In cases where the dispersion of light was incomplete the coma shape of the comet of 1862 would be afforded; the nucleus and the tail projected from the bottom of a cup-shaped luminous cloud. When the concave reflecting surface is thus presented the nucleus is merely the focused rays from such surface and the luminous tail is the illuminated paths of such rays, the principal axis being in the line of the radius vector, secondarily subjected to distortion through heterogeneity in density or reflecting surface of the coma (used in the sense of body instead of head).

The records heretofore made of comets may not admit of their elements being calculated upon this new hypothesis, but with future apparitions of the kind the following may serve as working suggestions:

Obliquity of the incident ray would increase the nucleus and tail sizes and their light.

The nature of the nebular globe penetrated by the sunlight would modify the comet appearance.

Extreme distance would cause the coma to appear as an opaque body through regular and irregular reflection.

Cæteris paribus, double the tail length would be the radius of the concave reflecting surface, remembering that this curvature is not that of the entire sphere of the coma, but an apparent concavity made in that sphere by the line of vision with a radius much greater than that of the cloudy globe, hence the tail may describe, at times, nearly the diameter of the entire gaseous globe.

If, through relativity of apparent sun and coma sizes, the sun rays are not parallel, but diverge upon the coma concave, then the nucleus and tail behind it will be longer than one-half the reflecting convexity radius. And as the sphere approaches the sun a greater elongation would occur because the rays are concentrated in conjugate foci in advance of the principal focus.

If the sun occupied the apparent principal focus, the extinction of the nucleus and tail would follow. Sudden disappearances have occurred, and the failure of Biela's comet may be owing to solar malposition for the production of the conical reflection.

Were it possible for the sun to occupy a point between the principal focus and apparent concave surface then the virtual focusing would turn the tail toward the sun. With density and distance suitable the convexity of the cloud would afford a virtual image with similar relations, but both of these suppositions are questionable, and where a tail proceeded toward the sun the usual one from the sun existed as a rule, and secondary optical causes must be sought for.

The reflections producing the nucleus and tail indicate that the spherical aperture is usually limited to a few degrees, but in instances where a larger than 10° or 15° aperture existed spherical aberration by reflection would produce multiple effects such as the "beaded head" or caustic outlines.

Causticity produces many brilliant appearances resembling cometic illustrations. The reflections inside an ordinary china bowl or tea-cup from a lamp light, strikingly picture many of the shapes of Comet III 1862, and the complex light streams of 1861, July 2, drawn by Brodie, also the changing aspects in June and July of Comet III 1860.

Planetary proximity could alter the apparent densities of parts by real changes in the coma shape, comparable to tidal bulgings, and this distortion of the reflecting surface could produce apparent division of the comet such as Biela's underwent. Exchange in brilliancy between the two parts is undoubtedly reflection and not an illumination inherently derived.

Re-reflection from relatively plane surfaces could also induce multiple images and the gaseous globe could sustain relations with the sun and earth rendering such image multiplication possible.

Aberration of light would explain the tail bending, for rays emanating from the concave surface could not be seen at the same instant as those which had passed to the focused nucleus, and as the entire coma has a proper motion in space the apparition would be that of a bent cone ordinarily, unless the coma were advancing toward the observer and the sun.

The perihelion distance of 1843 I, was calculated at half a million miles. Secchi and others remarked the astounding acceleration of its passage in this part of its orbit. In two hours the head passed the sun and in one day it described 292° of its orbit, which if elliptical left but 68° undescribed to its next return. This is best explained by the supposition that with the disappearance of the focused rays behind the sun, the slow passage of the immense coma afforded quickly a new concave surface for reflection. It is quite probable that a comet may thus appear upon each side of the sun, reflected from the same gaseous sphere.

Irradiation certainly augments the coma appearance. High powers often dissolve away apparent solidities. Comets notoriously are best observed through lowest powers, and the tail still better by the unassisted eye.

Prof. A. W. Wright (*Silliman's Journal*, Vol. VIII, p. 156) deduces from polarization observations that the cometic light is largely due to reflection from the sun. Tyndall fully demonstrated the reflecting powers of diaphanous gases, while Webb affirms that "polarization experiments concur with spectrum analysis in showing that tails as well as comæ shine by reflected light," and that coruscations are due to teluric atmospheric conditions.

Interference of light can be advantageously studied with reference to subsidiary appearances, such as Donati's nucleus and streamers presented, and real or apparent density differences in the comæ would undoubtedly refer many peculiarities to refraction. In all the preceding estimations the apparition evidently does not occupy the entire field of the coma globe; the earth or several planets may be surrounded by a portion of the cloudy agglomeration, and many facts lend color to this supposition. Hind remarked an auroral glare in the sky while the comet of 1861 was conspicuous in the north, and Chambers states that candles were lighted while the sun was still shining, the comet appearing hazy in the darkness. Other unaccounted-for dark days are historically noted, and the passage of a tenuous mass, such as a coma, would explain such circumstances, even though no comet had been observed at the time. If, as Keplar stated, "comets are as thick in the heavens as fishes in the ocean," and 4,000 of them appearing during the Christian era seem to justify the remark; we may consider that but a small number have been seen of those which really exist.

Bessel, D'Arrest, Hevelius and Herschel confess to having often mistaken nebulæ and comets, one for the other. The assumption of inter-planetary comæ really constituting comets, in this connection, may well suggest the strong probability of inter-stellar nebulæ being of the same nature. Of course the resolvable clusters are out of the question, but the remaining gaseous, and even some associated with a few stars, seem to answer the conditions needed to place them in the list of cometary masses. Hind described in Taurus a new nebula in 1852, which D'Arrest in 1861 found had vanished, but later it was re-discovered in the same year.

That there is some connection between the variable nebulæ and the stars, with which they are associated, there can be no doubt, and this connection we may safely affirm to be akin to what exists between our sun and comæ. Hind suggests that the nebula can shine by light reflected from a star. The nebula in Argo observed by Herschel underwent similar vicissitudes. Huggins speculates upon the " motions of some of the nebulæ toward or from the earth," in Proceedings of the Royal Society, March, 1874. Some, he conjectures, approach with a velocity of 35 miles per second and have a motion in rotation and internal translation of portions. We may well question whether the appearance of nebulæ is even approximately real, for all parts of such thin clouds may not be equally lighted.

Proctor questions " whether any meteor system exists without a cometic nucleus and whether any comet exists without a meteoric train," as the only meteor systems whose paths have been recognized are found to travel in the orbits of known comets (" Essays," p. 143). But, he adds, " how can flights of solid bodies come to be associated with gaseous comets ?"

Tempel's telescopic comet of 1866 accords in its orbit with that of the November meteors, as assigned by Dr. Oppolzer, and Newcomb estimates that 146,000 millions of meteoric bodies fall each year upon the earth, but Proctor suggests that these bodies appear luminous at 70 miles and vanish by vaporization at 50 miles.

The main questions are, What is the connection between comets and meteors, and how do solid meteoric substances come to be associated with gaseous meteors ?

The condensation of cometic matter into meteorites and their vast occlusion of hydrogen is the most rational supposition, based upon the determinations of Graham with the Lenarto meteorite. This condensation might occur at such parts of the coma as surrounded the earth in its passage through it, the remainder of the cloudy globe continuing to be gaseous. Radiant points for meteoric displays thus consist of such meeting places of earth and comæ ; the olden supposition of there being a zonal or circum-orbital envelope of meteorites being thus rendered unnecessary. Repeated contacts with the earth or other planets would by loss of substance reduce nebulous masses, and account for the successive palings of returning comets and their entire disappearance.

TIME.*

J. G. PORTER.†

Time is one of the most common and universal of all conceptions; and yet when we begin to analyze this conception, how mysterious, how unfathomable does it become! We say time is measured duration. When did this duration commence? Up the stream of time we gaze through the generations of human life, past the landmarks of history, back through unknown ages of geologic and astronomic time, up to that dim, mystic point, "In the beginning." Amid the throes of creation's birth did time also spring into being? Was it then that its pulse first began to beat, and the fleeting seconds to multiply into minutes, the minutes to glide into hours, the hours into days and years, and the years to roll up into centuries and ages? If so, what was back of this birth-day of time? A void, incomprehensible, unthinkable! A timeless blank, an unbounded, unfathomed ocean of nothingness!

We turn to look down this stream of time, but the mists of futurity gather darkly and it is soon lost to our view. If we cannot trace its source, neither can we track its onward course

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*Written for the Astral Club, Jan. 1887.

†Astronomer in charge of the Cincinnati Observatory.

nor discover the sea wherein its waters are finally lost. It is true that in the Book of Revelation we read of the angel who, standing upon the sea and upon the earth, swears that there shall be time no longer. But the following verse seems to me to render the meaning of this declaration plain: "But in the days of the voice of the seventh angel when he shall begin to sound, the mystery of God shall be finished as he hath declared to his servants the prophets." It is, then, the time of redemption, the mediatorial dispensation which shall be ended and exist no longer, and not time itself abstractly considered.

And here let me ask if it be possible when speaking of created beings to distinguish between time and eternity. Is eternity other than endless extension of time? Finite intelligences are and must ever be limited in time as well as in space. There is for them a past, a present and a future, as well as a 'here' and a 'there.' As long, therefore, as created finite intelligences exist there must be a succession of 'nows' which constitutes duration, and there must be some measure for this succession which constitutes time. There is, in fact, no true eternity except for the uncreated, self-existent God. Of Him it is said that He inhabiteth eternity: a thousand years are with Him as one day. As His presence fills all space, so does it fill all time. Eternity past and future is ever present to Him. But even the highest created beings dwell in time and are subject to its never-ceasing flow. The archangel Gabriel we are told, when sent forth to minister unto Daniel, was caused to fly quickly and touched him at the time of the evening oblation, thus showing that he was limited both by space and time, and could act only in one place at any given moment. Surely we can not hope, even when we cast off these weak and perishing bodies which so clog the spirit's flight, even when clothed with immortality we cannot hope to rise above the state of the archangels. We shall still be subjects of time. Its streams will not be lost in the ocean of eternity, but through all the ceasless eons of never-ending duration it will flow onward, still onward. Its measure may not be the same as now. Indeed, astronomy teaches us that the divisions of time as we have them belong only to this earth. Transferred to other members of the solar system we find different lengths of the day and year. Mercury's seasons run their course in less than three terrestrial months. Saturn's day is about ten hours, while his year is twenty-nine earth years. Upon slow-coursing Neptune Methuselah's life-time would have been but six years, and the whole period of human history would have occupied less than half a century. As we proceed upward in the order of celestial movements, the periods swell and expand into absolute infinity. Even our solar system contains cycles which confound the imagination. The revolution of the earth's pole among the stars occupies twenty-five thousand years, and the oscillations of the planetary orbits range all the way up to two million years. Yet these are nothing compared with periods which we know must exist in the realm of the fixed stars, eras so far-reaching and grand that they have well been called "great clocks of eternity which beat ages as ours beat seconds."

I do not here enter upon the question whether the universe in its present form is destined to be eternal, or whether in our future state we shall still have material surroundings. All I have sought to show is that as created intelligences we can never rise above the limitations of time, nor progress beyond it. However we measure it, it is still the same wonderful, impalpable, yet grandly real mystery! Can you grasp the conception? Can you explain this mystery, forever here, yet constantly slipping away?

Sometimes I have stood before the clock in yonder observatory and watched the hands as they approached the hour of midnight. Steadily the seconds glide away: twelve o'clock strikes, and I slip from to-day into to-morrow—from yesterday into to-day. Where has the day gone? Yea, where do these beating seconds go? So fleet-winged that ere we realize that they are here, they have flown, vanished into the dim past! Do they travel onward forever through universal space, as some have fancied our words and actions do? Might we by some magic overtake them and live them over again? Vain hope! They are gone irretrievably. Only the present is ours. What we give to that present as it passes will indeed endure forever, but can never be altered or recalled.

Have you ever thought of the difference in this respect between time and space? We are limited by the latter as well as by the former: we can be in one place only at a time; and if we speak of absolute space, perhaps we can never revisit the regions we are now passing through, unless the endowments of a future life enable us to track the devious course we are now pursuing; for owing to the motion of the solar system, it comes to pass that even at the end of the year we do not arrive at the spot where we were twelve months before, but strike out into before untrodden realms of cosmic space. But practically, as far as place has any interest for us, we are free to come and go at will. We can revisit old scenes and explore new ones, being only limited by the earth's extent. But with time how different! Past seasons we can recall only in memory, future ones we can anticipate only through hope or imagination. So that time is a more evanescent and, perhaps we might say, ideal conception than space. Both conceptions have, when we come to analyze them closely, many elements of mystery. But time partakes pre-eminently of the marvelous. The fraction that is with us now we accept without thought, but this infinitesmal fraction is linked by indissoluble bands to an eternity past and to an eternity to come. These fugitive moments are golden, for though we shall never see them again, they are building up for us character and destiny which shall last forever.

THE METEORITES, THE METEORS AND THE SHOOTING STARS.

PROFESSOR H. A. NEWTON.

(Continued from page 72.)

Again the meteoroids do not cause the acceleration of the moon's mean motion. In various ways the meteors do shorten the month as measured by the day. By falling on the earth and on the moon they increase the masses of both, and so make the moon move faster. They check the moon's motion and so bringing it nearer to the earth, shorten the month. They load the earth with matter which has no momentum of rotation, and so lengthen the day. The amount of matter that must fall upon the earth in order to produce in all these ways the observed acceleration of the moon's motion has been computed by Prof. Oppolzer. But his result would require for each meteoroid an enormous mass, one far too great to be accepted as possible.

Again, the supposed power of such small bodies, bodies so scattered as these are even in the densest streams, to break up the comets or other heavenly bodies, and also their power by intercepting the sun's rays to affect our weather must in the absence of direct proof to the contrary be regarded as insignificant.

So too their effect in producing geologic changes by adding to the earth's strata has without doubt been very much overestimated. During a million of years, at the present rate of say fifteen millions of meteors per day, there comes into the air about one shooting star or meteor for each square foot of the earth's surface.

To assume a sufficient abundance of meteors in ages past to accomplish any of these purposes is, to say the least, to reason from hypothetical and not from known causes.

The same may be said of the suggestion that the mountains of the moon are due to the impact of meteorites. Enormously large meteoroids in ages past must be arbitrarily assumed, and, in addition, a very peculiar plastic condition of the lunar substance in order that the impact of a meteoroid can make in the moon depressions ten, or fifty, or a hundred miles in diameter, surrounded by abrupt mountain walls two, and three, and four miles high, and yet the mountain walls not sink down again.

The known visible meteors are not large enough nor numerous enough to do the various kinds of work which I have named. May we not assume that an enormous number of exceedingly small meteoroids are floating in space, are falling into the sun, are coming into the air, are swept up by the moon? May we not assume that some of these various forms of work which cannot be done by meteoroids large enough for us to see them as they enter the air, are done by this finer impalpable cosmic dust? Yes, we may make such an assumption. There exist no doubt multitudes of these minute particles traveling in space. But science asks not only for a true cause but a sufficient cause. There must be enough of this matter to do the work assigned to it. At present, we have no evidence that the total existing quantity of such fine material is very large. It is to be hoped that through the collection and examination of meteoric dust we may soon learn something about the amount which our earth receives. Until that shall be learned we can reason only in general terms. So much matter coming into our atmosphere as these several hypotheses require would without doubt make its presence known to us in the appearance of our sunset skies, and in a far greater deposit of meteoric dust than has ever yet been proven.

A meteoroid origin has been assigned to the light of the solar corona. It is not unreasonable to suppose that the amount of the meteoroid matter should increase toward the sun, and the illumination of such matter would be much greater as we approach the solar surface. But it is difficult to explain upon such an hypothesis the radial structure, the rifts, and the shape of the curved lines that are marked features of the corona. These seem to be inconsistent with any conceivable arrangement of meteoroids in the vicinity of the sun. If the meteoroids are arranged at random there should be a uniform shading away of light as we go from the sun. If the meteoroids are in streams along cometary orbits, all lines bounding the light and shade in the coronal light should evidently be approximately projections of conic sections of which the sun's centre is the focus. There are curved lines in abundance in the coronal light, but as figured by observers and in the photographs they seem to be entirely unlike any such projections of conic sections. Only by a violent treatment of the observations can the curves be made to represent such projections. They look more as though they were due to forces at the sun's surface than at his center. If those complicated lines have any meteoroid origin (which seems very unlikely) they suggest rather the phenomena of comets' tails than meteoroid streams or sporadic meteors.

The hypothesis that the long rays of light which sometimes have been seen to extend several degrees from the sun at the time of the solar eclipse are meteor streams seen edgewise seems possibly true, but not at all probable.

The observed life of the meteor is only a second, or at most a few seconds, except when a large one sends down stones to remain with us. What can we learn about its history and origin?

Near the beginning of this century, when small meteors were looked on as some form of electricity, the meteorites were very generally regarded as having been thrown out from the lunar volcanoes. But as the conviction gained place that the meteorites moved not about the earth, but about the sun, it was seen that the lunar volcanoes must have been very active to have sent out such an enormous number of stones as are needed in order that we should so frequently encounter them. When it was further considered that there is no proof that lunar volcanoes are now active, and that when they were active they were more likely to have been open seas of lava, not well fitted to shoot out such masses, the idea of the lunar origin of the meteorites gradually lost ground.

But the unity of meteorites with shooting stars, if true, increases a hundred-fold the difficulty, and would require that the comets have the same origin with the meteorites. No one claims that the comets came from the moon.

That the meteorites came from the earth's volcanoes is still maintained by some men of science, particularly by the distinguished Astronomer Royal for Ireland. The difficulties of the hypothesis are, however, exceedingly great.

In the first place, the meteorites are not like terrestrial rocks. Some minerals in them are like minerals in our rocks. Some meteoric irons are like the Greenland terrestrial irons.

But no rock in the earth has yet been found that would be mistaken for a meteorite of any one of the two or three hundred known stone falls. The meteorites resemble the deep terrestrial rocks in some particulars, it is true, but the two are also thoroughly unlike.

The terrestrial volcanoes must also have been wonderfully active to have sent out such a multitude of meteorites as will explain the number of stone falls which we know and which we have good reason to believe have occurred.

The volcanoes must also have been wonderfully potent. The meteorites come to us with planetary velocities. In traversing the thin upper air, they are burned and broken by the resisting medium. Long before they have gone through the tenth part of the atmosphere the meteorites usually are arrested and fall to the ground. If these bodies were sent out from the earth's volcanoes they left the upper air with the same velocity with which they now return to it. This the law of gravitation demands. What energy must have been given to the meteorite before it left the volcano to make it traverse the whole of our atmosphere, and go away from the earth with a planetary velocity! Is it reasonable to believe that volcanoes were ever so potent, or that the meteorites would have survived such a journey?

No one claims that the meteors of the star-showers nor that their accompanying comets came from the earth's volcanoes. To ascribe a terrestrial origin to meteorites is then to deny the relationship of the shooting-star and the stone-meteor. Every reason for their likeness is an argument against the terrestrial origin of the stones.

To suppose that the meteors came from any planets that have atmospheres involves difficulties not unlike to, and equally serious with, those of a terrestrial origin.

The solar origin of meteorites has been seriously urged, and deserves a serious answer.

The first difficulty which this hypothesis meets is, that solid bodies should come from the hot sun. Besides this, they must have passed without destruction through an atmosphere of immense thickness, and must have left the sun with an immense velocity.

Then there is a geometric difficulty. The meteorite shot out from the sun would travel under the law of gravitation nearly in a straight line outward and back again into the sun. If in its course it enters the earth's atmosphere, its relative motion, that which we see, should be in a line parallel to the ecliptic, except as slightly modified by the earth's attraction. A large number of these meteors, that is most, if not all, well observed fireballs, have certainly not traveled in such paths. These did not come from the sun.

It has been a favorite hypothesis that the meteorites came from some planet broken in pieces by an internal catastrophe. There is much which mineralogists can say in favor of such a view. The studies of M. Stanislas Meunier and others into the structure of meteorites have brought out many facts which make their hypothesis plausible. It requires, however, that the stone-meteor be not regarded as of the same nature as the star-shower meteor, for no one now seriously claims that the comets are fragments of a broken planet. The hypothesis of the existence of such a planet is itself arbitrary; and it is not easy to understand how any mass that has become collected by the action of gravity and of other known forces should by internal forces be broken in pieces, and these pieces rent asunder. The disruption of such a planet by internal forces after it has by cooling lost largely its original energy would be specially difficult to explain.

We cannot then look to the moon, nor to the earth, nor to the sun, nor to any of the large planets, nor to a broken planet as the first home of the meteoroids, without seeing serious if not insuperable objections. But since some of the meteoroids were in time past certainly connected with comets, and since we can draw no line separating shooting stars from stone-meteors, it is most natural to assume that all of them are of a cometary origin. Are there any insuperable objections that have been urged against the hypothesis that all of the meteoroids are of like nature with the comets, that they are in fact fragments of comets, or it may be in some cases, minute comets themselves?

If such objections exist they ought evidently to come mainly from the mineralogists, and from what they find in the internal structure of the meteorites. Astronomy has not as yet furnished any objections. It seems strange that comets break in pieces, but astronomers admit it for it is an observed fact. It is strange that groups of these small bodies should run before and follow after comets along their paths, but astronomers admit it as a fact in the case of at least four comets. Astronomically, there would seem to be no more difficulty in giving such origin to the sporadic meteor, and to the large fireball, and to the stone-meteor, than there is in giving it to the meteor of the star-shower. If then the cometic origin of meteorites is inadmissible, the objections must come mainly from the nature and structure of meteoric stones and irons. Can the comet in its life and history furnish the varied conditions and forces necessary to the manufacture or growth of these peculiar structures?

It is not necessary in order to answer this question to solve the thousand puzzling problems that can be raised about the origin and the behavior of comets. Comets exist in our system, and have their own peculiar development, whatever be our theories about them. It will be enough for my present purpose to assume, as probably true, the usual hypothesis that they were first condensed from nebulous matter; that that matter may have been either the outer portions of the original solar nebula, or matter entirely independent of our system and scattered through space.

In either case the comet is generally supposed, and probably must be supposed, to have become aggregated far away from the sun. This aggregation was not into one large body to be afterwards broken up by disruption or by solar action. The varieties of location of the cometic orbits seem inexplicable upon any such hypothesis. Separate centers of condensation are to be supposed but they are not *a priori* unreasonable. This is the rule rather than the exception everywhere in nature.

Assume then such a separate original condensation of the comet in the cold of space, and that the comet had a very small mass compared with the mass of the planets. Add to this the comet's subsequent known history as we are seeing it in the heavens. Have we therein known forces and changes and conditions of such intensity and variety as the internal structure of the meteorites calls for?

(*To be continued.*)

THE TOTAL SOLAR ECLIPSE OF AUGUST NEXT.

JACOB ENNIS.

As the changes in the size and the form of the solar corona are so frequent, and as the time for observation at any one point is so very short, it will be important at the solar eclipse in August next to make observations on its entire line from western Europe to eastern Asia and further on through Japan. This will afford two or three hours to view the coronal changes. The importance of this is illustrated by the eclipse across the Americas in July, 1878, when about half an hour intervened between the observations in Montana and those in Texas and when such enormous changes astonished all reflecting astronomers. They are recorded in a quarto volume compiled by the naval observatory in Washington, D. C. At one point two projections or streamers of the corona on opposite sides of the sun extended outwardly to the vast distance of 5,000,000 miles. At another point a third projection was seen at right angles to the other two, and equally as long. At another station one of these streamers reached outwardly 10,000,000 miles. The latter observer, Prof. Langley, says, "It was evident that I was witnessing a real phenomenon heretofore undescribed. The distance of twelve solar diameters through which I traced it, I feel great confidence in saying *was but a portion of its extent.*" The italics are his own. Later observers in Texas saw the extensions to be shortened to only a single solar diameter and less.

For solving the mysterious problem of the cause of the solar corona, no facts are more important than the suddenness and the greatness of its changes, both in form and in size. These changes are admirably collected and portrayed in the 41st volume of the Memoirs of the Royal Astronomical Society, London, which is devoted entirely to the total solar eclipses of modern times. Among all these many drawings of the corona there are no two at all resembling one another. The same must be said of the ten drawings of the forms of the corona given by Prof. Young in his volume on the sun. The coronal changes take place even during the few minutes of observation of a single eclipse in a single locality. Prof. Plantamour drew very carefully three pictures of the corona in the eclipse of July 18, 1860. They appear along with many others in the 41st volume of the Memoirs just named. The first and the third are quite different, while the second shows the gradual transition from the one to the other. Mr. Gilman also gives three drawings of the corona in the eclipse of December 22, 1870. While they are all as different as can be from those made ten years before by Plantamour, they yet show the same shifting panorama, the first and the third are very unlike and the second exhibits the manner in which the change went on from first to last. In the report made concerning this latter eclipse by the deputation from our own Government Observatory, that of Captain Tupman is especially interesting, for he particularizes with special emphasis the changes which were in progress in the corona during the time of totality.

Besides these changes of the solar corona in size and in external form, there are others of an internal character. In the eclipse of 1878 Trouvelot said, "The soft, pearly light which the streamers emitted at once suggested the zodiacal light." Newcomb said, "The streamers shaded off by insensible gradations, and struck me as having a great resemblance to a representation of the zodiacal light on a reduced scale." Langley said, "I should compare it to the zodiacal light with more confidence than to anything else." Now the zodiacal light has rapid pulsations, cloud-like forms of light, running through it from below upwards, but on account of this faintness they are seldom observed. Humboldt was the first to mention them, which he did in his Cosmos while watching the sky during his residence in South America. I have seen them, though rarely. One observer reports seeing these pulsations in the corona in the eclipse of 1878. Prof. Ormond Stone writes, p. 238, of the eclipse of 1878, "Around the sun was a narrow, bright halo, very nearly white, but slightly tinged with orange yellow." Prof. Lewis Swift says, "The pencils or streamers of light appeared to have a rapid motion in the direction of their length. The motion appeared not pulsating, but a constant outflow of what I consider to be electric light of high tension streaming off in straight lines into space. This flow in one direction suggested to me, at the time, the jets of water from a street sprinkler."

E. J. Brookings, Esq., of Washington, D. C., made a communication to *The Evening Star* of that city which is worthy of a record in some astronomical work, and it is appropriate here. He wrote: "During the winter of 1860 I was living in Maine, and after the perihelion of the wonderful comet of that year (the nights being cold and the atmosphere very clear) I saw distinctly, and for several nights in succession, scintillations of light in the form of waves, pass very rapidly from the coma of the comet to the extremity of its tail. The scintillations or waves were so rapid and continuous that my curiosity was excited and I called the attention of my father to the fact, and he, too, distinctly saw the curious phenomenon. That this was no atmospherical disturbance, the wavy motion alone would determine. The action was precisely similar to that of the aurora borealis, and, although a boy, as it were, I came then to the conclusion that this action was electricity, no matter what might be the material composing the tail."

I was also in the State of Maine on Mount Desert Island in the summer of 1873 when Coggia's comet appeared above the northern horizon with its large tail projecting straight upward. Almost every night the streamers of the aurora borealis appeared and also shot straight upward. No difference could be detected between the two phenomena. The streamers of the comet and the streamers of our planet seemed identical. Sometimes they overlapped and became intermixed on the northern sky so that there was no telling which was which.

It is objected by one of the observers of the eclipse of 1878 that if the corona were an outgo of electricity, it would be even on all sides of the sun alike. But we know that the aurora borealis is an electric effect, and yet the tall streamers are often wide apart with vacant, or nearly vacant spaces between. Some comets also have more tails than one; these are of quite different dimensions.

About the eclipse of 1878 Prof. Boss wrote: "These radiating streamers resembled very much the white ones frequently seen in auroras." "An apparent pulse or luminous wave motion continually occurred from the limb outward, reminding me again of the aurora, in which a similar motion is often seen. Perhaps as good a comparison would be the white streams emitted from the head of a bright comet." Prof. Keeler wrote: "I took one glance with the naked eye, and could see the corona extending apparently much farther than I could with the telescope, but could not see the streamers of the inner corona. I think that the variations of light in the corona and its forks and branches are more easily recognized without the aid of the telescope." Prof. Trouvelot wrote: "The naked eye views of the corona differed considerably from the telescopic views, as should have naturally been expected."

The above facts show that five phenomena in the corona are to be looked for in August next,—the size, the form, the changes of form and size, the pulsations, and the colors. They show also that the telescope may be dispensed with. The same is true of the photographic apparatus; the time is too short at any one locality to photograph the faint and delicate extensions of the corona. This is important in the wide regions of central Asia, of China and of Japan, where instruments cannot easily be taken.

Nothing need here be added of my theory about the causes and the identity in nature, of the solar corona, the tails of comets, the zodiacal light, and the aurora borealis. Already this was done in the last October number of the SIDEREAL MESSENGER.

KEPLER'S CORRESPONDENCE IN 1599.

[Through the kindness of Professor J. Hagen, S. J. College of the Sacred Heart, Prairie du Chien, Wis., we have had the pleasure of examining a pamphlet of 118 pages which contains three letters written by Kepler in 1599 to Chancellor Herwart von Hohenburg of Bavaria. In 1881, the editor of Kepler's works, who was a member of the royal Wurtemburg board of national education, said some letters were missing that Kepler must have written in 1599, and he presumed they were lost.

The editor of the pamphlet before us, C. Anschutz, says in the introduction:

"As I was recently looking over the Latin manuscript of 1607 at the Munich royal national library which, thanks to the kindness of the library management, I had the pleasure of examining while there, I found three original letters from Kepler to Herwart in the year 1599. A closer examination proved beyond a doubt that they were the letters sought. The first of them, dated April 9 and 10, is evidently an answer to Herwart's letter of March 10."

These letters were first published in 1886 at Prague; they are in Latin and are followed by 34 pages of interesting notes in German. Professor Richardson, of Carleton College, has translated the first letter, a portion of which we give below.—ED.]

Letter I.

Your letters, most learned and noble sir, written on the tenth of March, I received on the sixth of April. You raise many questions in them; of these, I will defer for the present, those which require work and computation, since in the two days which the courier has announced to me both other and sacred matters must be treated: but I will briefly touch upon those which can be solved by ready memory, not indeed, because I think that you so greatly need my work; both whose ready memory, and careful reading, and discretion especially in judging I have seen not only on many former occasions, but especially in these letters and in the question concerning the birth-day of Octavius; but because I see that you are delighted with literary discussions of this nature. For unless your nature and genius drew you hither, long since would this very dull letter of mine have persuaded you to silence.

The first question you raise is regarding the calculation of eclipses, and, although you are pleased that a reform in this has been attempted by me, yet you oppose my experience and observation. Therefore I should prefer to excuse myself to you, blaming my youthful zeal, rather than to give a reason for my rashness in boasting in public of that which I had scarcely well dreamed of in private. For what am I among the masters who have long been trained and practiced in observations? Let all be silent, and give heed to Tycho Brahe, the Dane, who now for the thirty-fifth year is devoting himself to observation, who sees more with his eyes than many others with mental vision, a single instrument of whom cannot be counterbalanced by my whole thought and being, in comparison with whom Ptolemy, the Alphonses, Copernicus, may be considered as boys, except that he ascribes to them a great part of his wisdom and the advantages derived from their discoveries: as indeed it is always easy to add something to what has been discovered. But to speak most truly what I think: he alone has understanding and they as shadows flit about. Nor does it embarrass me that he was a little opposed to the motion of the earth. For he does not refer it to education, by which as a tradition one may follow any motion. But to reality. You will read, I hope, a part of his letter, which he gave to Mästlin last year, joined to these letters. From that you can exclaim concerning me and those like me: "Oh human griefs, oh what an empty world!"

Certainly it is more than a puzzle to me, that anything seems smaller near at hand, than if it is further removed. For before this, the opticians had taught me the opposite. The moon, he says, in solar eclipses is quite low and yet it appears smaller in diameter than in eclipses of the full moon, in which, other things being equal, it is in the same proximity to the earth. I do not know what I shall say to him. The theory of parallax certainly shows this proximity, other observations show a perceptible quantity. A wise astronomer will not strive for either. Therefore I await the publication of his works with great eagerness. Since I have already entered upon this discussion, I will defend my temerity against your experience, and although undertaken by a plan not the wisest, yet the worst result may not follow. You place before me six eclipses, in all of which I am wonderfully strengthened in my reform, I would better say correction, of the calculation.

1. You write Dec. 29, 1591, that you have carefully observed the Prutenic tables. Good! For if anyone should correct the Prutenic calculation in this way which I[4] have mentioned in my suggestion, he will make no change in the Prutenic tables about the end of June and December, while the sun is in apogee or perigee. For although there is the greatest difference between the moon of December and June or January and July (other things being equal) nevertheless the effect of this difference is chiefly augmented in the quadrants Aries and Libra. Take this example. Although the daily motion of the sun is greatest in Capricornus, least in Cancer, nevertheless the difference of the true motion from the mean in Capricornus and Cancer is nothing, in Aries and Libra the greatest. There is the same reasoning in my suggestion concerning the moon also.

2. The second eclipse February 20, 1598: I think you are mistaken in the observation. I do not say this alone, but Tycho also. And although I am without instruments, nevertheless from the time of setting, it was evident without doubt that the Prutenic calculation was in error by about a whole hour. By which the eclipse followed it. I remember that I point this out in my appendix to the prognosticon from which you see I do this also for myself.

3. In the month of February of this year I had a clear night

and a comparison having been again made to the setting, I found the eclipse about three quadrants later than the Prutenic calculation.

(To be continued.)

COMETS OF 1886.

W. C. WINLOCK.

The following list is in continuation of that published in the SIDEREAL MESSENGER for February, 1886, (Vol. V, p. 50):

Comet 1886 I = Comet *d* 1885.
= Fabry's comet.
Comet 1886 II = Comet *e* 1885.
= Barnard's comet.
Comet 1886 III = Comet *b* 1886.
= Comet 1886. (Brooks 2.)
Comet 1886 IV = Comet *c* 1886.
= Comet 1886. (Brooks 3.)
Comet 1886 V = Comet *a* 1886.
= Comet 1886. (Brooks 1.)
Comet 1886 VI = Comet *d* 1886.
= Winnecke's comet.
Comet 1886 VII = Comet *e* 1886.
= Finlay's comet.
Comet 1886 VIII = Comet *c* 1887.
= Barnard's comet.
Comet 1886 IX = Comet *f* 1886.
= Comet *f* 1886. (Barnard, Oct. 4.)
= Comet 1886. (Barnard-Hartwig.)

EDITORIAL NOTES.

Correspondents are respectfully urged to take great pains in preparing copy for publication. No time is set apart for deciphering or copying bad manuscript, though this has been done too often in the past. It is also unpleasant to return good matter to the writer on this account when a little more pains at first would make it wholly unnecessary.

This is a grand year for comets, judging from the record of the last two months. It is plain that Barnard and Brooks have been sitting up nights to get even with Dr. Warner for reducing the prizes last year. We guess he wishes now he hadn't, for certainly Professors Chandler, Boss and Egbert will have to rub their eyes and sit up nights also, to get out all the orbits and ephemerides on regulation time after discovery.

Comet a 1887 (Thome).—As stated last month this comet was discovered by Dr. Thome, director of the observatory at Cordoba, South America, on Jan. 18, in the constellation *Grus*. The tail was seen at Melbourne observatory Jan. 19, and at Adelaide observatory Jan. 20. Though the comet is not yet brilliant, it is said to be easily visible to the naked eye, even in strong twilight, and to appear somewhat like the Great Southern Comet of 1880. The *Observatory* speaking of it about the time of its discovery said, "the tail is long, narrow and straight and has been traced for a length of about 30.°"

With Meyer's elements of Comet 1880 I, and an assumed perihelion passage on Jan. 11, 1887 G. M. T., Mr. Chandler computed an ephemeris for the month of February which gives the comet a north-easterly motion with increasing distance from the earth and sun and rapidly diminishing brightness. By this calculation the comet should now (Feb. 22) be on the western boundary of Eridanus 5° south of a lone third magnitude star and 4° east of β Fornax. The comet will be watched by Southern observers with interest.

Comet b 1887, (Brooks).—Under date January 25th, Mr. Brooks writes: "On Saturday evening, January 22, I discovered a new comet in the constellation Draco in approximate R. A. 18 *h* 0 *m*; decl. north 71°. It required but a short time to detect motion and the discovery was telegraphed to Dr. Swift the same evening, and he at once instructed Harvard to cable the discovery to Europe. Last night and this morning, Jan. 24th, I observed the comet again. Dr. Swift was enabled to verify the discovery at about the same time in the evening, and he found its position to be at that time R. A. 18*h* 20*m* 28*s* + 73° 36′ 34″. The comet is round, with some central condensation, and easily visible with modern apertures. Its motion is slowly in a northeasterly direction."

Red House Observatory, Phelps, N. Y.

WILLIAM R. BROOKS.

Professor L. Boss' elements for this comet, as published in A. J., No. 152, are:

$$T = 1887 \text{ March } 16.7117 \text{ G. M. T.}$$

$$\left.\begin{array}{rl} \omega & = 158^\circ\ 53'\ 30'' \\ \Omega & = 279\ \ 43\ \ 18 \\ i & = 104\ \ 22\ \ 33 \\ \log.\ q & = 0.21372 \end{array}\right\} 1887.0$$

Comparison with middle places gives (C — O) :—

$$\Delta\lambda \cos\beta = -6''; \quad \Delta\beta = -14''$$

EPHEMERIS FOR GREENWICH MIDNIGHT.

Date.	A. R. h	m	s	Decl. °	′	Light.
March 1	3	26	16	+55	58.2	1.1
5	3	38	37	51	47.4	1.1
9	3	48	58	47	53.7	1.0
13	3	57	56	44	17.5	0.9
17	4	5	52	40	58.6	0.8
21	4	13	4	37	56.1	0.8

The unit of light is referred to the date of discovery. By the above elements this comet was brightest about the middle of February, and March 5 it will pass *a Perseus* a few degrees to the northeast. Professor Boss remarks in the *Science Observer* the singular resemblance of the elements of this comet to those of the comet of 1491, excepting in perihelion distance, this being more than twice as great as that found by Professor Pierce for the former.

Comet 1886 (Barnard c 1887, Jan. 23).—February 14, Mr. Barnard writes:- "On the evening of January 23, a telegram came announcing the discovery of a comet by W. R. Brooks. It was raining heavily at dark, but, suddenly it turned very cold and cleared at midnight. Mr. Brooks' comet was picked up with the 6-inch Cook equatorial, and its accurate position determined with the ring micrometer, ten comparisons being obtained. As soon as these observations were finished, I began comet-seeking with the 5-inch refractor, and at about 5 o'clock A. M. I ran upon a dim hazy object several degrees south-west of β Cygni which I recognized as a stranger. The 6-inch was again brought into use and nine good ring micrometer observations obtained before it was blotted out by dawn. These observations showed clearly a northeasterly motion, but circumstances prevented any reduction of these observations until evening, so the motion was not certainly established until that time. The usual notification was given to Dr. Swift.

The discovery position, the mean of nine comparisons gave,

Comet preceded star 1*m* 31.03*s*.
Comet south of star 11′ 56.6″

Comparison star's mean place,

R. A. 19*h* 9*m* 15.22*s*; Decl. +25° 33′ 55.3.″
Red'n to app.—1.55*s*; +1.0″

Hence the resulting apparent place of the comet was,

January 23, 1887, 17*h* 36*m* 47*s*. Nash. M. T.
R. A. 19*h* 7*m* 42.64*s*; Decl. +25° 21′ 59.7.″

I am indebted to Professor Boss for accurate place of the comparison star. I have no accurate place for the star used with Mr. Brooks' comet. Bad weather has prevented many observations of this comet. E. E. BARNARD.

Vanderbilt University Observatory, Feb. 14, 1887.

The elements of this comet have been computed by Mr. Egbert of Dudley Observatory and E. Weiss, Wien, the latter elements being referred to the mean equinox of 1887.0, but the former, as printed in A. J. No. 152, are without reference, (probably an unintentional omission) and hence can not be compared to account for the apparent disagreement.

From a recent private letter Mr. Egbert states that the elements on which the following ephemeris is based will soon be published, and kindly sends in advance the places for March as follows:

EPHEMERIS.

1887.	A. R.	Dec.	Light.
Gr. 12 *h.*	*h.* *m*	° '	
March 2,	21 06.5	+48 00	.65
" 6,	21 21.8	50 06	.62
" 10,	21 37.6	52 06	.58
" 14,	21 54.0	53 58	.54
" 18,	22 11.0	55 42	.50
" 22,	22 28.5	57 17	.47
" 26,	22 46.4	58 44	.44
" 30,	23 04.8	+60 03	.40

Light at discovery taken as unity.

Distribution of the Minor Planets.—In the *Scientific American Supplement*, Jan. 22, 1887, it was stated that the gap at the distance 3.277 is the only one corresponding to the first order of commensurability. The distance 3.9683, where an asteroid's period would be two-thirds of Jupiter's, is immediately beyond the outer limit of the cluster as at present known; the mean distance of Hilda being 3.9523. The discovery of new members beyond this limit is by no means improbable. Should a minor planet at the mean distance 3.9683 attain an eccentricity of 0.3—and this is less than that of eleven now known—its aphelion would be more remote than the perihelion of Jupiter. Such an orbit might not be stable. Its form and extent might be entirely changed after the manner of Lexell's comet, or the orbit might be thrown into a cometary form without greatly modifying the period. Two well known comets, Faye's and Denning's, have periods approximately equal to two-thirds of Jupiter's. In like manner the periods of D'Arrest's and Biela's comets correspond to the hiatus at 3.51, and that of 1867 II to the first order of commensurability. (Distance 3.277.) A second mode of elimination from particular parts of the zone is thus suggested.

DANIEL KIRKWOOD.

The Swift-Watson Intra-Mercurial Observations.—Mr. Colbert's statements in your last number regarding Mr. Swift's observation are interesting, and some of them new to me; but they do not affect my opinion that "the two objects seen by Swift * * * were certainly not the two seen by Watson."

Swift's two objects, according to his repeated and persistent assertions, were both visible in the same telescopic field, at a distance first estimated as 7′, but afterwards as 12′; Watson's two stars were *four degrees* apart. I have not said that *one* of Swift's stars might not by some possibility be identical with *one* of Watson's; but even that is violently improbable, since Watson could hardly have failed to see them both, if he saw either.

As regards Professor Hough's excellent work upon Jupiter, I failed to notice it in the paper referred to, simply for want of room. I did write something about it in my rough draft, but was obliged to cut down my material to the utmost in order to keep within any tolerable limits. Possibly I may have erred in judgment as to what should be omitted, but *something*, and a great deal, had to go overboard. As it was, the audience was nearly tired to death before I finished. C. A. YOUNG.

"*Astronomy and the Ice Age.*"—In the February number of this journal there is an article under the above head, criticising Dr. Croll's theory. The following is the principal statement, viz: "Now the difficulty is to show how this effect could be produced by an increased cold in winter *accompanied by a corresponding increase of heat during the ensuing summer.*" When this assertion is divested of a manifest error, there is no difficulty at all. An elementary acquaintance with the subject should make it plain to any one that when the northern hemisphere has its long cold winter in aphelion, the summer is correspondingly short—too short to melt the accumulated snow and ice of the preceding winter. The objector forgets that *time* is a factor in the melting. That the summer in the above case is short, is an integral part of the theory. It is a fact of possibly universal application, that ice under an open sky is formed more rapidly than it melts. Anyone who has lived in a cold climate knows that ice is frequently formed in three or four days—which a month's sunshine scarcely suffices to melt.

Many so-called objections are too trivial to notice; the principal ones were long ago substantially and, in most cases, triumphantly answered by Dr. Croll in his various articles published in the journals of Scotland and in the *American Journal of Science*—to which answers I would respectfully call attention. The principal points have brought together in his late work, "Climate and Cosmology," a work which it behooves most objectors to read. Even Wocikoff's objections were nearly all answered before they were published.

Miami University, Oxford, O. R. W. MC FARLAND.

Temporary Stars.—In the Monthly Notices of December last, Dr. Ralph Copeland, speaking of the Nova in Andromeda, says "the light curve falling by a succession of steps," is as the result of his own comparisons. In the SIDEREAL MESSENGER of the preceding March, I called attention to the "well marked waves, when the star grew brighter for a few days and then recommenced its diminution." In the *Astronomische Nachrichten* of January 27, (No. 2769,) the light curve of the Nova in Orion, of last year, is plotted from the observations of Paul Stroobant, at Brussels, showing similar waves, which are confirmed by the light curve of M. Muller in the *Astronomische Nachrichten* No. 2734.

In a paper by Prof. Wallace Goold Levison, read before the American Astronomical Society, the theory is suggested that long periods of uniform heat may be accounted for by chemical association. When substances in a condition of dissociation cool sufficiently to combine chemically, the temperature remains uniform until the combination is completed. This is illustrated by well known phenomena. If a body of steam is gradually cooled, its temperature diminishes until it reaches 212°. At that point, while the cooling is continued, the temperature remains uniform until it is all converted into water.

The temperature then begins again to fall, at the same rate as at first, and continues until it reaches 32°. Then there is another period of rest while the water is being converted into ice.

It is possible that in cooling so rapidly that the light is reduced one half in a few days, these periods of rest may be correspondingly short. It may be, therefore, that we see in these temporary stars, a corroboration of the theory of Prof. Levison. And so far as the phenomena correspond with his theory, they tend to show that the diminution of the light is caused by cooling, and not by occultation.

Unless such successive periods of rest have been observed in ordinary variable stars, this resemblance would point to the conclusion that the Nova in Orion was last year a new temporary star. It is remarkable that if a variable hitherto undiscovered, it has been so long overlooked; and it is also noteworthy that the new star does not appear upon Pickering's photograph of November 9, 1885; although that may perhaps be accounted for by its red color. These are at least indications that it may possibly be in fact a new variable, discovered at its first maximum.

HENRY M. PARKHURST.

Brooklyn, N. Y. Feb. 16, 1887.

Warner Observatory.—Volume I of the publications of Warner Observatory is received. It is a pamphlet of 70 pages, giving a brief history of the observatory, a description of its instruments and an account of the work done from 1883 to the close of 1886. The frontis-piece is a beautiful picture of the observatory, the observer's dwelling house attached and the spacious lawn surrounding. Opposite pages 8 and 10 are two full page engravings showing the working parts of the 16-inch Clark equatorial and its accessories; then follow four of the lists of new nebulæ, containing 100 each, discovered by Dr. Swift within the time before mentioned.

The greater part of the volume is taken up with the Warner prize essays. The first was by Professor Lewis Boss, of Albany, on *Comets*. It may be remembered by some of our readers that this paper was the leader in the first number of this journal in 1883. The remaining four essays are written on the theme, *The Red Sky-Glows* in 1883 and 1884. The writers were Professor K. I. Kiessling, Hamburg, Germany, James E. Clark, York, England, Henry C. Maine, Rochester, N. Y., and Rev. Sereno E. Bishop, Honolulu, Hawaiian Islands. The first and second prizes were awarded in the order of the foregoing names, the third being divided between the last two.

Another interesting feature in the management of this observatory is the offer of prizes for the discovery of comets, varying from $100 to $500, at different times. Mr. Warner, the founder of the observatory, began this in 1881. Since that time he has given twenty-one different prizes in this way, amounting to $3,750. Of this number W. R. Brooks, Phelps, N. Y., has received 8; E. E. Barnard of Nashville, 7; Dr. Swift, Rochester, 3; Schaeberle, Ann Arbor, Thome, Cordoba, South America, and Finlay, of Cape of Good Hope, each, 1.

The influence of this observatory and some of its lines of work have been unique, yet all has been very useful and helpfully stimulating to amateurs.

The total list of 540 new nebulæ discovered mainly by Dr. Swift in so short a time is a piece of work of very unusual magnitude of its kind.

Comets Chiefly Optical Phenomena.—The writer of the leading article of this number has evidently thought and read much concerning the physical constitution of comets, and has made an honest and earnest effort to harmonize a wide range of phenomena belonging to them, that have not been satisfactorily explained either by the physicist or the astronomer. The mode of doing it most of our readers will fully comprehend, as only principles of elementary physics are assumed as the basis of explanation. While the ideas advanced may assist in explaining some phenomena, it seems to us that too much is claimed for the theory in view of the known physical nature of merely tenuous bodies in gaseous form, to say nothing of the definite knowledge of comets obtained by the spectroscope.

The Sidereal Messenger,

CONDUCTED BY WM. W. PAYNE,

Director of Carleton College Observatory, Northfield, Minnesota

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THE COMETS OF DE VICO, 1844 I, AND FINLAY, 1886 V.

H. C. WILSON, WASHINGTON, D. C.

For the Messenger.

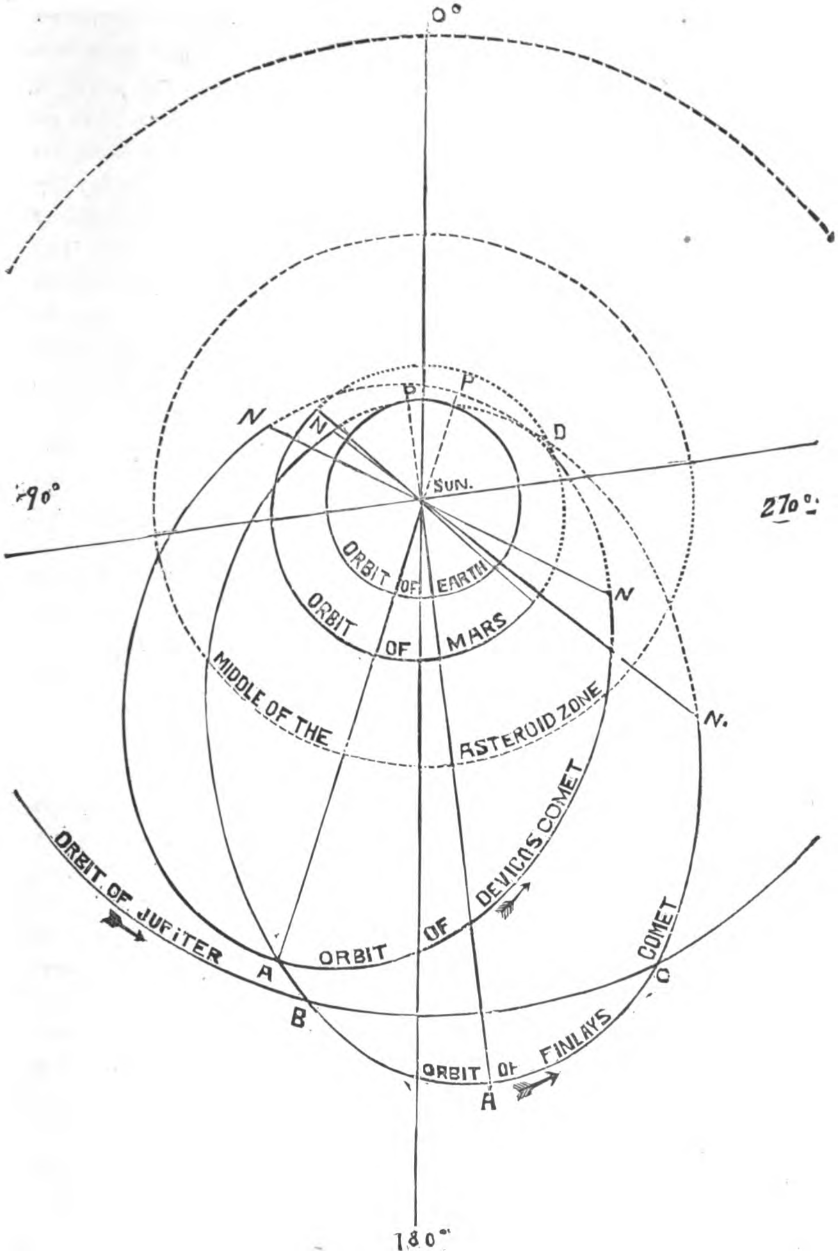
The little faint telescopic comet discovered by De Vico, at Rome, August 22, 1844, would have excited little interest, but for the fact, first made known by Faye, that it was moving in an elliptic orbit of short period. The seven sets of elements of this comet, derived by different computers, agree very closely; the last and most accurate set, computed by Brünnow, was based upon all of the observations made during that apparition, and was corrected for the perturbations of all the planets. The resulting period was about 5.47 years. The comet ought therefore to have been observed, if not at any of the previous apparitions, at least at each second return since 1844. Such, however, has not been the case.

With regard to previous apparitions, Le Verrier in a memoir communicated to the *Astronomische Nachrichten*, Vol. 25, page 375, has discussed the possible identity of De Vico's comet with those of 1585, 1678 and 1770, whose elements bear some points of resemblance. The conclusion reached was strongly negative with regard to the comets of 1585 and 1770, but was quite favorable to the identity of the comets of 1678 and 1844. In this memoir Le Verrier pointed out the folly of inferring identity from mere similarity of the elements of two comets. He showed that in the case of this particular comet, the perturbations by the planet Jupiter under certain conditions might have produced a change of as much as 100° in the longitude of the nodes and 30° in the longitude of perihelion, between the years 1770 and 1844. He showed also that this change must take place in certain directions, namely, that the longitude of perihelion must advance in the direction of Jupiter's motion, while the line of nodes must retrograde. A reference to the diagram may help to make this plain to the reader. Suppose the comet of De Vico and the planet Jupiter to pass the vicinity of the point *A* at the same time; the planet moves faster than the comet and its influence will draw the comet outward and around in the direction *AC*. The aphelion distance will thus be increased and the whole orbit will be swung around in the direction *AC*. Again, owing to the greater inclination of the comet's orbit, the point *A* is elevated above the plane of the planet's orbit, and the tendency will be to pull the comet down, causing its path to intersect the plane of the ecliptic before it reaches *N*. The line of nodes will therefore be revolved in a retrograde direction. A difference of 5° in the wrong direction in either of these elements necessitates, as a condition of identity, that the perturbations have produced a change of 355° instead of 5°; so that the elements of two comets may be apparently quite similar while they are really very different.

The closing words of Le Verrier's memoir will be of interest here:

"The comet of 1844 has probably come like the others from the far distant regions of space, and has been fixed among the planets by the powerful influence of the attraction of Jupiter. Its coming dates back, without any doubt, several centuries. Since that epoch it has passed quite frequently in the vicinity of the earth; but has been observed only once in the past centuries, 166 years before the apparition of 1844.

"This comet will for a very long time yet traverse the restricted orbit which we see it describe to-day. In a certain number of centuries, it will again reach to the orbit of Jupiter, in a direction opposite to that by which it came into the planetary system; and its course will certainly sometime be again changed. Perhaps even Jupiter will restore it to the space whence he has stolen it."


0°
90°
270°
180°
SUN.
B
P
D
N
N
N
N.
ORBIT OF EARTH
ORBIT OF MARS
MIDDLE OF THE ASTEROID ZONE
ORBIT OF DEVICOS COMET
ORBIT OF JUPITER
A
B
C
ORBIT OF FINLAYS COMET
A

Since the apparition of 1844 this comet has not been seen, if we except a single doubtful observation by Goldschmidt in 1855, although the conditions for observation, according to Brünnow's elements, have been quite favorable at several returns. The comet has become known as the Lost Comet of De Vico. It was natural, therefore, that when the computers found that the preliminary elements of Finlay's comet differed so little from those of the comet of 1844, they should conjecture that the two comets were the same. The later and more accurate computations do not tend to confirm this conjecture, but, as Professor Boss puts it, "render the question of identity with De Vico's comet rather problematical, to say the least."

Below I give, side by side, the elements of De Vico's comet by Brünnow, and the latest set of elements of the Finlay comet by Boss, from *Astronomical Journal* No. 150. The latter elements are based upon 35 observations, extending over the interval of time from September 29 to December 29, 1886. Both sets of elements are referred to the equinox of 1886.0.

	De Vico's Comet.	Finlay's Comet.
Time of perihelion passage	Sept. 2, 1844	Nov. 22, 1886
Longitude of perihelion	343° 06′ 20″	7° 33′ 02″
Longitude of node	64 24 30	52 26 14
Inclination	2 54 50	3 01 46
Perihelion distance	1.18640	0.99777
Eccentricity	0.61765	0.71855
Semi-major axis	3.10294	3.54519
Period	5.47 years.	6.675 years.

The most striking point of resemblance is in the inclination of the orbits to the plane of the ecliptic; this being almost exactly the same for both comets and but little greater than the inclination of the orbits of Mars and Jupiter.

The graphical method is the most satisfactory for comparing the other elements. The accompanying diagram shows the projection upon the plane of the ecliptic, of the orbits of the two comets and of the planets Earth, Mars and Jupiter. In the case of each of these bodies the inclination is so small that, on the scale used, the dimensions of the real orbits do not differ appreciably from their projections. Those parts of the curves which lie below the plane of the ecliptic are represented by dotted lines.

A glance at the diagram shows that, to satisfy the hypothesis of identity, the comet must have been subject to violent perturbations at some time between 1844 and 1886. The differences in direction of the major axes, *AP* and *A'P'*, and the lines of nodes, *NN* and *N'N'*, accord with Le Verrier's criterion in regard to the perturbations by Jupiter, namely, that the perihelion must advance while the nodes must retrograde. The intersection of the two comet orbits at *A*, the point where De Vico's comet approaches nearest to Jupiter's path, would also lead us to suspect that Jupiter might have had something to do with the disturbance; but we encounter serious difficulty in bringing the three bodies together in that vicinity. The heliocentric longitude of the point *A* is approximately 168°, and if we work forward from 1844.67 with the period 5.47 years, and backward from 1886.90 with the period 6.68 years, we find the following dates when the three bodies were in that longitude:

De Vico's Comet.	Finlay's Comet.	Jupiter.
1847.40	......	......
52.87	1848.72	1850.26
58.34	55.40	......
63.81	62.08	62.12
69.28	68.76	......
74.75	75.44	73.98
80.22	1882.12	......
1885.69	......	1885.84

It appears that the Finlay comet and Jupiter passed the point *A* together about 1862.1, but that the De Vico comet was then 1.8 years behind. In order to bring the three together at that epoch, the period of De Vico's comet must be either diminished to 5.0 years or increased to 6.9 years, both of which suppositions are improbable.

There is an interesting conjunction of intersections of the paths of the comets and Mars at *D*, in longitude 285°—300°. The distance between the planes of the orbits is there very small. Examining, in the same manner as before, the dates at which the comets and the planet were in longitude 288°, we find the following:

De Vico's Comet.	Finlay's Comet.	Mars.
1849.96	1853.26	1852.96
55.43	......	54.84
60.90	59.94	60.49
66.37	66.62	66.14
71.84	......	71.79
......	73.30	73.67
77.31	......	77.44
......	79.98	79.32
1882.78	......	83.08
......	1886.66	1886.84

The nearest approach to coincidence of dates here is in 1866, but the divergence is still so great that it can hardly be accounted for by errors in the assumed periodic times of the comets.

From this superficial examination it would seem that the probability of identity of these two comets is extremely small. Before undertaking a rigorous investigation of the question, it will be necessary to know the mean motion of Finlay's comet with exactness. This we cannot hope to know with certainty until a second apparition shall have been observed. The next perihelion passage will occur in the summer of 1893, and, unless the period 6.68 years is considerably in error, will be only a little less favorable for observation than the present one. As no remarkable perturbations are likely to take place meanwhile, it is quite probable that the comet may then be re-observed. Until then it will be hardly worth while to attempt to compute the past perturbations of Finlay's comet, in the vain hope of identifying it with that of De Vico. It will be advisable, however, to obtain the best possible elements from the observations at this apparition and to compute the perturbations by all of the planets during the next seven years, in order to predict the course of the comet as closely as possible in 1893.

Washington, February 12, 1887.

THE METEORITES, THE METEORS AND THE SHOOTING STARS.

PROFESSOR H. A. NEWTON.

(Continued from page 105.)

What that structure is, and to some extent what conditions must have existed at the time and place of its first formation and during its subsequent transformations, mineralogists rather than astronomers must tell us. For a long time it was accepted without hesitation that these bodies required great heat for their first consolidation. Their resemblance to the earth's volcanic rocks was insisted on by mineralogists. Professor J. Lawrence Smith in 1855 asserted without reserve that "they have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes." Director Haidinger, in 1861, said "With our present knowledge of natural laws the characteristically crystalline formations could not possible have come into existence except under the action of high temperature combined with powerful pressure." The likeness of these stones to the deeper igneous rocks of the earth as shown by the experiments of M. Daubrée strengthened this conviction.

Mr. Sorby in 1877 said, "It appears to me that the conditions under which meteorites were formed must have been such that the temperature was high enough to fuse stony masses into glass; the particles could exist independently one of the other in an incandescent atmosphere, subject to violent mechanical disturbances; that the force of gravitation was great enough to collect these fine particles together into solid masses, and that these were in such a situation that they could be metamorphosed, further broken up into fragments, and again collected together."

Now if meteorites could come into being only in a heated place, then the body in which they were formed ought, it would seem, to have been a large one. But the comets, on the contrary, appear to have become aggregated in small masses.

The idea that heat was essential to the production of these minerals was at first a natural one. All other known rock formations, are the result of processes that involved water or fire or metamorphism. All agree that the meteorites could not have been formed in the presence of water or free oxygen. What conclusion was more reasonable than that heat was present in the form of volcanic or of metamorphic action?

The more recent investigations of the meteorites and kindred stones, especially the discussions of the Greenland native irons and the rocks in which they are imbedded, are leading mineralogists, if I do not mistake, to modify their views. Great heat at the first consolidation of the meteoric matter is not considered so essential. In a late paper M. Daubrée says "It is extremely remarkable that in spite of their great tendency to a sharply defined (*nette*) crystallization, the silicate combinations which make up the meteorites are there only in the condition of very small crystals all jumbled together as if they had not passed through fusion. If we may look for something analogous about us, we should say that instead of calling to mind the long needles of ice which liquid water forms as it freezes, the fine grained texture of meteorites resembles rather that of hoar frost and that of snow, which is due, as is known to the immediate passage of the atmospheric vapor of water into the solid state."

So Dr. Reusch from the examination of the Scandinavian meteorites concludes that "there is no need to assume volcanic and other processes taking place upon a large heavenly body formerly existing but which has since gone to pieces."

The meteorites resemble the lavas and slags on the earth. These lavas and slags are formed in the absence of water, and with a limited supply of oxygen, and heat is present in the process. But is heat necessary for the making of the meteorites? Some crystallizations do take place in the cold; some are direct changes from gaseous to solid forms. We cannot in the labratory reproduce all the conditions of crystallization in the cold of space. We cannot easily determine whether the mere absence of oxygen will not account fully for the slag-like character of the meteorite minerals.

Wherever crystallization can take place at all, if there are present silicon and magnesium and iron and nickel with a limited supply of oxygen, there silicates ought to be expected in abundance, and the iron and nickel in their metallic form. Except for the heat the process should be analogous to that of the reduction of iron in the Bessemer cupola where the limited supply of oxygen combines with the carbon and leaves the iron free. The smallness of the comets should not then be an objection to considering the meteoric stones and irons as pieces of comets. There is no necessity of assuming that they were parts of a large mass in order to provide an intensely heated birth-place.

But although great heat was not needed at the first formation there are many facts about these stones which imply that violent forces have in some way acted during the meteorites' history. The brecciated appearance of many specimens, the fact that the fragments in a breccia are themselves a finer breccia, the fractures, the infiltrations and apparent faultings seen in microscopic sections, and by the naked eye,—these all imply the action of force.

M. Daubrée supposes that the union of oxygen and silicon furnishes sufficient heat for making these minerals. If this be possible those transformations may have taken place in their first home. Dr. Reusch argues that the repeated heating and cooling of the comet as it comes down to the sun and goes back again into the cold is enough to account for all the peculiarities of structure of the meteorites. These two modes of action do not, however, exclude each other.

Suppose then, a mass containing silicon, magnesium, iron, nickel, a limited supply of oxygen and small quantities of other elements, all in their primordial or nebulous state (whatever that may be) segregated somewhere in the cold of space. As the materials consolidate or crystallize, the oxygen is appropriated by the silicon and magnesium, and the iron and nickel are deposited in metallic form. Possibly the heat developed may, before it is radiated into space, modify and transform the substance. The final result is a rocky mass (or possibly several adjacent masses) which sooner or later is no doubt cooled down throughout to the temperature of space.

This mass in its travels comes near to the sun. Powerful action is there exerted upon it. It is heated. How intense is that heat upon a cold rock unprotected apparently by its thin atmosphere, it is not possible to say. We know that the sun's action is strong enough to develop and drive off into space, that immense train, the comet's tail, that sometimes spans our heavens. It is broken in pieces. We have seen the portions go away from the sun, to come back probably as separate comets. Solid fragments are scattered from it to travel in their own independent orbits.

What is the condition of the burnt and crackled surface of a cometic mass or fragment as it goes out from near the sun again into the cold? What changes and re-crystallizations may not that surface undergo before it comes back to pass anew through the fiery ordeal? We have here forces that we know are acting. They are intense, and act under varied conditions. The stones subject to those forces can have a history full of all the scenes and actions required for the growth of such strange bodies as have come down to us. Some of our meteors, those of the star-showers, have certainly had that history. What good reason is there for saying that all of them may not have had the like birthplace and life?

Before I close let me recite one lesson that has been taught us by the recent star-showers. The pieces which come into our air, in any recurring star-shower, belong to a group whose shape is only partly known. It is thin, for we traverse it in a short time. It is not a uniform ring, for it is not annual, except possibly the August sprinkle. How the sun's unequal attraction for the parts of a group acts as a dispersive force to draw it out into a stream, those most beautiful and most fruitful discussions of Signor Schiaparelli have shown. The groups that we meet are certainly in the shape of thin streams.

It has been assumed that the cometic fragments go continuously away from the parent mass so as to form in due time a ringlike stream of varying density, but stretched along the entire elliptic orbit of the comet. The epochs of the Leonid star-showers in November, which have been coming at intervals of thirty-three years since the year 902, have led us to believe that this departure of the fragments from Tempel's comet (1866, I) and the formation of the ring was a very slow process. The meteors which we met near 1866, were, therefore thought to have left the comet many thousand years ago. The extension of the group was presumed to go on in the future, until, perhaps tens of thousands of years hence, the earth shall meet the stream every year.

Whatever may be the case with Tempel's comet and its meteors, this slow development is not found to be true, for the fragments of Biela's comet. It is quite certain that the meteors of the splendid displays of 1872 and 1885 left the immediate vicinity of that comet later than 1840, although at the time of those showers they had become separated two hundred millions of miles from the computed place of the comet. The process then has been an exceedingly rapid one, requiring, if continued at the same rate, only a small part of a millennium for the completion of an entire ring, if a ring is to be the finished form of the group.

It may be thought reasonable in view of this fact about Biela's comet established by the star-showers of 1872 and 1885 to revise our conception of the process of disintegration of Tempel's comet also. The more brilliant of the star-showers from this comet have always occurred very near the end of the thirty-three year period. Instead of there being a slow process which its ultimately to produce a ring along the orbit of the comet, it certainly seems more reasonable to suppose that the compact lines of meteors which we met in 1866, 1867 and 1868 left the comet at a recent date. A thousand years ago this shower occurred in the middle of October. By the precession of the equinoxes and the action of the planets, the shower has moved to the middle of November. One-half of this motion is due to to the precession of the equinoxes, the other half to the perturbing action of the planets. Did the planets act upon the comet before the meteorids left it, or upon the meteoroid stream? Until one has reduced the forces to numerical values, he may not give to this question a positive answer. But I strongly suspect that computations of the forces will show that the perturbations of Jupiter and Saturn upon that group of meteoroids hundreds of millions of miles in length, perturbations strong enough to change the node of the orbit fifteen degrees along the ecliptic, would not leave the group such a compact train as we found it in 1866. If this result is at all possible, it is because the total action is scattered over so many centuries. But it seems more probable that the perturbation was of the comet itself, that the fragments are parting more rapidly from the comet than we have assumed, and that long before the complete ring is formed the groups become so scattered that we do not recognize them, or else are turned away so as not to cross the earth's orbit.

Comets by their strange behavior and wondrous trains have given to timid and superstitious men more apprehensions than have any other heavenly bodies. They have been the occasion of an immense amount of vague and wild and worthless speculation by men who knew a very little science. They have furnished a hundred as yet unanswered problems which have puzzled the wisest. A world without water, with a strange and variable envelope which takes the place of an atmosphere, a world that travels repeatedly out into the cold and back to the sun and slowly goes to pieces in the repeated process, has conditions so strange to our experience and so impossible to reproduce by experiment that our physics cannot as yet explain it. Yet we may confidently look forward to the answer of many of these problems in the future. Of those strange bodies, the comets, we shall have far greater means of study than of any other bodies in the heavens. The comets alone give us specimens to handle and analyze. Comets may be studied, like the planets, by the use of the telescope, the polariscope and the spectroscope. The utmost refinements of physical astronomy may be applied to both. But the cometary worlds will be also compelled, through these meteorite fragments with their included gases and peculiar minerals, to give up some additional secrets of their own life and of the physics of space to the blowpipe, the microscope, the test-tube and the crucible.

KEPLER'S CORRESPONDENCE IN 1599.*

(Continued from page 112.)

4. In the year 1595, April 23, I myself saw, as you also did, that the eclipse of the moon occurred later than it was placed by the Prussian calculation. This again confirms my view. For in the month of April this seems to me to happen of necessity.

5. You see the eclipse of the sun for the year 1598 is placed by Tycho himself 10¾ digits north and later (for the one follows the other, as I have shown in my appendix) by myself also it is received in this very way, and is given a fundamental position.

6. In the year 1590 the eclipse of the sun was seen at Tübingen by Mästlin in my presence, to appear in like manner earlier and smaller. This is just in accordance with my principles, for the sun was beyond apogee in Leo.

Finally you recount in order the eclipses before CHRIST, which are obtained by each calculation with a difference of almost three hours. This also confirms my view. For since my calculation is accommodated to daily observations, the observations approximate the calculation of Alphonso, therefore I also approximate the calculation of Alphonso, and the more constantly I do this, with greater accuracy. But even before the time of CHRIST I do this, which I thus prove. My correction and difference of the lunations introduced depend upon the eccentricity of the sun. That was once greater, wherefore my difference also was necessarily greater: so also the Prussian tables evidently differ more at that time from the tables of Alphonso. I do not say this with accurate knowledge but conjecturing. For Reinhold in his Prussian tables shows how much must be attributed to the tables of Alphonso in making investigation of antiquity, who commends Coperni-

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* Translated from the Latin by Professor Louisa H. Richardson, Carleton College.

cus for this especially, because he teaches how to compute ancient eclipses, when no one before could do it. But yet the tables of Alphonso seem to have been especially accurate in the case of the moon. And they, perchance, looked chiefly to the eclipses of spring, as Copernicus to those of summer and winter. Hence the former err in the eclipses of summer and winter, Copernicus in those of spring and autumn. Copernicus was devoted to matters of greater importance, determining the eccentricity of the sun, measuring the length of the year. To obtain these, it was impossible to teach otherwise concerning the eclipses, unless he should introduce at the same time a new and yearly irregularity in the moon ; and I do this, although he was not inclined to. Perhaps it may also be important to state, if the Ptolemaic eccentricity of the sun be retained, the eclipses of Copernicus would agree with the observations to-day. For I attribute this yearly irregularity to the moon, that is the same, as if the eccentricity of the sun increased, while the moon continued in uniform motion.

Beside testing my opinion, you also set forth that of another, concerning the introduction of a new motion of the earth. And I indeed have already said how it could be by a simple increase of the eccentricity which is not a new motion.

And Tycho also makes another suggestion in his letters written to me: for he says, the orbit of the sun (according to Copernicus the earth's orbit) is sometimes increased, sometimes diminished: that this may be physically possible, he eliminates the real or solid orbits from the heavens, and I do not object to it, though for other reasons. This then we shall see in his works if God permit us. On the celestial globe, which you perchance have, he says that he observes a certain and very slow declination of the Zodiac, so that the same constellations do not remain fixed in the Zodiac. This in like manner Copernicus refers to the earth. And thus let a suggestion be enough for us regarding a new motion of the earth. But in my appendix you have a two-fold reason why the moon rather should be the cause of these errors in eclipses. For, in the first place, the same irregularity of the moon is seen also when it is compared with the fixed stars or planets, not so much when it is compared with the sun and its shadow. Then, the whole theory concerning the equinoxes and the length of the year would be thrown into great confusion, if the eccentricity were changed. I am unequal to settling this confusion. Tycho has the power, let him then see to it. For he maintains that this, which Copernicus gives, is not the reason for the equinoxes. To depart now from the eclipses, since this subject is ended, it would please me to discover something concerning the declination of the ecliptic, but philosophically from my cosmography which I am thinking upon. Certainly, the declination is not to-day at its minimum, but will decrease through many centuries (if time continue) even to a mean of 22° 30′, and perhaps beyond. For I am convinced that the mean was 22° 30′ in the beginning of the world; then it increased through four thousand years to 24°. So that in the first or second century before Christ the mean was found by the masters to be 23° 52′, which Copernicus makes the maximum. Further, in a single revolution the motion is quite frequently unequal. Then by Ptolemy again it was observed 23° 52′. (Notice that I do not here reply to the Obelisk of Pliny.) Then it decreased more quickly, to-day it decreases slowly, and is 23° 28′, but this decrease will continue (to increase again) even to the 2400th year from this returning to the primeval mean, 22° 30.′ Copernicus used to say, the earth was moved from the center, the greater the eccentric was made, the more also it was inclined to the Zodiac at the poles. But from what dream, you say, do you derive this 22° 30′? From the cosmography, I answer. Consider what it would have been, if the equator had not deviated from the ecliptic; what also, if it had deviated a whole quadrant, for these are extremes. Afterwards consider the varying phenomena of the declination of orbits 45°, the mean between the extremes, then of the declination 67° 30′, the mean between 45° and 90°, also of the declination 22° 30′, the mean between 0° and 45°. Examine these yourself, for the matter is easy to think upon, troublesome and long to write. You will find that the equality of any declination corresponds to the laws of the universe and the too great inequality is neither 90° nor 67° 30′ nor 45°. There remains then only the 22° 30′, and since this number approximates the number 23° 28′ of to-day's declination and the declination is still decreasing, for this reason have I fallen upon this suspicion of mine. For in the whole bodily structure, God has given laws of the body, number and proportions, the best chosen and well regulated laws. Wherefore the parts of the orbits are rational, as is 22° 30′, which is the sixth part of any meridian, while 23° 28′, 23° 52′ or the mean 23° 40′ are not rational parts. This also favors my suggestion, that all may be varied by motion, and return to themselves that there may be as great variety as possible. Wherefore I agree with Tycho also concerning the obliquity of the Zodiac. In this way the latitude of the zones will be made equal in creation, on a meridian which is divided into eight parts of which each holds one from the frigid, two from the temperate and torrid, in opposite places. Hence God accomplished this, that the mean should lie between the extremes, the temperate between the frigid and torrid, and this law has been of great value in the arrangement of the world. For this law is also among the planets, if not all, certainly the greater ones, Saturn, Jupiter, Mars, Venus, Mercury. These five: for we may except the earth, since even God has called it an exception adorning it alone with the orb of the moon. Accordingly you see Saturn pale, Jupiter yellow, Mars red, Venus golden, Mercury silvery. Saturn and Mercury, the extremes, correspond in color, so Jupiter and Venus, lying between, while Mars has no corresponding planet. Thus God seems to have given the planets to the zones, or the zones to the planets. For Saturn is frigid, Jupiter, temperate, Mars, torrid; the color is an indication, and the astrologers bear witness; unless the same nature and condition does not extend far within to Saturn and Mercury, Jupiter and Venus, as to the frigid and temperate zones each. For Venus and Mercury are nearer the sun, separated from the earth by the higher ones. Wherefore they are more brilliant also than Jupiter and Saturn.

Let us cease therefore to investigate celestial and incorporeal things, more than God has revealed to us. These things are within the grasp of human understanding. God wishes us to know these, since he has fashioned us in his own image, that we may come into participation of the same reasoning with himself. For what is in the mind of man beyond numbers and quantities? These things alone do we rightly understand and if it can be said with reverence, with the same kind of thought as God, as much indeed as we do comprehend of these things in our mortality. Foolishly do they fear that we shall make man a God; the counsels of God are inscrutable, not his corporeal works. For what are the works of God, if they are compared with his counsels? The counsels of God are God himself, but the works are his creatures, and it is not a great thing for God to create man capable of understanding his works. But let us return to your letter. I think the opinion of Pliny and of the ancients concerning the equinoxes—when on 8 of Aries—together with Scaliger arose from this, because the solstices continued apparently sixteen days, (athough from this I do not affirm anything disparaging the Obelisk of Pliny, that I may not make him too unskilled in astronomy. For it is one thing to remain fixed and another thing to revolve; the former is rest, the latter motion. Wherefore as long as the day did not increase perceptibly, the sun was thought to stand still, and the solstice was said to continue. But as soon as the increase of the day was observed, then the sun was said to turn. Hence, revolution on the eighth day. Afterwards even the days of the equinoxes were computed from the revolutions.

The pearls of philosophy, gathered from Arabic nonsense, belong to that matter which Aristotle treats of in his books on generation and destruction and on meteors. Some I have shown in the preface of this year's prognosticon, which you have. They are simply wonderful, and because they rightly try one's patience, the zeal of the Academic philosophers grows cold in things so noble. I think that on account of the countless trifles, which are rightly to be despised by the wise, it happens that the pearl also is scorned. Just as a cock will have little faith in me advising him to hide something good, for he will see the dung-hill and will dig. I will set forth some points through questions.

1. How are all moistures connected with the light of the moon? 2. Why are the ocean tides caused by the motion of the moon? 3. How can position effect anything, and, indeed, not all position, but only the *rational.* For every appearance is a rational position, a harmonious part of the four right angles, caused by the rays of the stars coming together on the earth. And since there are eight harmonies, unison, minor third, major third, fourth, fifth, minor sixth, major sixth, octave, there will also be eight radiations, as is in my little book, cosmography: conjunction, sextilis, quintilis, quadratus, trigon, sesquiquadratus, biquintiiis, opposition. (Symbols omitted.) And why should they not have an influence, since the same plan embraces these as the ancient and accustomed. This whole question is worthy the genius of natural philosophers. Behold to-day, when two planets are distant 89°, nothing new happens in the meteors. To-morrow, when they are full 90° distant, that is, a quadrant, suddenly a storm arises. How little addition is made to the light of each within one day, and how on the day after to-morrow can that be again quickly diminished? Therefore it is not the effect of a star, but of stars; not of light but of the number 90°, that is the angle counted by the number 90°, a rational and harmonious part of the whole circle. The earth itself, then, by its own position aids this effect, since if it were situated any where else, it would be in another angle. But what can position accomplish? What can reason do, unless that which acts, comprehends reason? Or shall we give life to the light? I preferred to give life to the earth, which is suited for understanding these appearances, as you will comprehend more clearly n my preface.

(To be continued.)

THE SUN-SPOTS AS VORTEX RINGS.

FRANK H. BIGELOW.*

For the Messenger.

In order to emphasize the need of a new theory on the origin of sun-spots, it will be advisable to enumerate the most important suggestions heretofore made. The most superficial observations indicated the following general appearances to be explained, namely, a circular spot having a dark center, a radiated hotter ring of remarkable uniform structure in width, brighter than the center and duskier than the average face of the sun, the whole region being hollow and depressed, and the scene of great activity as indicated by flames of different kinds. The early observers proposed "planets circulating very near the surface;" Galileo, "clouds floating in the solar atmosphere;" Derham and Capocci, "solar volcanoes;" Lalande, "solar mountain tops, like islands in an ocean of fire;" Herschel, "holes temporarily opening in the two enveloping clouds, the photosphere and the penumbral cloud, showing a dark, cool, solid sun;" Sir John Herschel, "great whirling storms boring down through the photosphere;" Secchi, "openings in the photosphere caused by gases bursting out in eruptions;" Zöllner, "the solar surface a liquid in which float slag-like cooler masses;" Secchi (modified), "dense clouds of eruption products settling down near but not at the place of eruption, as the resultant of component velocities;" Faye, "whirlpools caused by polar-equatorial currents, forming tangential vortices;" Young, "sinking spaces in the photosphere, due to diminution of upward pressure from below;" Norton, "diminished gaseous pressure and electric tension;" Siemens, "superficial convection currents from stellar regions circulating over poles toward the equator of the sun." A sufficient summary of objections can be found in Professor Young's book on the Sun, pp. 166–178. The statements which accompany these theories in many cases contain exact descriptions of conditions

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which must exist on the Sun, and the utmost value will always be attached to them.

That the comparison of vortex rings and sun-spot phenomena may be more readily apprehended, it will be expedient to recapitulate the mathematical relations known to exist, referring to the works of the illustrious authors (Helmholtz, Sir William Thomson, Tait, Craig, Lamb,) for details of the subject. Pure mathematics deals with the ideal, frictionless vortex ring as applied so beautifully in Thomson's Theory of Atoms, and hence it is to be expected that a modified though natural type shall be seen in the sun, where the elements of pressure and friction must be introduced. A perfect fluid is homogeneous, incompressible and devoid of all viscosity. In such a fluid, motion about an instantaneous axis is called irrotational, as distinguished from rotation about a fixed axis. When such a motion is impressed upon any portion of a perfect fluid, the mass in motion will remain the same; the volume is invariable, though the form may change; the density is uniform in all parts; the volume of the fluid contained in any closed surface is proportional to the volume contained within that surface; the stress between contiguous portions is always normal. The Theorem of Helmholtz is, that the points lying on a vortex line continue to lie on the same line during the whole motion; or the same row of particles forming a vortex line swims along with the fluid, in such direction as at every point to coincide with the axis of rotation. A vortex tube is formed of an integrated series of vortex filaments, which in turn are vortex lines integrated from consecutive points. The quantity which flows through any section of the same tube in the unit of time must be invariable, and at any section the product of the area of the section into the mean velocity of rotation is constant. The velocity of circulation at any section varies inversely as the area of that section, and that of any segment directly as the length of the segment. From these laws are deduced the properties that vortex rings are either closed and return into themselves, or have two sections in one free surface; are indestructible and indivisible;

tend to separate themselves from one another and from surrounding portions of the same fluid.

Such vortex motion is an ideal mathematical conception and cannot be reproduced by experiment, but the motion as modified in a viscous fluid can be reproduced to illustrate these laws, as has been shown in Tait's Lectures on Physics. Popular examples are seen in smoke rings of a locomotive. The types of modification may serve as a topic of research in physical laboratories promising a rich reward for labor and expense of apparatus. When such a ring is thrown into a medium of condensation and pressure, the natural expansion produced by the absorption of heat caused by the friction upon the revolving surfaces, instead of going on as a simple enlargement of the ring to the state of dissolution, will cause an intermediate process of folding the surface so as to follow the lines of the rotating sections, till the uniform ring disintegrates into a series of secondary rings at right angles with the core of the original ring. This generation of heat is accompanied by a retardation of the velocity of rotation, so that eventually the secondary rings, losing their momentum, will have only such a low velocity as fails to preserve the typical form, and they therefore float away like heterogeneous masses without character into the surrounding fluid wherein they were formed.

So far as we know, the origin of rings possible to be formed by experiments is accompanied by a blast of fluid having considerable velocity through a channel of relatively hard walls, upon whose sides the friction retards the outer layers of the column, while the inner being unimpeded give rise to a ring, all of whose particles rotate radially from center forward in the direction of translation, returning on the outside and thus preserving an integrity. When the ring is thus ejected it travels on indefinitely till the friction disintegrates it. Should it enter a denser medium the same destruction could only be hastened but not changed in type. The larger the volume of the original ring, the greater the power it has in any given medium to resist the influences of friction, since the volume increases more rapidly than the containing surface.

Let us apply this theory to the phenomena on the sun. On pages 175 and 286, Professor Young, quoting Maxwell and others, states what is doubtless the truth, that "at a high temperature the viscosity of gases is vastly increased, so that quite probably the matter of the solar nucleus resembles pitch or tar or putty in its consistency." The low density of the sun indicates either a large sphere, gaseous throughout, or a solidifying nucleus at the center, surrounded with a space occupied by convection currents only, and this surmounted by a shell of condensation at the extremity of the cooling radius, which forms the visible surface of the sun. The viscous surface of the nucleus is the agent of contraction upon the enclosed gases in relative stages of condensation, producing an accumulation of pressure by its volume diminishing in consequence of the continuous radiation of heat. The relief from this pressure is the frequent discharge of the confined gases, through the sticky, semi-solid outer wall, where they are puffed out under all the circumstances tending to produce a vortex ring. The liberated ring springs upward in a parabolic path under the components of velocity caused by the force of ejection, and the rotation of the nucleus on its axis, being practically unimpeded except by a retardation of its acceleration, till it strikes the envelope called the photosphere. The resolved tangential velocity at this point will give as the area of impact, that part of the ring which is towards the east, as seen from the center of the sun, while at the same time the normal component continues to act upwards. Under the influence of these two forces the ring begins to bore its way upward and eastward until it finally emerges as a perfect spot, or in case the normal component fails in this work, as a veiled spot. The hollow ring has the capacity of sending ahead of it a short, gusty current produced by the friction of its inside surface upon the medium in which it travels, and this will cause an antecedent rending of the superincumbent cloud in the irregular forms so well known, together with the elevated or bubble shape of the photosphere which heralds a spot at any locality.

It is clear that the retardation of the ring tends to enlarge the sectional area of circulation, in addition to the expansion of the gases freed from the pressure at the nucleus, so that by the time it appears at the surface of the photosphere it has become enormously larger than when it set out on its upward journey. It may be suggested also that this enlargement is so governed as to adapt itself in a degree to the general thickness of the photosphere at the place of apparition, as a consequence of which the thickness of the ring and of the photosphere is nearly the same ; or at least they may be supposed to differ by some such constant quantity as is detected in the usual depression of the spot below the surface, a result which would naturally be caused by the friction sucking down the medium in its immediate neighborhood.

If this view is correct there is needed only the simple confirmation of inspection to prove it. The umbra is the hollow of the vortex ring and is therefore a region of pure absorption such as the spectroscope demands, of low heating capacity crossed by fine spectrum lines as would happen in viewing the gaseous convection currents of the general regions between the photosphere and the nucleus. Even Dawes' dark specks in the nucleus are accounted for by a theory to be mentioned later. The penumbra is the vortex ring itself, at its early development quite free from filaments. These soon appear when the sectional rotation loses velocity, or as the surrounding pressure allows some circulating lines to grow, while others do not, and thus separates the smooth ring into creases or folds which tend more and more to rotate as individual sections or secondary rings. These grow in distinctness and are perceived as filaments with club shaped extremities resembling the perspective effect of a circle viewed nearly across its circumference, the rotation being observed in the spectroscope as upward from the interior and outward radially from the center till the motion disappears under the adjacent compressing photosphere. A fact in detail of almost unquestionable power in demonstration of this theory, is that towards the ripe stage or end of life of the ring a section is seen to emerge into the hollow of the ring and also to retreat into the photosphere just opposite to it. This is undoubtedly the result of an effort to preserve the law of vortex rings that the product of sectional area into the mean velocity of rotation tends to invariability, and hence that as the velocity diminishes the radius of rotation increases. In fact the whole ring enlarges under this law, but a section shows it clearly because the retreat in both directions is a sign of the motion continuing circular. The life of the ring endures while its force of velocity is being expended, but upon a sufficient retardation and enlargement, breaks up into meaningless wisps, while the photosphere hitherto thrust aside, encroaches in the reverse order of the manner of appearing. Several rings may be ejected successively from the same region of the nucleus and these following each other in close order would produce a combined disturbance in the photosphere, to be afterward regulated by the quick repulsive action between two or three adjoining rings so frequently seen to take place in a fresh group. The crevasses are irregular types where the rings become distorted, the umbra annihilated, while the penumbral appearance seen in contiguous positions indicates a rotation of true filaments which may endure a long while. The region of a group of spots becomes the center of principal activity whereby the internal tension would exhibit itself as flames bursting through the lines of fracture between the ring and the photosphere. In a study of the spots a large margin must be allowed for irregular appearances when dealing with such unstable elements, in the midst of the action of tremendous forces of all kinds, and overlying prominences and faculæ must be carefully distinguished from the true ring. The perplexity which has always attended the preservation of whatever uniformity of structure the spots possess seems to be fully met by regarding them as vortex rings.

This view of the origin of sun-spots would appear to exclude every theory of the interior constitution of the sun other than the one indicated. The question of the distribution of heat is at once raised and the vortex ring fortunately becomes a rough means of analyzing it. The heat of the surface is the maximum, that of the ring less, and the hollow within the ring relatively a minimum. But the ring is a courier direct from the interior of the nucleus, the umbra is a direct vision of the intra-convectional region and Dawes' black points are the surface of the viscous shell. Therefore unless we are prepared to admit a great change in transmission, we must assume that the envelope formed by condensation at the extremity of the cooling radius has become an area of combustion, and is actually the hottest layer of the sun, the convectional region having half the heat, and the interior of the core four-fifths, while the heat of the shell is as yet undetermined. This statement needs such modification as to allow for the loss of heat in the expansion of the ring. The eighty per cent at the surface may really indicate a temperature within the nucleus even higher than that of the photosphere, since more than one-fifth might be lost when released from the pressure restraining it before ejection. A measurement of the change of heat in the penumbra during the waning of the ring would throw some check upon a solution of this point. It is however clear that the high pressure inside the nucleus assumed by some theories can hardly be allowed, since relief at a fixed tension is afforded by the frequent discharge of the rings. Their pressure therefore indicates the maximum within the core. This argument would tend to revert to the medium temperature theory, say 10,000 degrees Cent., and it must be concluded from all the data that the state of solidification has set in at the surface, although but slightly advanced beyond thick liquefaction, while the gaseous condition of the interior is only approaching the formation of a liquid.

There is, however, one escape from this conclusion of a comparatively cool interior, although it lies in a direction which must at the outset be conceded to be conjectural. The analogy is based upon the laws of vibrations as known in sound and light. Beyond certain limits sound becomes inaudible as possessing periods of frequency too small, or too great; the same is true of light in every particular. In both directions of the few and the many vibrations in the unit of time, light and sound become super-sensible although it is not presumed that vibrations cease actually at the boundaries imposed by our sensations. The character which distinguishes heat from light no doubt consists more in the form of the wave pulses than in the frequencies; the like theorem being true of actinic rays. The analogy becomes fascinating to assume that heat vibrations at certain limits become super-sensible. Hence a type of heat may exist within the photosphere which would be practically inoperative, until reduced by cooling to the degree of frequency perceived in the photosphere; so that if the envelope were suddenly stripped off, other things being unchanged, the nucleus would seem cool and black from its condition of super-heat and super-light. Thus Dawes' black specks may be taken to indicate a comparatively cool or a super-heated surface of the core. A cool interior supplying by its contraction, fuel for the terrific conflagration of the granules is the simplest working hypothesis, and it is hoped that further analysis may justify it, inasmuch as the second theory of super-heat must remain beyond the range of verification.

This mechanical construction of the sun will account for all the phenomena of the spot periodicity, zones, and equatorial acceleration in longitude. First considering the figures of the envelope and the nucleus in a state of rest, it is perceived that the envelope is a true spherical shell of low specific gravity, held in position by the tension of radiation on the inside, and on the outside by the forces of condensation and gravitation, being a state of equilibrium so far as the average shape is concerned. The mutual adjustment continually going on tends to propagate great waves as resultant sinkings and heavings of the surface which are detected in the "réseau photosphérique" of Janssen, or in the change of diameters, measured by several observers, also in the continual interchange of pressure producing prominences both outside, as visible, and inside but hidden. The forces thus arrayed are sufficient to account for the fluctuation of the solar surface, while the general effects of combustion add the peculiar disturbances known as faculæ in the upper regions. The distinction of photosphere from reversing layer and this from chromosphere and corona, is somewhat arbitrary though of course followed closely by the superposition of the layers under the laws of specific gravity and tension produced by heat.

The nucleus is spherical before rotation, but after it revolves the figure changes strictly according to the laws of solid or viscous bodies, assuming that of an oblate spheroid, while the envelope partaking of the same motion has far less tendency to elongate, since the forces of tension are strong enough to overbalance the centrifugal velocity of the equator by a thrusting of the polar regions. The central oblate spheroid surrounded by a spherical shell contains the data for the solution of our problems.

As a viscous body under rotation assumes this new figure with major and minor axes, there is developed by the pull at the equatorial regions a circle of structural weakness in two latitudes above and below the equator, determined by the velocity attained. This circle of diminished strength on the outer layers is near the intersection of the circle and ellipse of revolution forming the original and ultimate bodies. The strain produced by the contraction of radiation will seek relief by the path of least resistance, so that the vortex rings should be ejected on the average at the two parallels north and south as before indicated. The question of maximum and minimum activity is simply the natural ebb and flow of forces seen in all such operations, and although probably following a general average of recurrence can hardly be constrained within rigid periods in all the details. Thus the eleven year period, with its fluctuations from seven and a half to fifteen years; the shorter time between minimum and maximum, with the longer interval from maximum to minimum; the contracted zones at minimum and the expanded belts of spots at maximum; the non-occurrence of spots at the equator and in higher latitudes, except near the poles where another weak structure should appear; the ejection of rings singly, in pairs, or in groups, and the observed recurrence of them in the same positions: these and other phenomena are the simple outcome of the mechanical and physical conditions that evidently exist.

The main feature of the figures as a combination, is that the equatorial regions of the nucleus must be nearer the envelope and the polar regions farther from it. The difference in length between the equatorial radius and the polar radius, is the advantage in length of path that a body projected from the equator would have over one projected from the poles, and the intermediate regions pass through a continuous variation of this distance. The shorter the path after projection, the greater is the impact in a ratio depending upon the square of the time, provided the change in acceleration has the same retardation in all cases. The projectile has the tangential velocity of the surface at the point of escape, which diminishes from the equator to the poles, and also the velocity of projection. The resultant path is a parabola, and the body projected at the equator will have a greater relative velocity on striking the envelope than would a body at the poles, supposing the force of normal ejection to be the same. The tangential component of this excess of velocity will tend to accelerate the equatorial regions of the envelope and will impart exclusively a movement in longitude in the direction of rotation. The continuous bombardment of radiation and ejection of the vortex rings will constantly accelerate the equatorial zones in which the spots are observed. Here we have a complete set of data for a study of the inner life of the sun. The sun-spot latitude, which is best obtained at minimum as freed from extraordinary circumstances, will give the circle of latitude drawn at the structural weakness of the ellipsoid, and from it there can be computed the ellipticity of the figure of equal volume with the sphere at rest. This latitude seems to be about 23 degrees north and south. The acceleration of the equatorial zones being about two days in twenty-five, will provide data, for the velocity of motion through a space equal to the difference between major and minor axes, counted from the velocity acquired at the extremity of the equatorial cooling radius, and thence the length of the path between the nucleus and the envelope being combined with the latitude of weakness, ought to produce a good result for the velocity of rotation of the nucleus of the sun, the mass and density of the same, and probably the forces of ejection. It seems useless to encumber this paper with any details of computation considering the paucity of the right kind of observations, therefore the outline only is briefly indicated. It is also to be suggested in this connection that the most minute observations upon the life of the vortex rings be instituted, including the exact length of days they possess, the rate of diminution of velocity to be discovered from the increasing sectional areas, a detailed study of the filaments as a means of measuring the pressure of the surrounding photosphere and hence the general tension of the enveloping shell. Numberless interesting solutions suggest themselves, as, for instance, the slight tendency of the spots to approach the equatorial or the polar edges of the zones from the positions where they were generated, doubtless following a natural law of seeking calmer regions; or the distinction between sharp crested granules, at the poles or equator, and the rounded granules in the spot zones, which would be due to the conflicting and more tangentially arrayed currents here than in the polar and equatorial portions of the photosphere; or to the tendency of the faculæ to lag behind the spots, caused by the advance of the ring under the influence of its tangential motion which results in the apparent western side first breaking in, while openings are made in the rear through which the prominences burst forth; the evident effort of two principal spots of a group to swing the connecting line so as to point east and west and thus follow the direction of the current; the appearance of the gray and rosy veils under the umbra, which may be shreds of the photosphere torn down and across the opening by the drag of the ring on its side; the fine dark lines interspersed with bright ones in the umbra, as surmised by Professor Young, due to the general action of the convection currents below, but which may proceed outwards by means of the puff of the motion of the ring;

and there are doubtless many more not mentioned in this hasty *résumé*.

The usual occurrence of electric terrestrial storms as an accompaniment of the outburst of new spots, which is detected in the disturbance of magnetic currents and auroral displays, yet remains to be noticed. From the 1st of September, 1859, when Carrington saw an electric discharge upon a spot and thus caught the sun in "the very act," this sequence of events has been often described. The vortex ring having an immense superficial area rotates with tremendous velocity in its fresh vitality, and encountering the resistance of the photospheric clouds becomes a machine for generating free frictional electricity upon a vast scale. The particles of condensing metallic vapor, possibly in the shape of incandescent dust as some suppose, become the points of ignition, so that we have a multitude of electric arcs. An analogy is known in the electric flashes accompanying the eruption of volcanoes where the impact of fluids upon suspended particles of dust in the aqueous vapors is a sufficient cause; also in the ejection of steam upon any hard substance. The rotation of the vortex ring thus generating electricity, is a striking confirmation of the theory, and the amount of electricity produced becomes a measure of the force expended in the rotation, so that we have here another path back to the vital power of the sun's nucleus. A final corollary seems to be desired, namely, that this proof of the production of free electricity by one type of phenomena, may be only a fraction of the same thing going on more persistently over the entire photosphere by the friction of the metallic clouds and prominences chafing against each other. In other words, frictional electricity, though resorted to for the explanation of some facts, has not been assigned a position of sufficient influence in extra photospheric phenomena. The sun may well be surrounded by an electric atmosphere extending far beyond our earth, of diminishing intensity as distance from the sun increases, in which there is a tendency not only of repulsion but physical suspension, causing both enormous velocities by its own motion, and enabling ejections to travel far in the presence of great forces of gravity. The prominences and the faculæ play about the lower and denser regions of it; the corona is a suspension of particles held between the rival powers of gravity and electricity, diminishing in extent during minimum periods of the sun's activity when less electricity is produced, thus enabling gravity to contract the dust and show it more clearly by reflection; the reverse taking place at maximum periods of activity. Possibly the quadrilateral or equatorial structure of the corona is directly related to the region of principal generation of the electricity while the lawlessness of its form is easily comprehended, if not explained. The phenomena of the zodiacal light, the aurora, and the structure of comet's tails are but manifestations of the contention of the laws of gravitation and electric repulsion, acting upon minute particles whose masses are much less affected by the force of gravity than their surfaces are by the force of frictional electricity.

The author will be gratified to feel that some light may have been thrown upon these perplexing problems, though it cannot be hoped that the best analysis has been bestowed upon the numberless details of operation. If a part is correct there opens before us a vast new field of investigation and research.

HINTS ON THE POPULAR STUDY OF ASTRONOMY.

MISS M. E. BYRD.*

For the Messenger.

Everybody looks at the sky, but it is well nigh painful to any student of astronomy to consider how thoughtless and aimless this looking is.

To-night if everyone would begin to look with some definite object in view, and would do some thinking as well as looking, many would be surprised to find how soon there would be a "new heavens" for them.

The first steps toward this end are very easy, and to any

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who care to help themselves or others to a little new pleasure in looking skyward, the following hints are addressed. They are given with no claim to originality, but with the hope that some one may find something suggestive. At first two things only are necessary, a note-book and a motto. For the former anything whatever in the shape of a blank book will answer the purpose. For the motto, nothing better can be found than a saying from a letter of the poet Gray, "Half a word fixed on or near the spot is worth a cart load of recollection." The first thing to be done is simply to go out and look at the sky. For once, turn an earnest, attentive gaze heavenward. Frankly face the question: "Do I have any clear idea of how the sky looks?"

I verily believe that if the spirits of the air some night should roll away the canopy of our heaven, and spread over us some section of the Milky way, with a different configuration of stars, many intelligent people would never know the difference until they were told.

So first and foremost the question is, "How shall I describe the sky to-night?"

The following are suggested as guiding questions that will help to answer the main one. Are there just as many bright stars in one part of the heavens as in another? Toward the south quarter do I find a number of bright stars that characterize that part of the sky? Can I arrange them in some group so as to fix them in mind, that is in a triangle, parallelogram, five sided figure or any easy configuration? Can I in like manner fix other groups toward the north and east? Is there any connection between the bright stars and the Milky way? How is the Milky way placed in the sky? That is, does it lie toward the north or south horizon? Does it intersect the horizon? If so, where on the horizon are these points of intersection? Is the breadth of the Milky way uniform? When these and similar queries are answered a description should be written out on the spot, accompanied by sketches of two or three striking groups of stars copied directly from different parts of the sky. Doubtless this first description will seem rather crude to the observer himself; and it will not be amiss to spend some time on a number of evenings in correcting and completing it. The sky can be divided into sections by imaginary lines, and several nights devoted to each. In mapping these sections it is well to employ two or three symbols for stars of different degrees of brightness. Directions are readily checked by connecting any three stars in the copy and noting whether the angle thus formed corresponds with the similar angle between the same stars overhead. In measuring distances in the sky have nothing whatever to do with feet and inches. Let the unit be the distance between two familiar stars, and in imagination lay it off a certain number of times from star to star as the sketch progresses.

The star watcher's experience will be very unique, if a score or two of questions do not suggest themselves to his mind before the sky-picture is finished. Whatever the number all should be carefully entered in the note-book. Some of them very likely lie beyond the ken of the most powerful astronomer, and some are not too difficult for the school girl to answer. But before entering upon the consideration of the easiest, it will be a matter of convenience to have names for some of the bright stars and groups of stars. Astronomers are not interested in the ancient grouping of the stars into constellations, and hardly anyone cares to puzzle out the figures of maidens, horses, beasts and birds, but for those who look at the heavens without telescopes it will be pleasant and convenient to have their hosts marshalled within the old boundaries, and under the old familiar names, Orion, Taurus, Gemini, etc. It will be hard to find any star map, old or new, that will not give all the information desired ; and anyone who has followed the preceding hints will have no trouble in locating the bright stars of the different groups within their proper limits.

Even thus humbly equipped, the star gazer is ready to find out the answers to an almost unlimited number of simple questions. Very likely on the first night the question comes to mind, How do the stars move ? The first step toward the answer is to break up the general question of motion into several specific questions. Are the stars moving away from or toward one another? Are those in the handle of *Ursa Major* coming together, or those in Orion's belt moving apart? Or, on the other hand, is the whole host of heaven moving on in marching order, each one always keeping just so far from every other? If careful sketches of the same groups of stars have already been made on different nights, a close comparison of these should enable the observer to decide whether or not the stars have, with regard to one another, a relative motion that can be detected in a short time by the naked eye. In case the question is decided by maps, made for the purpose, attention should be given to relative rather than absolute distances, and care should be taken not to be biased by any theory.

Having ascertained that for naked-eye vision the stars remain always at fixed distances from one another, it will perhaps require some patient watching and mapping to discover the path and direction taken in their orderly march. Orion is not for this purpose a favorable constellation to observe, nor is the answer that the stars rise in the east, pass across the heavens and set in the west, at all satisfactory. The question naturally following has very probably not been entered in the note-book at all. If the time when a particular star comes into line with two fixed terrestrial marks is noted one evening, and on the following evening the time is observed for the same position of the same star, will the interval be more or less than twenty-four hours? Or in other words, is the sidereal day longer or shorter than the mean solar day? As an illustration of a particular method of answering, the following note-book extract is given:

"Northfield, Minn., Feb. 28, 1887. I selected Beta of *Ursa Major* as my star, and the middle bar of the window sash as the object behind which I waited for the star to disappear. I took my position at the window and noted it carefully. At *7h* 15*m* P. M. I began watching my star, and at *7h 22m* P. M. it passed behind the window sash." A similar observation on the following evening enabled the youthful observer to secure quite an accurate answer to the question proposed.

The moon is the most accommodating of heavenly bodies in furnishing a large number of simple questions some of which can be answered in a single clear night. Does the moon move among the stars? In what direction? What is the hourly rate of motion in terms of its own diameter? Two sketches of the moon and a familiar star near it furnish the necessary data. It will be well to have an interval of three or four hours between the sketches, and to take the comparison star near enough to measure its distance conveniently in units of the moon's diameter, *i. e.*, in half degrees. The following are some of the queries that require longer consideration. In any given month is the moon's path in the sky constant? Helping questions are such as these: Does the moon uniformly rise and set at the same points on the horizon? Midway between these points is the zenith distant constant? If there are variations, is the moon moving toward or from the zenith? Are the points of rising and setting moving north or south? It is not at all difficult to fix the point where the moon rises. Choose some convenient place for watching just as the moon begins to appear above the horizon, select two fixed objects in line from the point of view, and so taken that the line prolonged will meet the horizon not very far from the moon, then calculate how many half degrees, *i. e.*, full moons, can lie between the imaginary point of intersection and the moon's center. In the note-book entry, the fixed objects should be sketched or described so as to be easily identified on following nights. After this there comes the still larger question of comparing the paths of the moon in different months. If one can only have patience to collect data throughout the year, some interesting results will be secured. In order to trace the real path of the moon among the stars for any month it is best to make a rough map of the constellations of the zodiac above the horizon, and locate upon it the position of the moon observed as often as possible on different nights and at different hours of the same night. An easy way to make the map is to lay tissue paper upon the printed map and then mark down such stars as are desired. If such maps are made for different months, the question can be answered whether the moon's path among the stars varies from month to month.

Many of the questions about the moon will apply to the sun and some will not be found too difficult. There is also a whole host of questions about the relative paths of the sun and moon which will naturally suggest themselves.

One can hardly look up at the bright planets without wishing to find out something about them by personal observation.

They are surely moving among the stars.

Are different planets moving with equal rapidity? Is the same planet always moving in the same direction? How is the direction in which a particular planet moves at a given time ascertained? For these and like queries, careful sketching is the key. As a more definite hint for the last, a portion of an entry from a school-boy's note-book is added. Saturn has been carefully located on two sketches including the principal stars in Gemini, the interval being nearly a month. "I compared these two maps, and found on the first map that Saturn was situated about midway between two stars in a small triangle [stars of triangle identified on celestial globe], which is about one half further from Pollux than Castor is. In the second map I find that Saturn has moved away from this first position toward the west. In this map it is seen a trifle above and away from this triangle and away from Pollux. So I find that Saturn is moving westward slowly and the motion is retrograde."

Looking at the sky according to some such method as has been suggested will not be found burdensome or difficult, and it cannot but give pleasure to replace the general vague notion of the spangled blue overhead with some simple but definite knowledge about the sky.

In Mr. Colbert's article in No. 52 entitled, "Motion of the Lunar Apsides," readers will please correct the ninth line from the bottom of page 53 by dropping *r*.

EDITORIAL NOTES.

For want of space last month much current matter was deferred, some of which we present this month though it seems late.

By adding sixteen pages to our ordinary size, we are able to give, in this number, a variety of extended articles and a table of news which our readers may deem acceptable dessert.

A Catalogue of 130 Polar Stars.—Some time ago we received the first paper of a series having the title, "A Catalogue of 130 Polar Stars for the epoch of 1875.0 resulting from all the available observations made between 1860 and 1885, and reduced to the system of the publication XIV of the *Astronomischen Gesellschaft* by William A. Rogers and Anna Winlock," of Harvard College Observatory.

In an introductory note, Professor Rogers says that his connection with the paper is limited to the methods of discussion, and to the examination of numerical results obtained. Beyond this that all credit belongs to his assistant, Miss Winlock.

The purpose of the paper is to discuss the modern observations of such polar stars north of +70° as are found in the H. C. Catalogue of 1213 stars. The reduction of stars under 85° north declination involves only the second or third powers of the time for sufficient accuracy, for a limit of fifteen years but for stars near the pole the problem is more difficult. To show how the work was done, the necessary formulæ are stated in order fully, and the tables of constants given, and then a single star, Groombridge 1119, for epoch 1875.0, is taken as an example and its reduction shown in detail. The method of the paper seems excellent and complete, and is a credit to its authors. We shall look with interest for succeeding papers, in which will appear the discussion of other kindred topics named at the close of the one before us.

Star Spectroscope for the Great Equatorial of Lick Observatory.—By kindness of J. E. Keeler of Lick Observatory, we have been furnished blue prints of the drawings designed by himself, for the new star spectroscope to be used with the 36-inch equatorial. The scale of the two drawings is each ⅓; one presenting the instrument in outline, and the other showing its details. In examining the details of the latter drawing, some points and facts are noticed which are new to us, and all are exceedingly interesting.

The ratio of the aperture to the focal length of the great objective is 1 : 18.7.

The aperture of the telescope object glasses for the spectroscope is 2 inches, and the focal length is 19.5 inches.

Electric illumination is provided for micrometer and vernier readings by a neat design which is new to us though it may not be to others. The instrument is to have a comparison apparatus for metallic and gaseous spectra, a diagonal eye piece for viewing the slit from behind and a reversion attachment. The spectroscope is also to be adapted to general laboratory work.

Some of the interesting matter in Mr. Barnard's article on Celestial Photography which appeared in number 52 was taken from the *Observatory* and the Annual Report of the Paris Observatory. He promptly says that he inadvertently failed to give credit to those publications as he should have done.

Kepler's Correspondence in 1599.—In the introductory note, given last month, to the first letter of this important correspondence, it was stated by whom these three missing letters were found, their respective dates, to whom written, and by whom published in 1886.

That part of the first letter already published is a specimen of the letter-writing of young Kepler at the age of 27. His style is easy and fascinating, often witty and sometimes sensitively delicate, in righting the opinions of his friend on topics belonging to his loved science.

We are not surprised to learn that our readers in different parts of the United States are already anxious to possess these letters and have written for the address of the publisher. It is probably true that all owners of Kepler's *Opera omnia* would also desire the pamphlet. Such persons should address Victor Dietz in Altenburg (Sachsen-A.), Germany.

The side lights to these letters which appear in the introduction and the supplemental notes are instructive. From them we learn that Herwart's correspondence with Kepler began in 1597, and Kepler was soon involved in it so much that it burdened him, and when so taxed he once said, "There is at the monastery —— Herwartus who keeps asking questions of such a nature that he torments me with great labor, driving me to all those things which Crusinus would have advised."

These questions pertained to the classics, astrology and astronomy. Herwart first touched on the declination of the magnetic needle in 1598 and Kepler became intensely interested in that subject in the following year on account of the "Historia navigationis in Arctum" which had been published the previous year. Kepler refers to that book in a postscript to the first letter. It is noticed with some interest that soon Kepler seeks information from Herwart concerning the magnetic declination in Portugal which the latter gives. Then Kepler advances the hypothesis that, "The magnetic needle points to the pole of the earth which was so by creation;" and later that, "Magnetic force is of the same kind as gravitation." Other points will be mentioned next time.

Haynaldshen Observatorium. — A report containing 178 pages, folio size, with numerous double-page plates prepared by Dr. Carl Braun, S. J., giving account of the work done at the above named observatory, during the last year finds welcome place on our table. While we have not space even in this large number to speak in detail of the many interesting topics presented therein, we must call attention to some, such as, the observation of sun-spots, method, reduction record and mapping of the same. Also the articles on electrical contacts, the passage micrometer, each of which is fully illustrated, and in which there are new and practical ideas that may be considered with profit.

Recent Showers of Meteors.—Between Nov. 17 and Dec. 29, 1886, I saw 375 meteors during 54 hours of observation. In January, 1887, the weather was very cloudy and only 57 meteors were counted during 13 hours of work.

A large number of radiant points have been determined but they apparently represent very feeble streams. The most active display observed during the last few months was from a center at 194°,+67°, between Dec. 18–28, which furnished 17 meteors. The following list of radiants is sent in continuation of that printed in the SIDEREAL MESSENGER for Dec., 1886, p. 309:

Epoch of Shower.	Night of Max.	Radiant Point.		No. of Meteors.	Appearance.
		R. A.	Dec.		
1886.		°	°		
November 17–18	Nov. 17......	53	+ 71	8	Swift, faint.
Nov. 18–Dec. 5...........	Nov. 30......	190	+ 58	8	Swift, streaks.
November 29–30	Nov. 30......	190	+ 79	6	Rather swift, bright.
November 29–30	Nov. 30......	81	+ 22*	6	Slow.
November 29–30	Nov. 29......	27	+ 71	5	Slow, short and faint.
Nov. 29–Dec. 1...........	Nov. 29......	64	+ 23	6	Rather slow.
Nov. 30–Dec. 5...........	Dec. 5......	105	+ 11	5	Swift, bright.
Nov. 30–Dec. 6...........	Dec. 2......	162	+ 58*	6	Very swift, streaks.
December 15–29..........	Dec. 20......	47	+ 65	5	Very slow, trained. Faint.
December 15–25..........	Dec. 18......	80	+ 24*	6	Slow.
December 18–28..........	Dec. 22......	194	+ 67	17	Swift, streaks.
December 18–29..........	Dec. 18......	134	+ 8	6	Very swift, thin streaks.
December 20–29..........	Dec. 25......	98	+ 31	5	Very slow, trained.
December 21–28..........	Dec. 28......	194	+ 32	7	Very swift, streaks.
December 22–24..........	Dec. 24......	129	+ 19	5	Rather swift.
December 22–28..........	Dec. 24......	218	+ 36	8	Very swift, bright streaks
December 22–28..........	Dec. 28......	115	+ 32*	7	Slow.
December 24–28..........	Dec. 24......	184	+ 39	6	Very swift, streaks.
December 24–31..........	Dec. 24......	77	+ 32	5	Slow.
December 28–29..........	Dec. 29......	231	+ 52	5	Rather swift, long.
1887.					
January 25...............	Jan 25.......	180	+ 24	5	Swift.

* Seen also in December, 1885. See SIDEREAL MESSENGER, Feb., 1886, page 61.

I recorded nothing of the Leonids, Andromedes or Geminids in 1886, observations being hindered either by moonlight or overcast skies at the special epochs of their return.

Bristol, England, Feb. 27, 1887. W. F. DENNING.

A valued friend of the MESSENGER calls attention to the fact that the photograph of the Nebula of *Orion* by Dr. Draper was made with his 11-inch photographic refractor and not with an 18-inch reflector as stated in Mr. Barnard's article in our February number. It is also true that the aperture of Dr. Draper's reflector was 28 inches instead of 18.

Comet d, 1887 (Barnard, Feb. 16).—February 17, Mr. Barnard writes, "A new comet was discovered here on the night of February 16, at about 10*h* 30*m*. It was very faint and moving with astonishing rapidity to the northwest, 14*m* 45*s* west, and 5° 17′ north daily.

Ring micrometer comparisons were made with a 7th magnitude star near which the comet passed. The star has not yet been identified. The following is the instrumental position of the comet at 10*h* 58*m*, Nashville M. T.:

$$\begin{array}{l} \text{R. A.} = 8h\ 4m\ 9s. \\ \text{Decl.} = -16^\circ\ 15.5' \end{array}$$

Vanderbilt Univ. Obs'y. E. E. BARNARD.

Orbit of Comet d 1887 (Barnard).—From my own observations of Feb. 16, 22, 28, I have computed the following orbit of the above comet:

$$\begin{array}{rll} T = & 1887\text{ March }26.5258 & \\ \omega = & 34^\circ\ 33.4' & \multirow{3}{*}{} \\ \Omega = & 134^\circ\ 47.8' & \\ i = & 139^\circ\ 46.7' & \\ \log q = & 0.002449 & \end{array}$$

$$\left.\begin{array}{rl} \omega = & 34^\circ\ 33.4' \\ \Omega = & 134^\circ\ 47.8' \\ i = & 139^\circ\ 46.7' \end{array}\right\}\ 1887.0$$

E. E. BARNARD.

Vanderbilt Observatory, Nashville, Tenn., March 16, 1887.

Orbit of Comet 1887 d (Barnard, Feb. 16).—From observations of Feb. 22, 25 and 28, I have computed the following orbit of Comet 1887 *d*.

ELEMENTS.

$$\begin{array}{rl} T = & 1887.\ \text{Mar. }28.5188\text{ Gr. M. T.} \\ \left.\begin{array}{rl} \pi - \Omega = & 36^\circ\ 39'\ 34'' \\ \Omega = & 135\quad 26\quad 22 \\ i = & 139\quad 45\quad 10 \end{array}\right\} & \text{Ap. Eq.} \\ \log q = & 0.00250 \end{array}$$

Middle Place $(C-O)$

$$\begin{array}{r} \Delta\lambda \cos\beta = -9'' \\ \Delta\beta = +2'' \end{array}$$

O. C. WENDELL.

Harvard College Observatory, Mar. 10, 1887.

Occultations of Stars by Dark Limb of the Moon.—At the Davidson Observatory, San Francisco, Cala. Clark equatorial, 6.4 inches.

Date, 1887.	Ob-server.	Power.	Star.	Magn.	Local Sider'l Time.			Remarks.
					h	*m*	*s*	
Jan. 28.	G. F. D.	90	(? Stone 139)..	7	5	46	41.6	Obsn. good. (*a*)
Feb. 2..	G. D...	90	70 Tauri	6	5	49	21.5	Obsn. good, but star faint; obj. partly covered.
" 2..	G. D...	90	Arg. 15: 630..	8.7	7	50	23.5	Disappearance sharp and sudden.
" 2..	G. D...	90	θ' Tauri	4	7	50	45.5	Disappearance sharp and sudden.
" 2..	G. D...	90	75 Tauri	6	7	51	23.4	Disappearance sharp and sudden.
" 2..	G. D...	90	Arg. 15: 633..	6.5	7	54	30.2	Disappearance sharp and sudden.
" 2..	G. D...	90	Arg. 15: 635..	8.5	8	31	21.7	Disappearance sharp and sudden.
" 2..	G. D...	90	B. A. C. 1391.	5	8	46	09.6	Disappearance sharp and sudden.
" 2..	G. D...	90	B. A. C. 1394.	7	8	53	38.3	Disappearance sharp and sudden.

(*a*) Identity of this star somewhat doubtful. Transit observations for time, for this and the observations of Feb. 2d, by G. D.

Observers: G. F. D. = G. Fountleroy Davidson.
G. D. = George Davidson.

Geographical position of Observatory:
Latitude = 37° 47′ 24.75″ N.
Longitude = 122° 25′ 40.54″ W.

Public Parks and Scientific Institutions.—We notice that there is a bill before the Legislature of Illinois to empower the corporate authorities of public parks to set apart so much, or such parts of the public grounds, as may be desirable or necessary for Botanical Gardens, Astronomical Observatories, or other Scientific Associations, and to give these societies or associations exclusive control of the sites so designated during their maintenance or continuance. The bill further provides that the corporate authorities before named shall have power to make, from time to time, such appropriations for establishing and maintaining the same as they may deem necessary for the advancement of scientific knowledge, or the promotion of the general welfare.

In the main this seems to be a wise provision, although certainly a novel one in this country. Such a plan would evidently awaken interest in scientific pursuits in the public mind and doubtless lead to useful support which ought to come from the public generally on account of corresponding benefit.

The drawback at once suggested is, that interruption which will seriously impair quiet and continuity in the pursuit of certain classes of scientific work. This and like objections can, however, be overcome in a considerable degree by wise planning. We sincerely hope the experiment will be tried, for no one can doubt the popular educating influence of such a plan if managed wisely.

Occultations of Stars in the "Hyades" at dark limb of the moon, Feb. 2, 1887: observed at the Chabot Observatory, Oakland, Cala., Supt. F. M. Campbell, Director.

Star.	Local Sidereal Time.			Remarks.
70 Tauri	5*h*	50*m*	14.8*s*	Apparently not instantaneous.
θ' Tauri	7	51	27.1	Good, instantaneous.
Arg. + 15:633	7	55	16.2	Good, instantaneous.
Arg. + 15:635	8	32	03.6	Good, instantaneous.
B. A. C. 1891.........	8	46	50.7	Good, instantaneous.

θ' Tauri was observed to emerge at 8*h* 36*m* 54.9*s* ± 05.0*s*, a weak observation, at bright border.

These were all observed with the 8-inch Clark equatorial, by Mr. Chas. B. Hill.

The geographical position of the Observatory is

Latitude, 37° 48′ 05″ N.
Longitude, 8*h* 09*m* 06.3*s* W.

The Boyden Fund of Harvard Observatory.—By will of U. A. Boyden $230,000 was left in trust to Harvard College for the purpose of astronomical research, "at such an elevation as to be free, so far as practicable, from the impediments to accurate observations which occur in the observatories now existing, owing to atmospheric influences." The first step in carrying out this will is to obtain knowledge of suitable places of sufficient altitude to secure the kind of observations sought. To this end Professor Pickering has issued a circular asking for detailed information on the following points:

1. Latitude and longitude. Distance and direction from some town, or other well-known point. Height, and how determined.

2. Peak, pass, or table land. Character of surface: ledge, broken rock, gravel, or covered with trees, shrubs, or grass. Prevalence of snow in summer, and period during which the depth of snow in winter might obstruct the paths of access, or occasion other inconvenience or damage. Proximity of wood for fuel, and of water.

3. Means of access, distance from and height above the nearest railroad station, wagon road, bridle-path, or footpath. Time of ascent and descent. Nearest post-office and telegraph station, and their distances from the proposed station. Nearest point of road kept open in winter.

4. Observation of the rainfall at different seasons of the year. Proportion of the sky covered with clouds at different hours and seasons. These observations are desired at sunset, sunrise, and late in the evening. Such observations may also be made of a distant mountain peak, confining the evening observations to moonlight nights. Observations of the barometer and thermometer are also desired. Information is wanted regarding the prevalence of very high winds; the presence of dust, haze, or the smoke from forest fires, rendering distant points invisible; and all other meteorological phenomena affecting the value of the station for astronomical purposes. If there is a rainy or cloudy season, its duration; also the regular recurrence of clouds, thunder-storms, or wind, at any given hour of the day.

5. Sketches or photographs of the proposed location, and of points on the road; also of the view.

We are sure that our genial friend J. A. Brashear, of Allegheny, Pa., will not object to our telling the readers of the MESSENGER what is now going on in his new shops in the interest of astronomy. He has the order for the great spectroscope for the 36-inch equatorial of Lick Observatory, elsewhere particularly spoken of; the mounting of a 6½ inch glass for Francis G. du Pont, of Wilmington, Delaware; a spectroscope for the University of California; one for another college; one for R. R. Beard, Pella, Iowa; a concave grating spectroscope of large size for the University of Madison; one for Geo. E. Hale, of Chicago; and other astronomical instruments for some prominent American astronomers.

Mr. Brashear's work is receiving very favorable notice in Europe as well as at home, another mark of respect for American industry and skill.

Brightness and Masses of Binary Stars.—In *Observatory* No. 120, W. H. S. Monck has an interesting paper on the relation of the brightness and masses of binary stars, depending mainly on the assumption that the mass of the smaller star of each pair is very small compared with that of the larger. The relation is independent of distance or parallax, but does use constants of photometry easily obtained by observation. Readers of the MESSENGER who may refer to the *Observatory* for the discussion of this method will please notice that the factor, in the final formula, $P \div P'$ should be affected with the fractional exponent ¾.

Mr. Common's Photograph of Orion.—By our request, Mr. A. A. Common of England has favored us with a copy of his photograph of Orion which astronomers in America have recently and justly spoken of with very favorable comment. This picture was taken by the use of Mr. Common's 3-foot reflecting telescope with particular details of exposure of which we are not informed. The results obtained are excellent.

In January last Mr. Common was himself at work on a larger telescope with which he expects to obtain even better results.

U. S. NAVAL OBSERVATORY.

The Great Equatorial.—Observations of double stars will be continued. Observations of previous years will be discussed if time and force will permit.

Continuation of measurements of the fainter stars in the Pleiades. Completion of these observations if possible.

Observations of the conjunction of the five inner satellites of Saturn with the minor axis of the ring, and the angles of position and the distances of the faint satellite Hyperion.

Reduction and discussion of the observations of former years as time and force will permit.

The Small Equatorial.—Observations of comets whenever possible.

Observations of stars that need to be identified for the preparation of the third edition of Yarnall's catalogue, now in progress.

Observations of stars and asteroids for identification for the transit circle, and of such asteroids as cannot be observed with that instrument.

Observations of occultations of stars by the moon whenever practicable.

The Transit Circle.—Completion of the observations of miscellaneous stars for the proposed Transit Circle catalogue; concurrently with these, observations of the sun, moon and planets.

After the completion of the above observations (which, it is hoped, will be accomplished by the 1st of March), the instrument will be dismounted for repairs, and will thereafter be used in observations of—

The sun daily.

The moon throughout the whole lunation.

The planets, major and minor.

Stars of the American Ephemeris, required for the determination of instrumental and clock corrections.

Miscellaneous stars, as may be deemed advisable.

The observations of preceding years will be reduced as rapidly as possible.

The Transit Instrument.—Observations for the correction of the standard mean time clock daily.

The Repsold Circle (at Annapolis).—Observations of the 303 southern stars.

The 59 refraction stars of the Leiden Observatory.

Auxiliary stars of the Berlin Jahrbuch.

CAPT. R. L. PHYTHIAN, U. S. N. SUPT.

BOOK NOTICES.

The Elements of Geometry, by Webster Wells, S. B., Associate Professor of Mathematics in the Massachusetts Institute of Technology. Messrs. Leach, Shewell & Sanborn, Publishers, Boston and New York.

This book covers the usual ground of Plane and Solid Geometry, and in matter and arrangement its author lays no claim to originality. The course in Plane Geometry is divided into five books, followed by an appendix on symmetrical figures, and a few interesting theorems on maxima and minima of plane figures.

The remainder of the book is devoted to Solid Geometry, and is divided into four parts, the first treating of lines, planes and angles in space; 2, polyedrons; 3, cylinder, cone and sphere; 4, the measurement of the cylinder, cone and sphere. The considerable number of brief and simple exercises given with the various topics is a helpful feature for the class-room as well as the private student. The publishers have done their part well and very neatly.

A Treatise on Algebra, by Professors Oliver, Wait and Jones, of Cornell University, Ithaca, N. Y. Dudley F. Finch, 1887.

In working out the plan of this new book the authors have widely departed from the ordinary course of treating the subject-matter of Algebra. They have aimed to prepare a treatise in such a way as to assume no previous knowledge of the branch, by laying down first the primary definitions and axioms and building on these to develop the elementary principles in logical order, particular attention being given both to the matter and the form of its presentation. The new things that frequently meet the eye of the student of some experience will be somewhat surprising, and some may stop reading to ask if these enthusiastic Cornell men are not getting a little unorthodox in their notions touching the good old creeds of the fathers in Mathematics, for there seems here to be great liberty of conscience that may unsettle time-honored requisites for college standing in Algebra if their standard should obtain general favor.

The first twenty pages which treat of primary definitions and signs are largely given to the explanation and illustration of the mathematical language of the book, including the use of the ordinary signs and many others new, as subscript numbers and superior numbers, letters, accents and signs, Greek letters and fractional forms. On page 2, we notice that $x \div y$ is to be read "x over y." Is not that reading both inaccurate and inelegant? . Carleton students are taught so.

On page 16, under the first group of exercises for expression a, with exponent b, in parenthesis, with exponent c is to be read "the cth power" of a with exponent b. Suppose c is fractional or itself a negative quantity, would the direction be the best reading possible? When the exponent is a letter, variable or constant, is it not more accurate to call the superior an exponent? No fractional or negative superior can be called, strictly speaking, a power or a root unless it be a fraction with unity for its numerator. The general plan of all this introductory work is very excellent, and the thoroughness and completeness of the system will pay for the extra effort to get the increased vocabulary. The student will be led to think and express his thought more commonly in mathematical terms, and so easily acquire the peculiar habit of conciseness and directness that the study ought to furnish.

Further on, some new topics and some unusually treated are found; as, Absolute and Relative Error; Continued Fractions, Limits, Infinitesimals, Incommensurables, Derivitives and Imaginaries. The subject of Infinitesimals finds suitable place in this Algebra as it ought to in every one, and the Imaginary is dealt with after the graphic method, which is certainly interesting if its language does sound like that of Quaterinons. This is an excellent book of reference for teachers, but too full for ordinary classes. It is best suited to students who have some knowledge of Algebra and Geometry before undertaking it.

A Treatise on Trigonometry, by Professors Oliver, Wait and Jones, of Cornell University. Second Revised Edition. New York: John Wiley & Sons, 1884.

Though not a new book, it is a pleasure to look over this Trigonometry, and call attention of the readers of the MESSENGER to it as presenting the ordinary themes of Plane and Spherical Trigonometry in a very judicious and scholarly way. The copy before us is not provided with the ordinary tables. The publishers always do their part handsomely.

The Sidereal Messenger,

CONDUCTED BY WM. W. PAYNE,

Director of Carleton College Observatory, Northfield, Minnesota.

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THE ECCENTRICITIES AND INCLINATIONS OF THE ASTEROIDAL ORBITS.*

PROFESSOR DANIEL KIRKWOOD.

For the Messenger.

The average eccentricity of the 264 asteroids whose orbits have been calculated is 0.157. This is about equal to the mean eccentricity of Mercury, and exceeds the maximum of any other major planet. An inspection of the table of elements shows that while but one orbit is less eccentric than the earth's, sixty-eight depart more from the circular form than the orbit of Mercury. These large eccentricities seem to indicate that the forms of the asteroidal orbits were influenced by special causes. It may be worthy of remark that the eccentricity does not appear to vary with the distance from the sun; being nearly the same for the interior members of the zone as for the exterior.

The inclinations of the orbits are thus distributed:

From 0° to 4°	69
4 to 8	82
8 to 12	59
12 to 16	31
16 to 20	8
20 to 24	8
24 to 28	6
28 to 32	0
above 32	1

One hundred and fifty-two—considerably more than half—have inclinations between 3° and 11°, and the mean of the whole number is about 8°,—slightly greater than the inclina-

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* Extracted from an unpublished work on the Minor Planets.

tion of Mercury, or that of the plane of the sun's equator. The smallest inclination, that of Massalia, is 0° 41′, and the largest, that of Pallas, is about 35°. Fifteen minor planets, or nearly six per cent of the whole number, have inclinations exceeding 20°. Does any relation obtain between high inclinations and great eccentricities ? These elements in the cases named above are as follows :

Asteroid.	Inclination.		Eccentricity.
Pallas	34°	42′	0.238
Istria	26	30	0.353
Euphrosque	26	29	0.228
Gallia	25	21	0.185
Æthra	25	0	0.380
Eukrate	24	57	0.236
Eva	24	25	0.347
Niobe	23	19	0.173
Eunice	23	17	0.129
Electra	22	55	0.208
Idunna	22	31	0.164
Phocea	21	35	0.255
Artemis	21	31	0.175
Bertha	20	59	0.085
Henrietta	20	47	0.260

This comparison shows the most inclined orbits to be also very eccentric ; Bertha and Eunice being the only exceptions in the foregoing list. On the other hand, however, we find over fifty asteroids with eccentricities exceeding 0.20 whose inclinations are not extraordinary. The dependence of the phenomena on a common cause can therefore hardly be admitted. At least the forces which produced the great eccentricity failed in a majority of cases to cause high inclinations.

THE PHENOMENA OF COOLING ENVELOPES.

FRANK H. BIGELOW.[*]

For the Messenger.

The argument for the sun-spots as vortex rings indicates that the sun is composed of a nucleus and a cooling envelope. An extension of the analysis to the characteristics of the solar system seems to fortify this conclusion by its explanation of several unsolved problems.

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It becomes evident, therefore, that the questions of solar gravitation ought to be transferred generally from the apparent radius of the envelope to that of the nucleus, when the data for the sun's contraction are selected. The envelope of the photosphere now forming is the type of similar ones that have been constructed during the development of the sun from its original nebulous material. When the densities and angular velocities of the nucleus and its successive envelopes are compared, it is clear that they are relatively greater for the former. Being intimately connected, the densities are in both cases increasing quantities, since the contracting core is giving off its lighter material to form the envelopes, which in turn must become heavier as they are formed later in time. The tangential velocity of the surface of the nucleus is a maximum which upon being transferred by radiation and ejection to the envelope induces in it a diminished velocity when reckoned in angular rotation. The central body of the sun is more viscous and dense, and rotates more rapidly than the observations of the envelopes would indicate.

The normal reaction of forces resulting from the discharge of envelope material pursue parabolic paths which, extended backward into the nucleus, do not pass through its center, but on the side from which rotation proceeds, causing the angular acceleration of the nucleus due to contraction to be retarded by a very significant amount in its integral of time. The discharge of the same material produces also a heat reaction proportional to the work expending in the lifting.

These considerations enable us to introduce important modifications into the usual statements of Laplace's Nebula Hypothesis. The principal feature therein contained is that equatorial rings are thrown off from the surface of the contracting sun whenever its angular velocity is sufficient to make the centrifugal overbalance the central forces, and that the planetary distances mark the stages of this process. The objection is clear that such an accelerating velocity is incompatible with the apparent circular disk of the sun, being a total violation of the laws of mechanics. Upon the theory of cooling envelopes no such acceleration is demanded, for it is certain that wide intervals, increasingly so at the earlier stages of condensation, have existed between the nucleus and envelope, and that thus the formation of planets was never a phenomenon of the surface of the contracting sun. Also it is seen that the mechanical work of lifting these immense envelopes through the convectional spaces is balanced only by a retardation of the central angular velocities.

An inspection of the orbital velocities of the planets, in miles per second, indicates approximately the acceleration of the successive envelopes, from which it is possible to compute the radius of the nucleus at each critical position. The problem is very complicated, however, since it includes the consideration that the first materials to be thrown off were generally of low densities and would cool at greater radial distances. These suggestions tend to relieve analysis of some objections that have hitherto hindered mathematical investigations in this direction.

The same theory becomes applicable to formation of the secondary systems. The envelope which changes into any particular planet, after having passed through the stages of solar shell and ring, and finally secondary sphere, still retains the capabilities of further contraction. Following the example of the central body it develops a succession of envelopes which become its attendant satellites. The uniformity in this process as applied to the sun and the planets should likewise be extended to the stars, and it therefore is a law of universal operation.

The explanation of the origin of vortex rings proceeded upon the lines that the elliptical nucleus was surrounded by a spherical shell in which was produced an acceleration of the equatorial regions by the residual forces of the continuous bombardment. As the condensation of these materials increases, the figure of the spherical shell changes to elliptical, since the gaseous constitution tends towards greater densities, while the polar regions will be spirally drawn towards the equator and finally be laid bare under the tangential stresses.

At this stage the acceleration produced by the nucleus upon the ring will proceed rapidly, while local concentrations enable the ring to be drawn by the principal point of accumulation into a secondary sphere. This center of mass receives a motion of rotation either direct or retrograde, from the moment of indifferent equilibrium, according as the following portion of the ring advances over and the preceding part is retarded under its center of gravity, or conversely. In the case of Neptune, when the general motion of the ring was very slow, it seems to have coiled up in a retrograde spiral, but from some minimum of orbital velocity the formation has been uniformly direct. It should be considered that the motion of the central mass at first was very uncertain, and it is not inconceivable that there was an oscillation in it before its first rotation took place.

Some of the planetary problems now receive an obvious solution. The exterior group of planets are of great mass because the volume of their respective envelopes is in proportion to the law of the radii. Their density depends upon the material originally composing them and it must have been quite gaseous. They have each gone through the process of forming several successive envelopes, Neptune one, Uranus four, Saturn seven, Jupiter four. The surface belts of Jupiter and Saturn each indicate such typical envelope phenomena, as equatorial acceleration, unequal diameters, and the varying aspects due to adjustment of the effects of radiation, cooling and gravity. The great red spot on Jupiter and the spots of considerable duration often compared to the sun-spots are therefore signs of processes in the envelope now forming. Whether another satellite can be cast off depends upon the degree to which condensation has already gone. The rings of Saturn look like the theoretical section of an unbroken envelope when the density layers are exposed. The observation that the principal ring is thicker, as shown by the bend in the shadow at that place, is also quite probable if they were formed by the evolution of a proper envelope. The present appearance of equilibrium may or may not be an anomaly of the natural process. The asteroids present another peculiar type of development wherein a series of envelopes and a number of local condensations indicate their probable origin. The inner group of planets are of greater density than the outer, since a long continued expenditure of material from the sun would leave only heavy matter for their constitution. The asteroids therefore seem to mark a decided increase in the density of the remaining matter left for envelope formations. The cooling of the planets proceeds more rapidly as solid particles preponderate in the mass considered. It should be observed that the friction of such solids in gaseous suspension generates the force of electrical repulsion which we perceive has reached an enormous development in the sun's photosphere.

The envelopes during their transformations will pass through many irregular forms and when they are regarded as screens upon the central luminary, it is apparent that the emission of light and heat to distant parts of the system will be controlled by their temporary condition. If the envelope is transparent a full transmission of solar energy takes place, but on being cooled absorption is greatly increased. Hence a long secular thermal variation occurs which will cause cosmical changes of a corresponding nature in the planets dependent upon the sun. They have gone through a series of fluctuations to be counted by the number of envelopes produced after themselves. The Earth has thus endured two cycles of diminished heat and light, while Venus and Mercury in turn surrounded the sun as envelopes. At the epoch of the Venus-shell the Earth itself was enveloped by the Moon and was thereby screened from the influence of solar changes, its own internal energies of heat being also vigorous. The rupture of the Venus and Moon envelopes occurring somewhat simultaneously would again expose the Earth, by that time much cooled, to the full radiant heat of the sun. The production of the Mercury envelope gradually reduced this heat to the amount remaining during the geological Glacial Period, but the disruption of it allowed the quick recovery of the maximum at the Earth, thus restoring the warmer climatic conditions existing after the ice retreated to the poles. The cooling of the photosphere, which is not relatively far advanced, points to a similar period of glaciation to come, only more severe and fatal by reason of continuous development in the same direction of expenditure.

Finally the phenomena of the variability of the stars are seen to depend upon the same cause. The stars are necessarily going through these processes of envelope development, exhibiting a secular waning of light, so that Gould's surmise of a general variability of the stars is well founded. During the time of the disruption of envelopes the rotation about the central body of shells more or less broken will produce the quick maxima and minima changes of light observed to exist in individual stars. It may be possible, by close examination of the sequence of intermediate magnitudes, to reconstruct the approximate figure of the rotating interposed screen. This subject opens up a wide field for conjecture in the study of details, but they can be verified by laboratory models of light effects, showing the velocity of revolution and the portions of the shell or ring broken away by its local attractions.

Racine College, March 21, 1887.

A METHOD OF DERIVING THE RIGHT ASCENSION AND DECLINATION OF A HEAVENLY BODY FROM SEXTANT OBSERVATIONS, AND ITS APPLICATION TO THE DETERMINATION OF TERRESTRIAL LONGITUDE.

SEVERINUS J. CORRIGAN, WASHINGTON, D. C.

For the Messenger.

The determination of the apparent position of a heavenly body, with the greatest possible precision, can be effected only by means of the complicated and expensive instruments of a fixed observatory. But, for some practical purposes, the utmost attainable precision is not a prime requisite; approximate, yet entirely satisfactory, results can be easily obtained by any one expert in the manipulation of that simple but very efficient and comparatively inexpensive instrument the sextant.

This fact has led me to devise a method of computing, from sextant observations, the right ascension and the declination of any celestial body visible to the naked eye, or through the small telescopes which usually accompany the sextant.

This method, while it is sufficiently accurate, is extremely simple and can be readily comprehended by any one at all familiar with the most ordinary processes of spherical trigonometry and their application to astronomy, *i. e.*, to the solution of the triangles formed upon the celestial sphere by the coördinates and the angular distances apart, of any two heavenly bodies.

The principal observational data which are employed in the computation are the angular distances of the body, whose coördinates are to be determined, from any two prominent fixed stars whose right ascensions include that of the body. This condition that the right ascension of the body should lie between that of the stars is not an essential one, but is imposed simply as a matter of convenience in regard to the sign of a certain trigonometrical function as will hereafter be shown.

The observed distances are not the true ones, because they are affected by refraction which operates so as to render the observed distances always less than the true ones. Representing the difference in arc by q, or a correction to the observed distance, its value can be determined, in any case, through the solution of two triangles having a common angle Q, or the difference of the azimuths of the two bodies, and the sides z', Z'; z, Z; and d', d, the first two quantities being the observed zenith distances of the two bodies ; the second two their zenith distances corrected for refraction, while d' and d represent respectively the observed and the true angular distances of the bodies from each other. As q is always small its value can be accurately determined by means of the following equation :

$$\sin q \sin d' = \cos Q\,(\cos h' \cos H' - \cos h \cos H) + (\sin h' \sin H' - \sin h \sin H) \qquad (1)$$

in which the quantities h and H are the altitudes corresponding to z and Z, respectively. Since for each value of h' and of H' there is a corresponding value of h and of H, it is obvious that the second member of equation (1) can be easily tabulated in two parts, the arguments for both being h' and H'. Then, for any values of these arguments, we have only to take, from the first part of the table, the proper quantity, multiply it by cos Q, add to the result the quantity taken from the second part of the table, and multiply the whole by cosec d'. The resulting quantity will be the sine of the correction to be added to the observed distance in order to obtain the true one. The coefficient cos Q can be determined from the same triangles in the following manner: Let $\Sigma = \frac{1}{2}(z' + Z' + d')$ and $\sigma = \Sigma - d'$, then from the equation

$$\cos \tfrac{1}{2} Q = \sqrt{(\sin \Sigma \sin \sigma . \operatorname{cosec} z' \operatorname{cosec} Z')} \qquad (2)$$

Q can be obtained.

The second member of this equation can be tabulated in two parts, one for $\sqrt{(\sin \Sigma \sin \sigma)}$ and the other for $\sqrt{(\operatorname{cosec} z' \operatorname{cosec} Z')}$. The arguments for the first part will be $(z'+Z')$ and d'; for the second part z' and d'; or instead of the zenith distances z' and Z', it may be better to use the corresponding altitudes h' and H'.

Then all that is necessary in order to find the value of Q is to take, for any values of the arguments h', H' and d', the proper logarithm from each part of the table; the sum of these logarithms will be the log cos $\frac{1}{2}$ Q. It is to be remarked that into the above formulæ only the *mean* refraction enters; the corrections for the height of the barometer and of the thermometer being neglected.

Since these corrections are, in most cases, very small and as they will be greatly reduced in the subsequent process of computation, the omission of them will not, generally, materially affect the final result; but should it be necessary to regard it, x or the second refraction correction to the observed distance can be obtained from the equation

$$x = (q \div r)\, c \qquad (3)$$

in which q has the value given by equation (1), r is the difference between the mean refraction of the two bodies, and c the difference between the combined barometrical and thermometrical corrections to these mean refractions. If desired, x can be tabulated for values of q, r and c, which quantities are to be expressed in seconds of arc, and r and c are to be obtained from the ordinary refraction tables. For the tables to be constructed by means of equations (1), (2) and (3), four place logarithms are more than sufficient, and in taking out quantities from these tables, for any arguments, only first differences need be used in the interpolation.

A third correction to the observed distance is necessary when the body observed is the moon or a planet, this correction being simply the apparent semi-diameter of the body, which is to be applied to the distance in the usual way, additive if the limb observed be the nearer and *vice versa.*

The computed data required are the right ascension and polar distance of any two selected stars. These coördinates, for all prominent stars, can be taken from the ephemeris for any given time. The process is then as follows: Select any two stars, one of them to the west and the other to the east of the body. Designate, in any case, the more westerly of the two as first star, and the other as second star. Let a represent the polar distance of the former, b that of the latter, and D the included angle at the pole, or the difference of right ascension between the stars. These polar distances and the angular distance between the stars form a triangle in which it is necessary to find this angular distance which we will call $\varDelta$, and the angle A included by a and $\varDelta$. This can be done by means of the equations:

$$\left.\begin{aligned} \tan c &= \cos D \tan a \\ c' &= b - c \text{ (either being greater)} \\ \cos \varDelta &= \cos a \cos c' \sec c \end{aligned}\right\} \qquad (4)$$

By putting $S' = \frac{1}{2}(a + b + \varDelta)$, and $s' = S' - b$, we have, from which to find A, the equation,

$$\cos \tfrac{1}{2} A = \sqrt{(\sin S' \sin s' \operatorname{cosec} a \operatorname{cosec} \varDelta)} \qquad (5)$$

Then in the triangle formed by d_1 or the corrected distance of the body from the first star, d_2 or its corrected distance from the second star, and $\varDelta$, we have to find the angle B included by $\varDelta$ and d_1. It is given by the equation,

$$\cos \tfrac{1}{2} B = \pm \sqrt{(\sin S'' \sin s'' \operatorname{cosec} \varDelta \operatorname{cosec} d_1)} \qquad (6)$$

in which $S'' = \frac{1}{2}(\varDelta + d_1 + d_2)$, and $s'' = S'' - d_2$.

It is to avoid the use of the lower sign in this equation, that the condition was imposed that the right ascensions of the stars should include that of the body. But in some cases it may be advisable to take both stars on one side. In this case the lower sign would have to be used and the angle would be the supplement of $\frac{1}{2} B$. We have now a triangle in which the known quantities are the sides a and d_1 and the included angle $P = (A \pm B)$, the lower sign to be used when the polar distance of the body is less than b. But the comparison stars can be so selected that only the upper sign need be used.

It will be seen that P is the angle of position of the body with reference to the first star, and that this method is analogous to the differential method employed in connection with the equatorial, in which the distance is measured by the filar micrometer and the position micrometer measures the angle P.

The third side of the triangle referred to above is unknown, and is one of the required coördinates of the body, viz., the apparent polar distance.

Calling it p, we have for finding its value the equations,

$$\left.\begin{aligned} \tan e &= \cos P \tan a \\ e' &= e - d_1 \text{ (either being greater)} \\ \cos p &= \cos a \cos e' \sec e \end{aligned}\right\} \qquad (7)$$

In the same triangle the angle D_1 at the pole, included by a and p, is the difference of right ascension between first star and the body. Its value can be obtained from the equation,

$$\sin D_1 = \sin P \sin d_1 \operatorname{cosec} p. \qquad (8)$$

Adding D_1 to the apparent right ascension of first star, we obtain the apparent right ascension of the body. Thus, by means of the above equations, the apparent right ascension and declination of a celestial body can be determined. As a check, we can compute D_2, or the angle at the pole included by p and b, from the equation,

$$\cos \tfrac{1}{2} D_2 = \sqrt{(\sin S''' \sin s''' \operatorname{cosec} b \operatorname{cosec} p)} \qquad (9)$$

in which $S''' = \frac{1}{2}(b + d_2 + p)$, and $s''' = S''' - d_2$.

The value of D_2 derived therefrom is the difference of right ascension between the body and the second star, and if we *subtract* it from the latter it will give the right ascension of the body, and this will agree with that derived from the first star if the computation has been accurately made.

When p is great this check will not be very rigorous. The most accurate way will be to compute D_2 directly from b and d_2, as D_1 is computed from a and d_1.

It is obvious that in the method above proposed we have a means of determining terrestrial longitude; for, since it is applicable to all celestial bodies visible to the naked eye, it enables one to determine from sextant observations the apparent right ascension of the moon, whence, by means of the proper tabular reductions, the geocentric right ascension of that body can be deduced.

Then, by comparing this with the values given in the ephemeris for each hour, we can obtain the Greenwich mean time of the observation, using for this purpose the "difference of right ascension for one minute," which is given in connection with the hourly ephemeris. In view of the fact that the method of Lunar Distances for the determination of longitude at sea has fallen into comparative desuetude, and as the chronometer furnishes an easy and generally accurate means of obtaining a knowledge of the longitude, it may seem superfluous to propose this new method; but, since cases may occur in which other than a chronometrical determination is advisable or even necessary, and since the Lunar Distances still occupy a prominent place in the Nautical Almanac, the presentation of this method may be of interest, particularly as it possesses some points of superiority over the ordinary Lunar Distance method.

The first and probably the greatest is that while lunar distances must be computed for each and every year, the method here proposed suffices, with the assistance of simple and easily constructed tables, for an indefinite number of years before or after any epoch that may be chosen, and the only quantities that are to be taken from the ephemeris are already given therein for other purposes. Second, the required observations are fewer and more easily taken than those for the ordinary method, an interval of even an hour being admissible for the purpose of making them. Third, the process of computation tends to greatly reduce instrumental or observational errors, by reason of a smaller quantity being deduced from a larger, and on account of the fact that the observed distances will lie opposite sides of the moon. Fourth, it does not require any knowledge whatever of the Greenwich time, or of the longitude, and only an approximate knowledge of the latitude, and is thus, in one sense, an absolute method.

This method is as short and easy as that of the ordinary Lunar Distance. The process is about the same as that just given, differing mainly in this, that instead of the star places being the apparent ones for the time of the observation, they are the mean places for any chosen epoch.

Thus the right ascensions and the polar distances and the trigonometrical functions thereof, which are required in the subsequent computation, and also the quantities computed from equation (4), can be regarded as constants and tabulated for each set of stars that may be selected. We can have as many sets of stars and their corresponding tables of constants as may be necessary to determine the right ascension of the moon at any time during the period of its visibility.

The following is the form of the table referred to, arranged for a set of stars which I intend to use in an example illustrative of the method:

TABLE IV.

EPOCH 1885.0.

First Star (Fomalhant).		Second Star (Achernar).	
	h m sec		h m sec
R. A.	22 51 17.66	R. A.	1 33 25.53
a	59° 46′ 06.7″	b	32° 10′ 43.4″
Log a	0.234518	Log b	9.798799
Log a'	9.701995	Log b'	9.927571
Log a''	9.9365	Log b''	9.7264
A_1	33° 16′ 10″	A_2	117° 07′ 44″
Log m_1	8.3820	Log m_2	7.4472
Δ	39° 06′ 59″		
Log Δ	0.200041		
D	41°		
Log n	9.6757		

In the above table, a and b are the polar distances of first and second stars respectively. Logs a, a', a'', are the logarithms, respectively, of the tangent, cosine and sine of a. Logs b, b', and b'', are the same functions of b. Log m is the logarithm of the proper motion in right ascension of each star, expressed in seconds of time. Δ is the angular distance between the stars, and A is the angle included by a and Δ. Log Δ is the cosec of Δ. D is the difference of right ascension between the stars, and log n is the value of log sin A sin a. The two last quantities, D and log n, are to be used only in cases where there is but one observer.

When the observed distances have been corrected for refraction, by the application of quantities taken from tables constructed by means of equations (1), (2) and (3), and also for semi-diameter, the direct computation of D_1 or the difference of right ascension between the moon and the first star can be made by means of equations (5), (6), (7) and (8). Equation (7) will give the apparent polar distance or the declination of the moon, and equation (8) the value of D_1, hence the apparent right ascension of the moon referred to the mean equinox of the epoch but uncorrected for the star's aberration.

The moon's place must be reduced to the equinox of the time of observation, by means of precession and nutation tables constructed with the moon's right ascension and declination as arguments. The values taken from the precession table are to be multiplied by T or the time from the epoch expressed in years and fractional parts thereof. If the time of observation be after the epoch, T will be positive, but if before, it will be negative. The nutation is to be tabulated in two parts, one of which is to be multiplied by the sine of the mean longitude of the moon's ascending node, or ☊, and the other by the cosine of the same quantity. The second part of the nutation will be, generally, very small and in most cases can be neglected. The value of ☊ can be taken from the ephemeris for any given date, or it can be derived from the equation,

$$☊ = 92.6^\circ - T.\ 19.3^\circ$$

in which T is the time from 1890.0.

The right ascension of the first star must receive a correction for aberration, which can be tabulated with the day of the year as the argument, and intervals of half a month are small enough for an accurate interpolation using only first differences.

When these corrections have been applied, we will have the apparent right ascension of the moon for the time of observation. The parallax in right ascension must then be applied in order to obtain the geocentric right ascension for comparison with the hourly ephemeris of the moon.

The value of this parallax, in seconds of time, can be obtained from the equation,

$$\varDelta a = \frac{(\rho \cos \varphi' \sec \delta) \sin \pi \sin t'}{15 \sin 1''} \qquad (11)$$

in which φ' is the geocentric latitude of the place, ρ the corresponding radius of the earth, π the moon's equatorial horizontal parallax, and t' its apparent hour angle. Since for each value of φ or the geographical latitude there is a corresponding value of φ' and of ρ, it is evident that the second member of equation (11) can be tabulated for values of φ and of δ, and for given values of π. If we take π equal to 54′, 58′ and 62′, and form three tables, one for each value of π, and with the arguments φ and δ, the value of the parallax factor can be readily interpolated therefrom.

The value taken from the table is then to be multiplied by sin t' or the moon's apparent angle, which is computed from the equation,

$$\sin \tfrac{1}{2}\, t' = \sqrt{(\cos S \sin s \sec \varphi \operatorname{cosec} p)} \qquad (12)$$

in which $S = \frac{1}{2}\,(\varphi + p + H)$, and $s = S - H$. The value of t' need be only approximate and four place logarithms are sufficient for an accurate computation.

Finally a correction for the proper motion of the first star is necessary when that quantity is considerable, or when the time of observation is so far from the epoch as to produce a sensible correction.

As I propose to illustrate this method by a practical example, a reference to the Tables becomes necessary, but since it is impracticable, in this connection, to set forth these Tables at length, a recapitulation of them in their proper order must suffice. Reference will be made in the examples only to the number of the Table.

Table I (Parts 1 and 2).—For the determination of cos Q, the coefficient for first part of Table II. Arguments for first part are $(h' + H')$ and d'. For second part, h' and H'. (See equation 2.)

Table II (Parts 1 and 2).—For the determination of first refraction correction to the observed distance. Arguments for both parts are h' and H'. (See equation 1.)

Table III.—For finding the second refraction correction, due to the height of the barometer and the thermometer. (See equation 3.)

Table IV.—Constants for each set of comparison stars.

Table V.—Factor of parallax in R. A. to be multiplied by sine of t', or moon's apparent hour angle. Arguments, latitude and moon's declination. (See equation 11.)

Table VI.—For precession correction. Arguments, moon's R. A. and Dec.

Table VII.—For nutation correction, in two parts, both with arguments, moon's R. A. and Dec. First part to be multiplied by sin ☊, and second by cos ☊.

Table VIII.—Correction for aberration. Argument, day of the year.

An important point in favor of this method is its practicability, or the facility with which the observations can be made. The two distances d_1' and d_2' are the only quantities that need be determined with considerable accuracy, and even in them an error of 20″ will not materially affect the resulting longitude.

The moon's altitude will in most cases be sufficiently exact if its error be under 5′, while the altitudes of the two stars need be only roughly observed; it will suffice if the observed values are within a degree of the true ones. With two observers the measurements can be easily made, the distances being observed simultaneously, and the local mean time carefully noted; then the moon's altitude is taken and also the time. As the moon's hour angle which is to be computed from this altitude is for the time of the observation of the distance we must apply to the hour angle a correction due to the interval between the times of the two observations. This correction is simply the interval itself, converted into arc at the rate of 14.5′ to one minute of time.

The altitudes of the stars are then observed or they may be taken *before* the other measurements. If the stars are west of the meridian they should be observed first, and *vice versâ*.

It will probably happen in the majority of cases that there is but one observer, yet, even in this event, accurate results can be easily obtained by a device of computation. Plenty of time can be taken by the observer for his observations, an interval of even an hour being admissible.

He begins by taking the distance d_1'; then the moon's altitude is observed and then the distance d_2'; the local mean time of each of these observations being carefully noted. The two star altitudes can be taken before or after, as may be most convenient.

On account of the interval between the observation of the distances the resulting longitude must receive a correction Δl, which is expressed by the equation,

$$\Delta l = \pm i (1 - k n) \qquad (13)$$

in which i is the interval between the observations of the distances expressed in seconds; $k = d_1 \div (d_1 + d_2)$; n represents sin A sin a, these quantities being constants and placed in Table IV, as already stated. The upper sign is to be used when d_2 is increasing during the interval, and *vice versa.*

In order to determine which sign is to be used we must have a criterion, and one is readily obtained in the following manner: Let $D_2 = D - D_1$, in which D is the difference of the right ascensions of the two stars and is placed as a constant in Table IV, and D_1 is the value already derived in the computation from equation (8).

$$\text{Then } \cos Y = \sin D_2 \operatorname{cosec} d_2 \sin b \qquad (14)$$

sin b being constant and taken from Table IV. Then in the equation $X = I + Y$ (c), I is the inclination of the apparent path of the moon and is determined by the equation $\tan I = \frac{Diff.\ of\ Dec.}{Diff.\ of\ R.\ A.}$ these differences being given in the hourly ephemeris for one minute. I will be positive if the moon be moving northward, otherwise negative. If in equation (c), X be greater than 90°, d_2 will be increasing and the upper sign in equation (13) is to be used, but when X is less than 90°, d_2 will be decreasing and the correction Δl will be negative. Since I lies between 0° and 29°, it will suffice to take 15° as its mean value, except in cases where X is very nearly 90°.

We are now prepared to present examples illustrative of this method. Two will be given, one representing a case where the observations are taken simultaneously and the date is only *3 years* from the epoch, while the second example will treat of

Nat number	+.000038
Table II (Part 2)	+.000050
	+.000088
Log	5.9445
Log cosec d_1'	0.2702
Log sin q	6.2147
d_1' =	32° 27′ 05″
1st cor. for ref. = q =	34
2nd " " " =	00
d_1 =	32 27 39

" " ½t'	8.8234
t' —	7° 40′
π =	0° 59′ 10″
☊ =	150 10 00
Epoch	1890.0
Date of observation	1887.0
T = —	3.0
Log coeff. for precession and proper motion = log T =	— 0.4771
Log coeff. for par. in R. A. = log sin t' =	— 9.1252
Log coeff. for nutation = log sin ☊ =	9.6968
Log coeff. for nutation = log cos ☊ =	— 9.9383

Nat number	— .000052
Table II (Part 2)	+ .000076
	+ .000024
Log	5.3802
Log cosec d_2'	0.5304
Log sin q	5.9106
d_2' =	17° 09′ 06″
1st cor. for ref. = q =	17
2nd " " " =	00
d_2 =	17 09 23

COMPUTATION OF CORRECTIONS TO MOON'S R. A.

Parallax.	Precession.	Nutation (1st part).	Nutation (2nd part).	Aberration.	Proper Motion.
				Sec.	
Tab. V, log 2.2921	Tab.VI, log 0.5494	T. VII, log — 9.9507	T VII, log 8.3502	T.VIII, + 1.7	T. IV, log ∞
Log sin t' — 9.1252	log T — 0.4771	log sin ☊ 9.6968	log cos ☊ — 9.9383		log T ∞
— 1.4173	— 1.0265	— 9.6475	— 8.2885		∞
Nat number — 26.1 sec.	— 10.6 sec.	— 0.5 sec.	0.0 sec.	+ 1.7 sec.	

Sum of Correction, — 35.5 sec.

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COMPUTATION OF MOON'S R. A.

Table IV, Δ	34° 15′ 22″	Log Δ	0.24962	Log cos P	—9 87819	Log sin P	9.81641
d_1	32 27 39	" cosec	0.27325	Table IV, log a	9.98658	" " d_1	9.72975
d_2	17 09 23			Log tan c	—9.86477	" cosec p	0.02401
	83 52 24					" sin D_1	9.57017
S''	41 56 12	" sin	9.82498	c^1	143° 46′ 45″		
d_2	17 09 23			d_1	32 27 39	D_1	21° 49′ 10″
s''	24 46 49	" sin	9.62232	c'	111 19 06	(in time)	1*h* 27*m* 15.7*s*
			2)19 96717			Tab. IV, R. A.	5 08 33.8
		Log cos ½ B	9.98358	Log cos c'	—9.56056		6 35 50.5
				" sec c	—0.09326	Sum of cor.	—35.5
		B	31° 18′ 46″	Tab IV, log a'	9.85609	Moon's R.A.	6 35 15.0
		Table IV A	107 44 56	Log cos p	9.50991		
		P	139 03 42				
				p	71° 07′ 25″		

	h m sec		*h m sec*	
Ephemeris value of Moon's R. A.	6 35 15.0	at	16 00 00	G. M. T.
Computed " "	6 35 15.0	corr	0 00 00	
Diff.	0 00 00.0		16 00 00	G. M. T.
Local M. T.			10 51 48	
Longitude			5 08 12	West

a case in which the observations are taken at different times, the date of observation is nearly *30 years* from the epoch, and the refraction corrections are large. Thus the method is illustrated in all of its phases. The second example is based upon one given in Bowditch's Navigator, revised edition, page 139, for the Lunar Distance method. In this case there was an observation of d_1 only, and d_2' is a computed quantity deduced for the time of observation and the longitude, and therefore represents a correct observation.

In both cases the longitude comes out exact.

It will be seen that some of the work could be abridged. In some cases the refraction could be neglected. The second nutation correction in both examples given above could be disregarded and in most cases no attention need be paid to proper motion. *All* the corrections have been given so as to illustrate the *modus operandi* in any possible case.

On account of the interval between the observations of the distances, in the first example, the value of the moon's apparent polar distance will differ from the value for the time of observation of the altitude, and therefore will not give the proper value of t', or the hour angle. In general the difference will be too small to be regarded; but in some cases it may produce an appreciable effect. This can be avoided by computing the hour angle from the equation $t' = \theta - a'$, in which θ is the sidereal time of the first observation of distance and a' is the moon's apparent right ascension. The computation is carried on as before; the value of D_1 and the moon's right ascension (uncorrected) are obtained; then all the corrections to the latter, *except* that for parallax are applied. We will then have the value of a'. The sidereal time is derived in the usual manner, and then the value of t' by the above equation. Using this we obtain the moon's geocentric right ascension by the same process as in the first example. This method of obtaining t' is perhaps easier and better, even when the observations are simultaneous, than that of computing it from the moon's altitude, polar distance and the latitude. The computer can use either.

If we use this second method of determining the hour angle, the moon's altitude need be only very roughly observed, since, like the altitudes of the two stars, it will be used only for the refraction tables. The estimated center of the moon can be brought down to the horizon so that no correction for semi-diameter will be necessary. The other corrections may also be neglected.

This method is therefore shorter and easier than any in use, and the advantages claimed for it will be apparent to any one who will give it a close examination.

In the second example the only variable quantities that enter into the computation of D_1, being the distances d_1 and d_2, it is evident that the value of D_1, and therefore of the moon's apparent right ascension, can be tabulated for any values of d_1 and d_2, for each set of stars. Thus the right ascension can be taken directly from the table, doing away with the direct computation, and greatly reducing the labor.

This tabulation will also serve for the purpose of identifying the comparison stars; for the observer has only to take the approximate right ascension of the moon from the ephemeris and this will indicate the approximate values of d_1 and d_2 in the table. Then if he sets the index of the sextant at any one of these values, brings the reflected image of the moon into the field and revolves the instrument, the line of sight will pass near to the star. As the stars will generally be of not less than the second magnitude and in approximately known direction from the moon, no trouble will be found in picking up the right one even when the observer is not well acquainted with the geography of the heavens.

WORK OF THEODOR VON OPPOLZER.

Professor Theodor von Oppolzer, the only son of the celebrated physician Dr. Johann von Oppolzer, was born at Prague, October 26, 1841. His early years were spent at Leipsic, whence, while yet in his childhood, he moved to Vienna, as his father had been appointed professor at the university of that city. Here he studied physics, but his abilities and great predilection for mathematics induced him to devote all his spare time to the study of natural philosophy. In these studies he was aided by Dr. E. Weiss, the present Director of the Vienna Observatory, who soon recognized his eminent talents for Astronomy, and encouraged him to devote his life to that science. Oppolzer, who was possessed of independent means, sufficient to remove all necessity for seeking immediate employment, therefore constructed a well-appointed observatory after he had taken the degree of a Doctor of Physics. At this observatory he observed asteroids and comets during the years 1862 to 1872 with great zeal, and his computations of the orbits of such bodies were the first papers by which he introduced himself to the astronomical world. For such work he was especially qualified by the quickness and certainty with which he conducted extended numerical computations. He published numerous papers on this subject.

He also published all his valuable theoretical researches in this branch of Astronomy in an excellent "Lehrbuch zur Bahnbestimmung der Kometen und Planeten," of which the first volume appeared 1870; and in a second, totally revised and much enlarged edition, 1882. The second volume was published in 1880. In this classic work the author has in fact reconstructed on a more comprehensive plan the *Theoria Motus* of Gauss. The theories upon which the calculation of the orbits of comets and planets depend are discussed so fully and exhaustively, that the work forms, indeed, a Treatise on Astronomy, " not only useful to the practical Astronomer, but of the highest value to the Mathematician who desires to know the exact manner in which the dynamical formulæ he is acquainted with are actually applied in practice to the calculation of orbits." This most important work has been recently translated into French by Prof. Ernest Pasquier, of Louvain.

In the year 1873 Prof. Oppolzer was entrusted by the Austrian government with the astronomical work for the *Europaische Gradmessung*. In this position, which he held till his death, being in his last years chairman of the Austrian Commission of the *Europaische Gradmessung*, he showed an extraordinary ability as an organizer, and executed a long series of telegraphic longitude operations between various cities of Austria amongst each other, and with the capitals of other countries. For many weeks in the summer of 1876 operations were undertaken at the Royal Observatory, Greenwich, by Professor Oppolzer's assistants for the purpose of making the time observations and telegraphic observations necessary for determining the longitudes of Vienna and Berlin, connected also in some measure with Munich and Paris. All the observations for this very extensive work are finished, but unfortunately the reductions are not very far advanced. In connection with these investigations he also conducted a series of pendulum experiments for determining the intensity of gravity at Vienna, and he made a very interesting and important study of the vibrations of the stand of the pendulum and their influence on the time of its oscillation. But here also his very premature death prevented him from discussing more than a small part of these extremely delicate researches, for which he devised special methods of observation and reduction.

In the year 1881 Oppolzer published "Syzygien-Tafeln für den Mond," which were intended to supply a simple and convenient means of finding very approximately the time of New and Full Moon, particularly in the case of an ecliptic syzygy, and of calculating all the circumstances of an eclipse without the necessity of having recourse to the Solar and Lunar Tables.

But the most important work undertaken by Oppolzer is his "Canon der Finsternisse" [Table of eclipses], which will be found to be an invaluable basis for all historical researches connected with solar eclipses, and will form an enduring monument of the power of its author.

This masterly paper contains not only the elements of eclipses of the Sun, but also of those of the Moon from 1207 B. C. to 2162 A. D. The elements are given for all the eclipses partial and central occurring during this period of time.

The tables furnish the computations of 8,000 Solar Eclipses, of which about 2,220 are total, 355 annular and total, 2,605 annular, and 2,820 partial.

Of the Moon, 5,200 eclipses are computed; for which the tables give not only the date, but also the magnitude, the time of the middle of the eclipse, and the geographical position of the point on the earth where the Moon at that moment is at the zenith.

Appended to the work are 160 charts, in which is laid down the central line of all Solar Eclipses visible in the northern hemisphere, and as far as 30° south latitude. Partial eclipses of the Sun, and those which are central only in regions more south than 30° are not mapped.

The work forms vol. lii. of the *Memoirs of the Imperial Academy of Sciences* at Vienna.

This noble undertaking Oppolzer was only just able to bring to a conclusion, for it was but a few hours before his death that he read the last proof-sheets of it.

Besides all this, Oppolzer was engaged during the last years of his life in researches on planetary disturbances, especially on the theory of the motion of the Moon, on astronomical refractions, and on the resisting medium, &c.

From this brief record of his more important researches it will readily be understood that his life was one of constant work, and that he was enabled to perform all he did only by an indefatigable diligence, a very remarkable power of memory, quickness of perception, and intuitive grasp of whatever he studied.

Von Oppolzer was appointed in 1876 Professor of Astronomy at the University of Vienna, and received besides this, various other distinctions from the Austrian Government and several foreign sovereigns. He was also Member of the Imperial Academy of Sciences at Vienna, Honorary Member of the Royal Academy at Munich, Correspondent of the Institute of France, and Fellow of many other scientific bodies. He was elected an Associate of the Royal Astronomical Society on Jan. 9, 1874.

In the middle of November 1886 Oppolzer returned to

Vienna somewhat ill, after attending the Paris International Conference on Weights and Measures, and the meeting of the Permanent Commission of the *Europaische Gradmessung* at Berlin. No one would have considered, however, from his appearance, that his end was so near. At first his illness was thought to be a kind of malaria, but it very soon proved a serious disease of the heart, of which he suddenly died, on the morning of December 26, at the early age of 45 years.—*E. W. in "Monthly Notices."*

EDITORIAL NOTES.

Space will be given hereafter in the MESSENGER, regularly for the publication of the titles, prices and the names of publishers of standard and useful books on Astronomy. Good books for sale, or books wanted will be included. The charge for such notices will be 5 cents per nonpareil line for each issue. Subscribers are offered one line monthly free.

The Holden-Proctor Unpleasantness.—It is reported that President Holden of the California State University and Director of the Lick Observatory, published an article in the San Francisco *Examiner*, March 1, in which he said something that greatly displeased Professor R. A. Proctor who was in Florida at the time. Seeing the article Mr. Proctor replied through the same paper (March 27) condemning Professor Holden's treatment of him in very severe language, and implying that the animus of the attack was due to the detection of a gross literary offense therein named, and besides, woefully berating some of Professor Holden's astronomical work while in connection with the U. S. N. Observatory at Washington. We have not seen the articles referred to in the *Atlantic Monthly*, nor that of March 1 in the San Francisco *Examiner*, and do not personally know of the merits of these accusations. If true and fairly presented, Mr. Proctor is right, in just severity and dignified silence both of which he has chosen to use.

Professor Hough's Catalogue of 209 New Double Stars.—It was an agreeable surprise to find the *Astronomische Nach-*

richten Nos. 2778–79 wholly given to the publication of Professor Hough's catalogue of two hundred and nine new double stars. Observers giving attention to this kind of work well know how thoroughly the northern heavens have been explored and that the easy doubles remaining uncatalogued are comparatively few.

The following synopsis in reference to distance of components is interesting:

Distance.	Stars.
0." to 0.5"	25
0.5 " 1.0	25
1.0 " 2.0	43
2.0 " 5.0	77
over 5.0	39
Total	209

The Astronomical Theory of the Ice Age.—I am glad to see the attention of astronomers again called to this subject. I am, however, unable to acquiesce in the criticisms of Professor McFarland and ask your permission to make a short reply.

First. I did not imagine that I could dispose of the elaborate theory of Dr. Croll in two pages of your journal. I thought Sir Robert Ball had been induced to give his adhesion to that theory on insufficient grounds and as Sir Robert's name is likely to carry a good deal of weight with astronomical readers I wrote to point out the defect of his argument.

Second. I think Professor McFarland mistakes the correspondence between the summer heat and the winter cold at the period of greatest eccentricity. The earth receives on the whole the same (or rather a slightly greater) amount of heat from the sun in the course of the year. Consequently if the summer is shorter than the winter its heat is *more* above the average than the winter cold is below it; and the excess of summer heat above winter cold is greater the more the summer is shortened and the winter lengthened. The shorter summer brings with it no diminution of the total heat.

Thirdly. Though we have no means of producing a sufficiently intense heat on the earth to render the melting of snow or ice absolutely instantaneous, time as such does not enter into the problem of melting at all. What is required to change a certain quantity of ice or snow at a given temperature into water at a given temperature is a certain quantity of heat, and (if we could exclude interference from other sources of heat or cold) it is perfectly indifferent whether this quantity of heat is applied to it in an hour or in twenty years. As to the effects of intense heat in removing a snow-cap, I believe snow-caps on volcanoes have sometimes been melted in a single night. Time as such is not a factor in the melting.

One fact cited by Prof. McFarland tells, I think, against himself. If intense cold can in a day or two form an ice-cap which long exposure to the oblique rays of the sun fails to melt, it would seem that the great agent in forming snow or ice-caps is not duration but intensity. Is it not natural to conclude that in melting them also intensity, not duration, is the great element to be looked to? W. H. S. MONCK.

Dublin, Ireland, March 17, 1887.

Daylight Occultation of a Tauri, observed at the Chabot Observatory, Oakland, Cala., March 29th, 1887, F. M. Campbell, Director.—The *Disappearance* was observed with a power of 90 on the 8-inch Clark equatorial; nearly at middle of moon's limb at 3*h* 28*m* 59.6*s* by sid. chronometer; star bright and steady.

For the *Reappearance* a power of 200 diameters was used; the star flashed out sharply, near the expected spot, and apparently exactly on the bright edge at 4*h* 56*m* 49.8*s* by chron. The time carefully determined on the evening of the 30th (29th cloudy), and kept by standard sidereal and mean time clocks, the rates of which agreed exactly in giving the chronometer used a correction for true local sidereal time of + 3*m* 16.3*s* for Dis., and + 3*m* 16.4*s* for Reap. We have, therefore:

Dis., "*a Tauri*": 3*h* 32*m* 15.9*s*, local sid. time.
Reap., "*a Tauri*": 5*h* 00*m* 06.2*s*.

Position of Observatory: Lat. = + 37° 48′ 05″; Long. = 8*h*

09*m* 06.3*s* W. All the observations and reductions have been made by myself. CHAS. B. HILL, ASSISTANT.

Occultation of Aldebaran.—This occultation occurred during daylight. At immersion the sky was clear, but the atmosphere was very unsteady. The instrument was the 6.4 inches equatorial, objective, by Clark & Sons. The star was quite red, and was visible in the finder of $1\frac{15}{16}$ inches.

The moon was five days old and the star disappeared instantaneously about 88° from the north point; but the ash grey limb of the moon was not visible.

At the reappearance from behind the bright limb the atmosphere was much disturbed, and a slight haze was gathering over the sky. The border of the moon was blurred and very unsteady, and the star did not reappear with the brightness expected, but its almost sparkling red color left no doubt whatever of the time to the nearest tenth of a second. The outermost limit of the moon's apparent disc was fuzzy and decreasing in brightness outward. The time which elapsed from the reappearance until the star left this factitious limb was two and a half or three seconds, when it appeared much brighter. The star emerged about 240° from the north point. The error of the chronometer was determined by transit observations during the evening; the rate is one-tenth of a second per diem and very regular.

RECAPITULATION.

Name of Star.	Mag.	Obsr.	Power.	Local Sidereal Time of Obsn.	Remarks.
				h *m* *s*	
a Tauri..	1	G. D.	90	3 31 18.8	Immersion; Instantaneous; good; star red.
a Tauri..	1	G. D.	250	4 59 09.2	Emersion; " " " "

Geographical position of the observatory: Latitude 37° 47′ 24.75″ north; Longitude 122° 25′ 40.54″ west.

Observer G. D. = George Davidson.

Davidson Observatory, San Francisco, March 29, 1887.

Astronomical Observations at Dresde (French).—The first part of the published observations of the observatory of B. d'Engelhardt at Dresde for the year 1886 are promptly before us, and find welcome place in the astronomical library.

The description of instruments, instrumental constants, geographical position of the observatory, observations of the moon, double stars, culmination of the moon, occultations, phenomena of the satellites of Jupiter, temporary stars in the nebula of Andromeda and X^1 Orionis, besides much of the ordinary work common to observatories like that of Dresde, is found in this large quarto volume of 220 pages. Four full page plates of the fine buildings, instruments and observing facilities are a neat and fitting supplement to the volume.

Intra-Mercurial Observations July 29, 1878.—After reading Professor Young's statement published in the March MESSENGER, that "the two objects seen by Swift were certainly not the two seen by Watson," etc., we were not fully satisfied with our own knowledge concerning the intra-Mercurial observations made at Denver, Colo., July 29, 1878, and accordingly asked Professor E. Colbert of Chicago, who was the chief of the observing party referred to, if we might examine the original papers in his possession concerning the observations made at Denver under his direction. He kindly assented, and we have before us as we write,—

1. The lithograph star-map of the neighborhood of the sun, at the time of the eclipse, prepared by Mr. Colbert for the use of his party,—the particular copy which he tacked to his own observing post.
2. Professor Watson's private letter to Mr. Colbert, Dec. 24, 1878, giving a pencil drawing of the position of the sun, and his two planetary objects as related to Theta and Zeta Cancri after reduction.
3. Professor Watson's private letter to Mr. Colbert, May 27, 1879, concerning Dr. Henry Draper's statements and Professor Swift's observations.
4. Professor Watson's brief private letter to Mr. Colbert, Feb. 23, 1880, about negative evidence of the existence of

intra-Mercurial planets given by a Washington astronomer and others.

5. A copy of Professor Colbert's letter to Dr. C. H. F. Peters concerning the observations.

6. Other papers, most of which have already appeared in print:

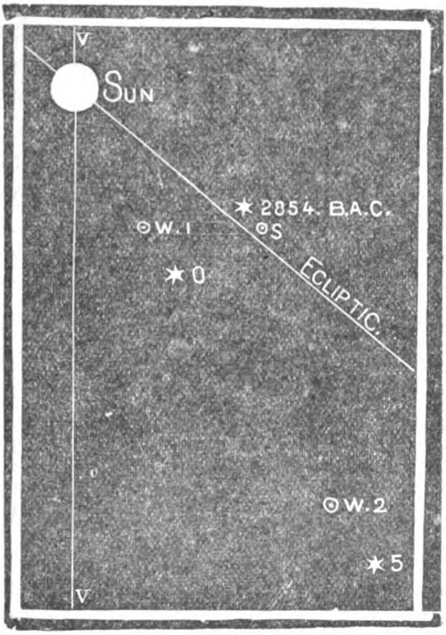
(1) Intra-Mercurial Planets by J. C. Watson, *American Journal of Science and Arts*, Oct. 1878.

(2) Report of the Solar Eclipse by the Chicago Astronomical Society.

(3) The Problematical Vulcan by Professor Watson as published in the Wisconsin *State Journal* under date of Feb. 18, 1880.

(4) Other papers with Prof. Colbert's statements.

From the maps above mentioned, and the correspondence explaining them the following cut has been made:



Little explanation is needed. *VV* represents a line perpendicular to the horizon at Denver at the time of the eclipse. W. 1, is Watson's planetary object first discovered, about 3° west of the sun and east of Theta Cancri. W. 2, is Watson's second planetary object, about 7° west of the sun and near Zeta Cancri (5) on the east side. The difference of declination of the sun and Theta at that time was less than ten minutes of arc ; that of the sun and Zeta more than half a degree. Professor Watson had less confidence in the position of the second object, although carefully read from the circles of his instrument as was also the position of the first object.

On Professor Colbert's observing star-map is found a pencil mark west of star B. A. C. 2854, which, it is said, Professor Swift placed there soon after the observation and after hesitating a while in deciding whether it should be on the one side or the other of the star. That point is *s* in the cut. It is not a wonder that Professor Swift should be uncertain of part of the details of this important observation, in view of his sudden surprise and annoyance in finding his telescope hampered by the pole attached preventing motion to the eastward. Professor Colbert further says that Professor Swift's later adoption of Theta as probably one of the stars he observed was after he had called his attention to the fact that he could not possibly have seen 2854 (6½ mag.) during the eclipse.

Now, it is noteworthy that Professor Swift's object is very nearly in line with the sun and star, and so is Watson's first object. If Professor Swift was not certain on which side of the star to place his object, and had no means of measuring distance, and on the way home, as he says in his report, changed the first supposed distance between his objects from 12′ to 7′ (not from 7′ to 12′ as stated by Professor Young,—see Report of Chicago Astr. Soc.), is it not strongly probable that the two objects which he saw were Theta Cancri and Watson's first planet, especially since Professor Swift thinks Theta Cancri was one of the objects he saw in his single sweep to the west, and since Professor Watson swept carefully over the same ground twice and is sure that no other planetary object as bright could have been near Theta? It should also be added that at the outset Professor Swift had set his instrument to follow a star in its diurnal motion, and hence it moved approximately in a parallel to the plane of the equator. Now, in reality, the stars Theta and Zeta Cancri, the sun and Watson's objects were all nearly on the same parallel of declination, and by sweeping to the west but twice the field of his glsss Professor Swift would certainly have picked up Zeta and Watson's object number two. His distance of 7′ between his two objects is the only troublesome element in making the two observers wholly agree. These statements, if entirely true, seem to us in no wise to weaken or lessen the value of Professor Swift's observation of *what* he saw, or its *direction;* but on the contrary his observation is a strong kind of testimony in support of the claim that Watson and himself independently discovered *something* near Theta Cancri which they both took for an intra-Mercurial planet. Hence it is simply just to say that all the evidence in the world of what other astronomers have not seen should not weigh much against such observations. What two experienced observers have seen independently can not be impeached by what a hundred others have not seen, not observing in the same field at the same time. Professor Watson's letters also throw light on some other points of interest, especially regarding statements of Dr. C. H. F. Peters and Dr. Henry Draper pertaining to Professor Watson's mode of observation.

Charlier's General Disturbance of Thetis by Jupiter (German).—The MESSENGER is favored with No. 2, Bandet 22, a paper published by the Royal Swedish Academy recently, being prepared by C. V. L. Charlier and entitled An Inquiry Concerning the General Disturbance of the Planet *Thetis* by Jupiter. The object of the writer is to furnish a more general method for the computation of disturbances than astronomers now have in view of the rapidly growing number of minor planets between Jupiter and Mars, thereby avoiding in some degree the special yearly work that must now be done for particular orbits. Hansen's well known treatise, entitled, "Explanation of a Suitable Method for Computing the Absolute Disturbances of the Small Planets," is the work probably most commonly used. This author speaks of the excellence of Hansen's methods as consisting of the compact form of development with large terms only in a single coördinate, though first presented by Cauchy was improved by Hansen for numerical computation in the complete system of checks furnished for the computer.

In the development of the function of perturbations generally this writer has given a few methods which he believes are to be preferred in many cases to those of Hansen. One slight modification is that his method makes it possible to take the coefficients of the development from tables, thereby lessening the labor of computation somewhat. Another peculiarity is the use of Gauss' transformation of elliptical integrals in computing the terms of the same order in relation to eccentricity simultaneously. A further advantage is claimed in tabulating results, which work is scarcely less than that of the development of the main function. The two arguments involved make the work intricate, which the writer seeks to simplify by noticing that one is constant during a half revolution of the planet, but that during the remainder of the path it changes by finite amounts. In this way the terms are made so small that tabulation is unnecessary.

This paper is published in quarto form, in large clear type, and contains 98 pages of well ordered matter.

General List of Observatories (French).—The most useful small publication that has recently come to hand for those who read the French is the "General List of Observatories, Astronomical Societies and Astronomical Publications" of the world which has been prepared by A. Lancaster, librarian of the Royal Observatory of Brussells. We take pleasure in calling the attention of our readers to it.

The Sidereal Messenger,

CONDUCTED BY WM. W. PAYNE,

Director of Carleton College Observatory, Northfield, Minnesota.

VOL. 6, NO. 6. JUNE, 1887. WHOLE NO. 56.

COMETS OF THE YEAR 1886.[*]

Comet 1885, V. Cf. V. J. S. 21, p. 21. The last observation was made, as subsequently became known, by Pechüle in Copenhagen, March 1, 1886.

The following observations have been published since the last report :[†]

Brussels 115. 291.
Copenhagen 115. 387.
Lyons B. A. 3. 135.
Marseilles B. A. 3. 167.
Nashville 115. 323.
Padua 116. 107.
Pola 114. 211.
Rome 114. 205.
Vienna 114. 347.

Comet 1886, I. (Fabry). Cf. V. J. S. 21, p. 21. During the first days of April, the comet was visible to the naked eye. In the telescope, its nucleus could be seen surrounded by a brilliant coma, to which was appended a nearly straight slender tail over 1° in length; toward the end of the month, the comet reached the brightness of a star between the 2nd and 3d magnitudes with a length of tail from 4° to 5°; yet, because of the low position of the comet in the twilight, its appearance could not be called especially conspicuous. Photometric measurements of brightness by G. Müller show that at least during the time from the beginning of March to the end of April the nucleus of the comet gave out almost solely reflected sunlight, and that the light of its own emitted by it really

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[*] Extracts from the *Astronomischen Gesellschaft*, Vol. 22, No. I. Translated from the German by Miss Mary B. Cutler, Senior Class Carleton College, Northfield, Minn.

[†] The following periodicals are compared: *Astronomische Nachrichten* (without further designation) to Vol. 116, p. 256; *Monthly Notices* (M. N.) to Vol. 47, p. 120; *Comptes Rendus* (C. R.) to Vol. 104, p. 614; *Bulletin Astronomique* (B. A.) to Vol. 4, p. 48; *Astronomical Journal* (A. J.) to Vol. 7, p. 64.

furnished but a very small contribution to the total light. The Potsdam spectroscopic observations come to similar conclusions, according to which the spectrum of the bands in comparison to the continuous spectrum was rather faint, while in contradiction of this, Trêpied in Algiers expressly emphasizes a strong prominence of the bands in the spectrum. The observations on the northern hemisphere closed with April 25 at Vienna and Copenhagen; in the southern hemisphere the comet appeared first on May 1st as a striking object to the naked eye with a perfectly straight, sharply defined tail, 9° long. It was still visible to the naked eye with diminishing brightness until the middle of May; with a telescope it could be followed until July 30, on which day Finlay at the Cape last observed it. The following elements by A. Svedstrup are derived from six mean positions of 1885 from Dec. 3, 1886, to March 22, and will probably not differ very much from the final results:

$$\begin{array}{rl} T = & 1886 \text{ Apr. } 5.99962 \text{ Berlin M. T.} \\ \left.\begin{array}{r} \pi = \\ \Omega = \\ i = \end{array} \begin{array}{rrr} 162^{\circ} & 58' & 5.3'' \\ 36 & 22 & 38.7 \\ 82 & 37 & 17.1 \end{array}\right\} & 1886.0 \\ \log q = & 9.807767 \end{array}$$

To the observations quoted from the V. J. S. 21, p. 22, may be added the following:

Algiers C. R. 102. 731; B. A. 3. 234.
Bothkamp 114. 171.
Brussels 115. 291.
Cape 114. 235.
Christiania 114. 379.
Copenhagen 115. 385.
Cordova 116. 59.
Dresden 114. 205.
Glasgow, Mo. 115. 107.
Gothe 115. 139.
Greenwich M. N. 46. 303, 348, 399.
Kremsmuenster 115. 391.
Leipzig 115. 235.
Lyons B. A. 3. 134, 236.
Marseilles B. A. 3. 166.
Melbourne 116. 145.
Munich 114. 315.
Nashville 115. 323.
Nice B. A. 3. 277.
Orwell Park 115. 289.
Padua 116. 107.
Paris B. A. 3. 450, 493.
Plonsk 114. 395.
Pola 114. 211.
Prague 116. 57.
Rome 115. 329.
Scarborough M. N. 47. 28.
Sidney M. N. 46. 495.
Taschkent 114. 235.
Turin 115. 331.
Vienna 114. 347.
Washington 115. 109.
Windsor 115. 393.
At Sea M. N. 46. 457, 498; M. N. 47. 117.

Comet 1886 II. (Barnard). Cf. V. J. S. 21, p. 22. The comet became visible to the naked eye with increasing brilliancy toward the 1st of May and could be observed for 14 days in the northern hemisphere as a round bright nebula with distinct but wasted nucleus and a tail 3° long. On the 12th of May, already situated low in the horizon, it reached the brightness of a star of the 3d magnitude, without, however, presenting a striking appearance. Also, G. Müller in connection with spectroscopic observations was able to observe in it a striking falling off of its own light, in comparison with the sunlight reflected. In the northern hemisphere it was observed last by Pechüle in Copenhagen on the 15th of May; in the southern hemisphere the observations began on the 29th of May, after the comet had already passed the maximum of brightness, and closed with the Cape observation of the 26th of July.

We have not at present better parabolic elements than those given in the V. J. S. 21, p. 23. Two more orbit computations from three observations by Thraen (A. N. 115. 79) and Morrison (SID. MESS. 5. 118) have indicated a hyperbola; but these can, as the computers themselves perceive, merit no confidence so long as the impossibility of representing the observations by a parabola is not proved.

The following observations are added to those quoted in the V. J. S. 21, p. 23:

Algiers C. R. 102. 731; B. A. 3. 234, 496.
Brussels 115. 291.
Christiania 114. 379.
Copenhagen 115. 387.
Cordova 116. 61.
Dresden 114. 205, 379.
Glasgow, Mo. 115. 107.
Gotha 115. 141.
Greenwich M. N. 46. 303, 348, 399.
Kiel 115. 107.
Kremsmuenster 115. 391.
Leipzig 115. 235.
Marseilles B. A. 3. 167.
Melbourne 116. 147.
Munich 115. 47.
Nashville 115. 323.
Nice B. A. 3. 275.
Orwell Park 115. 289.
Padua 116. 107.
Paris B. A. 3. 584.
Plosnk 114. 395.
Pola 114. 211; 116. 193.
Prague 116. 57.
Rome 115. 329.
Scarborough M. N. 47. 28.
Sydney M. N. 46. 497.
Taschkent 114. 235; 115. 109.
Turin 115. 331.
Vienna 114. 347.
Virginia Univ. 115. 43.
Washington 115. 109.
Windsor 116. 123.
At Sea M. N. 47. 117.

Comet 1886, III. (Brooks 2); discovered in the morning sky by Brooks at Phelps, April 30. The comet presented in the telescope the exact image of a great comet. According to Pechüle there followed the extremely small nucleus a straight nebulous tail some 12″ broad, ending in a second somewhat wasted nucleus from which the tail, 10′ long, curved, fan-shaped towards the south. B. von Engelhardt mentions, besides, a faint off-shoot 6′ in length that split off by a single cleft from the main tail and bent toward the south.

When the comet was looked for again, after the full moon on May 20th, its appearance had entirely changed. Instead of the bright comet, Knorre saw only a light vapor from 5′ to 10′ in length. Tempel describes it during these days as a fusiform nebula 12′ in length and 1½′ in breadth, without head or bright nebulous spot in the place of it; a measurement of this headless mass seemed to him impossible. The last observation is that of Celoria in Milan, May 24; on June 3d it was, to be sure, still visible, but no longer to be observed.

The following elements by Celoria extend over the whole time of visibility, and will surely come very near to the final results:

$$\begin{aligned} T &= 1886 \text{ May } 4.482162 \text{ Berlin M. T.} \\ \left.\begin{aligned} \pi &= 326^\circ\ 19'\ 6.5'' \\ \Omega &= 287\ \ 45\ 33.4 \\ i &= 100\ \ 12\ \ 6.7 \end{aligned}\right\} & \ 1886.0 \\ \log q &= 9.925294 \end{aligned}$$

Observations:

Algiers C. R. 102. 1096; B. A. 3. 496.
Berlin 114. 317, 331.
Brussels 115. 295.
Copenhagen 115. 387.
Dresden 114. 288, 317, 379.
Hamburg 114. 301.
Kiel 114. 237, 317.
Leipzig 115. 237.
Lyons C. R. 102. 1052.
Marseilles B. A. 3. 276.
Nice C. R. 102. 1149; B. A. 3. 277.
Paris C. R. 102. 1051.
Rome 114. 301; 115. 329.
Washington 115. 109.

Comet 1886 IV. (Brooks 3) was found by Brooks on May 22nd as a faint nebula with a diameter of 2′. Unfortunately the extreme faintness of the comet's light did not permit its being followed longer than until July 3, which is the more to be regretted, as this seemed to belong to the interesting class of comets with short periods of revolution.

The following elements by Hind are based on three observations, May 25, June 3, and July 1, and show the last observations at Nice, of July 3, to be without certainty:

$$
\begin{array}{rl}
T = & 1886 \text{ June } 6.60866 \text{ Berlin M. T.} \\
\left.\begin{array}{r} \pi = \\ \Omega = \\ i = \\ \varphi = \end{array}
\begin{array}{rrr} 229^\circ & 45' & 58.0'' \\ 53 & 3 & 25.7 \\ 12 & 56 & 1.8 \\ 37 & 27 & 10.2 \end{array}\right\} & 1886.0 \\
\log a = & 0.5329478 \\
\mu = & 563.0992'' \\
U = & 6.301 \text{ years.}
\end{array}
$$

A second elliptical orbit by S. Oppenheim during the same interval gives a period of revolution of 9.05 years, but appears not to represent the observations quite so well as the orbit of Hind.

Observations:

Albany 114. 365.
Algiers 114. 403; C. R. 102. 1438; B. A. 3. 496.
Arcetri 114. 365; C. R. 115. 47.
Lyons 114. 365; C. R. 102. 1303.
Melbourne 116. 147.
Nashville 115. 323.
Nice 115. 47; C. R. 102. 1230; 103. 119; B. A. 3. 278, 535.
Pola 116. 193.
Rome 114. 365.
Strasburg 114, 365.
Sydney M. N. 46. 497.
Vienna 114. 365, 399.

Comet 1886 V. (Brooks 1) discovered by Brooks before the two preceding ones on the evening of April 27 as a moderately bright round mass of light 2' in diameter with an eccentric condensation. The comet, its brightness increasing, could be followed in the northern hemisphere until the end of May. The observation of position ended at Milan with May 25, at Vienna with May 28. The observations in the southern hemisphere began after the maximum of brightness was past on the 3d of July and closed with the Cape observation of July 30.

The following elements by A. Krueger are based on three observations, April 29, May 9 and 21, and appear, from the numerous provisory elements existing, to represent the observations best:

$$\begin{aligned} T &= 1886 \text{ June } 7.42622 \text{ Berlin M. T.} \\ \pi &= \left.\begin{array}{l} 33^\circ\ 55'\ 26.9'' \\ 192\ \ 42\ \ \ 6.5 \\ \ 87\ \ 44\ \ 23.1 \end{array}\right\} 1886.0 \\ \log q &= 9.431999 \end{aligned}$$

(In the display above, the three bracketed rows are π, Ω and i.)

Observations:

Algiers C. R. 102. 1096. B. A. 3. 495.
Ann Arbor 114. 397.
Arcetri 114. 299, 332.
Berlin 114. 299, 329.
Brussels 115. 293.
Copenhagen 115. 387.
Dresden 114. 287, 299, 379.
Gotha 115. 141.
Greenwich M. N. 46. 400, 459.
Hamburg 114. 237, 317, 329.
Harrow 114. 299.
Kiel 114. 223, 237, 299, 317, 329.
Leipzig 115. 235.
Lyons C. R. 102. 1052.
Milan 115. 159.
Marseilles B. A. 3. 275.
Munich 114. 381.
Nice C. R. 102. 1149; B. A. 3. 277.
Orwell Park 115. 289.
Padua 116. 107.
Paris C. R. 102. 1008.
Plonsk 114. 395.
Pola 116. 193.
Prague 116. 57.
Rome 114. 237; 115. 329.
Sydney M. N. 46. 497.
Turin 115. 331.
Vienna 114. 299.
Windsor 116. 123.

Winnecke's Comet 1886 VI. For last year's appearance of Winnecke's comet, A. Palisa had, on the basis of the elements derived by Oppolzer from three observations, found the following elements, which, on account of the great disturbances, are only closely approximate:

Epochs and osculation 1886 Aug. 31.26 mean time at Berlin.

$$\begin{aligned} T &= 1886 \text{ Sept. } 16.5 \text{ Berlin M. T.} \\ M &= 357^\circ\ 15' \\ \left.\begin{array}{r} \pi = 276\ \ \ 4 \\ \Omega = 101\ \ 56 \\ i = \ 14\ \ 27 \end{array}\right\} & 1890.0 \\ \varphi &= 46\ \ 37 \\ \mu &= 610.48'' \\ \log a &= 0.509557 \end{aligned}$$

On the basis of an ephemeris computed from these elements by E. Lamp, Finlay at the Cape succeeded in finding the comet on the 19th of August as a circular nebulous mass 1′ in diameter and of the brightness of a star of the 10th magnitude.

Towards the middle could be seen a slight condensation without a real nucleus; there was no tail. The passing of perihelion, according to this observation, took place 12 days earlier than was to be expected according to the elements of Palisa. On account of the unfavorable position for the northern hemisphere, it was only at the observatories lying in the South, Palermo, Nice and Algiers, that some few observations of the comet could be taken; so much the more reason is there for gratulation that Finlay himself was able to follow it through several months till Nov. 29. His great range, embracing 32 observation days, will be of great importance in the correction of the orbit.

Observations:

Algiers C. R. 103. 457; B. A. 3. 497.
Cape 115. 111.
Nice 115. 329; C. R. 103. 516; B. A. 3. 535.
Palermo 115. 143.
Rio C. R. 103. 918.
Sydney M. N. 47. 67.

Comet 1886 VII. (Finlay) discovered Sept. 26 by Finlay at the Cape as a round, faint, nebulous mass 1′ in diameter with traces of a central condensation. The very first computation of the orbit showed such a resemblance to the elements of the comet of de Vico 1844 I, which had not been found again up to this time, that the identity of the two heavenly bodies was for a time scarcely doubted. Unfortunately, the later computations of Prof. Krueger and Prof. Boss, although they also indicated an ellipse with a short period of revolution for Finlay's comet, yet make the identity of the two comets at least extremely doubtful. The periodic time of 2433 days shown by the elements of Prof. Krueger given below, is 440 days longer than that found by Brünnow for the comet of de Vico, and it is impossible to conceive how, in the time from 1844 to 1886, such a change of the orbit can have taken place. Consequently the assumption that we have before us two different comets with similar orbits must provisionally be regarded as by far the more probable.

The elements by Prof. Krueger, which are derived from single observations from Sept. 29, 1886, to Feb. 23, 1887, run as follows:

$$\begin{array}{lllll} T = & \text{1886 Nov. 22.42429 Berlin M. T.} & & & \\ \pi = & 7^\circ & 34' & 14.6'' & \\ \Omega = & 52 & 29 & 58.8 & \left.\right\} 1886.0 \\ i = & 3 & 1 & 39.2 & \\ \varphi = & 45 & 54 & 22.7 & \\ \mu = & \multicolumn{4}{l}{532.6894'' \pm 0.395''} \\ U = & \multicolumn{4}{l}{2432.937 \text{ days.}} \end{array}$$

After the comet had reached its greatest southern declination —26° in the middle of October, it turned toward the north, and soon, on account of the increasing brightness and its favorable position in the evening sky, became the object of eager observation for the astronomers of the northern hemisphere. From the middle of December on, the bright mass was observed to be slowly on the wane, without the observations having to be discontinued up to this time (middle of March).

Observations:

Albany 115. 269; A. J. 7. 21, 52.
Algiers B. A. 3. 586.
Bethlehem, Penn. A. J. 7. 54, 61.
Bordeaux C. R. 103. 1170.
Cape 115. 223.
Dresden 116. 43, 111, 247.
Goettingen 116. 219.
Hamburg 116. 111, 219.
Kiel 116. 13, 77, 111, 127, 219
Kremsmuenster 116. 41.
Lyons C. R. 103. 590.
Marseilles B. A. 3. 533,
Nashville 115. 267.
New York (Searle) A. J. 7. 15, 16.
Nice 115. 239; 116. 151; C. R. 103. 590.
Padua 116. 215.
Palermo 115. 239; 116. 151.
Pola 116. 193.
Rome 115. 237, 253, 267, 283, 303; 116. 27, 43.
Sydney M. N. 47. 68.
Taschkent 116. 247.
Turin 115. 397; 116. 153.
Washington A. J. 7. 8, 31, 62.

Comet 1886 IX. (Barnard-Hartwig)* discovered in the morning sky of Oct. 4 by Barnard in Nashville, on Oct. 5 by Hartwig in Bamberg and by Pechüle in Copenhagen, the last named of whom, meantime, was first able to verify the discovery on the following day. The comet was bright, round, with a distinct condensation of the brightness of a star of the 8th magnitude. With increasing brightness it became visible to the naked eye by the end of October. By the beginning of

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* The notations for 1886 VIII. are reserved for the comet discovered by Barnard Jan. 23, 1887.

December, it had developed to a beautiful object with a brilliant nucleus between the 2nd and 3rd magnitudes. On the very day after the discovery, Barnard had been able to perceive traces of a tail; towards the end of the month, this developed more clearly and reached, by the end of November, the considerable length of 5°. A second shorter tail already showed itself by the beginning of November; this, like the main tail, increased in brightness, so that the comet by the beginning of December presented a really characteristic appearance. Barnard reports besides, Nov. 23, still a third tail which, however, on Nov. 28, he was no longer able to perceive.

In the spectrum of the comet, the three usual bands stand out clearly on the continuous spectrum, without any other specially characteristic properties having shown themselves. Of especial interest are the photographs which Gothard in Herèny took of the comet. Among them all, the plates of Nov. 27 and 28 are of the greatest beauty; these show the form and structure of the tail with such distinctness that we have a right to expect great disclosures in future from the application of photography to comets for the discernment of the nature of these heavenly bodies.

The following elements are derived by A. Svedstrup from three mean positions, Oct. 8, Oct. 28, and Nov. 18:

$$
\begin{array}{rl}
T = & \text{1886 Dec. 16.51908 Berlin M. T.} \\
\pi = & 223^\circ\ 43'\ 46.1'' \\
\Omega = & 137\ \ 21\ \ 50.1 \\
i = & 101\ \ 39\ \ 36.0 \\
\log q = & 9\,821442
\end{array}
\left.\begin{array}{l} \\ \\ \\ \\ \\ \end{array}\right.
\begin{array}{l} \\ \left.\begin{array}{l} \\ \\ \\ \end{array}\right\} 1886.0 \\ \\ \end{array}
$$

In the beginning of January, 1887, the comet became invisible to the observers in the northern hemisphere; on Jan 8, so far as is now known, the last observation took place in Dresden. There is still hope, however, that in March and April, 1887, there will be successful observations in the southern hemisphere.

Observations:

Algiers B. A. 3. 586.
Bothkamp 115. 283; 116. 125.
Copenhagen 115. 253.
Dresden 116. 247.
Gotha 115. 317; 116. 171.
Greenwich M. N. 47. 27, 65, 116.
Hamburg 115. 283.
Kiel 115. 283, 317; 116. 125.
Kremsmuenster 116. 43.
Liege 115. 317.
Marseilles B. A. 3. 533.
Padua 116. 215.
Palermo 115. 255; 116. 27.
Pola 116. 193.
Prague 115. 255; 116. 155.
Strasburg 115. 285.
Turin 115. 397; 116: 153.
Vienna 115. 253.
Washington A. J. 7. 8, 31.

The last year's appearance of the periodic comet Tempel 3, which, according to the computations made beforehand by J. Bossert, was to pass its perihelion May 9.5, has, unfortunately, on account of the extreme faintness of the light of the comet and its unfavorable position near the sun, passed by unobserved.

The comet of Olbers (Cf. V. J. S. 21, p. 24) also has not been found the past year.

PRESIDENT HOLDEN'S REPLY TO PROFESSOR PROCTOR.

In the issue of the *Examiner* (Cal.) of March 27th there is an article by Mr. Proctor which refers to me in such terms that I am bound to notice it, and as space has been given to his personal attack upon me, I beg to be allowed the space for reply.

In order to present the basis for a judgment I must give the whole history of our relations, which, fortunately for your readers, are not complex.

In the *Atlantic Monthly* for September, 1874, I wrote a review of two of Mr. Proctor's recent books, from which the following words are taken:—After speaking of his book on "Saturn" I said: "This book was a success, as it deserved to be, and it led its author (as too candid biographers have told us) to attempt to earn a large sum of money by writing similar books. The titles of these books are well known and are an index to the rather sensational character of the books themselves—'The Sun, Ruler, Light, Fire and Life of the Planetary System,' 'Other Suns than Ours,' 'The Orbs Around Us,' 'Other Worlds than Ours,' etc."

To these remarks Mr. Proctor replied in a letter to the editor of the *Atlantic Monthly* (Mr. W. D. Howells), in which he objects to my having included "Other Suns than Ours" in the list of books above given for the reason that although it had been announced three years before it had never been published. This was quite true, as it had not been published as a book, but what I presume to have been its substance had been given in a lecture delivered in New York under the title "Other Suns than Ours," which I had read. As soon as possible it was replied to Mr. Proctor that the mention of the book-*title*, "Other Suns than Ours," even in so incidental a casual way, was "an undoubted slip for which Mr. Proctor has our apology."

In extenuation of the acknowledged inadvertence it was said that from 1868 to 1875 Mr. Proctor had published at least twenty (20) separate volumes, and the public was left to infer the impossibility of even the most devoted critic reading and remembering the particular contents of each of three volumes per year from the same hand on similar subjects, especially as the titles of these seemed to be chosen for sensational and advertising purpose only, and as my criticisms applied to the contents of nearly all of them.

At the time I wrote the first review I truly believed I had read some rather popular but not very valuable writing between the covers of a book—one of Mr. Proctor's twenty—and that the title of the book was "Other Suns than Ours."

I know now that I read the not very valuable writing referred to as a lecture by Mr. Proctor which was reported in the *New York Tribune* under the same title.

I submit that the mistake of confounding a printed lecture with a printed book was, under the circumstances, pardonable. I have moreover apologized in print for this mistake, and at this moment I regret that it occurred.

Mr. Proctor has not accepted an apology made in good faith and he does not now accept it. I regret this also; but I do not see that there is anything more required of me in this especial matter.

Mixed in with his remarks upon this particular point, I find various attacks upon me, personally, and upon my scientific reputation generally.

I decline to enter upon the question of my personal merits as a gentleman. This is a question to be decided by my contemporaries and noc by Mr. Proctor nor by myself.

I further decline to enter into the question as to my merits as a scientific man. This, again, is a matter to be settled by my contemporaries and by those who will come after, and neither by Mr. Proctor or by myself.

I have the personal satisfaction of knowing that I have honestly striven to do my duty and that my contemporaries have been more than kind to me.

The Royal Astronomical Society which forced Mr. Proctor out of the position of its Secretary has honored me by selecting me as one of its fifty Foreign Associates.

I have held public positions of various kinds since 1870 and I have no reason to be ashamed of the results of my work at the U. S. Naval Observatory at Washington, at the Washburn Observatory at Madison, Wisconsin, and at the University of California. Some of the men with whose names Mr. Proctor says I ought not to presume to couple my own are glad to look forward to being associated with me at the Lick Observatory. I remark in passing that Mr. Proctor couples his own name, characteristically, with Sir Isaac Newton's.

Finally, looking back over the twelve years during which Mr. Procter has nursed his wrath, I have to regret that in writing my original review I made a mistake which did him an unintentional injustice and which obliges me to make this reply. I reflect, however, that reviewing his books adversely was the real offense, not accidentally mentioning one of his book titles as sensational.—*Examiner.* EDWARD S. HOLDEN.

KEPLER'S CORRESPONDENCE IN 1599.[*]

[Continued from page 138.]

4. How will you make the character of man depend upon his horiscope, in a point of the heavens? For it influences a

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[*] Translated from the Latin for the MESSENGER by Professor Louisa H. Richardson, Carleton College.

man as long as he lives, just as those fetters put upon the gourds by the skill of the farmer: although they do not increase the gourd, yet they give it shape. So the constellation, although it does not give character, nor deeds, nor fortune, nor children, nor riches, nor wife to a man, nevertheless it forms all things which come to the man. And yet this, while the man lives, takes an infinite number of forms from the natal hour, never remains:—and so the natal position is lost. How can that act which does not exist? For it has an influence while it is in this position, but the position does not remain. Or is some mark of that position impressed upon the body, upon the soul, kindred to light, and suited to this thing? And how is that, which has no existence, impressed upon fortune? Experience proves all these things, and that, experience of men by no means foolish. Look at a man at whose birth the planets, Jupiter and Venus, the means between the extremes, as I said above, are not favorably situated: although such a man may be upright and wise, yet you will see that he experiences a fortune as sad and gloomy as possible. Such a woman is known to me. She is praised throughout the whole city on account of her virtue, her modesty and her chastity. Yet she is plain and of a coarse body. She was restrained with severity by her parents in her early life, and when she had scarcely attained womanhood was married against her will to a man of forty years. In a short time he died, and she married with more willing mind, another of the same age, but one who could not be called a man, and the whole four years, which she lived in this marriage, was passed in sickness. She married for the third time a poor and despised man, though she herself was rich before. Her property was wrongfully held from her on all sides. She could never have a maid but proved faithless. She became entangled and involved in all her business. She even bore children with sorrow. Everything else was of this nature. Here you may see the same mark of mind, body and fortune, analogous indeed to the position of the constellation; and so it would be impossible that this mind was the cause of this whole fortune, since it was beyond her power and control.

Since Saturn and the sun agreed in the sexangular radiation for me (I speak more freely concerning very well known facts) my body is dry and knotty, not large, my spirits low, wholly driven into straits, suspicious, timid, coming through difficulties and embarrassments, and lingering in them, my character is very similar. It is my delight to gnaw bones, to eat dry bread, to taste the bitter and acid; it is my pleasure to walk through rough places, declivities, and thickets. I have no charms of life except letters, nor do I desire any, and when offered I scorn them. My fortune is similar in the smallest detail. Other things are dispaired of. I have an access, by no means great, to property and reputation. For I am continually hard pressed among the rising and circumstances indeed change, the form remains the same. With whatever I have striven thus far, I have been harshly opposed. I know not whether genius also is drawn into the alliance while I defend the human race for appealing to the motion of the earth, while I with resolute boldness urge on an orb of so great weight, with swift motion through the stars, while the senate of terrestrials strive in opposition. But let this indeed be ascribed to the common lot of illustrious men. Let this prevail, "hardships are blessings," etc., and this used by Cicero, "the sweat of virtue," etc. And I am not opposed to that theory of the wise, nay rather, most certain demonstration, by which they prove that truth is ever opposed by the multitude. Let there be, then, talent and zeal begun with reason, free from those things which belong to the heavens. Let the former be retained. And you see again one characteristic in me depending upon the heavens, not indeed so plainly from the sixth position of the sun and Saturn, but thus for the sake of brevity have I spoken. For those who are wont to speak thus plainly selling much material, stones and cement, for a house, would quickly have done me a wrong. For I myself was born when the sun, Venus and Mercury were in Capricorn. And so you see Wirtemberg will be at length destined for me. Let these therefore be the examples and forms of experience. I wonder why these things are not treated by philosophers. Either because many vain things are handed down, or because rules are made (such even as you seem to demand, canons--and guides of very little use to a philosopher, who does not busy himself with every detail) rules, I say, easy and deceptive in almost the same elements, which are contradicted also by the first experience. Or are these again becoming obsolete, of themselves, on account of the difficulty of astrology, although formerly they crept into the minds of philosophers? Of such a nature also is that question concerning the magnet and countless other questions which are regarded among miracles, since they do not coincide with the axioms of physics, and it is deemed sufficient that each is known separately. But since there are so many, let some one of you who compare many things with each other, come forth and render the reasons not of one thing alone, which is impossible, but of many such, united. While I was attempting this in my office before this, two philosophical methods came to my mind, one of the type and archetype, Platonic as I think, the other of the Genii derived from letters. For the world is the corporeal image of God, the soul is the incorporeal image of God and yet created. The body is the image of the world, hence microcosmos; the forms of bodies, of souls, the diversities of fortune are the images of diversities which exist between the positions of the heavenly bodies. Thus the birth of man corresponds to the rising of a planet, the death of man and what follows corresponds to the setting. And that which is between forms the actions of man and those things which follow man in life. And inasmuch as the setting looks back to the rising with a certain relation of opposites, so also man's correlatives are shown in the seventh or setting, as wife, purchaser, physician, slave, etc. And the position of the heavenly bodies, since it is regarded in a point, has something lasting which corresponds to it in man, and this is that same character which I have mentioned of mind, body and fortune. But the motion of the heavenly bodies, which is considered with time, is an example of temporal things in man, namely his actions, of which I shall speak later. But nevertheless because I do not yet know where that imaginary canopy is kept in the mean time, since the constellation moves on after the moment of birth, I have therefore introduced Genii. For the body is too material—for undertaking this character—but the soul, although it is kindred to light, although it has no less wonderful duties entrusted to it by God, the formation of necessary parts and other things—and thus it could well be made the subject of this character impressed by the constellation; — nevertheless I do not know, how it could, outside the man, form his fortunes according to the standard prescribed by that character given by the constellation. And so those tutelary Genii taken from the Bible are pleasing to me,—who preside over the birth of men by a certain divine law, and receive the character of the nativity in place of the men themselves, either in their own being, or in memory alone, and offer themselves to the bonds of heaven, and are not of free thought at all, but either grow weak or strong according to the movement of the constellation. We have an example of this mingling, in light, which, although it is not corporeal, but something divine, nevertheless it is subject to the laws of body without time indeed and motion: it is reflected, refracted, it is thrown on stronger, weaker, it is cut off, it is lessened by distance, etc. It is a good example in this case. For above I made the nature of the light and of souls the same, here also I make the nature of the Genii either the same or kindred.

5. Related to these is the question concerning the rise of man. I have a very pleasant sight in the relations of the natal hours. Who, then, is there who puts off birth to that time and moment that the offspring may be brought forth under a constellation favorable to the parents. This thing alone would convert even Mirandola himself (if he fights more than against trifles). Who, I say, is that director, immediate God? Is it the soul of the mother, or of the child? And how could he be skilled in astronomy when man himself is ignorant? No one ever lived so wise that, freed from the material body, he

learned anything of astronomical subjects through ecstasy. Or then does the heavenly light itself with so great reason measure out the moments of births? But can the rays of light and of the stars accomplish so much? Even to the rays of the stars so much must not be granted that, with so great intelligence, they can do a thing which it is scarcely credible any mind can do. For in the town also many obstacles would be opposed to the rays of light, and we must say here what was said above—if it is the effect of the rays, it is not then of the light, but of the position, not of the star, but of the stars. It would be absurd that the light and ray itself should become a body, as it were, which should be formed by its position or by a soul for this work, and should be instructed by the best reason. For even this relation also of children to parents exists in the radiations.

(To be continued.)

THE SECOND CENTENNIAL OF THE PRINCIPIA.

The mathematician, physicist or astronomer who thinks of 1887 as the second centennial year of the Principia will instinctively and eagerly go back, in thought, two hundred years to live over again the memorable scenes attending the publication of this first great work of the illustrious Newton. How he came to compose the Principia and the stir that it made in the world of science and letters are inspiring details of his biography that any may read, and all should know, to judge well of his genius and nobleness of character. Newton was 45 years old when this book was given to the world, although some of its great thoughts had been kept in mind patiently for more than twenty years. When he learned in 1682 of the successful measurement of an arc of the meridian by the French astronomer, M. Picard, in 1679, he took note of the result, and quickly computed therefrom the diameter of the earth, and with these new data he resumed his former calculations by which he had tried in 1666 to show the identity of the law of falling bodies at the earth's surface with that which guides the moon in her orbit. As the computation progressed and he saw that his method was right, the amazing magnitude of his discovery began to dawn upon him, and its stupendous results overpowered him so much that he was obliged to entrust the finishing of his computation to the hand of a friend. His biographer has pictured this scene in these fitting words:

"It were difficult, nay impossible to imagine, even, the influence of a result like this upon a mind like Newton's. It was as if the key-stone had been fitted to the glorious arch by which his spirit should ascend to the outskirts of infinite space—spanning the immeasurable—weighing the imponderable—computing the incalculable—mapping out the marching of the planets, and the far-wanderings of the comets, and catching, bring back to earth some clearer notes of that higher melody which, as a sounding voice, bears perpetual witness to the design and omnipotence of a creating Deity."

Under an inspiration like this we may imagine that Newton wrote out, in bold outline, his great thought of universal gravitation, choosing for a title of the entire work the broad phrase, The Mathematical Principles of Natural Philosophy. It consisted of three books, the first two dealing with the motion of bodies, and the last titled "The System of the World," the matter of all being given in the form of mathematical propositions and proofs, with suitable drawings.

Although Newton appreciated in some degree the great work that he was doing, yet the low estimate that he placed on his own way of doing it, and the utter dislike that he had for the discussion that his new ideas and principles were constantly eliciting are well expressed in his own words, as follows:

"I had indeed composed this third book in a popular method, that it might be read by many; but afterwards considering that such as had not sufficiently entered into the principles could not easily discover the strength of the consequences, nor lay aside the prejudices to which they had been many years accustomed, therefore to prevent disputes which might be raised on such accounts, I chose to reduce the substance of this book into the form of propositions, in the mathematical way, which should be read only by those who had first made themselves masters of the principles established in the preceding books."

Newton was without means or influence at this time, and the noble Halley who stood high in the Royal Astronomical Society befriended him, and generously assumed the cost of publishing the Principia in a better form and with more matter than Newton had intended to use.

How Dr. Hook and a host of others prominent in name, in England, either claimed precedence or share in Newton's brilliant discoveries that they were obliged to acknowledge as true, or assailed him and his friends bitterly for promulgating and supporting such ideas as the compound nature of white light, and the fact that the solar spectrum could possibly be more than three and one-half inches long, the devotees of science in the year 1887 will remember with impatience and pity while they wonder that the sensitive and pure-minded Newton could withstand all this and still triumph so royally. It is a wonder also to the best minds of this memorial year of the Principia that any one person in Newton's time could possibly have given to the world the first thoughts of the infinitesimal analysis, now the most powerful instrument of investigation known to the whole realm of mathematics and science in general. When the scholar of to-day opens the Principia and drinks in the breadth and depth and sublimity of its thought, can any such hesitate to acknowledge the divine help that guided the pen of the immortal Newton?

EDITORIAL NOTES.

The next MESSENGER will be published in September, July and August being vacation months.

Dr. H. C. Wilson of Washington, D. C., has been invited to a position in the new observatory at Carleton College, Northfield, Minn. His instruments will be the Meridian Circle and the Equatorial. As soon as his part of the reductions of the observations of the Transit of Venus is completed at Washington he is expected to assume the duties of the new position.

The fine new observatory building at Carleton College is now very nearly completed. The instruments will be remounted within the next sixty days. In our September issue we hope to give a cut of the new building with detailed description of its plan, instruments, and something of the work contemplated.

Quite soon, articles on interesting topics are expected from Professors Hall and Harkness of Washington, and S. C. Chandler, Jr. of Cambridge, Mass.

Anuario for 1887 from the national observatory at Tacubaya, Mexico, is received. It contains the usual matter, with two beautiful photographs of phases of the annular eclipse for 1886.

Elements of Comet e 1887 (Barnard).—On May 13, a telegram was received at Harvard College Observatory, announcing the discovery of a comet on the night of May 12, by Mr. E. E. Barnard, of Vanderbilt University, Nashville, Tenn. The discovery position was with reference to an 8th magnitude star, in *a* 15*h* 11*m* 41*s*, δ S. 30° 48′, the comet being north preceding 0*m* 58.93*s* in *a*, and 11′ 53″ in δ. The comparison star being recognized as *Cordoba General Catalogue* 20734, the following position was deduced by Mr. S. C. Chandler, Jr., and circulated as the discovery position:

1887, May 12.706515*d* Gr. M. T.
App. *a* 15*h* 10*m* 49.18*s*
App. δ —30° 35′ 50.6″

A letter from Mr. Barnard states the position to be the result of nine ring-micrometer comparisons, the exact data being, in *a* 0*m* 58.96*s*, in δ 11′ 52.8″.

On the night of May 13, Mr. Chandler observed the comet at Cambridge, his position being distributed on the following morning to astronomers in this country. All the positions which have come to hand are here given, the Nashville positions being communicated by telegraph by Mr. Barnard, those from Albany by Prof. Boss, and the Harvard College (Wendell) positions being taken from a local newspaper.

Greenwich M. T.	R. A.			Decl.			Observer.
d	*h*	*m*	*s*	°	′	″	
May 12.70651	15	10	49.18	—30	35	50.6	Barnard.
13.63591	15	12	16.3	—30	7	26	Wendell.
13.65590	15	12	19.79	—30	6	32	Chandler.
13.67772	15	12	21.33	—30	6	4.4	Barnard.
13.67994	15	12	20.42	—30	6	1.3	Boss.
14.64053	15	13	53.6	—29	35	20	Wendell.
15.71349	15	15	38.41	—28	59	44.7	Boss.

In *Science Observer* and the *Astronomical Journal*, each of May 18, are found the following elements and ephemeris of Comet *e* 1887:

$$T = 1887 \text{ June } 26.682 \text{ G. M. T.}$$

$$\left.\begin{aligned}\omega &= 27^\circ\ \ 42.6'\\ \text{☊} &= 244\ \ \ 41.4\\ i &= 17\ \ \ \ 2.4\end{aligned}\right\}\text{App. Equinox.}$$

$$\log q = 0.10216$$

Comparison with middle place gives $C - O$:

$$\Delta\lambda \cos\beta = -0.06'$$

$$\Delta\beta = -0.24$$

The following is the ephemeris for Greenwich midnight:

1887	α			δ		log Δ	light
	h	*m*	*s*	°	′		
June 3.5	15	57	4	— 13	21	9.4869	2.4
" 7.5	16	8	32	8	54	9.4714	2.6
" 11.5		20	48	— 4	17	9.4618	2.7
" 15.5		33	44	+ 0	21	9.4586	2.8
" 19.5	16	47	4	+ 4	47	9.4614	2.9

The light of May 12 is taken as unity.

Finlay's Comet.—I think Mr. Wilson's interesting article in your April number renders it highly improbable that the comets of Di Vico and Finlay are identical. There seems however to be a more probable case of identity between Finlay's comet and the comet of 1585, the resemblance of whose elements to that of Di Vico has been already noticed. For comparison I give the leading elements of the three, but those of the comet of 1585 having been computed for a parabolic orbit will no doubt require modification:

	π		☊		i		q
Comet of 1585	9°	8′	37°	44′	6°	5′	1.0948
Di Vico's Comet	343	6	64	24	2	55	1.1864
Finlay's Comet	7	33	52	26	3	2	0.9978

If the first and third of these comets are identical and the number of revolutions is 45 the mean period would be 6.69 years. This agrees sufficiently closely with the period of 6.675 years assigned to Finlay's Comet.

It seems past doubt however that in many instances there are families of comets. This fact renders the identification of individual comets somewhat precarious. But these three comets plainly belong to the same family. W. H. S. MONCK.

Dublin, Ireland, May, 1887.

Under date of May 2, Mr. Barnard kindly sent to the MESSENGER the following, designed for the May number, which was received too late for publication:

Orbit of Comet d 1887 (Barnard Feb. 16).—From my own observations of Feb. 16th–28th and March 12th I have come puted the following orbit of Comet *d*. The observations were first corrected for parallax and aberration by an approximatorbit (SID. MESS. for April).

ELEMENTS.

$$T = 1887 \text{ March } 28.39633 \text{ G. M. T.}$$
$$\left.\begin{array}{rl} \omega = & 36^\circ\ 28'\ 50'' \\ ☊ = & 135\ \ 27\ \ 17 \\ i = & 139\ \ 48\ \ 39 \end{array}\right\}\text{Mean Eq. 1887.0}$$
$$\log q = 0.00295$$

E. E. BARNARD.

Vanderbilt University Observatory, Nashville, Tenn.

Photographic Study of Stellar Spectra.—The first annual report of the photographic study of stellar spectra, conducted at the Harvard College Observatory, through the patronage of the Henry Draper Memorial, has just appeared.

The report begins by citing the facts that Dr. Draper was the first to photograph the lines of a stellar spectrum and relating how he pursued the work with skill and characteristic ingenuity until interrupted by his death in 1882; and how Mrs. Draper has made liberal provision for carrying forward the work at Harvard College Observatory, on a larger scale, by the aid of the improved dry-plate process and other modern facilities incident to the rapid growth of stellar photography. The instruments used are an 8-inch Voigtländer photographic lens, Dr. Draper's 11-inch photographic lens, the 15-inch refractor, Dr. Draper's 28-inch reflector (soon to be added) and a 15-inch mirror constructed by Dr. Draper with which his photograph of the moon was taken.

A brief description of the experimental work with the instruments, the stars tried, and plates showing the results obtained make a prominent part of the paper.

The investigations to be pursued in this department of the observatory work are,

1. A catalogue of spectra of bright stars.
2. A catalogue of spectra of faint stars.
3. A detailed study of the spectra of the brighter stars.
4. Faint stellar spectra.
5. Absorption spectra.
6. Wave lengths.

It is thought that this photographic apparatus will be sufficiently delicate to determine the motions of the stars with a high degree of precision; as it is claimed an outfit has been furnished, on a scale unequaled elsewhere, and that Mrs. Draper has provided means for continued observation, reduction and publication suited to the capacity of the instruments. This being true the Draper Memorial is a noble monument to the useful memory of one of America's noblest sons of Science. May it endure untarnished. But the Director of the H. C. Observatory disappoints his friends when he speaks weak words concerning the encouragement of small observatories.

New Method for Time and Azimuth.—The method perfected by Professor Wm. Döllen and widely used by Russian geodesists, according to which time and azimuth are determined, in a few minutes at any time of day, by setting the portable transit instrument, a universal instrument, in the vertical of the pole star, is one that is perhaps not so well known to American astronomers as it deserves to be. It is peculiarly adapted to the needs of explorers and those who have to do their observing by daylight. For the great convenience of the increasing number of those who are learning to use Döllen's method, he has published "Stern Ephemeriden auf das Jahr 1887, zur Bestimmung von Zeit und Azimut mittelst des tragbaren Durchgangsinstruments im verticale des Polarsterns." Copies of this ephemeris can be had (by any who will study or teach or practice this method) by application to Professor Cleveland Abbe, Washington, D. C. We are obliged to Professor Abbe for kindly calling our attention to this interesting matter.

New Red Star near 26 Cygni.—T. E. Espin, observer to the Liverpool Astronomical Society, discovered March 23 and 27 a new red star 7.5 magnitude, 0*h* 0*m* 5*s* F. 0° 3′ S 26 Cygni. Its spectrum III !!!.

Characteristic Curves of Composition.—The applications of the mathematics seem to be unlimited, and Professor Mendenhall is out with a late genuine surprise in this direction, as neat in principle as it may prove useful in practice. In a paper of 14 pages he gives a plan for analyzing composition on representative counts of words used by a writer, in regard to length and relative frequency. When these counts have been made for any composition, the resulting numbers are placed in order on the adjacent sides of paper ruled in squares like mathematical or computing paper, and a curve is drawn on the paper as directed by the numbers. Several interesting graphic illustrations are given in the paper before us and the tests of the system are surprisingly satisfactory. What need longer for Mr. Donnelly to be in doubt whether Bacon is the author of Shakespeare or not. A school boy who will take the time to count words enough of Bacon's writings and arrange the resulting numbers in proper form will doubtless find a satisfactory answer to the great question.

The Origin of Nebulæ.—We have not before had the privilege of reading Dr. James Croll's *Origin of the Nebulæ*, an article which was published in the *Philosophical Magazine* for July, 1878, a copy of which this distinguished author has recently kindly furnished us. Parts of it, if not all, will appear later as space may be given to this important theme.

Sur Les Spectres Invisibles is a translation of Professor Langley's publication into the French by Charles Baye, and published by Ganthier Villars of Paris. It is an honorable notice of American work.

The American Astronomical Society of Brooklyn, N. Y., has published papers read before it during the last two years in neat pamphlet form, consisting, thus far, of 55 pages in two numbers. These papers are excellent and deserve a fuller notice than space at this time will allow. The writers are, most of them, practical observers or workers who already have reputation in astronomical circles, as, for example, Professor Geo. W. Coakley, Henry M. Parkhurst, Garrett P. Serviss, and others that should be named. That society seems to be doing good work and should prosper.

Warner Prizes for 1887.—From April 1st, 1887, to April 1st, 1888, I offer ($100) one hundred dollars for each and every discovery of a new comet made between the above dates, subject to the following three conditions:

1. It may be discovered either by the naked eye or telescope, but it must be unexpected, except as to the comet of 1815, which is now looked for.

2. (a) The discoverer, if residing in the United States or Canada, must send a *prepaid telegram immediately* to Dr. Lewis Swift, Director, Warner Observatory, Rochester, N. Y., giving the time of discovery, the position and direction of mo-

tion with sufficient exactness, if possible, to enable at least one other observer to find it. (b) Discoverers in the other countries must send *by immediate mail* a full account of the discovery, as above required, to Dr. Lewis Swift, as above.

3. In the United States and Canada this intelligence must *not be communicated to any other party or parties*, either by letter, telegraph or otherwise until publicly announced through the press by Dr. Swift, which he will do at once on information of the discovery. Great care should be observed regarding this condition, as it is essential to prevent duplication of announcements and for the correct transmission of the discovery, with the name of the discoverer, which will be immediately made by Dr. Swift.

Discoverers living in *Continental Europe*, will receive their prizes from Warner's Safe Cure Establishment, 10 Schæffergasse, Frankfurt on the Main, Germany; those living in *Great Britain*, from H. H. Warner & Co's Safe Remedies office, 47 Farringdon St., E. C., London; those in Australasia and Asia, from H. H. Warner & Co's Safe Cure Branch House, 147 Little Lonsdale St. W., Melbourne, Australia; for other parts of the world, prizes will be paid here.

Prizes will be awarded four (4) months after discovery and verification of claim.

Three disinterested scientists will be selected to settle any dispute that may arise regarding comet discoveries.

ROCHESTER, N. Y., March 15, 1887. H. H. WARNER.

The Great Southern Comet.—Unusual interest was felt in astronomical circles concerning the appearance of comet *a* of 1887, which was also called the Great Southern Comet. It was first seen by Thome of Cordoba, South America, June 18, and soon after telegraphed to astronomers generally. This comet was without head or nucleus so that observations for place were very uncertain always, and worth very little for determining an orbit. It was observed through the month of January while passing through the constellations of Phœnix and Eridinus, and from the 22nd to the 25th the long, slim train extending to a length of 40° was a beautiful sight to the naked eye. Dr. Thome computed the elements of its orbit finding that the comet had passed its perihelion Jan. 8, at a distance of about 18,000,000 miles from the sun with motion retrograde. In A. J. 156 S. C. Chandler has given a very full and thorough study of this comet, having computed a second orbit with the following elements:

$$\begin{aligned} T &= 1887,\ \text{Jan. } 8.730 \\ \omega &= 174^{\circ}\ 48.6' \\ \Omega &= 132\ \ 48.6 \\ i &= 57\ \ 52.1 \\ \log q &= 8.36280 \end{aligned}$$

$$C-O$$

$$\begin{aligned} \Delta\lambda \cos\beta\ . &\ -1.2' \\ \Delta\beta &\ +2.3 \end{aligned}$$

From recent foreign papers we notice that the computation of final orbits for the following comets has been undertaken by the persons named below:

Winecke's Comet by Baron E. von Härdtl.
Comet 1840 I by Mr. Rechenberg.
Comet 1848 I by Mr. F. Bidschof.
Comet 1865 I by Mr. F. Koerber.
Comet 1879 V by Prof. T. Zona.
Comet 1882 II by Mr. H. Kreutz.
Comet 1882 III by Mr. L. Stutz.
Comet 1885 III by Prof. J. Gallenmüller.
Comet 1886 III by Prof. G. Celoria.
Comet 1886 IV by Dr. S. Oppenheim and Mr. F. Bidschof.

Paris Astronomical Congress.—This gathering of distinguished astronomers and physicists was in session at the large hall of the Paris Observatory from April 16 to April 25. The following persons were present: Auwers, Berlin; Baillaud, Toulouse; Bakhuyzen, Leyden; Bertrand, Paris; Beuf, La Plato; Bouquet de la Grye, Paris; Brunner, Paris; Christie, Greenwich; Cloué, Paris; Common, Ealing; Cornu, Paris; Cruls, Brazil; Donner, Helsingfors; Dunér, Lund; Eder, Vienna; Elkin, America; Faye, Paris; Fizeau, Paris; Folie, Brussels; Gautier, Geneva; Gill, Cape of Good Hope; Gyldén, Stockholm; Hasselberg, Pulkowa; Henry (Brother), Paris; Janssen, Meudon; Kapteyn, Gröningen; Knobel, London; Krueger, Kiel; Laussedat, Paris; Liard, Paris; Loewy, Paris; Lohse, Potsdam; Mouchez, Paris; Oom, Lisbon; Oudemans, Utrecht; Pechüle, Copenhagen; Perrier, Paris; Perry, Stronghurst; Peters, Clinton; Pujazon, Cadiz; Payet, Bordeaux; Roberts, Liverpool; Russel, Sydney; Schönfeld, Bonn; Steinheil, Munich; Struve, Pulkowa; Tachini, Rome; Tennant, Ealing; Thiele, Copenhagen; Tisserand, Paris; Trepied, Algiers; Vogel, Potsdam; Weiss, Vienna; Winterhabter, Washington; Wolf, Paris.

Regarding instruments to be used in celestial photography, this conference decide,

(1.) The instruments employed shall be exclusively refractors, and may be made locally provided the conditions laid down by the conference be fulfilled.

(2.) The stars shall be photographed as far as the fourteenth magnitude inclusively, this magnitude being indicated provisionally by the scale actually in use in France, and with the reservation that the photographic value shall be definitely fixed afterward.

(3.) The aperture of the object-glasses shall be 0 33 meter, and the focal length about 3.43 meters, so that a minute of arc shall be represented approximately by 0.001 meter.

Concerning photographic plates it was decided:

(1.) All plates to be used should be prepared according to an identical formula to be subsequently determined.

(2.) A permanent control of these plates from a point of view of their relative sensibility of the different radiations shall be instituted.

(3.) The aplanatism and achromatism of the object-glasses employed shall be calculated for the wave lengths near Fraunhofer's G.

The account given in *Nature*, No. 1, Vol. 6, of the discussions of these and many other kindred points is instructive reading.

Haynald Observatorium.—The publications of this observatory at Kaloosa, in three parts for 1886, have been received. The work is under the direction of Hüninger, Adolf S. J., and the illustiations of solar protuberances for 1884-1886 are instructive.

Astronomical Photography.—The Princeton *Review* for May has a full and delightful article on the theme of astronomical photography by Professor Young. He first glances at the history of this new branch of astronomical work, then notices the comparative merits of refracting and reflecting telescopes, after which follows a particular and classified statement of work now going on:

(1) Photographs of the solar surface, (2) photographs of the corona, (3) of the solar prominences, (4) of the moon, (5) planetary photography, (6) photography of comets and (7) of nebulæ. Under the head of the use of photography as a means of *precision* in astronomical work, he speaks of photographs of eclipses, the transit of Venus, star-charts, double-stars, star-groups and clusters, and stellar parallax, of meridian observations, of photometry and of stellar spectra. In point of historical development, range of efficiency and judicious statement of astronomical facts, this article is very valuable for reference.

Brass Micrometer Screws.—M. D. Ewell, of Chicago, noticing the irregularities in the work of his micrometer, which had the appearance of coming from the heat of the hand on the brass milled head screws of the instrument, has recently had them all removed and replaced by the material of hard rubber. In a late private note he says he now has no further trouble.

Intra Mercurial Investigations.—The total eclipse of the sun which will occur near our midnight of Aug. 18 next may prove to be one of the most important of those witnessed in this century. The path of totality will pass across Europe and Asia, giving an opportunity for observations from scores of easily accessible stations. The greatest duration will be only three and eight-tenths minutes, but wonders may be accomplished in, that short interval between the disappearance and reappearance of the sun. The United States will be well represented by a party in Japan, which will take especial pains to photograph the corona and surrounding sky. It is understood that Old World observers will also work largely on the same general plan.

It now looks as if the question of existence or non-existence of an intra-mercurial planet will come to the front on this occasion, after having been quietly shelved for several years.—*Chicago Tribune.*

The Holden-Proctor Correspondence.—We gladly give place to the reply of President Holden to Professor Proctor, whose letter was mentioned in the May MESSENGER. The dignity and courtesy of the answer in general is a credit to President Holden and the prominent position he occupies.

Orientation of Photographic Plates.—The late conference of astronomers, at Paris, passed the following resolutions in regard to the orientation of photographic negatives for star catalogues:

Besides the negatives giving the stars down to the fourteenth magnitude, another series should be made with shorter exposures, to assure a greater precision in the micrometrical measurement of the fundamental stars, and render possible the construction of a catalogue.

The supplementary negatives destined for the construction of the catalogue shall contain all the stars down to the eleventh magnitude inclusive. The Executive Committee shall determine the steps to be taken to insure that this condition is fulfilled.

The photographic plates to be used in formation of the catalogue shall be accompanied by all the data necessary to obtain the orientation and the value of its scale; and as far as possible these data shall be written on the plate itself.

Each plate of this kind shall show a well centered copy of a system of cross-wires to insure the determination of errors of the field, and to eliminate those which may be produced by a subsequent deformation of the photographic film.—*Nature.*

Variable Stars for June.—The following are the days on which the maxima of the variable stars named below occur as given by the *Astronomischen Gesellschaft*, except in the case of the fourth star which is at minimum :

June 2, T Hydræ
5, T Delphini
5, Vulpeculæ
7, W Scorpii
9, R Hydræ, *Min.*
14, U Virginis
June 17, S Persei
18, R Delphini
19, R Virginis
22, R Geminorum
23, R Orionis
23, R Capricorni
23, T Acquarii

New Minor Planet (266) was discovered by Palisa of Vienna. In G. M. T. its place, May 17.5, was,

$$\alpha = 16h\ 13m\ 12s$$
$$\delta = -\ 19^\circ\ 8'$$

Planets for June.—June 5, *Mercury* sets $8h$ $45.7m$ in the evening. June 30 the planet is in greatest eastern elongation, 25° 51′, good position for observation.

Venus sets June 5 at $10h$ $45.2m$.

Mars is too near the sun for observation.

Jupiter sets June 6 at $2h$ $12m$ A. M., and is stationary on the 22nd of the month.

Saturn was in conjunction with *Venus* May 30. Its distance is now so great that observation is unfavorable.

Uranus sets June 6 at $1h$ $29m$ A. M., is stationary on the 16th, and in quadriture with the sun June 20th.

Neptune is near the sun.

BOOK NOTICES.

Elementary Treatise on Determinants by William G. Peck, Ph. D., LL. D., Professor of Mathematics and Astronomy in Columbia College. Publishers, A. S. Barnes & Co., 1887; pp. 47.

The subject of Determinants has recently proved so valuable in the study of modern coördinate Geometry and kindred branches that there is a growing demand for a knowledge of the elementary principles of the subject, at least, either for those who teach, or those who wish to pursue modern methods of mathematical investigation. This little book treats of the elementary principles of the subject in the usual way through the definitions, principles and the properties of Determinants, minors, co-factors and reduction, and then follows the application to the solution of equations, consistence of equations, eliminants and homogeneous equations of the first degree. Following this the principles of Determinants are more fully developed and their operation on higher and unequal orders studied when the student is asked to apply to them the principles of the infinitesimal analysis which, though brief, forms an interesting feature of the work. Every teacher or live student of mathematics who has not already a good work on this theme may profitably examine this one.

Electricity and its Discoverers, by Rev. M. S. Brennan, of St. Louis. New York: D. Appleton & Co., publishers, 1885; pp. 191.

The recent extraordinary progress of knowledge in the different branches of Electricity and the possibilities of its extended uses in the practical affairs of life have turned the attention of all classes to it during the last few years. Many large and excellent books have been written, showing complex and intricate phases of theory belonging to the science for the benefit of the scholar and the specialist; but this little book has aimed only to give a knowledge of its principles to the exclusion of the mechanics of the subject. The author believes in the identity of all forms of electricity, and has depended on this relation in his treatment of it. He also speaks of it as woman's science, and gives credit to scholars of that class for original work in its development. Though not new, as a little book for the popular reader it may find a useful place in many libraries.

The Sidereal Messenger,

CONDUCTED BY WM. W. PAYNE,

Director of Carleton College Observatory, Northfield, Minnesota.

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THE TIME SERVICE OF THE LICK OBSERVATORY.

JAMES E. KEELER.*

For the MESSENGER.

The distribution of time by electric signals to railroad and telegraph companies, cities, private business firms, and other corporations or individuals, for commercial purposes, forms at the present day a part of the routine work of a number of the larger observatories in the United States. The advantages of these "time services" are manifold, and hardly need to be pointed out. A greater degree of accuracy and uniformity is secured than would be possible in any other way, and these are considerations which are daily becoming of greater importance. Chronometers and other time-pieces can be easily and accurately rated without the expensive instrumental outfit of an observatory and the skill required for its use, at any place where telegraphic communication can be had. The astronomer on his part finds the establishment of an already perfected system of time distribution of great service on occasions of special interest to him, such as transits and eclipses, when it is not usually possible to make the necessary observations at fixed observatories, and many observations of value are obtained which would otherwise be lost. A less obvious, but not the less important consideration, is the connection which is thus formed between the more abstruse work of the observatory and the ordinary affairs of every day life, bringing continually before the mind of the public the practical applications of astronomical science, and inspiring confidence in the accuracy of its methods.

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* In charge of the Time Service at Lick Observatory, Mt. Hamilton, California.

It was eminently proper that the Lick Observatory, the only large and completely organized institution of the kind on the Pacific coast, should be the center of such a system of time distribution for the surrounding country, sending its clock signals far enough to connect with the already established systems in the East. Accordingly in the early part of the year 1886, contracts for supplying time by automatic signals from a clock were made by Professor Holden, on behalf of the Lick trustees, with the Southern Pacific and other companies. I was invited to come out and take charge of the time service, and arrived on the mountain in April of the same year, but on account of unexpected delays in running the necessary telegraph lines and providing proper instruments, formal notice of the beginning of signals according to the contracts was not given until the first of January, 1887. Since that time the performance of everything connected with the system has been gradually improved until it is now perfectly smooth and satisfactory.

In the following account of the time service which has thus been established, I do not purpose to describe the well known methods for finding time, regulating clocks, etc., but to state the practice at this observatory, with such modifications of the usual methods as experience has shown to be advantageous, either on account of the instruments at command, or the unusual circumstances in which the observatory is placed. It will be understood that a great part of the apparatus which is used in the ordinary process of determining time is not specially intended for these purposes only, but forms an integral part of the instrumental outfit of the observatory.

The transit instrument is mounted in a room adjoining the large meridian circle house on the west. It has an object glass by Clark & Sons, 4.1 inches in clear aperture, and of 47.0 inches focus. Its optical qualities are excellent, and I have several times seen the companion of Antares with it, when that star was in transit. The mounting is by Fauth & Co., is well made and satisfactory. The piers are heavy, open frameworks of cast iron, covered by walnut jackets, and were used as at first set up for some time, but as the azimuth and level constants of the instrument varied considerably from night to night, they were subsequently filled with brick and Portland cement, by which their stability was greatly increased. The pivots are of bronze, 2.1 inches in diameter, and rest in agate Y's. They are sensibly cylindrical, but slightly unequal in size, the correction to the level constant on this account being —0.021*s* for clamp west. Part of the weight of the instrument is relieved by counterpoises. For reversing, which requires about 3½ minutes, a special carriage is provided. On one end of the axis is a circle 16 inches in diameter, coarsely divided on the edge to 20′, with a level alidade reading by two verniers to 1′. This circle is used as a finder, and is set to read zenith distances.

In 1885 the instrument was sent back to the makers and remodeled by them according to designs by Professor Holden, so as to serve also as a zenith telescope. A finely divided circle reading to 10″, with latitude level, was put on the other end of the axis, and the micrometer was made to rotate 90° between adjustable stops, so as to move in declination when desired. The finely divided circle is symmetrical with the finder, and marks the clamp end of the axis.

The glass reticle plate contains thirteen vertical lines, and one horizontal line to mark the center of the field. Of the vertical lines only the central group of five is used. The equatorial interval between two lines of this group is 1.170*s*, which is perhaps a little too small. At the zenith the interval is 1.472*s*. Whole revolutions of the micrometer, which carries a single spider's thread, are registered on a dial outside the box. The value of one revolution is 2.931*s*.

The illumination is effected by a lamp, placed on a stand about six feet from the west end of the axis. It is so easy to rotate the illuminating mirror in the telescope when the transit is reversed that I have not placed a lamp on the opposite side.

The system of electrically controlled finding clocks which has recently been introduced into the observatory deserves especial mention. All the standard clocks are in a room in the main building, and can only be seen from the window opening from this room into the long hall. At all instruments where an approximate knowledge of the sidereal time is necessary, clocks provided with electric control (Gardner's patent) have been set up, and connected by the same line with a Leclanché battery of four cells in the battery room. The wires lead also through a button near one of the standard sidereal clocks in the clock room. When this button is touched the minute and second hands of all the finding clocks in the circuit spring to zero, and it is only necessary to do this at the proper time, *i. e.*, at any even hour, to set all the clocks.

These finders, which will run several days without varying more than a few seconds, are even more convenient than a standard sidereal clock near at hand would be, as they can always be set to show the true sidereal time, and the observer is not troubled with allowance for error of the clock. The dial and hands are moreover larger, and easier to see at a distance. Other clocks of the same kind regulated to mean time, have been placed in the dwelling houses below the observatory, and are set by touching a button in front of the standard mean time clock. The same battery furnishes the current for these clocks, as well as for other electrical instruments in the observatory which are only occasionally brought into use. The cost of each clock, including the electric control, was $25.

The sidereal finder in the transit house is on a stone pier, originally intended for a standard clock, near the wall on the west side of the transit, where it can be seen from any part of the room.

The chronograph ordinarily used is on the north side of the same room. It was made by Fauth & Co., and its performance is good. It is unnecessary to say that everything tending to the convenience and comfort of the observer has been provided.

The meridian of the Fauth transit has been adopted as the standard meridian of the observatory. The dome of the 12-inch equatorial has been connected by the Coast Survey with the primary triangulation of the Pacific coast, and reducing the coördinates of this point to the center of the mercury basin of the transit, we have for the position of the latter,

$$\varphi = 37^\circ \quad 20' \quad 24.56''$$
$$\lambda = 8h \quad 6m \quad 34.29s \text{ west of Greenwich.}$$

The latitude determined by Prof. Comstock from a preliminary reduction of observations made with the meridian circle during the summer of 1886 differs from that of the Coast Survey by only 0.4″.*

The observatory possesses five astronomical clocks and five chronometers. All of the clocks are mounted on piers in a room specially constructed for the purpose in the main building, where they are protected from dust, fog and sudden changes of temperature. The sidereal clock which has been most used in connection with the time service is mounted in a closet, itself with double walls and doors, on the west side of the clock room. It was made by E. Dent & Co. of London, has a gravity escapement and mercurial pendulum, and is of very beautiful workmanship throughout. On account of changes made in the positions of the clocks, and on account of experiments which were made for the adjustment of the temperature compensation of their pendulums, etc., none of these clocks has been allowed to run for any length of time without disturbance. Below are given the rates of the Dent clock for the longest interval during which it has been suffered to run uninterruptedly in its present position.

Date.	Mean Temp.	δT	Date.	Mean Temp.	δT
	°	s		°	s
1887, March 1...	41	— 0.08	1887, March 20..	52	— 0.03
4...	44	— 0.06	25..	53	— 0.11
6...	45	— 0.08	27..	54	— 0.10
8...	45	— 0.13	29..	56	— 0.05
10...	48	— 0.05	31..	56	+ 0.03
13...	51	+ 0.09	April 3..	57	+ 0.01
15...	52	+ 0.05	5..	58	— 0.04
18...	54	+ 0.03	7..	58	— 0.02

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* SIDEREAL MESSENGER, December, 1886.

The average height of the barometer was 25.80 in., and the extreme variation 0.30 in.

The other sidereal clocks have been occasionally used. Two by Hohwü of Amsterdam, with dead beat escapements, run quite as well as the Dent clock.

The mean time clock was made by E. Howard & Co. of Boston. It is mounted on a pier in the clock room, in front of the window opening into the main hall, where it can be seen from the switch board outside. This clock is kept 6*m* 34.29*s* fast of the local mean time of the transit instrument, and therefore indicates the time of the 120th meridian from Greenwich, or Pacific Standard Time. In construction it is similar to the clock used for the same purpose at the Washburn Observatory in Madison, Wisconsin. The pendulum carries a cluster of four steel jars filled with mercury, and has a dead beat escapement. Through inexcusable carelessness of construction the verge of the escapement, when the instrument was received from its makers, was so put on its arbor that the teeth of the scape-wheel struck sometimes on the jeweled pallets and sometimes on the steel frames in which the pallets are set. The rate of the clock was naturally very irregular. After this had been remedied by resetting the verge, the clock performed very well, and has since given entire satisfaction. The pendulum is provided with a nut turned by short spokes, traversing a thread cut on the pendulum rod a short distance above the bob, for regulating its rate, but as the act of turning this nut disturbs the pendulum to some extent, and the rate is greatly affected by variations in the arc of vibration, its use has been abandoned and the rate adjusted entirely by the use of weights, the smallest of which produces a change of 0.1*s* per day. The weights most frequently used for correcting small errors are cut to produce a change of 0.1*s* per hour. Each is provided with a projecting pin for lifting and weighs 4.64 grammes. For convenience the clock is kept with a slightly losing rate, and the application of one of these weights to the top of the pendulum is required about an hour every day.

The apparatus for repeating the beats of the clock by electricity is arranged as follows: The scape-wheel arbor carries a steel wheel originally with 30 teeth, the 30th being cut away. At every even second except the 58th one of these teeth lifts a spring and breaks an electric circuit. The second wheel of the train drives a five-minute wheel with smooth rim, in which a notch ten seconds long is cut. Immediately after the fiftieth second, preceding every even fifth minute a catch falls into this notch and short-circuits the two-second break, so that the latter does not operate until the beginning of the next minute. The relay connected with the clock beats therefore even seconds of Standard Pacific Time, omitting the 58th second of *every* minute, and the 52, 54, 56 and 58th seconds preceding every *even fifth* minute.

On the switch board outside the clock room are, besides other instruments, five "back contact" relays of low resistance, one for each of the five clocks. From these relays, which take the place of the clocks for all time observations, either make or break-circuit signals can be sent, no matter what the arrangement of the electric apparatus in the clocks may be. Only feeble currents are sent through the clocks. The switch board need not be described. Its arrangement is so simple, notwithstanding the number of instruments leading to it, that any desired connection can be made in a moment even in the dark, and with any battery power required. At the left end of the switch board is a relay marked "receiving magnet," which through the day is connected with the back-contact relay of the No. 7 or mean time clock. Through the relay points of the receiving magnet passes the main line from the observatory to San Jose, and the current on this line is therefore interrupted simultaneously with the beats of the No. 7 clock relay.

The observatory is connected with a large relay for repeating the time signals in the railroad office of the Southern Pacific Company in San Jose, by twenty miles of No. 9 iron wire, and it is responsible for the proper working of this relay. This telegraph line runs for a great part of the way through wild and mountainous country, and for several miles at the upper end is exposed in winter to violent storms. The care of the transmitting line is therefore much greater than in most other time services, where sometimes the instrument at which the responsibility of the observatory ceases is in the building itself.

The relay in San Jose opens or closes simultaneously four different circuits, which will be described separately further on. It is at one end of the operator's table in the railroad office, and is enclosed in a glass case to exclude the dust. Above it on a shelf are four indicators, one for each pair of relay points, through which the circuits interrupted by the relay are led to show that good contact is made at every beat of the instrument. Four corresponding switches outside the glass case allow the relay points to be cut out of circuit when the clock signals are not desired on the local lines. The relay is wound to a resistance of 150 ohms. The responsibility of the observatory for the proper transmission of time-signals ceases at this point.

The line to San Jose is led at the observatory through a lightning arrestor (not often needed in this climate) on the switch board, and a relay having a resistance of 150 ohms, placed on a bracket near the clock room window. This relay serves merely as an indicator. The end of the line at the observatory is grounded by connection with the system of water pipes. The resistance of the line, including the indicator and the four point relay at the other end, is about 800 ohms. A battery of 18 gravity cells is used. In fine weather 5 of these cells are just sufficient to work the instrument in San Jose.

On the same poles with the time line is strung a telephone wire of the same size, also the property of the observatory, connecting the dwelling house on the mountain with the central telephone office in San Jose, which is about a quarter of a mile nearer to the observatory than the railway station. There are three intermediate telephone stations on this line, at approximately equal distances apart, known as "Smith's Creek," "Snell's Ranch" and the "Junction House," in the order of distance from the summit. The two wires are liable to cross in severe storms, by breaking of the poles or insulators, and it is a matter of importance to have some means of locating the fault.

When both lines are in good order, the beats of the clock can be distinctly heard in any of the telephones, by induction between the two wires, although they are not loud enough to interfere with conversation. When a cross occurs, however, the powerful current from the main battery divides between the lines, a part reaching the earth through both ends of the telephone wire, and violent concussions are produced in all the telephones. If the observatory end of the telephone wire is then disconnected from the ground, the beats will cease to be heard (except very faintly) at stations between the cross and the observatory, and continue as before between the cross and San Jose. In this way the two stations can be determined between which the cross occurs.

Accidents are most liable to happen between the summit and Smith's Creek, where the line is most exposed to severe weather, and it is desirable to locate the position of a cross here with some degree of precision. A tangent galvanometer is kept near the clock room at the observatory, and can be introduced at any time into the main line. In fine weather, with the line in good condition, the 18 cells will produce a deflection of the galvanometer needle of 47°, and the battery is cleaned when the deflection falls below 40°. When a cross occurs the current divides as above mentioned, the total resistance of the circuit is diminished, and the galvanometer deflection is increased. By observing the deflection (1) with both ends of the telephone line open, (2) with the observatory end open and the San Jose end grounded, (3) with the San Jose end open and the observatory end grounded, the position of the cross can be determined with considerable accuracy. The changes in the connection of the San Jose end are easily arranged by communication with the attendant in the central telephone office, the clock being temporarily disconnected for the purpose. The battery of 18 cells is sufficiently powerful to work the relay in San Jose, even when a large part of the current is diverted by contact with the telephone wire, but all communication on the latter under these circumstances is stopped.

The San Jose relay is allowed to beat from 9 A. M. to 6 P. M. every day except Sundays. The mean time and sidereal clocks are compared just before 9 A. M., and the former adjusted so that its error shall be as nearly as possible zero at noon, when a special signal is sent, according to the following programme: At 23*h* 50*m* the usual break-circuit signal is changed to a make-circuit one (as such signals are less liable to interruption in telegraphing), by reversing the switch of the back-contact relay, and made long by proper adjustment of the tension of the armature springs. At 23*h* 56*m* the clock is cut off, and the armature of the receiving magnet is rattled or *time* called until 23*h* 57*m*. The clock remains disconnected from 23*h* 57*m* to 23*h* 57*m* 58*s*, when it is switched in again, so that the first beat heard at a distance after the pause of one minute is 23*h* 58*m* 0*s*. The clock is then allowed to beat two minutes, omitting as usual the 58th second of the 58th minute and the 52, 54, 56 and 58th seconds of the 59th minute. At 0*h* 0*m* 1*s* the clock is cut off, and the last beat heard at a distance is therefore 0*h* 0*m* 0*s* or noon. The usual break-circuit signals are begun again at the end of about five minutes.

It is evident that the time can be told precisely at any distant instrument, at any time between 9 A. M. and 6 P. M. provided that a person there has the means of identifying the fifth minute indicated by the long pause of 10 seconds, *i. e.*, provided he knows the time already to within 2½ minutes.

Besides the standard sidereal clock, two mean time chronometers are used, and are compared with the Howard clock three times daily; at 9 A. M., at noon (immediately after the special signals) and at some time in the evening. One of the chronometers, Negus 1719, has nearly as uniform a rate as a good clock. The other is a fair instrument. The comparisons are made by ear, using a sounder in the clock room worked by the relay points of the receiving magnet. Taken in connection with the morning comparisons, they give a check on the error of the noon signals.

The sidereal clock correction is determined, on an average, about every other evening. In arranging a system of observation which should give the greatest degree of accuracy with the least expenditure of time and labor, regard was paid to the following facts, due to the peculiar situation of the observatory and the manner in which the instruments are mounted: 1. For the greater part of the year, during the summer and autumn months, observations can be made whenever desired, so far as the atmospheric conditions are concerned. 2. During the winter months very heavy fogs prevail, which, however, seem often to extend very little above the top of the mountain, and frequently bright stars near the zenith can be very well observed when the whole mountain top is shrouded in dense clouds. 3. Facilities were at hand for erecting a suitable mark from which the azimuth and collimation of the instrument could be obtained at any time.

Twenty-nine feet immediately south of the Fauth transit, on a brick pier, is the 5-inch object glass of the photoheliograph, an instrument in all respects similar to those employed by the parties sent out by the United States government to observe the last Transit of Venus, and forty feet south of this, on a pier in a photographic laboratory constructed for the purpose, is the plate-holder of the same instrument. The objective thus forms an admirable collimator for the transit, and as both piers are substantially built and well protected from the weather, it was probable that the azimuth of a mark observed through the collimator would be subject to but little change. A mark was made by fastening on to an adjustable iron frame secured to the base plate of the photographic plate-holder a piece of glass cut from an old "fogged" dry plate, on which fine lines had been ruled with a knife so as to scratch through the collodion film. The visual focus of the photoheliograph lens is about three inches longer than the photographic focus, so that the mark does not at all interfere with the plate-holder. When the mark is illuminated by a hand lamp and viewed with the transit instrument, the scratches appear as fine, bright vertical lines on a darker background, and they can be bisected by the micrometer thread with great precision.

Experience showed that the azimuth of the mark was very nearly constant. It has been determined carefully about once a month since first set up, but the changes have hardly been more than the probable error of observation. In determining the azimuth of the instrument from that of the mark, the central line of the latter is assumed to be 0.13*s* west of south.

For the regular routine determinations of the clock correction for time-service purposes, I have arranged the stars of the Berliner Jahrbuch and the American Ephemeris in fifty-three lists, which are consequently about half an hour apart. The ideal construction which I have endeavored to approach as closely as possible in making out these lists, is as follows: Each list consists of four stars, not differing more than twenty minutes in Right Ascension, arranged in two pairs, one pair to be observed in the direct and one in the reversed position of the axis. Each pair consists of a north and a south star culminating at zenith distances not exceeding 15°, and such that the mean of the clock corrections given by the members of the pair shall be free from error of azimuth. The mean of the four stars is then almost entirely free from errors of azimuth and collimation.

The following lists very nearly fulfil these conditions. The column B, for the level factor, has been omitted.

No. 10.

Star.	Mag.	R. A.	Dec.	Zenith Dist.	A.	C.
		h *m* *s*	° ′	° ′		
ε Persei...............	3.3	3 50 16	+ 39 41	2 21 N.	— 0.05	+ 1.30
ξ Persei..............	4.0	3 51 38	+ 35 28	1 52 S.	+ 0.04	+ 1.23
A′ Tauri...............	4.7	3 58 1	+ 21 46	15 34 S.	+ 0.29	+ 1.08*
c Persei...............	4.0	4 0 28	+ 47 25	10 05 N.	— 0.26	+ 1.48*

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* Usually the sin of C after reversing is — instead of + as given above.—Ed.

No. 39.

Star.	Mag.	R. A.	Dec.	Zenith Dist.	A.	C.
		h m s	° ′	° ′		
β Draconis	2.7	17 27 53	+ 52 23	15 03 N.	— 0.40	+ 1.64
α Ophiuchi	2.0	17 29 41	+ 12 39	24 41 S.	+ 0.43	+ 1.02
ι Herculis	3.3	17 26 17	+ 46 04	8 44 N.	— 0.22	+ 1.44*
μ Herculis	3.3	17 42 2	+ 27 47	9 33 S.	+ 0.19	+ 1.13*

These are among the best of the lists, but there are many others nearly as good, and in very few of them is the mean of the clock corrections from the four stars affected by so much as half the azimuth error. With lists like the above it has been my practice to merely verify the position of the instrument by inspection of the mark, and if no change is perceptible to use the azimuth constant of the last measurement. For determining the level constant, Fauth & Co. have provided two excellent levels, the least sensitive of which has hitherto been used. One division of the scale = 1.04″= 0.070*s*. The radius of curvature is 540 feet.

The probable error of a clock correction from one of these lists cotaining only Berliner Jahrbuch stars, with the instrumental constants well determined, is 0.02*s*. If the azimuth or collimation is in error, the probable error of the clock correction, determined from the discrepancy of the results of the individual stars, will be increased, but the absolute degree of accuracy of the clock correction remains the same. An error in azimuth will be pointed out by systematic differences between the north and south stars; one in collimation by a difference between the pairs.

Of course if one star out of the list is lost, the clock correction under these circumstances will be in error. In summer this is not likely to happen. In winter very frequently the regular list cannot be observed, and instead the brightest stars must be selected. The azimuth mark is then very useful, as

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* Usually the sin of C after reversing is — instead of + as given above.—Ed.

by means of it a good value of the clock correction can be obtained from a single star. The collimation of the transit telescope is fairly constant.

As the azimuth does not have to be deduced from the observations, the reductions are the simplest possible, and I doubt whether an equal number of observations could be made to give a more accurate value of the clock correction than that obtained by this arrangement.

Having now described the methods practiced at the observatory for the transmission of accurate time, I pass to a consideration of the different uses made of the signals outside of the observatory. Of the four pairs of points of the large relay in San Jose, one is used by the Southern Pacific Company, one by the Sunset Telephone Company, one by a jeweler in San Jose, and one was intended for use by the South Pacific Coast Railroad. The recent purchase of the latter road by the Southern Pacific Company leaves at present one pair of points unused.

The noon time signals are sent automatically over the Southern Pacific Company's telegraph lines to Oakland, where an operator, tapping his key in coincidence with the clock beats on his sounder, sends them over the general system of the company, causing the beats to be heard in every station on the lines as far as Ogden and El Paso. This method of repeating, although not quite so accurate for scientific purposes as an automatic one, is more certain and better adapted to the requirements of railroading. On the pier in Oakland, where the signals are received, a chronometer is kept which is rated by means of the signals, and is used in case of any accident to the lines connecting the office there with San Jose. Large clocks at the ferry slips in San Francisco, which are furnished with the correct time by signals transmitted by cable from Oakland, regulate the departure of the ferry boats, and also of the various lines of cable cars which start from the foot of Market Street.

The second pair of points is connected by a wire with the central telephone office in San Jose, about a quarter of a mile from the railroad station, and the clock signals can be repeated on a sounder in the office, or the current from four Fuller's cells sent directly through any telephone in San Jose, San Francisco, or elsewhere in the system. The city hall clock of San Jose is struck, by an electric arrangement, from the telephone office.

The third pair of points is connected by a private wire with the shop of Mr. W. D. Allison, a leading jeweler in San Jose, and repeats the clock signals on a relay near his regulator. Mr. Allison is introducing a local system of time distribution, with electrically controlled clocks like those already described in use at the observatory.

It will be seen that the observatory signals are sent over a great extent of country. There has nevertheless been no defect in the proper working of the whole and all the arrangements now in use have been found satisfactory. The only causes liable to interrupt the regular transmission of the signals are accidents in winter to the line connecting the observatory with San Jose, and it has been shown that means are provided for the detection of any fault here, and its speedy remedy.

It may be of interest, in closing, to give a few notes on the atmospheric conditions prevailing at the observatory at different seasons of the year. Much has been written on this subject, but the previous tests were all made during the most favorable period. The time and other observations which have now been made here continuously for some fourteen months, give the means for making a fairly accurate estimate of the chances for good observing throughout a complete cycle of the seasons.

An estimate of the "seeing" was made each night on a scale of 5; 5 representing perfect definition and 0 impossibility of observation. These estimates refer to the state of the atmosphere up to midnight only, and therefore represent conditions more unfavorable than those which actually obtain, as high winds after sunset, which are almost invariably accompanied by bad definition, frequently die away after midnight, and the "seeing" becomes good in the early hours of the morning.

The difference between the summer and winter months in regard to the suitability of the nights for astronomical observation is enormous. During the summer a bad night is exceptional, and the average "seeing" is at least 4 on the above scale. On a fine night I have watched the image of Polaris crossing the lines of the transit instrument with as little tremor as if it were a material point moving on the reticle. I have seen a bright star (Vega) with a small spy-glass, when at a true zenith distance of 91°. Stars culminating at a zenith distance of 87° can be well observed with the transit. This estimate of the seeing during the summer nights does not, I think, differ much from those of Mr. Burnham and Professor Comstock.

During the winter, on the contrary, the atmospheric conditions are very unfavorable, and for two months it is doubtful whether astronomical work could be pursued to advantage. The following table shows the record for the winter of 1886-87:

	Dec.	Jan.	Feb.	Mar.
Average seeing on scale of 5	2	2	1½	2½
Number of good nights (rated at 4)	3	2	2	5
Number of fair nights (rated at 3)	8	11	8	16
Number of nights on which observation was impossible	6	5	13	5

THE RELATION OF AEROLITES TO SHOOTING STARS.*

PROF. DANIEL KIRKWOOD.

The writer more than twenty years since gave reasons for believing that shooting stars, fire balls and meteoric stones move together in the same orbits.† The facts then collected were deemed sufficient to sustain the theory advanced, or at least to give it a high degree of probability. This view has been rejected, however, by several eminent astronomers, and

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* Read before the American Philosophical Society, April 15, 1887.

† Meteoric Astronomy, Chap. v.

especially by the present Astronomer Royal for Ireland, the distinguished author of "The Story of the Heavens." He remarks: "It is a noticeable circumstance that the great meteoric showers seem never yet to have succeeded in projecting a missile which has reached the earth's surface. Out of the myriads of Leonids, of Perseids, or of Andromedes, not one particle has ever been seized and identified. Those bodies which do fall from the sky to the earth, and which we call meteorites, never come from the great showers, so far as we know. They seem indeed to be phenomena of quite a different character to the periodic meteors" (Story of the Heavens, p. 349).

In pointing out the coincidence in the epochs of shooting stars and meteoric stones,* the present writer neglected to assign an obvious reason for the fact that star showers are so seldom observed at the same time with the fall of aerolites: a majority of the latter have been seen in the day time, when ordinary shooting stars would be invisible. At night, however, the phenomena have more than once occurred at exactly the same time. The writer called special attention to one of these epochs as long since as 1881.† In describing the shower of April meteors as it occurred in the year 1094, the historian says: "At this period so many stars fell from heaven that they could not be counted. In France the inhabitants were amazed to see one of them of great size fall to the earth, and they poured water on the spot, when to their exceeding astonishment, smoke issued from the ground with a hissing noise."‡ A few other examples are given below:

(1) During the meteoric display which continued through three consecutive nights in the latter part of October, A. D. 585, a globe of fire, sparkling, and producing a great noise, fell upon the earth.§

(2) A simultaneous fall of aerolites and shooting stars is

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* Metr. Astr., pp. 58-64.
† Science, Feb. 5, 1881, p. 59.
‡ Am. Journ. of Sci., Jan., 1841, p. 356.
Quetelet's Physique du Globe, p. 291.

indicated by the phenomena of 1029, as described in the catalogues of Herrick and Quetelet.

(3) But without quoting other records which imply the existence of aerolites and ordinary meteoric matter in the same streams or clusters, it is sufficient to refer to the recent and very decisive phenomena of November 27, 1885.* During the periodic star shower from the fragments of Biela's comet a mass of meteoric iron weighing about ten pounds was seen to fall near Mazapil, Mexico, in lat. 24° 35′ N., long. 101° 56′ 45″ W. from Greenwich. The evidence afforded by the phenomena of 1094 and 1885, apart from the other cases cited, renders the co-existence of large and small masses in the same meteor streams almost infinitely probable.

THE LIFE AND ACHIEVEMENTS OF ALVAN CLARK.

THE EDITOR.

During the last month much has been said, in various public journals, concerning the life and the achievements of Alvan Clark, senior, of Cambridge, Massachusetts, whose death occurred at a little past 3 o'clock on the morning of August 19, 1887. Most of the accounts which have come to our notice have been brief and fragmentary, and sometimes inaccurate in statement of fact. For these reasons another attempt is made for the benefit of the readers of the MESSENGER.

Alvan Clark was born at Ashfield, Mass., March 8, 1804. His early life was spent on a farm, and all the education he received was obtained in the public schools of western Massachusetts. At the age of twenty-two he secured the situation of calico engraver at Lowell, Mass., having an aptitude for drawing which he developed without assistance. For nine years he followed this occupation, and in 1835 he removed to Boston and opened a studio for painting miniatures. For twenty years he followed the profession of artist, producing some of the best portrait pictures made by the professional

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* Am. Journ. of Sci., March, 1887, p. 221.

artists of his day. At the age of forty he was led to the study of technical optics by the following circumstance, in his own words:

"My son Alvan G. Clark was at Andover studying to be an engineer. His young mind seemed to be absorbed in telescopes. I was a portrait painter then, and I began to study mechanics and astronomy so as to instruct my boy. We experimented together and succeeded in making a reflecting telescope. One of the Cambridge professors was much pleased with some instruments we made, and when we suggested to him that we would like to manufacture improved instruments, he gave us great encouragement and we went ahead."

Speaking of the character of the work which the Clarks did from the first, Professor Holden, director of Lick Observatory, in a late article in the *Examiner*, fittingly says:

> "Like the elder Herschel he was obliged to manufacture his own telescopes. Like Herschel, too, he began by making reflectors, but he soon abandoned them for refracting telescopes. His early efforts were most successful, and he began to make 6 and 8-inch object glasses of an unheard of excellence and precision. One of these found its way to England and came into the possession of the Rev. W. R. Dawes, an amateur of astronomy and a devoted observer of double-stars. The extraordinary defining power of Clark's object glasses admirably fitted them for this kind of work. It is no exaggeration to say that the separating power of one of Clark's 6-inch object glasses of 1850 was equal to the separating power of the nine-inch objectives of that day. Dawes made Clark's telescopes known in England by his own observations and by recommending his friends to purchase them, and it was not long before quite a number of Clark's object glasses were scattered over England and Scotland.
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> "They were mostly of small size, 6 to 8 inches. By this time Clark was able to give up his portrait painting, and to devote his life and those of his two sons, George and Alvan G. Clark, to the manufacture of object glasses exclusively.
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> "It must be remembered that this is no common manufacture; but that it is a process which involves special mechanical talent of the highest order, beside mathematical and optical learning.
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> "To give an idea of the perfection of a good object glass, it

may suffice to say that its geometrical figure is so precise that if the glass were to be rubbed with the finger only round and round in a circle for five or ten minutes there would be a perceptible falling off in the precision of the star-images seen with it; or to put the same thing in another way, I may say that theoretical optics shows us just how near two bright points (stars) may be and yet be seen separated with an object glass of a given aperture. And the Clarks long ago succeeded in making every one of their telescopes practically do what theory demanded. That is, the telescopes were perfect.

"After making the smaller telescopes, the Clarks began to make 12 inch object glasses, and some of these of their manufacture are probably the most perfect of any in the world. Mr. Clark has often said to me that the 12-inch objective at Middletown, Conn., and the 12-inch at the Lick Observatory (formerly the property of my friend Henry Draper), were the best that he had ever seen. A 6-inch which he made in 1870 for Mr. Burnham of Chicago, has had a wonderful history—no less than 1,000 new and difficult double-stars having been discovered by its use.

"The 18-inch telescope now at Chicago was finished just before the war for the University of Mississippi, and was at once made noted by the discovery of a companion star to Sirius, which theory predicted, but which no one had detected before it was seen by Alvan Clark, Jr.

"Alvan Clark, Sr., himself discovered quite a number of difficult and interesting double stars. In 1873 the great 26-inch telescope was made by the Clarks for the Naval Observatory in Washington. With this telescope Professor Newcomb and myself observed the faint satellites of Uranus, which had not been seen by any one but their discoverer, Lassell; and with this Professor Hall discovered the faint satellites of Mars. Some idea of the seeing power of such instruments may be had when I say that the faintest satellite of Mars has just such an angular magnitude as a small terrestrial school globe would have if it were placed on the State House in Boston and looked at from the Capitol at Washington.

"The 26¼-inch for the University of Virginia, the 23-inch for Princeton College, the 30-inch for the St. Petersburg Observatory soon followed, and finally the 36-inch for Mt. Hamilton."

The varying sizes of the lenses made by the Clarks range in inches as follows: 6, 8¼, 9⅓, 12, 15½ (Washburn), 18½ (Chicago Astronomical Society), 23 (Princeton College), 26 (U. S.

Naval Observatory), 26¼ (University of Virginia), 30 (Pulkowa Observatory, Russia), and 36 (Lick Observatory, Cal.). The cost of the Washington telescope was $46,000, the Pulkowa glass $33,000, and the Lick glass was $50,000. These large figures indicate another way of measuring the opticians' skill of which we are now speaking.

If we remember that this was the work of a man who had never seen a lens in the process of construction in the shop of another maker, we have an example of a self-made man emphatically. His skill was gained and his reputation earned by steady, hard and persistent work. If he had naturally a keen eye, a quick understanding and a most delicate touch, they were each and all used in long continued endeavor which was nothing less than the genius of successful and conquering patience. His skill as an observer was recognized by complimentary letters from the distinguished English astronomer Mr. Dawes, on account of his discovery of many new double-stars. In consequence of his discovery of the companion to Sirius, he was awarded the Lalande medal by the French Academy of Sciences, and for the successful construction of the 30-inch Pulkowa glass he was tendered a vote of thanks by the Russian Imperial Academy of Sciences, and the Czar of Russia gave him a gold medal.

Such was the career of Mr. Clark. Of his personal characteristics Professor Holden has spoken well in the article above referred to. He says:

"Mr. Clark was a man of perfectly plain and simple habits, but of singular distinction of face and manners. His countenance always showed the kindest and most sincere interest in his listeners and the truest enthusiasm for his art. His friends and admirers will remember the distinctions of his intellect and of his character longer than the memory of the honors given him by foreign governments will last. But so long as astronomy is followed by cultivated and intelligent man, so long will his real and conspicuous advances in the art of making telescopes be remembered and celebrated. His was a long, an honorable, a simple and a good life. It has come to a natural close, but it has left behind it kindly memories on all who knew him, and an undying fame in the annals of science."

THE YALE COLLEGE MEASUREMENT OF THE PLEIADES.*

A. M. CLERKE.

The Messrs. Repsold have established, and for the present seem likely to maintain, a practical monopoly in the construction of heliometers. That completed by them for the Observatory of Yale College in 1882 leaves so little to be desired as to show excellence not to be the exclusive result of competition. In mere size it does not indeed take the highest rank; its aperture is of only 6 inches, while that of the Oxford heliometer is of 7½; but the perfection of the arrangements adapting it to the twofold function of equatorial and micrometer, stamps it as a model not easy to be surpassed. Steel has been almost exclusively used in the mounting. Recommened as the material for the objective-cell by its quality of changing volume under variations of temperature nearly *pari passu* with glass, its employment was extended to the telescope-tube and other portions of the mechanism. The optical part of the work was done by Merz, Alvan Clark having declined the responsibility of dividing the object-lens. Its segments are separable to the extent of 2°, and through the contrivance of cylindrical slides (originally suggested by Bessel) perfect definition is preserved in all positions, giving a range of accurate measurement just six times that with a filar micrometer. (Gill, "Encyc. Brit." vol. xvi, p. 253; Fischer, *Sirius*, vol. xvii, p. 145.)

This beautiful engine of research was in 1883 placed in the already practiced and skillful hands of Dr. Elkin. He lost no time in fixing upon a task suited both to test the powers of the new instrument and to employ them to the highest advantage.

The stars of the Pleiades have from the earliest times, attracted the special notice of observers, whether savage or civilized. Hence, on the one hand, their prominence in stellar

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* "Determination of the Relative Positions of the Principal Stars in the Group of the Pleiades." By William L. Elkin. Transactions of the Astronomical Observatory of Yale University. Vol. I, Part I. (New Haven: 1887.)

mythology all over the world; on the other, their unique interests for purposes of scientific study and comparison. They constitute an undoubted cluster; that is to say, they are really, and not simply in appearance, grouped together in space, so as to fall under the sway of prevailing mutual influences. And since there is, perhaps, no other stellar cluster so near the sun, the chance of perceptible displacements among them in a moderate lapse of time is greater than in any other similar case. Authentic data regarding them, besides, have now been so long garnered that their fruit may confidently be expected at least to begin to ripen.

Dr. Elkin determined, accordingly, to repeat the survey of the Pleiades executed by Bessel at Königsberg during about twelve years previous to 1841. Wolf and Pritchard had, it is true, been beforehand with him; but the wide scattering of the grouped stars puts the filar micrometer at a disadvantage in measuring them, producing minute errors which the arduous conditions of the problem render of serious account. The heliometer, there can be no doubt, is the special instrument for the purpose, and it was, moreover, that employed by Bessel; so that the Königsberg and Yale results are comparable in a stricter sense than any others so far obtained.

One of Bessel's fifty-three stars was omitted by Dr. Elkin as too faint for accurate determination. He added, however, seventeen stars from the Bonn *Durchmusterung*, so that his list comprised sixty-nine, down to 9.2 magnitude. Two independent triangulations were executed by him in 1884-85. For the first, four stars situated near the outskirts of the group, and marking the angles of a quadrilateral by which it was inclosed, were chosen as reference-points. The second rested upon measures of distance and position-angle outward from Alcyone (η Tauri). Thus, two wholly unconnected sets of positions were secured, the close accordance of which testified strongly to the high quality of the entire work. They were combined with nearly equal weights, in the final results. A fresh reduction of the Königsberg observations, necessitated by recent improvements in the value of some of the corrections employed, was the preliminary to their comparison with those made, after an iterval of forty-five years, at Yale College. The conclusions thus laboriously arrived at are not devoid of significance, and appear perfectly secure, so far as they go.

It has been known for some time that the stars of the Pleiades possess a small identical proper motion. Its direction, as ascertained by Newcomb in 1878, is about south-south-east; its amount is somewhat less than six seconds of arc in a century. The double-star 61 Cygni, in fact, is displaced very nearly as much in one year as Alcyone with its train in one hundred. Nor is there much probability that this slow secular shifting is other than apparent: since it pretty accurately reverses the course of the sun's translation through space, it may be presumed that the *backward* current of movement in which the Pleiades seem to float is purely an effect of our own *onward* traveling.

Now the curious fact emerges from Dr. Elkin's inquiries that six of Bessel's stars are exempt from the general drift of the group. They are being progressively left behind. The inference is obvious, that they do not in reality belong to, but are merely accidentally projected upon, it: or rather, that it is projected upon them; for their apparent immobility (which, in two of the six, may be called absolute) shows them with tolerable certainty to be indefinitely more remote—so remote that the path, moderately estimated at 21,000,000,000 miles in length, traversed by the solar system during the forty-five years elapsed since the Königsberg measures, dwindles into visual insensibility when beheld from them! The brightest of these six far-off stars is just above the eighth (7.9) magnitude; the others range from 8.5 down to below the ninth.

A chart of the relative displacements indicated for Bessel's stars by the differences in their inter-mutual positions as determined at Königsberg and Yale, accompanies the paper before us. Divergences exceeding 0.40″ (taken as the limit of probable error) are regarded as due to real motion; and this is the case with twenty-six stars besides the half-dozen already mentioned as destined deserters from the group. With these last may be associated two stars surmised, for an opposite reason, to stand aloof from it. Instead of tarrying behind, they are hurrying on in front. An excess of the proper movement of their companions belongs to them; and since that movement is presumably an effect of secular parallax, we are justified in inferring their possession of an extra share of it to signify their greater proximity to the sun. Hence, of all the stars in the Pleiades these are the most likely to have a measurable annual parallax. One is a star a little above the seventh magnitude, distinguished as *s* Pleiadum; the other, of about the eighth, is numbered 25 in Bessel's list. Dr. Elkin has not omitted to remark that the conjecture of their disconnexion from the cluster is confirmed by the circumstance that its typical spectrum (as shown on Prof. Pickering's plates) is varied in *s* by the marked character of the K line. The spectrum of its fellow-traveler (No. 25) is still undetermined.

It is improbable, however, that even these nearer stars are practicable subjects for the direct determination of annual parallax. By indirect means, however, we can obtain some idea of their distance. All that we want to know for the purpose is the *rate* of the sun's motion; its *direction* we may consider as given with approximate accuracy by Airy's investigation. Now, spectroscopic measurements of stellar movements of approach and recession will eventually afford ample materials from which to deduce the solar velocity; though they are as yet not accurate or numerous enough to found any definitive conclusion upon. Nevertheless, M. Homann's preliminary result of fifteen miles a second as the speed with which our system travels in its vast orbit, inspires confidence both from the trustworthiness of the determinations (Mr. Seabroke's) serving as its basis, and from its intrinsic probability. Accepting it provisionally, we find the parallax of Alcyone = about 0.02″, implying a distance of 954,000,000,000,000 miles, and a light-journey of 163 years. It is assumed that the whole of its proper motion of 2.61″ in forty-five years is the visual projection of our own movement towards a point in R. A. 261°, Decl. + 25°.

Thus, the parallax of the two stars which we suspect to lie between us and the stars forming the genuine group of the Pleiades, at perhaps two-thirds of their distance, can hardly exceed 0.03″. This is just half that found by Dr. Gill for ζ Toucani, which may be regarded as, up to this, the smallest annual displacement at all satisfactorily determined. And the error of the present estimate is more likely to be on the side of excess than of defect. That is, the stars in question can hardly be much nearer to us than is implied by an annual parallax of 0.03″, and they may be considerably more remote.

Dr. Elkin concludes, from the minuteness of the detected changes of position among the Pleiades, that "the hopes of obtaining any clue to the internal mechanism of this cluster seem not likely to be realized in an immediate future;" remarking further: "The bright stars in especial seem to form an almost rigid system, as for only one is there really much evidence of motion, and in this case the total amount is barely 1″ per century." This one mobile member of the naked-eye group is Electra; and it is noticeable that the apparent direction of its displacement favors the hypothesis of leisurely orbital circulation round the leading star. The larger movements, however, ascribed to some of the fainter associated stars are far from harmonizing with this preconcieved notion of what they ought to be. On the contrary, so far as they are known at present, they force upon our minds the idea that the cluster may be undergoing some slow process of disintegration. M. Wolf's impression of incipient centrifugal tendencies among its components certainly derives some confirmation from Dr. Elkin's chart. Divergent movements are the most strongly marked; and the region round Alcyone suggests, at the first glance, rather a very confused area of radiation for a flight of meteors, than the central seat of attraction of a revolving throng of suns.

There are many signs, however, that adjacent stars in the cluster do not pursue independent courses. "Community of drift" is visible in many distinct sets; while there is as yet no perceptible evidence, from orbital motion, of association into subordinate systems. The three eighth-magnitude stars, for instance, arranged in a small isosceles triangle near Alcyone, do not, as might have been expected *a priori*, constitute a real ternary group. They are all apparently traveling directly away from the large star close by them, in straight lines which may of course be the projections of closed curves; but their rates of travel are so different as to involve certain progressive separation. Obviously, the order and method of such movements as are just beginning to develop to our apprehension among the Pleiades will not prove easy to divine.—*Nature.*

NOTE FROM MR. PROCTOR.

SIR:—I should have greatly preferred that what I have been forced to say respecting Prof. Holden (by his own most unwise renewal of an offense originally committed anonymously) should be forgotten. But, as you have published in the SIDEREAL MESSENGER for June, his letter in the *Examiner*, I must ask you to do me justice by inserting the enclosed reply. (The MESSENGER reaches me by the way of England and therefore very late.)

Prof. Holden, at a time when he was absolutely sickening me by false compliments at Washington (in 1874), wrote anonymously a review of a popular book of mine, in the *Atlantic Monthly.* In this he adopted the preposterous position that there is something degrading to science in writing books which more than pay their expenses; hinting plainly that since I can write books too abstruse to sell I ought so to employ all my time. I am not greatly concerned to point out that in writing books which will sell because simple (though sound), I am adopting one way of earning a livelihood, while a man who takes a position in an observatory or a professorship at college takes another, the only question affecting the propriety of any such course being whether the work is well done, and my livelihood absolutely depending on my doing my work well, whereas, as Prof. Holden's case at Washington shows, a man may be inefficient as an observer and yet retain a handsomely paid official post in an observatory. I should have cared nothing about the paltry sneers running through the criticism in the *Atlantic Monthly*, in point of fact they could deceive no one of any sense. But the review was full of untruths. It opened with a gross untruth, repeated in his latest letter in company with many others by Prof. Holden; and it either suggested or deliberately stated a string of untruths to the end. I had no knowledge who the author might be. Had I guessed a man associating as Prof. Holden did at the time, with men whom I esteem, was the author of it, I might have remained silent. But deeming that some dishonest penny-a-liner was the writer, I sent to the leading papers of New York a letter pointing out the dishonesty of the criticism and illustrating this by the case in which the critic deliberately denounced by name a book which had not yet appeared. He made, still anonymously, a feeble and foolish defense, offering an apology which I publicly declined to receive.

When, two years later, I found Prof. Holden was the author of the dishonest criticism, I was deadly ashamed to think a man in a responsible position in scientific circles could have been capable of such a thing. I remained absolutely silent for years. But it was the silence of contempt as well as shame,—contempt for him, and shame for the disgrace he had brought on the brotherhood of Science.

Recently he ventured, considerably to my surprise, to arouse the sleeping shame by deliberately making a series of statements about an article of mine implying that I needed to be told the very things that that article had most carefully explained,—and closing with the characteristic remark that only a Wiggins or a Vennor could make the mistake which my article anticipated him in correcting. (The reference to Wiggins, by the way, was rather dangerous for a man who had set all Washington astronomers laughing by detecting a third satellite of Mars with an impossible period and distance, and remaining deceived by it for months !)

Let me now simply state, and as briefly as possible, for this letter is running beyond the length I had intended and occupying more time than I can spare the subject, that—taking renewed and new falsehoods in the order in which they appear in Prof. Holden's letter:—

1. My "Saturn and its System" was *not* a success: the first edition was eighteen years selling. I estimate my loss on the work at fully a thousand dollars.

2. No biographers have ever asserted that Saturn was a success which led me "to attempt to earn a large sum of money by writing similar books."

3. No such book as "Other Suns than Ours" had appeared when Holden wrote his review.

4. I utterly disbelieve, though I cannot disprove, his statement that in writing a review which appeared in the summer of 1874, he confounded a two-column report of a lecture of mine with a book, and did not think of this even when first defending himself in 1879 from the rather serious charge of denouncing a book which had not yet appeared.

5. The Royal Astronomical Society not only did not force me from my position as its Honorary Secretary, but Prof. Holden has always known that it did not. For it was a subject of conversation with him at Washington in March, 1874, that I had had to resign the secretaryship and the editorship of the Society's Proceedings in October, 1873, when I sailed for America. The Society never suggested resignation, and never had anything to do with the matter, beyond accepting my resignation,—which, considering I was already in America when the letter reached the hands of the council, they could not well have declined to do.

6. Prof. Holden owes it entirely to me that his name, when suggested for a Foreign Associateship of the Society, was not rejected with contempt.

7. I never coupled my name with Newton's. I spoke of Newton as the "greatest of us all." If that is coupling my name with his, then a farm lad who chanced to say that "the President is the first of us Americans" would be coupling his name in the same sense with President Cleveland's.

8. Prof. Holden knows perfectly well that his review could

not have offended me as adverse; for it was not adverse. The level of my popular writings is not determined by myself but by my readers. Prof. Holden in admitting that my "Saturn" and my "Moon" deserved to succeed, was saying all that my personal desire for appreciation could possibly require. As a matter of fact, those books, my "Geometry of Cycloids," and similar works of mine have not succeeded. I write such books at a loss. For my livelihood, which I feel fully as much entitled to as any salaried official, I have had to write down to the general public (regarded astronomically). From the new editions of "Saturn" and the "Moon," all the more difficult matter has had to be excised, and matter added which appeals more directly to the mind of the general reader, as I find it, (still in the astronomical sense) to be. If I heard my friend Prof. Young addressing a junior astronomical class at Princeton and touched on the simplicity of his discourse, I should certainly not offend him by so doing; for that simplicity would be an important quality. If I went on, however, to say he was degrading science by lowering his tone to the capacities of his class, I take it he might be indignant at the charge, unless its absurdity left no room for any feeling but amusement.

Lastly, it is well known my real offense with Prof. Holden was that I wrote the astronomy of the American Cyclopedia.

Very truly yours,

St. Joseph, Mo., July 27, 1887. RICHARD A. PROCTOR.

THE TOTAL ECLIPSE OF THE SUN, AUG. 19, 1887.*

The following is extracted from *Ciel et Terre* of Sept. 1, 1887:

"According to the reports received up to date, observations of the total eclipse of the sun appear to have been generally prevented by the weather. At certain points on the line of totality, the sky was completely covered; at others partially

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Translated from the French for the MESSENGER by Dr. H. C. Wilson, Carleton College Observatory.

obscured by the clouds, and at Jurjewitz, among others, where our co-laborer M. Niesten installed himself, the phenomenon was observed only under very precarious conditions. However, the mission with which M. Niesten was charged was not a complete failure, and the letters and telegrams received from that astronomer permit us to hope that some new facts may be added to our knowledge concerning the nature of the principal body of our system.

"It is known that the eclipse commenced in western Europe, advancing through the north of Germany, Russia, Siberia, ending in Japan. The best stations for observing the phenomenon were to the north of Moscou in the valley of the Volga, and better still at Tobolsk.

"Few eclipses have been the objects of so considerable preparation, at Berlin, Poulkova and Moscou. Excursions were even organized at Berlin, carrying out a great number of persons desirous of contemplating the phenomenon.

"At Poulkova, M. Struve had organized fifteen or twenty scientific expeditions, national or foreign; at Rschew, to the northwest of Moscou, was M. Young; at Petrowska, on the route from Moscou to Jaroslaw, were MM. Glasenapp, Tattschaloff and Stanoiewitch, the last representing the observatory of Meudon; at Kineshma, on the Volga, (terminus of the railroad from Moscou), MM. Bredichin, Perry, Copeland, Tacchini and Ricco; at Kolkosé, M. Jegoroff.

"Beyond the Volga, access was more difficult; and even beyond the Ural the difficulties of travel became still greater, especially so near Tobolsk unfortunately, for that was the region most favorable for observation of the eclipse in its totality.

"The program of observations was thus fixed: Photography of the eclipse at all the stations; Spectroscopy, MM. Young and Tacchini; Photography of the spectrum and of the corona, MM. Copeland and Perry; Search for intra-mercurial planets, notably Vulcan, MM. Glasenapp and Tattschaloff; Solar diameter, M. Struve; Meteorological observations everywhere.

"Two balloons were arranged for observations above the clouds; the one of 700 cubic meters by the Imperial Institute of Technology of Russia for Professor Mendlejew; the other of 1,000 meters, at Twor, for Professor Swjerinzew.

"As stated above, the news received up to the present from a certain number of observatories show that the results are almost negative, because of the state of the sky, almost wholly covered. However, at the station at Petrowska, M. Glasenapp obtained two drawings and three photographs of the corona; M. Stanoiewitch obtained the lines in the spectrum of the corona and took some photographs; M. Kononowich (of Odessa) obtained a complete spectrum of the corona.

"In conclusion we may add that at Berlin some wags had posted upon the city wall that, in view of the bad weather, the eclipse was deferred until the following Sunday."

Then follow some telegrams and extracts from letters from M. Niesten, the last of which is quite interesting:

"The dispatch which I sent to the observatory will have informed you that the photographs taken during the eclipse have given some results. These photographs were obtained during totality with an apparatus having four objectives; Dallmeyer, Ross and two Darlot. Those given by Dallmeyer are very good. In all six photographs are good and two passable. The time of exposure was about 8*s* for the first, 12*s* for the second, 16*s* for the third, 20*s* for the fourth, 24*s* for the fifth and 30*s* for the last three. The chromosphere and the protuberances were shown upon all and two show some traces of the corona, also the impression of Regulus (*a* Leonis), which was near the sun. What is especially important, the photographs confirm the exactness of the drawing which I made during totality.

"M. Karinne, a skillful photographer of Moscou, also took, at Jurjewitz, with a Ross objective of 7 inches, several photographs which have given results analogous to mine. On the whole the records obtained at our station are passable and I can esteem myself fortunate to have been able to accomplish a little, since almost all the other stations were unfortunate.

"At Kineshma, where were MM. Bredichin, Perry, Copeland and Miss Brown, sky covered, nothing.

"At Viatca (Tacchini, Ricco, Kleiber), nothing.

"At Warnavin (Ceraski and Sokouloff), nothing.

"At Clinn (Müller, Kempf and Scheiner of Potsdam, Hasselberg of Poulkova), nothing.

"Thus, of the foreign station, ours only have any results.

"At Katinski about 55 *verstes* to the east of our station, on the Volga, the weather was clear; the phenomenon there displayed all its grandeur. The corona was splendid and extended, according to the observers, not in a *gloire* but in concentric circles. The protuberances even were visible to the naked eye; to certain observers they scintillated and presented the different colors of the rainbow.

"The terror among the natives was great; the infants cried, the women prayed and some, after the reading of the *Evangile* which had taken place the preceding Sunday,—religious as are all the Russians—were fearful of the end of the world. The passages of the *Evangile* which are prescribed to be read at that time of the year, state in effect, that the sun will be eclipsed, the stars will fall, etc. (See gospel according to St. Matthew, ch. 24, verses 27 to 33 and 42 to 51.)

"One fact to be noted is that the darkening was so great, that the pigeons flew against the houses and the sheep ran frightened toward the stables.

"If we had had plenty of time, what an admirable phenomenon it would have been to contemplate! Under the conditions where I observed, it was even then grand."

STAR OF BETHLEHEM.

THE EDITOR.

Intelligent people have been frequently asking during the past summer where the Star of Bethlehem could be found. The impression seems to have been general that there was now to be seen somewhere in the heavens, a very bright star which should be properly called the Star of Bethlehem, meaning that notable star spoken of in the Bible, as the "Star of the East," which the wise men saw and followed, in search of the birth-place of the King of the Jews, and which "went before them till it came and stood over the place where the young child was."

In 1884 very much was said about this star in newspapers and various periodicals in popular and religious lines of thought, awakening an interest in the minds of the uninstructed in the astronomy and the history of it, which was sometimes very intense with well-meant devotion and sometimes very ludicrous and fanciful. What lent peculiar zest to these popular fancies in the summer of that year was the appearance of the planet Venus, for a considerable time, as a bright and most beautiful object to look upon, in the western evening sky, and from this circumstance alone it was believed by many at that time that this queen of the starry host (unknown by name), was the real Star of Bethlehem.

In view of this, an article was prepared for the MESSENGER giving the substance of all that was known to the writer, about the history of that memorable star, arranged under four heads, as follows:—

1. The star may have been a miraculous light of some kind.
2. The conjunction of the planets Jupiter and Saturn first, and finally Jupiter and Mars.
3. A comet; and,
4. A new or temporary star.

On the first point astronomy, as such, could have nothing to say, because wholly beyond its province of investigation.

Regarding the second point, scholars and astronomers in the early part of the century thought and wrote much. In this Professor Encke took the lead and his conclusions that the conjunctions of the great planets which took place at the time of the birth of Christ must have been the star seen by the Magi. This opinion prevailed quite generally among astronomers, including so high authority as that of George B. Airy, Astronomer Royal of England.

Within the last few years, however, astronomers have been less confident that the theory of the conjunction of the planets is really sound. As satisfactory a statement of the theory and its objections from the side of science as the writer has seen is found in *Knowledge*, Dec. 29, 1882, as follows:

"If the Magi set out from Jerusalem as the shades of evening closed, and the time was winter, as commonly received, then it would have been about eight in the evening, at latest, when they reached Bethlehem. And if at that hour the star lay due south, or rather to the west of south, then it must have been sixty or seventy degrees west of the point opposite the sun. This would correspond fairly with the case of the conjoined planets, Jupiter and Saturn in the third year before the birth of Christ, if we suppose the journey to Bethlehem made nearly at the time of third conjunction of these planets, or about December 5. It is in fact, on this circumstance chiefly that the planetary conjunction theory of the star in the east has been based. But the whole character of the narrative is opposed to this interpretation apart from the fact that the taxing of the Roman Empire by Augustus when Joseph and Mary went to Bethlehem (according to Luke's account) certainly did not take place till two years after the triple conjunction of Jupiter and Saturn. For Matthew says, 'Lo! the star went before them' and 'when they saw the star they rejoiced with exceeding great joy.' But they would not lost sight of the planets Jupiter and Saturn from the time when these two planets had been visible in conjunction as morning stars in May (B. C. 7) though the time of their greatest conjoined brilliancy was in September and onward until the time of their third conjunction on Dec. 5. We had a similar triple conjunction in 1877. They were conjoined as morning stars on July 27, conjoined when nearly at their full brightness on Aug. 25, and conjoined again as evening stars on Nov. 3. But all through these months they were both visible and in tolerable close proximity. The exceeding great joy with which the Magi again saw the stars shows that it had been for a time lost to them, so that we must on this account reject the planetary conjunction theory which seems otherwise (and on several grounds) altogether untenable."

As determined by Encke in 1831, the distance of these planets apart, at conjunction, must have been one degree at least, and hence could not have appeared to an ordinary observer as one star at any time. On the other hand it is easy to see how such a striking phenomenon would arrest the attention of those interested in, or acquainted with, the Hebrew prophecy or astrology. The teachings of astrology interpreted the conjunctions of planets as foreshadowing great national events, and the sign of Zodiac called Pisces was known to belong to the Jewish nation, hence a conjunction in this sign (not only one but three) was significant of the birth of Christ the expected King. A trace of this same belief is found in Kepler's writings, in which he holds that the conjunctions of great planets coincides with the approach of climaxes in human affairs, and gives as examples, the birth of Enoch, the Deluge, the births of Moses, Cyrus, Jesus Christ, Charlemagne and Luther.

Regarding the theory that the star seen may have been a comet whose change of place might have answered the description given in the New Testament, nothing can be said by the astronomer, for he has no historical evidence to support such a claim, and he probably would not offer any other.

In the matter of the appearance of a new or temporary star at this time, as the object seen by the Magi, the records contain something of interest.

By a new or temporary star is meant one that suddenly flashes out where none has been noticed before, and as suddenly dwindles away to a telescopic star, or disappears altogether. The important stars spoken of in connection with this subject are Tycho Brahe's star and the star in *Coma Berenices*.

There is probably not another new or known variable star that has so wonderful a record as that which bears the name of Tycho Brahe. His own words best describe impressions at first sight, as follows: "Raising my eyes as usual, during one of my walks to the well known vault of heaven, I observed with indescribable astonishment, near the zenith in *Cassiopeia*, a radiant fixed star of a magnitude never before seen. In my amazement I doubted the evidence of my senses. However, to convince myself that it was no illusion, and to have the testimony of others, I summoned my assistants from the laboratory and inquired of them, and of all the country people that passed by, if they also observed the star that had thus suddenly burst forth." Going on with the description Tycho Brahe speaks of its brightness as greater than that of *Sirius*, *Vega* or *Jupiter*. For splendor it was only comparable to *Venus* when nearest to the *Earth*, and was seen by some at noonday. After a few weeks it began to decline and in sixteen months became invisible to the naked eye (the telescope being invented thirty-seven years later).

In waning the star passed through changes of color, from white to yellow and red and then to white again. These phenomena interested Tycho Brahe so much that he wrote a large book describing the appearance of the star as seen by himself and others, and gave theories to account for those wonderful changes. It has since been thought that this star appeared also in 945 and 1264. If it be a variable star with period of about 314 years, it would make its time of appearance about the beginning of Christian era and also its re-appearance probable, in some slight degree, in 1886. In consequence of this latter supposition astronomers in Europe have been watching its place in *Cassiopeia*, which is now closely marked by a faint star, with special attention, for the last ten years.

In Tycho Brahe's time it was claimed by one Cardanus that this was the star which the Magi saw.

The star of *Coma Berenices* is spoken of as appearing immediately preceding the birth of Christ and was so bright as to be visible by day. Hipparchus and Ptolemy speak of this star, and Ignatius says that it "sparkled in brilliancy above all stars." Chinese records also mention a new, bright star at this time; but none of these statements have we been able to verify from the best authority. In Dr. Seiss's view of the divine origin of the constellations, the theory of this last named temporary star is certainly very suggestive and possibly not too fanciful to be true. So uncertain is all our knowledge of the Star of Bethlehem from records within our reach at the present time.

NEW WORK ON ASTRONOMY.*

RICHARD A. PROCTOR.

On January 1, 1888, will appear the first part of a Treatise on Astronomy, to be completed in twelve monthly parts and a supplementary section. In each there will be sixty-four pages, imperial octavo, many cuts, and two plates, or one large folding plate. Thus the complete volume will contain, with index, preface, etc., about 800 pages and abundant illustration. The price of each part will be 2*s*.; that of the supplementary section, containing tables, index, and preface, 1*s*. The subscription price, if paid in advance, for the twelve parts will be 21*s*.; the price of the complete work, bound in one volume, 31*s*. 6*d*.

The chief object of the volume thus announced is to present in popular yet scientifically sound form those views of the heavenly bodies which are included in what, in his last poem, Tennyson calls the 'New Astronomy.' The life histories of worlds and suns will be dealt with; the planets will be studied as illustrating the stages of our own earth's life, while the record of our earth will be considered as illustrating the life histories of the planets. The sun will be studied as the one star we can examine, and thus as telling us all we know in detail about the nature of other suns; and the stars will be considered as throwing light on the probable past and future of the ruling and life-giving center of the solar system.

While thus the general characteristics of the new astronomy will be presented, points of detail in which the astronomy of to-day differs from the astronomy of a quarter of a century ago will be fully considered (for the first time) in the forthcoming volume. Among these points may be specially mentioned (in the order in which they have occupied my own attention and have been accepted, or in one or two cases still await acceptance)—

1. Changed views as to the structure of our galaxy, from

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* From a late printed circular.

the uniformity formerly imagined to a variety of forms, arrangement, and movements akin to what is found in the solar system, but on a much grander scale. (These views, first suggested—rather vaguely—by Dr. Whewell, were advanced definitely in 1857 by Mr. Herbert Spencer, and later, more fully by myself).

2. New views as to the sun's condition and surroundings, opening with Kirchoff's interpretation of the solar spectrum, but including many details and discoveries relating to the sun's surface, vaporous envelopes, the sierra, the colored flames, the corona, and the zodiacal.

3. Changed views (maintained by myself since 1869 and now generally adopted) as to the condition of the various orders of bodies—giant planets, terrestrial planets, moons, asteroids, etc.,—attending on the sun.

4. The recognition of the moon as presenting the history of our earth's past as well as future life—even of stages of vulcanian history whose records in the earth's case have long since been destroyed by denudation. (This I have recognized only during the last four years.)

5. Recent ideas respecting comets and meteors, by which all orders of both classes of bodies are included under one theory of volcanic ejection from suns and from planets when in the sunlike stage of their careers. (This theory is maintained in its general form at present by myself alone: but in detail by Daubree and Graham, who recognize ejection from stars; by Sorby, who recognizes ejection from our sun; by Tschermak and Meunier, who recognize ejection from our earth when in the sunlike stage. The theory is also accepted by many in one or other of these partial forms, each of which logically involves the general theory.)

I propose, further, to present in this treatise full explanations of several matters which hitherto have been either insufficiently or inexactly dealt with in all except one or two books on popular astronomy. Among them I may mention—

1. The Tides, correctly dealt with in no treatise on popular astronomy except Lord Grimthorpe's 'Astronomy without

Mathematics,' the old incorrect explanation being given in most books, and even in Herschel's admirable 'Outlines,' while a new and equally incorrect explanation (the centrifugal fallacy) appears in some recent treatises. In dealing with this subject I propose to present, in a simplified form, Sir George Airy's admirable geometrical demonstration.

2. The Precession of the Equinoxes, as a case of Gyration which is not explained accurately and sufficiently in any treatise on popular astronomy.

3. The Discovery of Neptune, correctly but not popularly dealt with by Professor Grant in his admirable 'History of Physical Astronomy,' and explained fully in Herschel's 'Outlines,' but nowhere else properly dealt with, most of the so-called explanations of the perterbations of Uranus being altogether erroneous. On this subject I have some considerations to present (not relating to the mathematical problems involved, about which there can be no question) which tend to modify the ideas commonly entertained about this interesting discovery.

The book will direct special attention to the departments of research in which astronomical work is now specially needed, endeavoring to attract towards profitable observations the many workers who are now wasting time in multiplying observations such as once had special value, but now, having achieved their purpose, possess none.

Throughout the work I shall endeavor to give a just estimate —neither exaggerated on the one hand nor inadequate on the other—of the discoveries, researches, and observations of all those students of astronomy whose work has come under my notice.

A special feature of the work will be the large number of original illustrations. I have long regarded it as unfortunate that so little has been done to improve the drawings in our books of astronomy, many of which belong, so far as style is concerned, to the astronomy of three centuries ago. The illustrations in the forthcoming work present the results of more than a thousand hours of work devoted to this feature alone.

The book itself may be regarded as the work for which all the treatises on astronomy I have hitherto produced, and also my astronomical essays and lectures, have been preparatory.

The work will be published by Messrs. Longmans & Co., to whom subscriptions (to include postage) may be sent either for parts or for the entire work.

COLORS IN THE SOLAR CORONA.

No systematic work on Astronomy, nor any special treatise, that I know of, has made any mention of the colors, or the changes of colors in the corona of the sun. Our present attempt, therefore, is altogether new and untried. We will begin by reviewing the testimony of seventeen different observers who have attentively watched the solar corona in four recent total eclipses, those which occurred in the years 1869, 1878, 1880 and 1883; all visible on the American continent, except the last which was seen by American observers on Caroline Island, in the Pacific Ocean. For convenience of reference the observers are numbered and paged. W. O. stands for the reports of the Washington Observatory for the year 1880. C. S. O. stands for the report of the Chief Signal Officer at Washington for the year 1881.

The eclipse of 1869. 1. The late Gen. Myer observed this eclipse from near Abingdon, Virginia, on the summit of White Top Mountain, 5,530 feet above the ocean level. With the telescope he saw an aureola of clear, yellowish bright light, closely surrounding the moon's disk, and fading gradually into the tint of the darkened sky, with a slight tinge of purplish green. To the unaided eye the vision was magnificent beyond description. Around the black disk of the moon there was an aureola of soft, bright light through which shot out, as if from the circumference of the moon, straight, massive, silvery rays, seeming distinct and separate from each other, to a distance of two or three diameters of the lunar disk, the whole spectacle showing as upon a background of diffused rose colored light.

The eclipse of 1878. General Myer was stationed on this occasion on the top of Pike's Peak, Colorado, 14,200 feet above the ocean. He describes the corona as being white, composed of silvery rays, the inner portion towards the moon brightening in intensity of its light, but not changing its character, unless by a possible tinge of yellow, deepening slightly as it reached close to the moon. The diffused rose color which beautified the eclipse of 1869 was absent. Hence in another place he speaks of "white eclipses and red eclipses." C. S. O. pages 832–3.

2. Prof. Cleveland Abbe, of the Chief Signal Office at Washington, D. C., observed this eclipse also from Pike's Peak. He describes the dark moon, then hiding the sun, as surrounded by a narrow, brilliant, yellowish white ring, probably in breadth one-eighth of the sun's diameter. He speaks of it also as the brilliant golden ring called the inner corona. Beyond this there extended outwardly several broad rays or streamers to the distance of from two to five millions of miles, and with a breadth of a million, or a million and a half of miles. He designates them numerically. No. 1 and 5, he says, had the most delicate imaginable bluish tint, each tapering outwardly to a fine point. "The same bluish tinge pervaded No. 2 as in the case of No. 1." "No. 3 was also broader at its base, tapering to its apex like No. 1, and of the same uniform intensity and tint." C. S. O., pages 845–6.

3. Lieutenant H. H. C. Dunwoody was also at the summit of Pike's Peak and he observed with the naked eye as follows: "The moment totality commenced, the moon presented the appearance of a black disk, without indentation, surrounded by a narrow ring of orange colored light, the ring not being concentric, but wider on the right hand side, as viewed by the observer. The sun was completely surrounded by bright, silver colored rays, those of the upper left hand quadrant extending 2½ and 3 diameters, and those in the lower left hand quadrant extending 3 or 4 diameters; the longer rays taken together presenting a fish-tail appearance." C. S. O., page 835.

4. H. T. Crosby, Chief Clerk in the War Department,

likewise on the top of Pike's Peak observed with the naked eye. He saw a circle of bright golden light consisting of fine rays radiating from the sun. The limit of this golden corona was about one-fourth of the sun's diameter above the solar surface. It was, however, seen to have a greater extension or bulging in the upper left hand and lower right hand quadrant. Beyond this circle of golden rays there extended two long streamers of silver light respectively to the left above and to the right below the sun, and to a distance of more than five solar diameters from the sun's limb. C. S. O., page 837.

5. Mr. J. H. Kerr observed from Colorado Springs, Col. He says the coronal beams were striated. The corona gave out a blue light, invisible with the telescope, but seen with the naked eye. C. S. O., page 950.

6 and 7. Lieutenant S. R. Whitall and private McCann observed together at Fort Sill, Indian Territory. The former made the discovery of the eclipse, and the latter wrote the description, which is as follows: "The sketch is, from my remembrance, a very creditable drawing. The white lines shot out and flickered uniformly; the inner circle of light during totality was of a purple hue, around which was a circle of pale yellow skirted with a circle of orange from the edge of which the irregular striæ were observed. During totality there was no appreciable change in the color of the striæ, which color was white with a yellow tinge." C. S. O., p. 875.

8. Mr. A. J. Bennet made his observations with the naked eye at Virginia City, Montana. "A ring of light of a yellowish cast surrounded the moon; brightest in the inner portion of the corona; the outer edge of the corona was sharply defined except at two opposite points where it shaded gradually away and was broken up into streaks." C. S. O., p. 883.

9. Mr. Thos. Bullock made a sketch of the eclipse at Coalville, Utah, from naked eye observations, and wrote as follows: "As it requires a master hand to imitate the beauty of a full-blown rose or a golden-banded lily, so it is utterly impossible for me to explain or describe the flickering, flaming streamers, as seen in the sketch herewith enclosed, or the gorgeous deep

golden border of rays around the sun, continually changing or moving. All who were near me exclaimed, 'Oh, how beautiful; what a glorious sight!'" C. S. O., p. 894.

10. Mr. Wm. H. Holmes, of the United States Geological Survey, was stationed in Wyoming Territory. He writes: "The sun was surrounded, while in eclipse, by minute pencils of light that played rapidly to the left and right, disappearing and reappearing; these pencils of light were very minute and quite brilliantly colored with all the colors of the spectrum. The groups of rays, as at B and C, were not constant in length and conspicuousness, but were so in position." C. S. O., p. 897.

11. Miss Olive Risley-Seward, at Colorado Springs, describes the several streamers or "shafts" which extended out from the sun. She says, "the light of these three shafts was pulsating like a flame in the breeze." "They differed distinctly in color; one showing white and brilliant like the diamond, another shining as topaz yellow, and the third as intensely yellow." W. O., p. 200.

12. Prof. Ormond Stone observed from Mount Lookout, Col. He says: "Around the sun was a narrow, bright halo, very nearly white, but slightly tinged with orange yellow." W. O., p. 238.

13. Prof. E. W. Bass, of the U. S. Military Academy, West Point, observed at Central City, Col. He says: "With the telescope I observed the corona proper. At first it appeared homogeneous, and without any particular structure, the brightness diminishing rapidly from the moon outward. But observing with care the same portion for some seconds, its minute structure became more evident. For a short distance outside of the moon's limb it apparently remained uniformly bright and white, then broke into radial streamers of the same color with intervals which were blue as seen. The streamers extended to the outer limit, expanding and blending with the blue spaces, becoming again uniform in structure." "These radiating streamers resembled very much the white ones frequently seen in auroras." "The bluish color between the streamers was faint, but unmistakable, and was due to the sec-

ondary spectrum of the objective of the telescope." "An apparent pulse or luminous wave motion continually occurred from the limb outward, reminding me again of the aurora in which similar motion is often seen. Perhaps as good a comparison would be the white streams emitted from the head of a bright comet." W. O., p. 166. This attributing the blue color in the corona to the object glass of the telescope is unhappy, because the blue was seen with the naked eye by at least two other observers here quoted, the Nos. 2 and 5.

The eclipse of 1880. 14. Prof. Frisby observed this eclipse from the summit of Santa Lucia Mountain, California, 6,000 feet above the ocean. He says: "I saw around the sun what appeared to me a concentric ring of yellow light, possessing great brilliancy. I did not see any streamers or outer corona." W. O., p. 399.

15. Lieutenant Christopher, U. S. N., observed from the same place and reported: "At the instant of totality a bright band of yellow light, uniform in color, and of great brilliancy, burst forth, forming a ring uniform in width, with the exception in some parts of distinct yellow protuberances rising up. The sun was slightly obscured by light cirrus clouds, preventing any view of the red flames, or a distinct view of the outer corona." W. O., p. 411.

The eclipse of 1883. 16. This eclipse was observed at Caroline Island, in the Pacific Ocean, by Lieutenant Qualtrough, of the United States Navy. He reports as follows: "The moon was surrounded by a beautiful halo of silvery light, its color reminding me somewhat of the electric light, and shining through or over this halo were long white streamers, which diverged ray-like from the edge of the moon's disk." See next.

17. Dr. W. S. Dixon, U. S. N., observing from the same position, reports: "At the upper part of the corona, the first large tail of light was filamentous to a great extent, irregularly arranged, a majority of the fibers, however, assuming a curved direction trending to the right. Nearly or quite all the fibers were tinted a delicate lilac. An apparent movement among them seemed to be due to changes in the depth of

color." "At another point there were numerous narrow threads or fibers, apparently colorless, but having a more perceptible movement even than the fibers in the tail of light before mentioned. These threads were also irregularly arranged, some of them almost bowed, and presented the appearance exhibited by a wheat field during the prevalence of a strong wind." This and the preceding one are from the Report of the National Academy of Sciences: pages 142–3. It will be seen that all these extracts are from the original reports, except the one for the eclipse of 1869 which I have taken at second hand, but which I suppose is correct.

The eclipses of 1880 and 1883 were visible only at places difficult of access, and only two persons at each place were appointed to report on the corona. It is remarkable that all, without any concert, bore testimony to the colors. The eclipse of 1878 had more observers, and it is again remarkable that thirteen described the colors. This they did without any collusion, each one not knowing what the others would report. Why some others said nothing about colors will soon be explained.

Along with the colors it is of the first importance to note the movements in the coronal rays. They are described as "flickering," "flaming," "playing rapidly right and left," "pulsating like a flame in a breeze," "the appearance exhibited by a wheat field during the prevalence of a strong wind," and other tantamount expressions. For these movements see Nos. 6, 7, 9, 10, 11, 13 and 17.

Now the momentous questions arise, What is the cause of these colors, and what information do they afford about the nature of the solar corona? Happily we can obtain the necessary answers from experiments with our philosophical apparatus. As we have from long ago known how to make actual thunder and lightning, so we can produce the aurora borealis and the solar corona on a scale so reduced as to do us no harm. On an electric machine we can obtain a flash and a snap which have long been known as real thunder and lightning. Now if we put the same machine in operation in the dark, then, from half a dozen or more parts of the machine, the electricity will be seen to fly off constantly in the form of luminous brushes. They are from two to three inches in length, and half an inch in breadth, and they are familiarly named electric brushes. They have the appearance of the tall streamers in the aurora and the corona. But they have not yet the pulsations nor the colors, for the air is too dense. But these can easily be added. Rarify the air; and pass the electric discharge through the tube whose air has been partially exhausted. Then we see a beautiful, mild, luminous stream filling the whole tube, and flowing from the machine to the opposite discharging end. This stream or streamer goes by pulsations, by cloud-like waves. Tubes of this kind have for a century or more been called auroral tubes, because the phenomena they present are like those of the aurora. Now let us exhaust the air still more,—exhaust it so much that almost none is left. Then the electric fluid as it flows through exhibits colors. All the colors of the rainbow can be produced. These tubes, including different gases, are called Giesler tubes from their first maker. In this way it is an easy operation to make thunder and lightning and auroral and coronal streamers.

My recollections are that, in small ordinary auroras, we see no colors. But in the very tall and grand auroras of 1869 and 1870 and 1871, and in some previous exhibitions, the colors were intense and impressive; "all the colors of the spectrum," as was said about the solar corona by observer No. 10 in this article. This difference may be because the small auroras are down in the denser air, as in the auroral tube, and the tall, colored ones are farther up in the rarer air, as in the Giesler tube. Probably the same causes operate in solar coronas. Hence the unusual display of colors in the tall corona of 1878 which were reported by at least thirteen different observers.

But why were these colors not seen by many others? Because so many other deeply interesting things demanded attention. The mere catalogue of subjects to be examined scientifically in an eclipse is too long to be inserted here. It is a time of deep emotion. The mind is attracted rapidly from one thing to another with the feeling that all will soon be over. Moreover the colors of celestial objects require time and special attention to bring them out. Often I have inquired of individuals about the colors of large stars, Sirius for example. Generally the answer is, white. But a short time of special looking brings out the true color for Sirius, white is green. Hence when there are plenty of observers each one should have but a single subject to examine. In the eclipse of 1878 very much was gained by the fact that Langley and Abbe confined their attention to the size and the form of the corona. In addition it should be stated that scientific men hitherto have felt not the least interest in coronal colors. Even after these colors have been reported by seventeen observers quoted here, and by how many more I know not, they have received during so many years not the least attention from professional astronomers. These colors are to them facts without any significance, and therefore they have been disregarded. They have been taken up by myself because my theory of the aurora and the corona makes such colors significant. It was the same with the changes in the colors of the stars. Astronomers had declared against such changes, except in the single case of Sirius. But my theory of the cause of stellar light and heat demanded such changes. Accordingly I examined and made a catalogue, with the evidences, of fifty-six stars whose colors have changed, several of them more than once. This catalogue was afterwards inserted in my volume, "The Origin of the Stars." Since then it has been added to by others.

All are familiar with the fact that when the aurora extends up in the heavens as high as 45 degrees or less, it is composed of streaks, streamers, or radiations up through which wave-like motions are seen to rise. These radiations vary in breadth, but considering the distance their width must be at least a few miles. The solar corona is composed of similar radiations. Observers call them striæ, filaments, and thread-like forms. Among the 22 plates in the report of the Washington Observatory on the eclipse of 1878, not less than 13 show these striæ or fine radiations. They require careful scrutiny to be seen. Observer No. 13, Prof. Bass, says that "At first the corona appeared homogeneous. But after observing with care a few seconds its minute structure of radial streamers became evident." The eminent observer Professor Langley reports, (W. O. page 209) "Now what I saw with the telescope in this brief view was a surprisingly definite filamentary structure." To be seen at all 92,000,000 miles away, they must be at least 500 miles broad,—broader, as might be anticipated, than the radiations in our northern lights. This brings out a new feature of identity between the solar corona and the aurora borealis. It is a new and a distinct subject for examination in total eclipses. My paper in the March issue of the SIDEREAL MESSENGER insisted on five separate points for observation in the corona. This one was accidentally omitted. Now it may be added as a sixth. It could profitably occupy the whole time of a single observer. Six men or women, therefore, each with his own special department, should be employed on the corona alone. These distinct rays or filaments are mentioned in this paper by observers Nos. 1, 4, 5, 6, 7, 8, 10, 13, 16, 17.

The corona cannot be composed of meteors falling into the sun. For the pulsations or wave-like motions all proceed outward. Nor can it be composed of meteors shot outwardly, because so dense a body of meteors constantly around the sun would most certainly stop and destroy the comets. Some of these pass within 100,000 miles of the solar surface. For further information I refer to my papers in this magazine for March and October last with the other paper therein mentioned.

J. E.

ASTRONOMY IN RECENT PERIODICALS.

Nature. In the issue of Aug. 4 of this periodical Miss A. M. Clerke reviews a late volume of Carl Braun, S. J., of Münster, Aschendorff, 1887, under the title, "A New Cosmogony." The first point raised is concerning the truth of the nebular hypothesis as given by Laplace, and the claim is made that it now no longer fits the facts, and that fairly speaking nothing is left of it, but the naked fact "that the solar system did, somehow, originate from a primitive nebula by condensation, but further than this the whole field is open to speculation."

Dr. Braun and Wolf belong to one of the two late schools of cosmogonists which are wrestling with the Kant-Laplace scheme, and M. Faye of France is a leader in the other. Faye would abolish the old hypothesis, but Braun and Wolf would build upon it as a foundation with a method radically different from that of Laplace.

Dr. Braun's theory assumes a nebulous mass, in the beginning, co-extensive with the whole sidereal world. He thinks of this vast mass as endowed only with gravity and atomic repulsion. It is motionless, and if at any time not homogeneous, centers of attraction would form, in the process of condensation, which would eventually break up the primitive nebula into fragments, and these in turn would later become individual systems. The theory of Laplace assumed rotary motion in the beginning. Dr. Braun accounts for axial motion by the falling or inward rush of cosmical masses from space which have eccentric collisions with one another. We have a feeble illustration of the theory, in the behavior of comets at the present time in reference to the sun in the solar system. Laplace, however, thought that comets moved, by nature, in hyperbolic orbits and in ellipses by perturbation; but Faye holds that out of 364 cometary orbits computed, not one is a decided hyperbola, thus showing that the assumption of Laplace was wrong.

Another objection to the nebular hypothesis of Laplace was that it requires too swift axial rotation for the central body. The principle was that a homogeneous rotating globe should revolve more rapidly, as it contracts, in porportion to the square of the radius. Thus, when the primitive nebula was spread out to fill the orbit of Neptune, if its rotation was 165 years, (the period of Neptune's revolution), when contracted to the sun's present size it ought to rotate in 127 seconds of time approximately instead of 25 days, as will be readily proved by comparing the square of Neptune's mean distance

(2,780,000,000 miles) with the sun's radius (434,000 miles). On the contrary Dr. Braun holds that the solar nebula never had, at any time, uniform axial motion, a hint of this now being seen in the present unequal rotation of different parts of the solar surface which began by external impacts, the outer portions of the primitive mass moving most rapidly, and the inner regions slowly following from external action.

The ring theory of the planetary formation is almost wholly abandoned, and yet the comparatively even plane of planetary revolution does not seem to offer to Dr. Braun's theory insuperable difficulties, though this point is less definitely stated. The mode of obtaining planetary rotation and the revolution of accompanying satellites is ingenious and very different from the old theory. In condensing, each planet of the solar system, for example, is assumed to be about five times its present distance from the center of revolution. From these positions they would move inward and onward spirally through the general nebulous mass, sweeping paths nearly clean of cosmic matter until a balance is reached between gravity and tangential force, and then the inward approach would slowly cease. The density of these young planets consequently would be slightly increased on the inward side which would cause a whirling movement thus accounting for planetary rotation. Mention is made that this rotation, so originated, must have been in the outer parts of the planetary nebula to account for the rapid revolution of satellites, yet unborn, as compared with the slow motion of the parent mass.

Considerable is said regarding the difference of inclination to the ecliptic shown by the axes of various planets, in attempting to account for this troublesome fact. Speaking of external impacts on the inchoate earth, it is claimed that the collision of a body with it, one-thousandth part of its present mass, would have changed the inclination of its axis one degree, and, that when earth's radius was seventy times as great as now, it only needed the adverse impact of a body less than one ten-thousandth part of its own mass to stop its revolution altogether. This fortuitous way of securing the varying inclinations of planets, satellites and the sun itself seems quite like begging a difficulty in the theory.

Dr. Braun makes Neptune the first planet in order of production, but Faye puts it last. Because of its retrograde motion Dr. Braun supposes that it may have been condensed from a nebulous ring, but Professor Kirkwood thinks this could scarcely have been possible, because it would have required 150,000,000 years for the opposite portions of the ring to come together.

Very briefly stated, these are the outlines of the new cosmogony which plainly show much laborious thought, hard work and honest inquiry respecting the origin of things which probably science will never fully understand. That Dr. Braun has taken steps in advance is plain, but that new difficulties beset this earnest explorer's path is equally plain.

Monthly Notices for June contains a brief article from Prof. Pritchard on the parallax 61_1 and 61_2 Cygni as obtained by photography which, we believe, is the first successful attempt of the kind. The observations began May 26 last year, and were completed May 31, 1887. The work involved 30,000 bisections of star-images on 330 photographic plates procured on 89 nights. The mean results obtained are:

Parallax of 61_1 Cygni = 0.4289
" " 61_2 " = 0.4353

Professor Pritchard claims that the photographic method is accurate and as reliable as other known astronomical methods.

Then follows A. M. W. Downing's comparsion of star places of the Argentine General Catalogue for 1875 with those of the Cape Catalogue for 1880 and with those of other Southern Star Catalogues.

Prof. A. Auwers' Catalogue of 480 stars to be used as fundamental stars for observations of zones between 20° and 80° south declination. This working list is drawn up for astronomers in southern observatories, that a new and systematic determination of their places may be secured as early as possible. The objects sought are, better knowledge of stellar proper motions of southern stars and aid to the Paris programme of a general photographic survey of the heavens for the epoch of 1900.

Measures of double-stars at Sydney observatory.

Professor Hough's observations of the companion of Sirius.

J. E. Gore on the orbit Σ 1757.

A. Marth's formulæ for correcting approximate elements of the orbits of binary stars.

J. G. Lohse's observations of Nova Cygni.

A. H. Wesley, the solar corona, as shown in photographs taken during total eclipses, interestingly illustrated.

Of the remaining brief papers, Edmund Neison's reply to Professor G. Hill's former article entitled "A reply to Mr. Neison's strictures on Delauney's method of determining the planetary perturbations of the moon," is prominent. Mr. Neison says that Professor Hill's criticisms of his paper show an imperfect acquaintance with the history of the question, and that they are not supported by evidence, and then notices two points in the reply.

1. That Professor Hill in justifying Delauney falls into the unquestionable error which his (Mr. Neison's) paper sought to correct by showing that Delauney had not represented Hanson rightly or fully.

2. Mr. Neison claims that by his methods of dealing with this problem there are obtained certain quantities in the coefficients of high order, depending on the higher powers of the disturbing force, which give rise to sensible values to these terms of long period, and that these values have not been obtained by Delauney's complex method, and that because they are not obtained by that method, he thinks it is bad logic to say the values themselves do not exist or are erroneous.

Astronomische Nachrichten, Nos. 2798–98 contains catalogue No. 6, 100 new nebulæ by Dr. Swift of Warner Observatory. A definitive orbit of Comet 1863 IV, by Aug. Svedstrup whose elements are :

$$\begin{array}{llll} T = 1863 \text{ Nov. } 9.52384 \text{ Berlin M. T.} & & & \\ \pi = 94^\circ\ 43'\ 16.2'' & \pi' = 113^\circ\ 16'\ 32.33'' & \left.\begin{array}{l} \\ \\ \\ \\ \\ \end{array}\right\} & \begin{array}{c}\text{Mean} \\ \text{Eq.} \\ 1863.0.\end{array} \\ \omega = 357\ \ 13\ \ 50.2 & \omega' = 21\ \ 13\ \ 11.72 & & \\ \Omega = 97\ \ 29\ \ 26.0 & \Omega' = 92\ \ 3\ \ 20.61 & & \\ i = 78\ \ 5\ \ 2.2 & i' = 76\ \ 6\ \ 8.31 & & \\ \log q = 9.8491730 & q = 0.7065990 & & \end{array}$$

in which the elements on the left are referred to the ecliptic and those upon the right to the equator.

Following appears an article entitled, "Can the Parallax of the Fixed Stars be made Perceptible?" by Chas. H. Kummell, Washington, D. C. The instrument suggested resembles a Helmholtz telespectroscope to be used as a photograph instrument. Views of the same stars (without proper motion) are supposed to be taken semi-annually, and through the stereopic effect of such celestial objects a new method of measuring parallax is expected. It suggested also that this means would also offer an easy method of searching for stars with sensible parallax.

In No. 2800 is found new elements for Comet 1846 VI (Peters) and 1884 III (Wolf) and John M. Thome's Cordoba Observations of the Great Southern Comet 1887 I. In No. 2802 elements of the orbit of Comet 1882 I are given, and the elements of a provisional orbit for the close pair (A B) of the triple-star 12 Lyncis (Σ 948) as follows:

$$\begin{array}{ll} P = 485.8 \text{ years} & \Omega = 166^\circ\ 30' \\ T = 1716.0 \text{ A. D.} & \lambda = 93\ \ 36 \\ e = 0.229 & a = 1.64'' \\ \gamma = 46^\circ\ 3' & \mu = -0.741 \end{array}$$

After the elements follows a comparison between the recorded measures and the positions computed from the above elements.

Astronomical Journal, No. 159 concludes John N. Stockwell's paper "On certain inequalities in the moon's motion arising from the action of the planets," in which he says:

"It is therefore manifestly useless to seek for an explanation of the empirical equation discovered by Professor Newcomb in the action of planets on the moon. In fact, the explanation of that equation which is given in (A. J.) No. 149, viz; that it is simply the correction of the adopted value of the inequality of longitude due to the oblateness of the earth, in order to reduce it to its true value, is so obviously correct that no further explanation seems necessary, at least until it has been clearly shown that the explanation there given is inadequate for the purpose."

In No. 160 appears improved elements and ephemeris of comet 1887 *e* by C. S. Chandler Jr.

A determination of the cöefficients of expansion of the glass plates used for stellar photography at Cordoba, in the years 1872 to 1876, and 1880 to 1883, by Professor W. A. Rogers of Colby University. The results of a long series of observations is the establishment of the invariability of the cöefficients of expansion of Bailey's metal, of Jessup's steel, and of Chance & Son's glass between the limits of — 3° and + 39° Fahrenheit. The critical time of quiescence for these observations was found to occur on clear days about half an hour after sunrise, and on cloudy days at a considerable later time. The absolute cöefficients of expansion of the specimens of photographic glass in hand were 0.438μ and 0.412μ.

EDITORIAL NOTES.

Necessary absence from home and the State, during the early part of September, made it quite impossible to publish the MESSENGER for the month of September on time, hence the present double issue.

Recent Showers of Meteors (continued from SIDEREAL MESSENGER, April, 1887, p. 160).—Between March 13 and July 31, 1887, I saw 759 meteors during 118 hours of observation. May was a very cloudy month here, but both June and July have been exceptionally clear. The following list of radiants includes the best showers derived from these new observations:

Epoch of Shower.	Night of Max.	Radiant Point. R. A. Dec.	No. of Meteors.	Appearance.
1887.				
March 13-24	March 24	161° + 58°	7*	Rather slow.
March 21-24	March 23	190 + 20	5*	Rather swift.
March 25-27	March 27	229 + 32	5	Swift, small.
March 28	March 28	263 + 62	5	Slowish.
April 12-25	April 20	189 + 20	7*	Slow.
April 17-19	April 18	213 + 53	7	Swift, short.
April 17-25	April 18	231 + 17	10	Very swift.
April 18-20	April 20	269 + 32	16	Swift, streaks.
April 18-26	April 25	272 + 21	10	Swift, short.
June 10-17	June 15	291 + 52	8	Swift, short.
June 10-28	June 20	335 + 57	10	Swift.
June 11-19	June 18	274 + 69	9	Very swift, short.
June 12-21	June 15	285 + 23	11	Slowish.
June 13-20	June 18	302 + 24	11	Rather swift.
June 13-21	June 17	270 + 47	5	Slow, bright.
June 14-20	June 17	268 — 24	5	Very slow, bright.
June 14-26	June 26	354 + 39	6	Very swift, streaks.
June 17-23	June 20	280 + 43	10	Swift, faint.
June 17-23	June 17	252 + 11	7	Very slow.
June 21-26	June 25	238 + 47	7	Slow, small.
July 12-27	July 12	333 + 12	17*	Rather slow.
July 14-31	July 29	269 + 49	13	Swift.
July 16-31	July 23	335 + 49	16	Swift, short.
July 22-29	July 22	16 + 31	7	Swift, streaks.
July 22-31	July 31	46 + 26	7	Swift, streaks.
July 25-29	July 28	337 — 12	27	Slowish, long.†
July 27-29	July 27	260 + 69	7	Slowish.
July 27-31	July 28	34 + 18	8	Swift, streaks.
July 27-31	July 31	5 + 10	6	Swift.

*** Including one or two meteors registered at same epoch in preceding years.**

† This shower in Aquarius represents the special display of the July meteoric epoch. In 1883, on July 28, I found the radiant at 337° — 11°; on July 28, in 1879, at 338° — 14°; and on July 27-31, 1878, at 341° — 13°.

In addition to the above streams I observed many others apparently of less importance, except the Perseids which have really formed the richest shower of all. Between July 18 and 31 I recorded 53 meteors belonging to it, and the radiant point has exhibited the same progressive motion amongst the stars as noticed here in previous years, as will be seen by the following positions determined on various nights:

Date.	Radiant.	No. of Meteors.	
1887.	R. A. Dec.		
July 19	19° + 51°	4	The Perseids are swift, bright meteors, leaving phosphorescent streaks.
July 22-23	25 + 52	9	
July 27	29 + 54	5	
July 28	30 + 55	10	
July 29	31 + 54½	10	
July 31	35 + 54	11	

In 1885, on July 13, I saw some swift, streak-leaving meteors from 11° + 48° and I believe these were a very early display of the same stream. There is very strong if not conclusive evidence that this celebrated shower endures from July 13 to August 22, and that the radiant advances during this interval from 11° + 48° to 76° + 57°. W. F. DENNING.

Bristol, England, Aug. 1, 1887.

Comet f 1887 (Brooks).—Under date Aug. 27, the following letter was received from Mr. Brooks concerning the discovery of the above named comet:

"On the morning of August 25th, 1887, while sweeping low down in the eastern heavens I discovered a new comet, in the constellation Cancer, approximate position R. A. 8*h* 33*m*, Decl. north 29 degrees. I verified the discovery by a second observation the following morning and found the comet to be moving at the rate of about ¾ of a degree daily in an easterly direction. Also observed it again this morning, August 27, when it was in field with the wide double-star *i* Cancri and about 20′ north of the star. Comet is brightish with eccentric condensation."

Elements and ephemeris of Comet *f* 1887 (Brooks), by H. V. Egbert. From observations on Aug. 26, 28 and 30, I have derived the following elements of Comet Brooks:

$$T = 1887,\ \text{Oct. } 6.480\ \text{Gr. M. T.}$$

$$\left.\begin{aligned} \omega &= 63^\circ\ \ 18' \\ \Omega &= 84\ \ \ 33 \\ i &= 44\ \ \ 10 \end{aligned}\right\}\ \text{App. Eq.}$$

$$\log q = 0.08719$$

$$C - O$$

$$\Delta\lambda \cos\beta = -0.05$$

$$\Delta\beta = -0.07$$

Ephemeris for Greenwich midnight:

1887.	A. R.	Dec.	Lg. r.	Lg. Δ.	L.
Sept. 6	9*h* 27*m*	—30° 11′	0.1164	0.3093	1.21
10	9 46	30 13	0.1096	0.3025	1.29
14	10 05	30 05	0.1036	0.2964	1.37
18	10 25	29 49	0.0983	0.2912	1.44
22	10 44	29 23	0.0939	0.2868	1.49
26	11 04	28 48	0.0906	0.2834	1.54
30	11 24	28 03	0.0884	0.2810	1.57
Oct. 4	11 43	27 08	0.0873	0.2796	1.59

These elements agree quite closely with those of the Olbers comet of 1815 and establish the identity of the two comets.

Dudley Observatory, Sept. 3, 1887.

This comet has a stellar nucleus and a faint tail. Before its orbit was known Mr. Brooks suspected its periodic character and of course is pleased to find that it is the return of the Olbers comet. We are also told by a ready computer, that the elliptical elements by Ginzel give a perihelion time which represents some observations within one minute of arc in both latitude and longitude.

This comet was first seen by Olbers in 1815, and at that time its periodicity was recognized. About three years ago its orbit was carefully investigated, and it was then announced that it would probably return in December, 1886, but the time was uncertain to the extent of one and one-half years, its period of revolution being about seventy-two years. This is especially interesting in view of the fact that it takes its place in the list of long-period comets as third, which has been established by observed return to perihelion. The other two are Halley's with period of 76 years and the Pons-Brooks comet of 1812, re-discovered by Mr. Brooks Sept. 1, 1883, and which has a period of 71 years, 4 months and 10 days. Its path for the month of September was eastward, though the constellation of Leo being nearly north of Denebola Sept. 30.

Memorandum of Miscellaneous Special Observations, recorded at the Chabot Observatory, 1886-87.—Lat. = + 37° 48′ 05″; Long. = 8*h* 09*m* 06.3*s* W.

1886.

June 7th. Transit egress of Jupiter's Satellite III noted at 9*h* 26.5*m* local mean astronomical time. Obs'r., C. B. H.

June 13th. The occultation of star η *Libræ* by moon observed with equatorial dis. by chron. No. 1744 at 15*h* 06*m* 49.3*s*, chron. fast of local sidereal time 1*m* 15.37*s*, and true sidereal time = 15*h* 05*m* 33.9*s*. Obs'r., C. B. H.

July 1st. Eclipse reappearance of Jupiter's Satellite I noted at 15*h* 43*m* 28*s* on sid. chron. No. 1744, which was fast of local sidereal time 1*m* 11.5*s*. Observation at 9*h* 01*m* 36.2*s* L. M. T. (By Amer. Ephemeris = 9*h* 01*m* 36.0*s*.) Obsr., C. B.

July 3d. Oc. dis. of Jupiter's Satellite II noted at 9*h* 13*m* L. M. T. Atmosphere bad, and obs'n. uncertain. Obs'r., C. B.

July 5th. Occultation of "*c Leonis*," 5th mag., observed through finder of Equatorial by C. B.; obs'r. at chron., C. B. H., and time 14*h* 58*m* 21.8*s*. Chronometer fast of local sidereal 1*h* 10.5*s* and observation at 14*h* 57*m* 11.3*s* L. S. T.

November 12th. Occultation of "*a Tauri*," 1st mag., observed with 8½ inch Equatorial; using sidereal chronometer No. 1744.

Dis., 23*h* 23*m* 45.7*s*, power 300.
Reap., 00 20 02.3 " 50.

Both observations considered excellent; atmosphere good. Corr'n. to chron. — 00*m* 16.9*s* and — 00*m* 16.8*s*, and true

Dis. = 23*h* 23*m* 28.8*s* local sid. time.
Reap. = 00 19 45.5 " " "

Obs'r., Burckhalter.

1887.

February 2d. Occultations of stars in the "Hyades" cluster, with powers of 40 and 100 on the 8½ inch Equatorial, viz.:

	Star.	True L. Sid. T.	Remarks.
Dis.	70 Tauri	= 5*h* 50*m* 14.8*s*	Not instantaneous.
"	θ Tauri	= 7 51 27.1	Good; sudden.
"	Arg. + 15.633°	= 7 55 16.2	" "
"	Arg. + 15.635°	= 8 32 03.6	" "
Reap.	θ Tauri	= 8 36 55 ± 05*s*	Poor observation.
Dis.	B. A. C. 1391	= 8 46 50.7	Good; sudden.

Obs'r., C. B. H.

March 29th. Occultation of "*α Tauri*," observed with Equatorial during full daylight on afternoon of this day. Dis. at 3*h* 28*m* 59.6*s* by sid. chron., power 90; star bright and steady. Reap., with power of 200 diameters, at 4*h* 56*m* 49.8 by chron. Corr'ns. to chron. = + 3*m* 16.3*s* and + 3*m* 16.4*s*.

Dis. = 3*h* 32*m* 15.9*s* local sid. time.
Reap. = 5 00 06.2 " " "

Obs'r., C. B. H.

C. B. = Mr. Chas. Burckhalter.
C. B. H. = Mr. Chas. B. Hill.

Magnitudes of Stars for Nautical Almanacs.—Since our last issue, we have learned with pleasure that Professor Pickering proposes to publish a series of short articles on astronomical and meteorological subjects to be prepared at Harvard College, Observatory. The first of this series will be on the magnitudes of standard stars of the various Nautical Almanacs now in use in different countries. Having noticed that Almanacs varied much in the brightness assigned to particular stars, and that generally the best determination was not in use, Professor Pickering proposed to the superintendents of Nautical Almanacs in different countries to furnish them a discussion of the magnitudes of the standard stars, as derived from best modern determinations. Favorable replies were received from all except England and Germany, so that the work will go forward. The plan proposed is to take for each star a magnitude which is the mean of those given by four standard authorities, the Harvard Photometry, the Uranometrea Argentina, Wolff's photometric observations and the Uranometrea Oxoniensis.

Total Solar Eclipse of Aug. 19.—The preparations for the observation of the total solar eclipse of Aug. 19 were probably more general and complete than ever before. The many questions that astronomers are anxiously waiting to settle by the aid of such rare and momentary opportunities as the eclipse affords, are a powerful incentive to use fully and well all such occasions when they come. The study of the corona and the chromosphere by the aid of the spectroscope and photography, the search for intra-mercurial planets, and attempts to measure the precise ratio of the apparent diameters of the sun and moon were objects of eager pursuit by a large company of observers stationed at all prominent points along the long path of the shadow. In eastern Prussia the failure was almost complete, likewise in Russia except at Petroosk, where six drawings of the corona were made and two photographs taken. At Spirous the total phase was visible twenty seconds, and at Odessa a complete spectrum of the cörona was obtained.

Elsewhere nothing was obtained of value on account of bad weather, unless in northern Asia and Japan, from which sources reports are not yet received.

Note on G. C. 4333.—The description of this nebula given in the General Catalogue differs so greatly from those made at this observatory as to give rise to a suspicion of change in its appearance. It has been but little observed and unfortunately never sketched until recently. The following descriptions of it are all that I have been able to collect:

GENERAL CATALOGUE. Pretty bright, pretty small, round, gradually brighter in the middle, suspicion of resolvability.

LORD ROSS 1848. Curious circular-shaped nebula with a large dark spot at one side, around which is a close cluster of well defined, very small stars.

DR. SCHÖNFELD 1863. Round, compact, middle bright, equals a 11 or 12 magnitude star, diameter 0.3′.

1868. Perfectly round, very compact, much brighter to the middle, middle bright, equals a 11 or 12 magnitude star, diameter 0.2′.

MULLER 1887. Two nuclei forming an elliptical nebula, elongated 150°, largest diameter 26″, northern nucleus brighter. A sketch shows each nucleus to be elongated in the direction 90° ±, the center being almost devoid of nebulosity. The nuclei are entirely separated from each other except by very faint nebulosity, and are of the 12.5 magnitude.

LEAVENWORTH 1887. Irregular ring nebula, consisting of two nuclei; north nucleus brightest and clearly defined, south nucleus rather diffuse, magnitude 12.0. Sketch shows a dark spot in the center.

1887. Sketch shows two nuclei which together form an elliptical nebula with a dark spot in the center. Four very faint stars seen in the nebula; otherwise no indication of resolvability, even with power 850.

The nebula observed at this place must have been the one observed by Dr. Schönfeld, since the difference in position as compared with a neighboring star is almost absolutely the same. While the difference in description may be due to change in the shape of the nebula, we think it more probably due to differences in size and especially power of the telescope used. Yet it is difficult to reconcile two such opposite statements as much brighter to the middle and annular with dark hole in center. F. P. LEAVENWORTH.

University of Virginia.

Ephemeris of Comet 1887 f.—As satisfactory material is not yet at hand to correct all the elements of the orbit of Comet Olbers 1815, I have, from four observations on Aug. 27, 28, 29, 30, determined the perihelion time by the use of Ginzel's ellipse, and have based the following ephemeris upon Ginzel's elements, using the perihelion time derived as stated above.

Perihelion time was found to be Oct. 8.50398 Gr. M. T., and the other elements by Ginzel as given in Viertel. der Ast. Gesell., 17 Yahr. Zweites Heft are :

$$\left.\begin{aligned} \text{☊} &= 84^\circ\ 31'\ 24.2'' \\ i &= 44\ \ 33\ \ 34.3 \\ \pi &= 149\ \ 48\ \ 40.3 \end{aligned}\right\}\text{Mean Equinox 1887.0}$$

$$\varphi = 68\ \ 31\ \ 03.0$$

$$u = 49.387785''$$

The correction of this ephemeris on Sept. 16 was $+ 4s$ and $- 9''$, showing that Ginzel's elements are not far from the truth.

Ephemeris for Greenwich midnight:

1887.	A. R.		Dec.		Lg. r.	Lg. Δ	L.
Oct. 4	11*h*	43.5*m*	+27°	06′	0.0799	0.2746	1.68
8	12	02.8	26	02	0.0794	0.2740	1.69
12	12	21.7	24	51	0.0800	0.2744	1.69
16	12	40.1	23	34	0.0816	0.2758	1.66
20	12	57.9	22	12	0.0843	0.2782	1.62
24	13	15.1	20	46	0.0880	0.2814	1.57
28	13	31.6	19	17	0.0927	0.2853	1.51
Nov. 1	13	47.4	17	46	0.0983	0.2900	1.44
5	14	02.6	+16	16	0.1047	0.2952	1.37

Unit of light = Aug. 26. H. V. EGBERT.

Dudley Observatory, Sept. 22, 1887.

Lick Observatory.—By kindness of Assistant J. E. Keeler, the MESSENGER has been informed of the progress of work at the Lick Observatory, Mt. Hamilton, Cal. The great dome for the 36-inch equatorial works beautifully. It revolves with a longitudinal pressure of 225 lbs., so that the hydraulic machinery provided is almost unnecessary, so that if water from the tank runs low the dome can be easily moved by hand. The shutters, when in place, will add something to the weight to be carried.

The castings for the lower part of the pier are already in place. The lowest section is 17 feet by 10, and is the heaviest piece, so far, hauled up the mountain. While in Cleveland recently, at the shops of Messrs. Warner & Swasey, we saw the fourth section of the pier to which the axes and telescope tube are to be attached. When in place the entire pier will be 36 feet high, measuring from the floor of the observing room to the center of motion of the telescope. The polar axis lay beside the head-piece of the pier, and it is apparently a noble piece of steel, as hard as it could be made, one foot in diameter and 10 feet long, weighing one and one-half tons.

The mechanical device resorted to by the makers to overcome the thrust on the lower bearings of the polar axis, occasioned by its own weight and the weight of the telescope when in place, is novel and ingenious. In the adjacent faces of the two main bearing plates two circular and concentric tracks are cut large enough to receive steel balls three-fourths of an inch in diameter. These grooves are filled close with these balls, allowing the plates to have only small pressure on each other, thus changing the large part of sliding friction into rolling. A method somewhat similar is adopted for the declination axis. By these auxiliaries it is confidently believed that this immense tube will be under easy control of a single person with the aid of dangling ropes or other inconvenient tackle used sometimes to harness large telescopes.

From the floor a winding stairway of iron is placed at the south end of the pier, with a landing at the base of the head-piece of the column, where an opening in pier is made and within is placed the great driving clock for the telescope. Within the pier, at this height, is space enough for a man to walk around the clock, examine and adjust all its working parts. Continuing up the stairway about twelve feet higher a second landing extending around the pier, with railing, is reached, which brings the observer near the great circles and all the quick and slow motion apparatus for controlling the telescope. This platform is the place of the assistant observer in working hours, when the astronomer desires such help. Any change of instrument or reading of circles, however, can be done independently by the observer at the eye-piece without the aid of the assistant.

The making of the great tube, 56 feet long and four feet in diameter at the ends, with greater diameter at the middle, seemed no small task, as its sections were being drilled and fitted by special machinery made for this particular work by Warner & Swasey.

The mounting is being pushed forward with all desirable haste, considering the many difficult problems which these makers have been compelled to solve by their own skill, as machinists, for they have no precedents to go by. A few days more only are needed to complete it, and then it will be transported to Mt. Hamilton where a great dome 75 feet in diameter will be in readiness for it.

Mr. Alvan G. Clark has just returned from Paris where arrangements were made, as we are informed, for a disc for a photographic corrector to take the place of one broken in the process of grinding a few months ago.

Concerning the work on subsidiary instruments in the hands of J. A. Brashear, of Allegheny City, and Messrs. Fauth & Co., of Washington, D. C., more will be said in our next issue.

Corregenda in Various Star Catalogues.—Dr. C. H. F. Peters has done further excellent service to astronomy in the detection of errors in various star catalogues and in publishing them under the above title as an eleventh memoir in the Proceedings of the National Academy of Sciences. The catalogues containing errors so noticed are Oeltzen's Catalogue of Argelander's Southern Zones, the Catalogues of Vol. VI of the Bonn Observations, Weisse's Bessel, Runker's Catalogue of 12,000 Stars (old and new series), Schjellerup's Catalogue, Bailey's Lalande, Yarnell's Catalogue (2d ed.), Glasgow Catalogue, Moesta's Santiago Observations, and the Geneva Observations 1842–49.

The paper also contains a list of "anonymous" identified in other catalogues.

The care with which Dr. Peters does all work is guarantee that no pains has been spared to give best results possible with means within reach.

The Sidereal Messenger,

CONDUCTED BY WM. W. PAYNE,

Director of Carleton College Observatory, Northfield, Minnesota.

VOL. 6, NO. 9. NOVEMBER, 1887. WHOLE NO. 59.

THE REJECTION OF DISCORDANT OBSERVATIONS.

ASAPH HALL.*

For the MESSENGER.

The question, what shall be done with observations that present large discordances from a mean value, is one that will continue to perplex investigators for a long time. The founders of the method of least squares have called attention to the fact that the law of error which is derived from the assumption of the arithmetical mean as the most probable value is not rigorously correct. This is expressly stated by Gauss in his Theoria Motus; and for a finite number of observations, a condition which always holds in practice, similar statements have been made by Laplace and Poisson, to whom we are indebted for the most general investigations of the law of error. The rule of the arithmetical mean is justified on the ground that it corresponds to common sense and universal practice. Criticisms and improvements of the method of least squares, therefore, will always be in order, whether they pertain to modifications in the law of error itself, or simply propose to change some of the constants into more general functions to be determined to suit special cases. Let us look at some of the results obtained by means of the methods proposed for dealing with this difficult question.

PEIRCE'S CRITERION.

This criterion was given by Professor Benjamin Peirce in *Gould's Astronomical Journal*, vol. 2, p. 161. Though warmly recommended by several eminent American astronomers, this

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* U. S. Naval Observatory, Washington, D. C

criterion has never come into general use, but it has been employed for a long time by Mr. C. A. Schott, chief of the computing division of the United States Coast and Geodetic Survey. Such a practical recommendation is important, even though Mr. Schott qualifies it by the warning that care must be taken in its use, since sometimes this criterion "cuts too deep." Professor Peirce applied his criterion to two examples. The first is that of thirty observations of the semi-diameter of Venus made with the mural circle of the Naval Observatory in 1846. After a reduction of these observations Professor Peirce finds the probable error of a single determination to be ± 0.28″. His criterion rejects no observation. The second example is that of fifteen observations of the same semi-diameter made with the meridian circle of the Naval Observatory in the same year. In this case the probable error of a single determination is ± 0.39″; and two observations giving the residuals +1.01″, and — 1.40″, are rejected by the criterion. After this rejection the probable error of a single determination is ± 0.21″. This diminution of the probable error is a suspicious circumstance. It is well known that observations with the mural circle were more accurate than those made with the old meridian circle; and the values of the probable errors found before the criterion was applied correspond very well to the relative accuracy given by a large number of observations. It is perhaps from such consideration as this, and from noticing how an apparent accuracy may be given to inferior work, that this criterion has been prevented from coming into general use. Since this second example is often referred to by writers on this subject it may be well to state, though it does not affect the argument, that the records show that not one of the fifteen observations was made by Lieutenant Herndon.

STONE'S CRITERION.

Mr. E. J. Stone, the present Radcliffe observer, does not accept the criterion of Professor Peirce, and in the *Monthly Notices* of the Royal Astronomical Society, vol. 28, p. 165, he gives a rule of his own. If a given observer makes one careless observation in 500, Mr. Stone finds that the limit of rejection is about five times the probable error of an observation. Thus if the probable error is ± 0.48″, and an observation gives the residual 2.5″, this observation should be rejected. This is a very simple criterion, and can be applied easily. We infer from the method of least squares that in a thousand observations there will be but a single error greater than five times the probable error. The application of Mr. Stone's criterion, therefore, could not do much harm, and yet I think astronomers would hesitate in applying it. The probable error ± 0.48″ is about that of an observation with the common meridian circle, and unfortunately residuals as great as 2.5″ are still apt to occur in this kind of work.

DE MORGAN'S METHOD.

Professor Augustus De Morgan proposed a method of weighting observations by means of approximate solutions until the assumed and deduced weights agree. Not having seen De Morgan's exposition of his own method I cannot speak of it with accuracy. It has been referred to as a method of meeting the difficulty of discordant observations by diminishing their weight. The weight depends on the probable error, which should be found from the whole series of observations. If the discordant observations are so managed as to be given arbitrarily a very small weight this process would become equivalent to a rejection. The criteria of Peirce and Stone seem to meet the question more fairly.

Besides these criteria two other methods have been proposed for use in the discussion of observations. The first is a proposal to change the law of error which is adopted in the method of least squares for one in which the first power of the error enters. In his earlier investigations Laplace adopted a law of this kind, but he soon discarded it for the one now used. It is true that the strict method of least squares sometimes leads to great labor of computation, and it may be well in some cases to shorten the work by grouping the observations. This can be done without changing the law of error. Again it may be sufficient for some observations to reduce the equations of condition by the old method of making the sign of similar terms the same, then adding all the equations, and thus deducing a set of normal equations. This will generally give a good result, but if the observations are accurate it is better to spend a little more labor on them.

Another method has been proposed for the treatment of discordant observations in which the law of error assumed in the method of least squares is retained, but the measure of precision is made a function of several unknown quantities, which are to be determined from the residuals found from a preliminary comparison. There can be no doubt that in a given series of observations the measure of precision is not a constant, as is commonly assumed; and it must certainly be subject to small fluctuations dependent on the condition of the observer and his instrument. It might vary with the daily allowance of food and stimulant provided for the observer. Now it seems probable that if we split an assumed constant into several parts, and attach various meanings to them we shall be able to represent the observations more accurately. In the case of the fifteen observations of the semi-diameter of Venus observed in 1846 such a process would permit us to represent the residuals very well. But into how many parts shall we divide the measure of precision ; two, five, or fifteen ? We might assume a periodical series and satisfy the residuals exactly; but after all the labor one cannot avoid the suspicion that this procedure is nothing more than an attempt to conceal ignorance under mathematical forms.

The question recurs whether there is any advantage in the discussion of observations by means of these rules over the judgment of an experienced investigator. I think the reply must be in the negative. For most cases of discordance that occur in practical astronomy the judgment of an experienced astronomer will be better than any arbitrary rule. Take for example the observations of a solar eclipse, or the transit of one of the interior planets. For several reasons such observations are liable to large errors. The heat of the sun and its effect on the eye and instrument in confusing the images, and the slow motion of the planet, make a little maladjustment of the instrument capable of producing errors of 20 or 30 seconds of time. The usual result is that many observations are made by inexperienced observers, and sometimes by those who have no good means of knowing their local time. Now an astronomer of experience will extract a better result from these observations than can be found by any set of fixed rules. His methods may seem arbitrary, but he is the last and best resort. Of course in cases where each observer has made a considerable number of observations the probable errors of the different series will furnish the combination weights.

There is one peculiarity of observations which may seem to partake of a discordant nature that should not be lost sight of. This is what Mr. M. H. Doolittle of the Coast Survey office calls their *instructive* character. Observations should not be rejected until they have been carefully scanned, since they may contain important information. Several inequalities in the motions of the planets and the moon have been discovered by apparently discordant observations, and although satisfied for a time by empirical formulæ they have frequently led to theoretical explanation. We sometimes see careful observations in astronomy that evidently contain systematic errors. These errors are commonly included under the vague terms of "flexure" and "refraction;" and generally they may be taken as good evidence that the astronomer has not become master of his instrument. Such results should spur him on to further investigation.

The method of least squares is a beautiful application of the theory of probabilities, and it is the best method of treating observations that has been devised. It is an instrument, and observers should become familiar with it and learn to apply it correctly. It was never designed, as Delambre sarcastically remarks, to make good observations out of poor ones, but it gives the best result fron the given data.

October 6, 1887.

THE NEW REPSOLD MERIDIAN CIRCLE OF CARLETON COLLEGE OBSERVATORY.

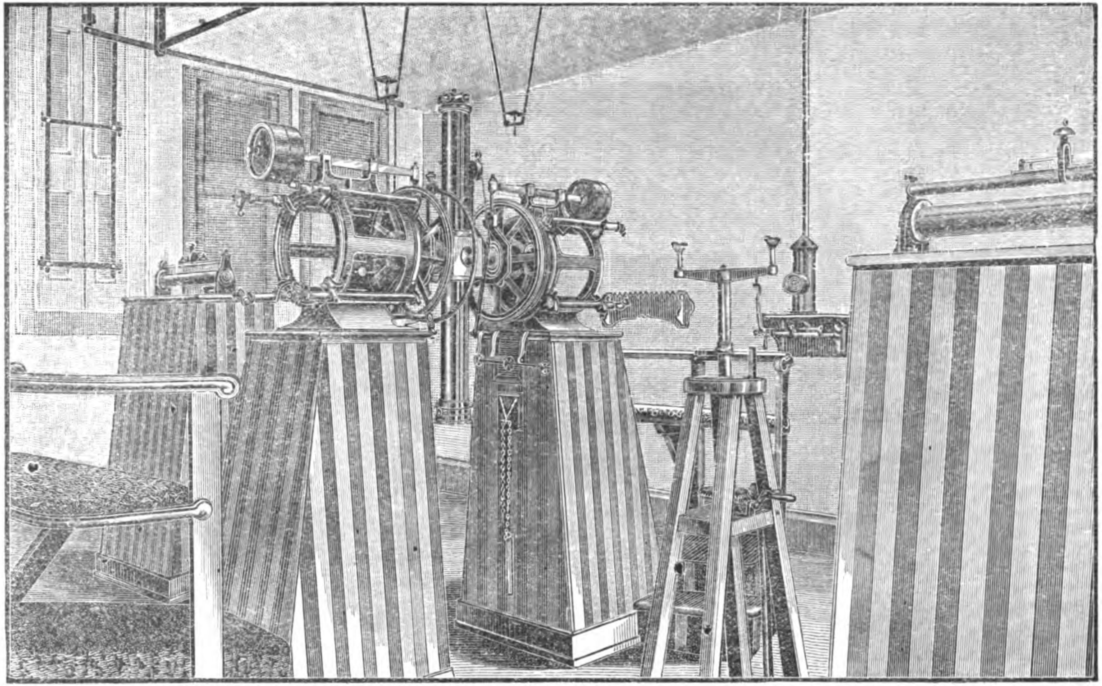
THE EDITOR.

The new Meridian Circle, now mounted in the new astronomical observatory of Carleton College, Northfield, Minnesota, was made by Messrs. A. Repsold and Sons, Hamburg, Germany. It was about two years in progress of construction, being completed in the summer of 1885. It was received late the same year and the erection of the new building for the observatory began in the summer of 1886, and it is not yet fully completed, though it has been occupied since August last.

For the purchase money and the cost of mounting the new instrument the observatory is indebted to James J. Hill, President of the St. Paul, Minneapolis and Manitoba Railway Co., whose generous gift of $5,000 has proved amply sufficient. While conversing with Mr. Hill concerning the projected lines of scientific research before the new observatory of Carleton College, we were delighted to recognize his intelligent interest and quick insight in, and ready comprehension of, present phases of scientific thought and the advantages of the best and most powerful modern appliances in promoting original investigation. Though Mr. Hill is well known for his many gifts to science and for the public good generally, it is all done so unostentatiously that no one competent to judge can know the facts without true admiration.

The east wing of the observatory is devoted entirely to the Meridian Circle. It is 27 feet 6 inches north and south, and 22 feet 10 inches east and west, inside measure. From floor to ceiling it is 12 feet 2 inches. The four piers, seen in the accompanying cut, stand on three separate foundations. All have good footing 10 feet below the floor in coarse gravel. They are built of limestone and the best cement. Above the floor each of the piers is covered with heavy felting and finished outside with cherry and ash. At the base below the floor the respective sizes of the piers are as follows:

Central one is 9 feet by 3 feet 6 inches.



Meridian Circle Room, Carleton College Observatory.

The bases of the north and south collimator piers are equal, being 4 feet by 3 feet 6 inches. The cellar is the same size as room above, with good wall 18 inches thick on all sides, with one inside entrance and two outside ventillating windows. The depth of cellar is 8 feet.

The north and south openings of the observing room for the meridian are 26 inches wide, and provided each with two shutters securely held in place by the iron adjustable frame, commonly used in observatories for this purpose, and plainly shown in the cut. There are two doors for the meridian opening in the roof, each about 14 feet long and 3 feet wide outside. These doors are opened easily and quickly by levers of steel 10 feet long and 2 inches in diameter, weighing each about 80 lbs. The doors are hinged to the roof and their weight is partly counterpoised by the weight of the levers so that a pull of 35 lbs is sufficient to open them under any circumstances if clear of snow or sleet. Their operation is very satisfactory.

By referring to the cut the collimators and the mode of mounting them will be easily understood. The glass cases which cover the telescopes have been removed and the levels put in place, to show their relation to the collimators. Their apparent unequal size is due, of course, to perspective, the point of view being the southwest corner of the observing room. The collimating telescopes are 33 inches long, with objectives 2.64 inches in diameter, and the main telescope between the middle piers (in vertical position) is 58 inches long. The object glass was made by A. Clark & Sons, Cambridgeport, Mass., and has a clear aperture of 4.80 inches and a focal length of 57.5 inches. The diameter of the graduated portion of the circles is 21.8 inches. One circle is movable, and is divided to degrees, the other is fixed and divided to two minutes of arc. The circles are each provided with four microscopes having micrometers which measure directly to one second of arc and which are adjusted closely enough to detect an error of one-tenth of a second of arc. These microscopes have already been placed symmetrically on the circular iron frame as is desirable to read the circles properly.

The micrometer of the telescope is provided with a spider-line reticle which contains seven groups of wires, numbering 27 in all. One revolution of the right ascension micrometer screw has been carefully determined and is 64.12″. The value of the micrometer screw in declination is yet only approximately ascertained. The measure of both micrometer screws for the collimators is the same, and was determined by the aid of the wire intervals of the transit instrument. Value of one revolution is 54.90″.

The wire intervals of the transit instrument have all been determined by transits of circumpolar stars, and the central groups also by micrometer measurement. These fixed wires all seem to be well in place except one.

In this connection, it may be well to mention a little experience that we have had, in the displacement of these delicate spider threads. After using the micrometer a short time, the motion of the screw was not entirely free through the run of the field. The head was carefully taken apart, and the movable frame of the reticle adjusted to easy motion. In doing this, in some unaccountable way, two of the nearest parallel fixed threads were thrown into the same groove, and were very closely side by side. To replace them without destruction seemed at first a problem of some difficulty. A pair of tweezers, with a hair about one inch long, was the instrument chosen for the work. In the steady hand of Dr. Wilson, this device accomplished the work in a very few minutes satisfactorily. The hair was also used to remove from the other threads particles of dust which had lodged upon them.

One division of the hanging level has been found approximately. The instrument has four eye-pieces which have been measured by three different observers with the following results for magnifying power:

I = 91.
II = 133.
III = 195.
IV = 268.
Diagonal V = 202.

The magnifying power of the eye-pieces of the collimators is 100, and the same for each instrument.

Observations for the latitude of the observatory have already begun and the work on twenty stars, on different nights, is yet unreduced. The work for the Meridian Circle in the near future, besides the study of the errors of the instrument, will be, observations for latitude, time, azimuth; comparison stars for comets; comparison stars used by Professor Stone, of Leander McCormack Observatory, in his work on the places of nebulæ, and a select list of stars from 4th to 8th magnitude for geodetic purposes.

Dr. H. C. Wilson is in charge of the Meridian Circle, and he is making good progress, as the preceding record shows, for a period of less than four weeks' time.

ON THE RELATIVE MOTION OF THE EARTH AND OF THE LUMINIFEROUS ETHER.

ALBERT A. MICHELSON AND EDWARD W. MORLEY.

(Abstract.)

To explain astronomical aberration according to the undulatory theory of light, Fresnel made two suppositions: First, that the ether is at rest except in the interior of transparent media; and secondly, that in such media it is moving with a velocity less than that of the medium in the ratio $\frac{n^2 - 1}{n^2}$ where n is the index of refraction. The second hypothesis is already established; the authors have now submitted the first to the test of experiment, by the method proposed and executed by Michelson in 1881, so modifying the scale of the experiment and the mounting of the apparatus as to obtain decisive results.

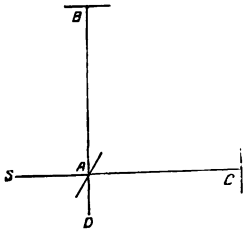


Fig. 1.

Let light from *s* be partly reflected to *b* and partly transmitted to *c*, and be returned by mirrors at *b* and *c*; part of the returning light will unite along *ad*, and, if the two paths are equal, will produce interference. If now, the ether being at rest, the apparatus moves in the direction *sc* with the orbital velocity of the earth, the directions and distances described by the rays will be altered; if we put D for the distance *ab*, and v and V for the velocities of the earth and of light, we shall find the difference of path to be $D\frac{v^2}{V^2}$ If the apparatus be now rotated so that *ab* is in the direction of the orbital motion of the earth, the difference will be in the opposite direction, and the displacement to be observed will therefore be $2D\frac{v^2}{V^2}$

In the experiment of 1881, D was such that the displacement according to theory would be 0.04 wave length; in the present case, D was, by repeated reflections, made about ten times as large. The former apparatus was extremely sensitive to vibrations and suffered distortion during the rotation; these difficulties have now been entirely overcome by mounting the apparatus on a massive stone floating on mercury.

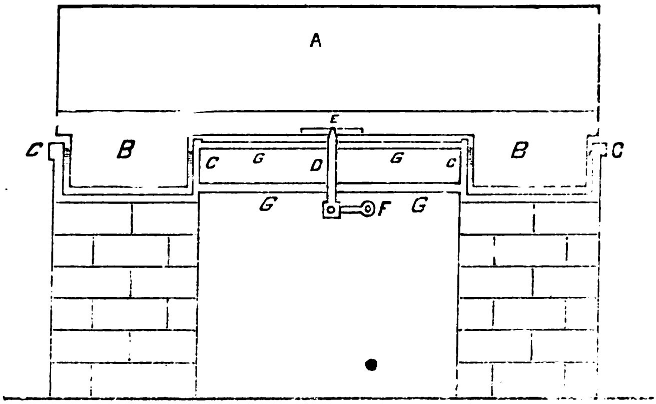


Fig. 2.

a is a stone 1.5 metres square, and 3 decimetres thick. *bb* is an annular wooden float. *cc* is an annular trough containing mercury; between the float and the trough is a clearance of one centimetre. A pin *d* can be pushed into a socket *e* so as to keep the float concentric with the trough; the pin bears no part of the weight.

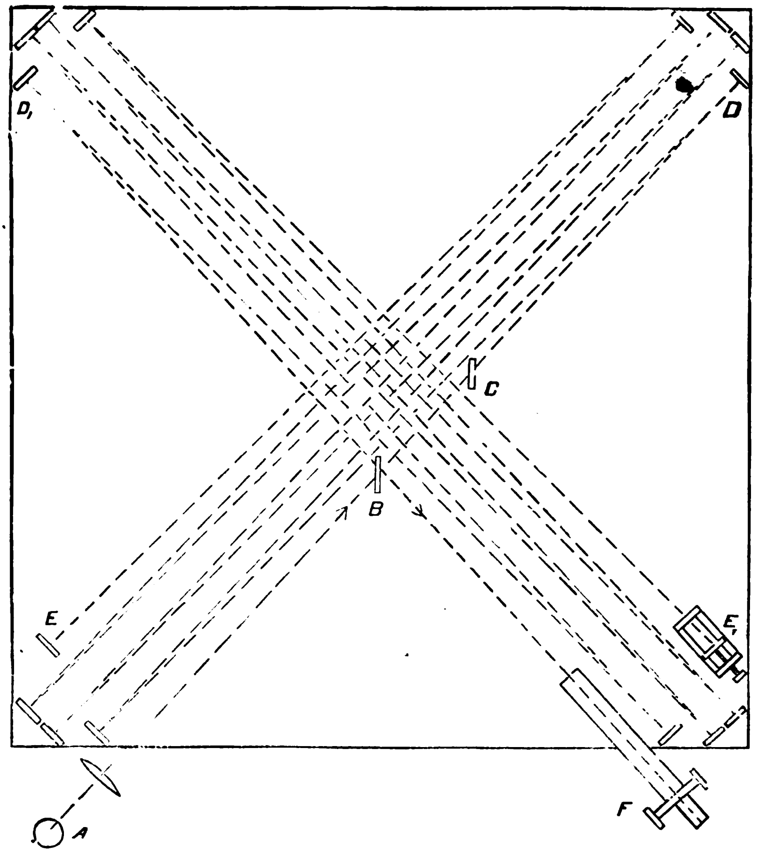


Fig. 3.

At each corner of the stone are four plane mirrors, *e*, *d*, of speculum metal. *b* and *c* are plane parallel glasses. Light from an Argand burner at *a* divides at *b*, follows the paths indicated, and reaches the observing telescope *f*.

The mirrors having been adjusted so that both rays entered the telescope, the lengths of the two paths were made nearly equal by measurement and by moving the mirror *e* which could be moved in the direction of its normal, keeping very accurately parallel to its former plane. The telescope being adjusted to distinct vision of the source, the two images were made to coincide. Then the telescope being adjusted to distinct vision of the expected interference fringes, sodium light was substituted for white light, and the interference fringes were made as clear as possible by adjustment of the mirror *e*. White light being restored, *e* was slowly moved in the direction of its normal till the fringes reappeared in white light, when they were adjusted to a convenient width and position and the apparatus was ready for observation.

While the apparatus was revolving once in about six minutes, the wire of the micrometer was set on the clearest fringe at the moment of passing one of the sixteen equi-distant marks on the iron trough ; the readings were continued for six revolutions.

The following are the means of three such sets of readings made at noon on three days and of three sets of readings made at six hours after noon on three days. The numbers are wave lengths, and are corrected for linear variations.

Marks.....	16	1	2	3	4	5	6	7	8
Noon.......	0.00	– 0.07	+ 0.02	0.00	– 0.01	+ 0.13	+ 0.23	+ 0.16	0.00
Evening...	0.00	+ 0.06	– 0.02	+ 0.07	+ 0.05	+ 0.16	+ 0.13	+ 0.03	0.00

These means are plotted in the following figure :

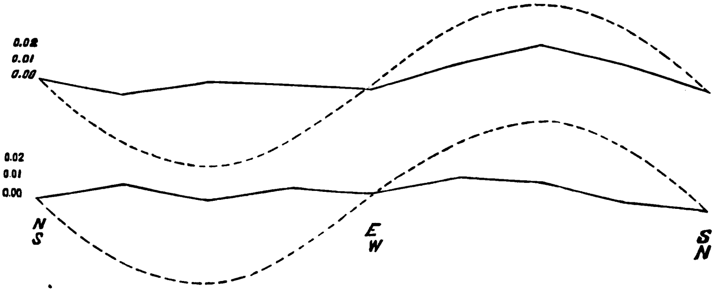


Fig. 4.

The upper curve represents the noon observations and the lower those at evening. The dotted curves represent *one-eighth* of the theoretical displacement. The actual displacement, it seems fair to conclude, was certainly not one-twentieth, and probably not one-fortieth, of the theoretical. The displacement varies as the square of the velocity ; therefore the relative velocity of the earth and the luminiferous ether was certainly not one-fourth and probably not one-sixth of the orbital velocity of the earth.

It is of course possible that the orbital velocity of the earth at the time of the experiment was equal and opposite to that of the solar system through space. Measurements will therefore be repeated at proper intervals.

A NEW MODE OF DETERMINING THE CONSTANTS OF REFRACTION AND ABERRATION.

GEORGE C. COMSTOCK.*

In a series of papers published during the past two years in the *Comtes Rendus*, M. Loewy has suggested the introduction into practical astronomy of a new instrument and has worked out in detail its application to the solution of two important problems of spherical astronomy, the determination of the so-called constants of refraction and aberration.

The fundamental idea upon which the proposed methods of research are based is the superior accuracy of differential as compared with absolute measurements. The determination of the astronomical refraction by the methods hitherto in use furnishes an excellent example of the difficulties attending investigations of the latter class. We have here to determine the absolute declinations of a group of circumpolar stars from observations made at their upper transits over the meridian and the declinations of the same stars from observations made at their lower transits. The value of the refraction depends ultimately upon the difference of the declinations thus determined. Consecutive observations of the same star are thus separated by an interval of at least twelve hours, frequently by a much longer one, and the astronomer engaged in a research of this kind has to fear not only the instrumental sources of

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* Director of Washburn Observatory, Madison, Wis.

error which affect a single observation, such as flexure, division errors, irregularities of micrometer screws, etc., but also the change in instrumental constants and in exterior surroundings during the interval elapsed between the observations. All these sources of error must be examined at an enormous expenditure of time and labor, their influence upon the resulting declinations determined, and when this has been done there remains the certainty that whatever instrumental error rests undetected has gone into the quantity to be determined and thus tends to vitiate it.

Suppose it possible to make the required observations simultaneously and to make them in such a manner that whatever error affects one observation shall affect the others in the same way, then when differences are taken the result is absolutely free from the errors which before were the chief source of uncertainty in the determination. This, then, is the problem which M. Loewy has proposed to himself and has solved : To devise a method by which the refraction can be so determined that the resulting value shall not be appreciably affected by instrumental errors.

The effect of refraction being to apparently displace all stars from their true positions toward the zenith it follows that the apparent angular distance between any two stars is affected by the refraction, and the amount by which it is so affected varies from hour to hour on account of the changed positions of the stars relative to the zenith. Thus, if one of the stars is in the zenith and the other in the horizon the distance is diminished by the whole amount of the refraction at the horizon. If the same two stars are by the diurnal motion brought into a position in which they are at equal distances from the zenith their apparent distance from each other will still be affected by the refraction, but the effect in this position will be much less than before. The exact amount of the effect in every case can be stated mathematically in terms of a certain quantity called the constant of refraction, and it is this quantity which is to be determined. Suppose the apparent distance of the pair of stars to be measured in each of two positions.

Each observation will furnish an equation containing two unknown quantities, the true angular distance of the stars and the amount of the refraction expressed in terms of the constant of refraction, and by the solution of the two equations we may determine the quantities required. Thus, if k denote the constant of refraction, ak and bk the effect of refraction upon the apparent distance of the stars at the times of the first and second observations respectively, D the true distance between the stars as it would appear if there were no refraction, and D_1 and D_2 the measured distances between the stars; we have the equations

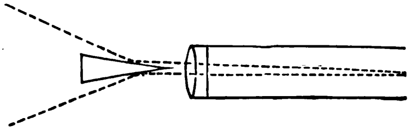
First observation, $D_1 = D + ak$
Second observation, $D_2 = D + bk$

by combining which we find, $k = \frac{D_2 - D_1}{b - a}$

We have here determined k by a method which involves only the numerical cöefficients b and a, whose values can be computed from the known positions of the stars, and the quantity $D_2 - D_1$ which is the difference between the results of two observations made at an interval of four or five hours.

Thus far, however, but little that is new has been developed. If we attempt to carry out the plan thus indicated and to measure the distances D_1 and D_2 with any of the instruments in ordinary use we shall find since the positions of the stars at the two times are quite different, that the observations will be affected with different errors and the difference $D_2 - D_1$ will not be free from their effects but may be influenced by them to an extent equal to the sum of the two separate errors. The merit of M. Loewy's method consists in a new mode of making the observations and in the proof that when so made the results are free from the effects of instrumental errors.

In front of the objective of an equatorially mounted telescope imagine a prism with silvered faces, placed as in the accompanying figure, in which the broken lines represent the paths of rays of light coming from two stars and



reflected from the surfaces of the prism so that images of the stars are formed at the focus of the telescope. It is evident that by choosing a proper value for the angle of the prism, the images of any two stars may be brought simultaneously into the field of view of the telescope and that the distance between the images may be measured with a micrometer. It follows from simple optical principles that the angular distance of the two stars is equal to twice the angle of the prism plus the measured distance between the images, so that the measurement of the large angular distance between the stars is reduced to the measurement of a small distance with a micrometer, an observation which can be made with great precision. To know the absolute distance between the stars, however, we still need to know the angle of the prism, but fortunately the determination of the constant of refraction, as has already been shown, does not require us to know the angle between the stars but only the variation of that angle, $D_2 - D_1$, due to the changed positions of the stars relative to the zenith, and this variation of the angle is equal to the difference of the micrometer measurements at the two times.

Let us recur now to the subject of instrumental errors and see how the method thus indicated is free from them. It is evident that between the two observations of the pair of stars whose distance apart is to be measured, several hours must elapse, and that the telescope must be turned to different parts of the heavens in making the observations, and it appears at first sight that the turning of the telescope into a new position, combined with the changed effect of gravity and possible variations of temperature, must produce some change in the position of the prism relative to the telescope which will alter the position of the images in the field of view and give rise to errors absolutely fatal to the accuracy of the results. But such is not the case. Any motion whatever of the prism relative to the telescope can be resolved into a motion of translation of the whole prism, and a motion of rotation about one or more of a set of three axes taken at right angles to each other. A motion of translation of the prism cannot affect the distance of the images from each other, unless the objective of the telescope is grossly defective and M. Loewy has shown by an elegant mathematical analysis that a small rotation of the prism about any axis, at right angles to the axis of the telescope, can have no appreciable effect upon the distance of the images in the field of view, its only effect being to displace both images in the same direction and by the same amount. The only remaining motion to consider is a rotation about the axis of the telescope itself. This does produce a change in the relative position of the images, but this very rotation may be made use of to determine the position in the field of view of the plane passing through the two stars and the eye of the observer. The projection upon this plane of the distance between the stars is unaffected by any small rotation of the prism whatever, and in the observations it is this projection of the distance between the images, and not the distance itself, which is to be measured, *i. e.*, this is the quantity which above we called D_1 and D_2.

Another source of error still remains to be noted. It is possible that the angle of the prism may be altered by change of temperature, or other causes, in the interval between the two observations and a change of this kind will appear with its full value in $D_2—D_1$; but even this source of error is eliminated by observing two pairs of stars so situated that if this error makes $D_2—D_1$ too great in one case, it will make it too small in the other, and will thus disappear from the mean of the two determinations. A determination of the constant of refraction is thus obtained from two observations made within a few hours of each other by the same observer with the same telescope, each observation consisting in measuring with a micrometer the distance between two stars which appear side by side in the same field of view.

It is not, however, to be supposed that the method of research thus suggested is in itself so perfect as to preclude the necessity for care and skill in the execution of the observations. As here presented it is given in its barest outlines only, and numerous precautions to be observed will suggest themselves to the mind of any one familiar with the art of observing, but their exposition does not fall within the scope of this article.

We have thus far treated M. Loewy's method as applied to the determination of the refraction only, but he has published an elaborate series of papers upon its application to the determination of the constant of aberration to which it is equally well adapted. The effect of the annual aberration is to displace all stars toward the point to which the motion of the earth in its orbit is at any moment directed. The amount of the displacement for any star can be expressed in terms of the angular distance of the star from this point, and of a certain quantity called the constant of aberration. The apparent distance of two stars is therefore affected by the aberration in much the same way, although not according to the same law, as in the case of the refraction, and as the point toward which the earth is moving changes constantly and runs through its complete cycle of 360° in a year, it follows that the effect of the aberration upon the apparent distances of two stars will vary from a maximum to a minimum in the period of six months, and if the distance between two stars properly chosen, is measured at the times when aberration has its maximum and its minimum effect, we may obtain from the variation in the distance the value of the constant of aberration.

Let us examine a little more closely some of the theoretical considerations involved in a determination of this constant. If we put

g = The constant of aberration.
L = The longitude of the sun.
D = The true angular distance between any two stars.
λ and β = The longitude and latitude of the middle point of the arc D.
dD = The effect of aberration upon the length of D.

then we shall have

$$dD = -2g \sin\frac{D}{2} \cos\beta \cos(\lambda - L)$$

a formula which will enable us to fix the conditions most favorable to a determination of g.

It will obviously be advantageous to make the variations of the distance, *dD*, as great as possible hence β must differ but little from 0°, *i. e.*, the stars must be so selected that the arc joining them will be bisected by the ecliptic. It would also seem advantageous to make the distance D nearly equal to 180° but for reasons to be given hereafter it will be best to make D as nearly as possible 120°. Owing to the sun's motion in longitude the angle $\lambda—L$ varies continuously throughout the year, assuming all values from 0° to 360°. If the distance between the stars be measured when $L = \lambda—90°$ and again when $L = \lambda+90°$, that is, after an interval of six months, we shall have from the two observations the two equations :

$$D_1 = D—2g \sin \frac{D}{2} \cos \beta$$

$$D_2 = D+2g \sin \frac{D}{2} \cos \beta, \text{ whence}$$

$$g = \frac{D_2—D_1}{4 \sin \frac{D}{2} \cos \beta}$$

or if we assume $D = 120°$, $\beta = 0°$, then

$$g = 0.2887 \, (D_2—D_1)$$

It thus appears that whatever error of any kind may be committed in measuring $D_2—D_1$ will be largely diminished by multiplying the observed quantity by a cöefficient less than 0.3.

It is obvious from the character of the observations to be made that the method of M. Loewy is as applicable to this case as to the refraction, but with this difference, that in a determination of the constant of aberration the two observations instead of being made within a few hours of each other are separated by an interval of several months, and the danger of a change in the angle of the prism between the observations is thereby greatly increased. To eliminate error from this source M. Loewy points out that the aberration has no effect upon the distance between two stars which have the same latitude and whose longitudes differ by 180°, and he recommends that such a pair of stars be measured in connection with a pair so situated that the aberration may have its maximum effect. By combining the two sets of observation both the aberration and the change in the angle of the prism, if one has occurred, may be determined. A more radical cure for this source of error would seem to be the use of a prism each of whose angles is, as near as may be, 60°. If the distance of the stars be measured with each angle of the prism and the mean of the three results taken this must be independent of any change in the angles of the prism, since from geometrical considerations the mean of the three angles must always be exactly 60°. It is even possible to so arrange the prism that a deformation of its faces, from planes into surfaces slightly curved, will have no systematic effect upon the observations. We thus obtain not only a measure of the change of distance of the stars, but also a measurement of the absolute distance at the instant of observation, a quantity which may be made very useful for other purposes. If such a prism is used, the distance between the stars must, of course, differ but little from 120° and it is for this reason that that distance was suggested above.

The mechanical problem of supporting a prism in front of the objective of a telescope in such a manner as to satisfy all the requirements of this method, appears not yet to have been solved, and an opportunity is here afforded the amateur astronomer of mechanical talent, to make an important contribution to the progress of astronomy by devising a mounting suitable for this purpose. The principal requirements to be satisfied are: The mounting must be light and rigid. The prism must rotate freely about the line of sight of the telescope. Some provision must be made for recording the position of the prism. The prism must be so supported that each of its angles can be used. Adjustments must be provided whereby the prism can be placed symmetrically in front of the objective. All manipulations of the prism should be made from the eye-end of the telescope. Suggestions as to the way in which these conditions may be realized mechanically will be very gladly received by the author of this article.

EDITORIAL NOTES.

The subscription price of the MESSENGER for 1888 will be $2 per year as usual, if paid in advance; if later, $2.50. The price is so low, in view of the kind and the amount of matter published, that prepayment is an important condition.

Foreign subscribers, and such only, are requested to draw money orders for payment of subscription on the post office of St. Paul, Minnesota. Collection on any other money order office causes delay and expense.

J. A. Brashear's Work-Shop.—Rarely in our lives have we spent a more enjoyable, or a more profitable, day than that given to a visit at the shops of Mr. J. A. Brashear, on Observatory Hill, in Allegheny City, Pa. The commodious rooms of the new shop, already well filled with fine machinery for the work of mounting telescopes, the apparatus for grinding and polishing plane and curved surfaces, and the arrangements, now in use, for testing finished surfaces are surprisingly complete, and they are another evidence of Mr. Brashear's purpose to do only the best work possible of any kind entrusted to him. We did not before know of his ingenious plans (now well under way) for casting and annealing optical glass. Is there any good reason why Americans should not lead in this industry also?

But to a novice in the practical part of the study of optical surfaces, the most enjoyable things we saw were the tests he made of plane surfaces on glass and mirrors for reflecting telescopes. The fact that the heat of a person's hand, a few inches from a mirror, should, in one minute, so disturb its figure as to make it useless for an hour, and the observer actually see the distortion of the mirror and the heated air curling about the hand, in the face of the mirror, like smoke in a clear sky, was an astonishing sight. It taught a lesson on the delicacy of telescope mirrors not before known. We shall, at another time, speak of other things seen at this place which deserve also special mention.

Carleton College Observatory.—As elsewhere stated, the new Repsold Meridian Circle is now in place, and Dr. H. C. Wilson is busy determining the errors of the instrument preparatory to regular observing work. As fast as results having public interest are determined they will be published in subsequent numbers of this journal.

The 8¼-inch Clark equatorial was dismounted in August last and removed to the new observatory. The lenses and telescope tube were sent to the Clarks for the purpose of fitting to the telescope a new correcting lens for photography. This work will soon be completed, and it is expected that the telescope will be in the new dome ready for use about the middle of the present month.

Messrs. Warner & Swasey, of Cleveland, O., have already put in place, at the new observatory, one of the fine domes which they have contracted to make. It is 17 feet in diameter, and is constructed wholly of steel. Its weight, including the iron truck, is 5,500 lbs., and its operation so easy that no mechanical appliance is needed to rotate it, for a child six years old moves it easily. The other large dome is well under way, but will not be ready for its place until early spring.

Miss Mary E. Byrd, assistant in mathematics and astronomy at Carleton College Observatory for the last four years, has been elected director of the new observatory at Smith College, Northampton, Mass. Miss Byrd's long experience as instructor in collegiate and preparatory schools, and her special studies in the higher mathematics and practical astronomy with instruments, fit her for the new and wider field of labor to which she has been recently called. Many readers of the MESSENGER know also of the value of her articles and will doubtless be favored by more of them. She is now busy at the new observatory of Smith College, putting instruments in order for regular work.

Flamsteed's Stars "Observed but not Existing."—Bailey's account of Flamsteed gives a list of stars under the above title, with the statement, that "the observations appear to be accurately recorded, but which still can not now be found in the heavens." From this fact it was common, subsequently, for astronomers to publish lists of "missing stars," probably from want of agreement of observations. By the more careful work of Bode, Caroline Herschel, Argelander and Bailey these long lists of missing stars were reduced to 22 which had not been accounted for.

In a late memoir for the American National Academy of Sciences, Dr. C. H. F. Peters gives a full and careful explanation of the observations of these 22 stars, and satisfactorily accounts for nearly every one of them. This, of course, refuted the erroneous belief that these stars had been actually extinguished. Dr. Peters suggests, very properly, that a new reduction of Flamsteed's observations is most desirable.

Elements of Comet Barnard (May 12, 1887) from three normal places.—The first normal place was computed from observations as follows:

Date.	Place.	Local Time.			Observed α'.			Observed δ'.		
		h	*m*	*s*	*h*	*m*	*s*	°	′	″
1. June 12.	Greenwich	12	4	25	16	11	2.35	—8	42	17.1
2. " 13.	Dresden	11	14	45	16	13	6.88	—8	00	49.1
3. " 13.	Leipzig	12	7	17	16	13	12.74	—7	58	42.3
4. " 13.	Harvard	16	41	58	16	13	41.93	—7	48	48.8
5. " 14.	Leipzig	11	54	39	16	15	26.04	—7	14	58.7
6. " 14.	Harvard	14	41	53	16	15	45.32	—7	8	9.2

The two Harvard Local Times are given in Greenwich Mean Time. From elements communicated by H. Oppenheim in A. N. No. 2791, an ephemeris was computed for dates comprising the observations, to Greenwich 12*h* M. T. There results:

The first normal place,—June 13.5; $\alpha = 16h\ 13m\ 16.62s \pm 0.18s$; $\delta = -7^\circ\ 57'\ 10.4'' \pm 3.5''$.

From observations,

7. June 17.	Hamburg	11	25	4	16	22	8.16	—5	6	1.5
8. " 17.	Leipzig	12	1	8	16	22	11.57	—5	5	34.2
9. " 18.	Leipzig	12	23	5	16	24	29.22	—4	23	14.1
10. " 19.	Leipzig	11	43	8	16	26	40.70	—3	44	3.4
11. " 19.	Greenwich	11	19	58	16	26	44.10	—3	43	11.8
12. " 19.	Greenwich	11	26	47	16	26	43.83	—3	42	50.3
13. " 19.	Hamburg	12	12	2	16	26	43.92	—3	42	48.8

The second normal place,—June 18.5; $\alpha = 16^h\ 24^m\ 31.91^s \pm 0.13^s$; $\delta = -4^\circ\ 22'\ 9.5'' \pm 2.1''$.

From observations,

14.	June 22.	Leipzig	11 22 58	16 33 26.36	— 1 49 8.3
15.	" 22.	Leipzig	12 2 0	16 33 29.88	— 1 48 5.2
16.	" 22.	Göttingen	11 56 41	16 33 30.76	— 1 48 0.3

The third normal place,—June 23.5; $\alpha = 16^h\ 35^m\ 51.30^s \pm 0.09^s$; $\delta = -1^\circ\ 10'\ 12.7'' \pm 1.1''$.

These places containing the perihelion passage give the elements of orbit,

$$\begin{array}{ll} T & \text{June } 16.868927 \\ \left.\begin{array}{l} \omega \\ \Omega \\ i \end{array}\right. & \left.\begin{array}{lll} 15^\circ & 20' & 9.1'' \\ 245 & 11 & 18.9 \\ 17 & 32 & 29.4 \end{array}\right\} 1887.0 \\ \log q & 0.144398 \end{array}$$

A comparison with elements previously published indicate these elements as favorable in a definitive determination of the orbit.

FRANK H. BIGELOW.

Racine College, Sept. 20, 1887.

[We are sorry that Professor Bigelow's paper came just a little too late for our last issue.—ED].

Lake Forest University is now planning for a large astronomical observatory of its own. The trustees of that institution had some conference with the Chicago Astronomical Society concerning the removal of the instruments of Dearborn Observatory to Lake Forest. The latest report at hand is, that the Chicago Astronomical Society have decided to relocate Dearborn Observatory at Evanston, Ill., in consideration of the offer of the Northwestern University located at that place. It is also reported that Governor Ross, who is president of the Tribune Company, Chicago, and president of the Board of Trust of Lake Forest University, is the financial backing of its new observatory.

Bright Meteor.—Saturday evening, Sept. 21, at eight minutes past nine o'clock, I saw a fine meteor at Madison, N. J., shooting down from a point well up in eastern sky toward the northeastern horizon. Its color was of a delicate bluish tinge, leaving behind it sparks of a deep red hue. Its apparent size was greater and its light many times brighter than that of Venus. It left no train behind it, at least none visible to the naked eye. Its motion was swift, and the portion of its course which I saw was from near Beta Andromedæ, past Beta Persei (Algol), ending two or three degrees below a latter star, being a distance of about twenty-five degrees. JOHN H. EADIE.

Iowa College Observatory.—This growing college, located at Grinnell, Iowa, is building a new astronomical observatory and pushing it to completion rapidly. The building will be ready probably during the present month, for the 8-inch equatorial from the shops of the Clarks. Mr. Grinnell, the founder of the town, is furnishing the necessary funds.

Double Meteor.—We regret that Mr. Barnard's note of a double meteor came too late for June, and was inadvertently omitted from the last MESSENGER. It was seen May 12 at 8*h* 57*m* while he was observing with a low power on the 6-inch equatorial; it appeared double, moved slowly across the field, filling one degree. The components were about 8 magnitude, and distant one minute of arc and were moving eastward, the line joining them extending north and south. There was no change in brightness or relative position while crossing the field. The telescope pointing was

$$\text{R. A.} = 12h\ 20m$$
$$\text{Decl.} = -19^\circ$$

This is the first double meteor which Mr. Barnard has seen in the telescope.

Eclipse Expedition to Japan.—The letters by Mrs. Todd, member of the eclipse expedition to Japan, to *The Nation* (Sept. 1 and 22) from Shirakawa, Japan, concerning the preparations of the party to observe the total eclipse of Aug. 19, are profitable reading, although scarcely anything worthy of record was accomplished on account of unfavorable weather.

The Forum for September has two important articles on astronomical themes. "Great Telescopes" is the title of one by Professor C. A. Young, in which he gives a historical sketch of the growth of great telescopes, reflecting and refracting, both in America and in foreign lands. He also compares the merits of large and small telescopes, rather to the disadvantage of the larger both on account of their cost and disproportionate advantage in the various lines of astronomical work. The second article is by Professor Alexander Winchell, of Michigan University, on "Ignatius Donnelly's Comet." Professor Winchell evidently is not a convert to the new theory of earth's drift formation, nor does he like the scientific imagination of the writer of "Ragnarok" better than astronomers do those terrible comet collisions in earth's primordial times.

Corrections to Catalogue No. 6 of New Nebula discovered at the Warner Observatory.—In the list of errata to this catalogue in the *Astronomische Nachrichten*, No. 2798-99, Dr. Swift notes that Nos. 2 and 7 are identical with Nos. 277 and 303 in the Leander McCormick Observatory list, previously published in the *Astronomical Journal*, No. 152. It should be added that Nos. 1 and 18 are identical with Nos. 276 and 397 of the last named list.

No. 12 was discovered by Tempel. The place and description are given by him in the *Astronomische Nachrichten*, No. 2212.

No. 22 is probably identical with No. 5348 of Dreyer's Supplement to the General Catalogue. The difference in right ascension is 2.0m and in declination 2.6′, but the descriptions agree in stating that G. C. 965 is in the field.

This opportunity is taken of noting a correction to No. 98 of the Leander McCormick list in the *Astronomical Journal*, No. 146. Prof. Barnard kindly calls attention to the fact that he had published the place of this nebula some time previously in the *Astronomische Nachrichten*, No. 2588.

Leander McCormick Obs'y, Oct. 13, 1887. FRANK MULLER.

Dearborn Observatory.—The latest news at hand concerning the Dearborn Observatory, is that the Chicago Astronomical Society is under orders to move from the present site to the Northwestern University at Evanston, Ill., a few miles only from Chicago.

As far as known the arrangement is that the University shall erect suitable buildings for the instruments of the Society and complete the outfit of minor instruments, so that the observatory at its new site shall be fully equipped for general astronomical work. The Chicago Astronomical Society, we believe, is still to own its astronomical instruments. This is certainly a grand opportunity for the University, and it will doubtless be well improved under the continued directorship of Professor Hough.

Ann Arbor Observatory is to lose Mr. J. M. Schaeberle, widely and favorably known in connection with Detroit Observatory under Director M. W. Harrington.

In July last President Holden offered Mr. Schaeberle the position of Astronomer at the Lick Observatory. Mr. Schaeberle has accepted the position. It appears, however, that the Regents of the State University of California will not take official action in the matter until the observatory is turned over to the State. In the mean time Mr. Schaeberle expects to remain at Ann Arbor. The observatory will feel his loss.

Morrison Observatory.—Publication No. 1 of the Morrison Observatory of Glasgow, Mo., has been received. It contains a description of the observatory and instruments. The equatorial telescope has a clear aperture of 12¼ inches and the Meridian Circle telescope an object-glass of 6 inches aperture. The work of the observatory shown consists of measures of double stars, observations of the Transit of Mercury May 5-6, 1878, occultations, measures of the diameter of Mars, observations of the Red Spot on Jupiter, observations of Saturn, Uranus, comets and the new star in the nebula of Andromeda. The volume is a valuable one for the astronomical library.

Dearborn Observatory.—By the kindness of a friend and member of the Chicago Astronomical Society, a neat bound volume of the annual reports of the Board of Directors of the Chicago Astronomical Society, together with the report of the Director of the Dearborn Observatory for the years 1885 and 1886, was sent us promptly. As heretofore, Professor Hough has used the large equatorial on a few special subjects, viz.: difficult double stars, the planet Jupiter, and miscellaneous observations. Of Jupiter, Professor Hough says:

As in former years, the object of general interest is the great red spot.

The outline, shape and size of this remarkable object has remained without material change from the year 1879, when it was first observed here, until the present time. According to our observations, during the whole of this period it has shown a sharp and well-defined outline, and at no time has it coalesced or been joined to any belt in its proximity, as has been alleged by some observers.

During the year 1885, the middle of the spot was very much paler in color than the margins, causing it to appear as an elliptical ring. The ring-form has continued up to the present time. While the outline of the spot has remained very constant, the color has changed materially from year to year. During the past three years it has at times been very faint, so as barely to be visible.

The persistence of this object for so many years leads me to infer that the formerly-accepted theory, that the phenomena seen on the surface of the planet are atmospheric, is no longer tenable. The statement so often made in text-books, that in the course of a few days or months the whole aspect of the planet may be changed, is obviously erroneous.

The rotation period of Jupiter from the red spot has not materially changed during the past three years. The "mean" period, 1884-5, was 9*h* 55*m* 40.4*s*. Marth's ephemeris for the present year is based on a period of 9*h* 55*m* 40.6*s*. The mean correction to this ephemeris is now (May 1887) only about minus 7 minutes, indicating a slightly less value.

A number of equatorial white spots were systematically observed in 1886; but, owing probably to the low altitude of the planet, they have not been so conspicuous during the present opposition.

The oval white spots on the southern hemisphere of the planet, 9″ south of the equator, have been systematically observed at every opposition during the past eight years. They are generally found in groups of three or more, but are rather difficult to observe. The rotation period deduced from them is nearly the same as from the great red spot.

These spots usually have a slow drift in longitude of about 0.5° daily in the direction of the planet's rotation, when referred to the great red spot; corresponding to a rotation period of 20 seconds less than the latter.

Under the appendix is found articles entitled, Motion of the Lunar Apsides by Professor Colbert, Catalogue of 209 New Double Stars, Nebulæ found at Dearborn Observatory, 1866-8, Description of the Printing Chronograph, and Observations of the Companion to Sirius.

Washburn Observatory, with Professor Asaph Hall as Consulting Director, and Professor George C. Comstock as Associate Director, confessedly is getting the cream of American astronomy for the daily sustenance of its scientific management. Western men can but be delighted at the willing approach of these eastern lights of the science. Congratulations to Washburn are certainly in order.

Mr. E. E. Barnard, well known observer formerly at Nashville, Tenn., has been appointed to the position of Astronomer at Lick Observatory, Mt. Hamilton, California. This is another wise choice on the part of Professor Holden. Mr. Barnard is a young man of proved ability who has gained his skill as an observer and his knowledge of astronomy by the dint of hard work and this appointment is a just recognition of his ability.

Charles H. Rockwell, of Tarrytown, N. Y., has in hand an intensely interesting astronomical problem for thought and observation. Astronomers becoming aware of the accuracy of the Almucantar for latitude determinations have asked Mr. Rockwell to use his fine instrument to observe the moon when in apogee and in perigee to see if any systematic difference could be noticed in the latitude results while in these different positions. Mr. Rockwell proposes to continue these observations for two years, that he may have ample data for the discussion of the problem. This work will be followed with no common interest.

General Bibliography of Astronomy.—In June, 1887, Professors J. C. Houzeau and A. Lancaster, of Brussels, Belgium, published the first volume of their great work entitled "Bibliographie Générale de L'Astronomie." In 1882 the same authors published a collection of memoirs, which together make volume II in a series under the above title. The themes of the memoirs of the volume of 1882 are: The History and Study of Astronomy; Astronomical Biographies, Spherical Astronomy; Theoretical Astronomy; Celestial Mechanics; Physical Astronomy, Practical Astronomy; Monographs on the principal members of the Solar System and Stellar Astronomy. In volume II there is much of interest both for the amateur and for the practical astronomer. Fuller reference to this important work will be given in the future.

Elements and Ephemeris of the Olbers Comet. — Normal places were formed on the dates, August 28, 31, September 6, 15, 18, 23, by means of Ginzel's elements and the perihelion time given in SIDEREAL MESSENGER, No. 57-58 and through the differential cöefficients corrections were obtained to these elements as follows:

$$\begin{array}{l} \Delta \pi = +\ 5'\ 55.8'' \\ \Delta \Omega = -\ 1\ \ 29.7 \\ \Delta i = +\ 1\ \ 19.6 \\ \Delta T = +\ 0.045223 da \\ \Delta q = -\ 0.0021360 \\ \Delta e = -\ 0.0006647 \end{array}$$

giving as the new elements,

$$\begin{array}{rl} T = & 1887, \text{ Oct. } 8.549199 \text{ Gr. M. T.} \\ \left.\begin{array}{rl} \omega = & 65^\circ\ 24'\ 41.6'' \\ \Omega = & 84\ \ 29\ \ 54.5 \\ i = & 44\ \ 34\ \ 53.9 \end{array}\right\} & 1887.0 \\ \log q = & 0.078619 \\ \log e = & 9.968420 \end{array}$$

From these elements the following ephemeris results, Ephemeris for Greenwich midnight (1887.0).

	A. R.			Dec.		lg. r	lg. Δ	L
Oct. 24	13	15	23	+ 20	42.8	0.0872	0.2810	1.50
28		31	54	19	13.6	0.0919	0.2850	....
Nov. 1	13	47	45	17	43.0	0.0975	0.2897	1.38
5	14	2	57	16	12.2	0.1039	0.2949	....
9		17	29	14	42.1	0.1110	0.3006	1.24
13		31	24	13	13.5	0.1188	0.3066	....
17		44	42	11	47.2	0.1270	0.3128	1.08
21	14	57	25	10	23.6	0.1358	0.3192	....
25	15	9	35	9	3.1	0.1449	0.3257	0.94
29		21	15	7	46.0	0.1543	0.3321	....
Dec. 3		32	25	6	32.6	0.1639	0.3385	0.82
7		43	7	5	22.8	0.1737	0.3447	....
11	15	53	23	4	16.8	0.1836	0.3506	0.70
15	16	3	13	3	14.5	0.1936	0.3564	....
19	16	12	40	+ 2	15.9	0.2036	0.3618	0.61

Unit of light, Aug. 27, 1887.

Dudley Observatory, Oct. 19, 1887. H. V. EGBERT.

The Orbit of Σ 1757, by J. E. Gore, as reprint from the "Monthly Notices," is on our table. The elements given in the Monthly Notices of November, 1886, failed to satisfy recent observations, hence the new determination, as follows:

$P = 276.92$ years $\qquad \Omega = 87° 36'$

$T = 1791.98$ A. D. $\qquad \lambda = 185° 23'$

$e = 0.4498 \qquad a = 2.05''$

$\gamma = 40° 56' \qquad \mu = + 1.30°$

New Minor Planet (269) was discovered by Palisa at Vienna Sept. 21, having position, Sept. 21.5201 G. M. T.,

$\alpha = 23h\ 15m\ 55.7s$

$\delta = -7° 15' 25''$ Twelfth magnitude.

New Minor Planet (270) was discovered by Peters. Oct. 8.5534 G. M. T., its position was,

$\alpha = 1h\ 17m\ 3s$

$\delta = + 12° 26'$ Twelfth magnitude.

New Minor Planet (271) (?) was discovered by Knorre, with position, Oct. 16.5218,

$\alpha = 1h\ 12m\ 32.8s$

$\delta = + 12° 1' 32''$ Eleventh magnitude.

The last two are probably the same asteroid.

The Sidereal Messenger,

CONDUCTED BY WM. W. PAYNE.

Director of Carleton College Observatory, Northfield, Minnesota.

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ON THE REPRESENTATION OF COMETS' ORBITS BY MODELS.

WM. HARKNESS.

When a comet is discovered the first question asked about it is "What are its elements," and yet to the vast majority of amateurs these elements are almost unintelligible, and even to adepts they often convey but a vague idea of the true form and position of the orbit. The best way to realize their exact import is by making a model; and by showing how easily that can be done, it is hoped a fruitful source of instruction and amusement will be brought within reach of every one interested in the subject.

The orbits of all heavenly bodies are conic sections whose size, form, and position in space are defined by six quantities called elements, which, for brevity, are usually designated by the following symbols:

T = instant of the body's perihelion passage.

π = longitude of the perihelion; in the case of a comet, measured along the ecliptic from the vernal equinox to the comet's ascending node, and thence along the comet's orbit to its perihelion; in the case of the earth, measured along the ecliptic from the vernal equinox to the perihelion.

☊ = longitude of the ascending node; measured on the ecliptic, from the vernal equinox to the ascending node of the orbit.

i = inclination of the plane of the orbit to the plane of the ecliptic.

e = eccentricity of the orbit, sometimes given in parts of radius, sometimes in seconds of arc, and sometimes as an angle, φ. Parts of radius are most convenient for our purpose, and seconds of arc may be reduced to that unit by dividing them by 206,265″. When φ is given, $e = \sin \varphi$.

q = perihelion distance of the body; expressed in terms of the mean radius of the earth's orbit as unity.

For a parabolic orbit e is unity, and in that case the elements are frequently given by stating T, ω, Ω, i, and log q. Here π has been replaced by

$$\omega = \pi - \Omega \qquad (1)$$

which is counted in ths comet's orbit, backward, from the perihelion to the ascending node; and the perihelion will lie on the northern or southern side of the ecliptic according as ω is less or greater than 180°.

As π and Ω are counted from the vernal equinox, and i is measured from the plane of the ecliptic, these quantities necessarily refer to a particular equinox which is always specified.

It was long customary to measure longitudes in comet's orbits in the direction of the earth's motion, to limit i to the first quadrant, and to specify the direction of the comet's motion, whether direct or retrograde; but most astronomers now prefer to follow Gauss in regarding retrograde motion as a result of the inclination passing into the second quadrant, and in accordance with that view they measure a comet's longitude always in the direction of its own motion, and permit i to take any value between 0° and 180°. The circumstance that i is measured at the ascending node limits its range to the first and second quadrants, for if it were to pass into the third or fourth quadrant the ascending node would be converted into a desending one. For a comet having direct motion the numerical values of the elements are the same in the old system as in Gauss' system, but for a comet having retrograde motion they are different, and in that case, if their values according to the old system are designated by a subscript 0, the equation requisite for passing from the old to the Gaussian system are

$$i = 180° - i_0 \qquad \omega = 360° - \omega_0 = - \omega_0$$
$$\Omega = \Omega_0 \qquad \pi = 2\Omega_0 - \pi_0$$

There is frequently much confusion respecting the angles π and ω, and as no model can be constructed without using the latter, it is important to have a clear understanding of its relations to π and Ω. In the old system of elements π is measured from the vernal equinox, along the ecliptic in the direction, of the earth's motion, to the ascending node of the comet, and thence along the comet's orbit, *still in the direction of the earth's motion*, to the comet's perihelion. In Gauss' system π is measured from the vernal equinox, along the ecliptic in the direction of the earth's motion, to the ascending node of the comet, and thence along the comet's orbit, *in the direction of the comet's motion*, to the comet's perihelion. These definitions may perhaps be elucidated by the following statement: Imagine a perpendicular to the plane of the ecliptic, erected from the sun. Then to an observer situated north of the ecliptic in that perpendicular, the motion of the earth will be counter-clockwise, and longitudes in the earth's orbit will increase in that direction. Now consider a comet's orbit, imagine a perpendicular affixed to it in such a way that when the inclination of the orbit to the plane of the ecliptic is i the inclination of the perpendicular shall be $(i + 90°)$, and suppose an observer so situated in the perpendicular that when $i = 0°$ he shall be north of the ecliptic. Then, according to the old system of elements, for all possible values of i the observer will remain north of the ecliptic, and the motion of the comet will appear to him as counter-clockwise when direct, and clockwise when retrograde; but according to Gauss' system of elements, he will be north of the ecliptic when i is less than 90°, south of it when i is greater than 90°, and to him the apparent direction of the comet's motion will always be counter-clockwise. Whichever system is adopted, from his point of view π will always increase counter-clockwise, and to find the intersection of the plane of the comet's orbit with the plane of the ecliptic, or in other words, the line of nodes, he must set off ω clockwise from the perihelion of the orbit.

In constructing a model of a comet's orbit two distinct processes are involved, namely: 1st, the drawing of the curves destined to represent the orbits of the earth and comet, and the marking upon them of the line of nodes, direction of motion of the bodies, etc., as they would be seen by an observer perched upon the imaginary perpendicular described in the preceding paragraph; and 2d, the putting of these curves together.

In general, the curves must be drawn from their equations, the constants of the latter being expressed in terms of the elements. For a parabola

$$y^2 = 2px \qquad (2)$$

and as the distance from the vertex of the curve to the focus is $\frac{1}{2}p$, for a parabolic orbit $\frac{1}{2}p = q$, and therefore

$$y^2 = 4qx \qquad (3)$$

The easiest way of constructing this curve is by drawing it through a series of points laid down by means of their coordinates computed from equation (3).

For an ellipse

$$y = \frac{B}{A} \sqrt{(A^2 - x^2)} \qquad (4)$$

in which the values of the semi-major and semi-minor axes are respectively

$$A = \frac{q}{1 - e} \qquad (5)$$

$$B = A\sqrt{(1 - e^2)} \qquad (6)$$

The lengths of the axes having been computed and plotted, the foci can be found by construction, and then the curve can be very readily drawn by a pencil moving in the bight of a thread whose two extremities are attached to pins stuck into the foci.

For an ellipse of small eccentricity, a circle of radius $\frac{1}{2}(A + B)$ may often be substituted, and then the distance from the centre of the circle to the focus will be $\frac{1}{2}(A + B)e$.

For the earth's orbit, at any time T, we may take

$$\pi = 100^\circ\ 21'\ 21''.5 + 61''.70\ (T - 1850.0) \qquad (7)$$

$$e = 0.0167711 - 0.000000424\ (T - 1850.0) \qquad (8)$$

whence, for the epoch 1887.0, $e=0.01676$, and by putting A equal to unity, formula (6) gives

$$B = 1\sqrt{(1 - 0.0002809)} = 0.999860$$

The difference between A and B is far too small to be sensible upon any scale likely to be adopted for a model, and the earth's orbit may therefore be represented by a circle, in which the sun's distance from the center is e multiplied by the radius. For most models the diameter of this circle may conveniently be from two to six inches, and its circumference may be graduated to show the position of the earth at intervals of 5, 10, or 15 days. The circle having been cut out of stiff cardboard, one of its surfaces should be marked "North Side," and the other "South Side." When the former side is viewed the direction of the earth's motion is counter-clockwise; when the latter, clockwise. For effecting the graduation to show the position of the earth, Table I will be convenient.

The position of the comet in its orbit is defined by its true anomaly, which is the angle at the sun included between the comet and its perihelion. If for any comet we put

m = mean daily motion;
M = mean anomaly at the time t;
E = eccentric anomaly at the time t;
ν = true anomaly at the time t;

then, in a parabolic orbit

$$m = \frac{188171''}{q^{\frac{3}{2}}} \qquad (9)$$

and the mean and true anomolies are connected by the equation

$$m(t - T) = M = 75 \tan \tfrac{1}{2}\nu + 25 \tan^3 \tfrac{1}{2}\nu \qquad (10)$$

where $(t - T)$ must be expressed in mean solar days. The labor of solving this equation is avoided by the use of Barker's table, some form of which is given in almost every treatise of the computation of orbits.

TABLE I.—LONGITUDE OF THE EARTH IN ITS ORBIT AT GREENWICH MEAN NOON.

NOTE.—This table is for the year 1886, being the second after bissextile; but for the purpose of model making, it will suffice for any year in the latter half of the nineteenth century.

Date.	Longitude.		Date.	Longitude.		Date.	Longitude.	
	°	′		°	′		°	′
January 1......	101	04	May 1......	221	01	September 1......	338	56
" 11......	111	16	" 11......	230	41	" 11......	348	39
" 21	121	26	" 21......	240	19	" 21......	358	25
February 1......	132	37	June 1......	250	52	October 1......	8	14
" 11......	142	45	" 11......	260	26	" 11.	18	06
" 21......	152	50	" 21......	269	58	" 21......	28	02
March 1......	160	52	July 1......	279	31	November 1......	39	01
" 11......	170	52	" 11......	289	03	" 11......	49	04
" 21......	180	49	" 21......	298	35	" 21......	59	09
April 1......	191	41	August 1......	309	06	December 1......	69	17
" 11......	201	31	" 11......	318	42	" 11......	79	26
" 21......	211	17	" 21......	328	19	" 21......	89	37

For an elliptic orbit whose semi-major axis is A

$$m = \frac{3548''.2}{A^{\frac{3}{2}}} \qquad (11)$$

$$\text{Periodic time in days} = \frac{360}{m} = 365.26\ A^{\frac{3}{2}} \qquad (12)$$

and the mean eccentric, and true anomalies are connected by the equations

$$m\,(t - T) = M = E - e \sin E \qquad (13)$$

$$\tan \tfrac{1}{2}\, \nu = \sqrt{\left(\frac{1+e}{1-e}\right)} \tan \tfrac{1}{2}\, E \qquad (14)$$

Number (13) is the celebrated Kepler's equation. Owing to its transcendental form it cannot be solved directly, and notwithstanding all the labor and ingenuity which have been expended in attempts to discover an easier mode of solution, that by trial and error is still in general use.

Having now given all the definitions and formulæ necessary in constructing figures of the orbits of heavenly bodies, we have next to explain how these figures must be united in order that their mutual relations may be the same as those of the orbits they represent. To make that part of our subject perfectly clear we will describe in detail the construction of several models belonging respectively to different classes of comet orbits.

EXAMPLE I.—Let it be required to construct a model of the orbit of the comet, 1840, III, from the following parabolic elements:

$T = 1840$, April $2^d\ 12^h$, Greenwich Mean Time.
$\omega = 138°\ 16'$ $\qquad i = 79°\ 51'$
$\Omega = 186\ \ 04$ $\qquad q = 0.7420$
Motion direct.

In these elements the mean distance between the earth and the sun is taken as unity. In our model let us make that distance two inches. Then, to represent the earth's orbit we draw a circle of 2 inches radius upon a piece of cardboard about 0.029 of an inch thick. The distance of the sun's place from the center of this circle is equal to the radius of the circle multiplied by the value of *e* found from equation (8), namely,

$$2 \times 0.017 = 0.034 \text{ of an inch,}$$

and for the longitude of the perihelion, equation (7) gives 100° 11′. Accordingly, we draw the diameter *AB*, Figure 1, through the circle to represent the line of apsides; mark the point *S* upon it, at a distance of 0.034 of an inch from the center *C*, to represent the sun's place; and draw through *S* the line of equinoxes, *DE*, making an angle of 100° 11′ with the line of apsides. The north side of the orbit being uppermost, the angle of 100° 11′ must be laid off clockwise (in the direction of the motion of the hands of a clock) from *A*, the perihelion end of the line of apsides; and the number 100° 11′ being affixed to the perihelion end of the line of apsides, the numbers 0° and 180° must be affixed to the two ends of the line of equinoxes in such a way that the numbers may increase counter-clockwise around the circle. These numbers represent longitudes in the earth's orbit, and the point marked 0° is called the vernal equinox because the *sun* passes through it in the spring. As the longitude of the earth is always 180° less than that of the sun, the earth does not reach the vernal equinox until about September 21. The circumference of the circle may now be graduated to show the place of the earth at every fifth day of the year, or less frequently if desired; but in making this graduation, the center from which the angles are laid off must be the place of the sun, *S*, and not the center of the circle. Lastly, we set off the longitude of the comets ☊ = 186° 04′ counter-clockwise from the vernal equinox, and through the sun and the point thus found we draw *FG*, the line of intersection of the plane of the comet's orbit with that of the earth.

The next step is to lay down the curve destined to represent the comet's orbit, and for that purpose the coördinates of a sufficient number of points in the curve are required. To compute them we need an expression for the relation between y and x, and recalling that the q of our model is the q of the elements multiplied by the mean radius of the earth's orbit, namely, two inches, we have from equation (3)

$$y^2 = 4(2 \times 0.742)\,x = 5.936x \qquad (15)$$

Then, having written down the values of x given in Table II, we find from (15) that when $x = 0.5$ the value of y^2 is 2.968 inches, and accordingly, that number is written opposite 0.5 in the table. As the interval between successive values of x is 0.5, for $x = 1.0$, $y^2 = 2.968 + 2.968 = 5.936$; for $x = 1.5$, $y^2 = 5.936 + 2.968 = 8.904$ inches, and so on; all the values of y^2 being formed by successive additions of 2.968, and the last one being verified by means of formula (15), to guard against possible errors of addition. The completion of the computation is then most conveniently effected by means of a table of square roots, from which the required values of y can be taken with the values of y^2 as arguments. Next, upon a piece of cardboard of the same thickness as that employed for the earth's orbit a straight line, *HI*, Figure 2, is drawn to represent the axis of the comet's orbit, and upon that line perpendiculars are erected at intervals of half an inch, as shown in the figure. Beginning at the left, these perpendiculars are marked successively 0.0, 0.5, 1.0, 1.5, etc., to correspond with the values of x in Table II; the ys belonging to these xs are set off upon the respective perpendiculars, both upward and downward, from the line *HI*; and the desired parabola is drawn with a free hand through the points thus laid down. As explained in connection with equation (15), the perihelion distance is 1.484 inches, and the place of the sun S', is found by setting off that distance from the vertex of the curve upon the line HI. Lastly, with the sun as a center the angle $\omega = 138° 16'$, is set off clockwise from the perihelion, H, and through the sun and the point so found the line KL is drawn to define the intersection of the plane of the comet's orbit with the plane of the earth's orbit. That surface of the cardboard upon which we have been working must be marked "North Side" because it represents the northern side of the comet's orbit; and as the comet's motion is direct, it will move counter-clockwise along the parabola. A comet with retrograde motion would move clockwise. The instant of the comet's perihelion passage is known, because it is one of the elements of the orbit; but if we desire to make the position of the comet for any other date, we must compute its true anomaly for date and lay it off from the perihelion in the proper direction. A general explanation of the method of performing the computation has been already given, and it is needless to enter into further details here because there are several different forms of Barker's table and whatever form the reader may possess will be accompanied by all necessary explanation for its use.

TABLE II.

x	y^2	y	x	y^2	y
Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
0.5	2.968	± 1.723	3.5	20.776	± 4.558
1 0	5.936	2.437	4.0	23.744	4.873
1 5	8.904	2.984	4.5	26.712	5.169
2 0	11.872	3.446	5.0	29.680	5 448
2.5	14.840	3.852	5.5	32.648	5.714
3 0	17.808	4.220	6.0	35.616	5.968

Having thus laid down the orbits of the earth and comet, they must next be cut from the cardboard with the utmost care, the lines of the orbits being followed very exactly; and then, to permit the union of the two discs so obtained, a slit of the same width as the thickness of the cardboard must be cut in each of them in the line of intersection of the orbits; that in the earth's orbit extending from the place of the sun, S, to F, the longitude of the ☊ $= 186° 04'$, and that in the comet's orbit extending from the place of the Sun, S' to L at longitude $(\omega + 180°) = 318° 16'$. Before cutting these slits the lines SM and $S'N$ should be drawn (the latter upon both sides of the cardboard) from the sun, at right angles to the intersection of the planes of the orbits, to serve as guides in putting the pieces together; and in cutting, care should be taken to hold the knife so that it makes the same angle with the cardboard as will be made by the other orbit when the two are united.

The slits having been cut, each orbit must be slipped into the other in such a way that the point F falls upon P, the point L near G, and the point S of the line SM upon S' of the line $S'N$, and then the orbits must be fixed together in that position by gumming a narrow strip of paper over the joint throughout its whole length. The strip of paper employed may be from one-quarter to three-eighths of an inch wide, and in order to make a good job, half its width should be gummed and applied to one of the orbits, and then the other half should be gummed and applied to the other orbit. The most convenient way of effecting this will be 1st, to fold the paper lengthwise down its middle, thus reducing it to half its width; 2nd, to gum one side of the folded strip, and apply that side to one of the orbits before the two are united, taking special care that the fold of the paper lies accurately along the edge of the slit, and extends beyond it in the same straight line sufficiently far to cover the slit in the other orbit when the two are united; 3d, to gum the other side of the folded strip, taking care before doing so to insert a piece of waste paper within the fold in order to prevent any accidental smearing of the model; and lastly, to put the two orbits accurately together as described above, and then to unite them by smoothing the gummed paper down upon the one to which it has not hitherto been applied.

In order to complete the model it yet remains to fix the angle between the planes of the two orbits by inserting a tri-angular piece of cardboard, $S''M'N'$, Figure 3, of the same thickness as that used for the orbits themselves. The sides $S''M'$ and $S''N'$ of this triangle must have respectively the same lengths as the lines SM, Figure 1, and $S'N$, Figure 2, and the angle included between these sides must be equal to i, the inclination of the plane of the comet's orbit to the plane of the ecliptic, which in the present case is 79° 51′. In the way described above, narrow strips of paper folded down the middle must be gummed to the edges $S''M'$ and $S'N''$ of the triangle, care being taken that the fold of the paper lies accurately along the edges in question; and then after gumming the free sides of the strips, the triangle must be inserted between the orbits with its angle S'' at the place of the sun, and its sides $S''M'$ and $S''N'$ coinciding with the lines SM and $S'N$ on the orbits, and it must be fixed in that position by means of the gummed paper.

EXAMPLE II.—To illustrate the difference between the two systems of stating a comet's elements, let it be required to construct a model of the orbit of the comet 1874, VI, from the following parabolic elements, which are given both according to the old system and according to Gauss' system:

Old System.

$T =$ 1874, Oct. 18*d* 23*h*, Paris Mean Time.
$\omega = 343° \ 43'$ $i = 80° \ 47'$
$\Omega = 281 \ \ 58$ $q = 0.5083$
Motion retrograde.

Gauss' System.

$T =$ 1874, Oct. 18*d* 23*h*, Paris Mean Time.
$\omega = \ 16° \ 17'$ $i = 99° \ 13'$
$\Omega = 281 \ \ 58$ $q = 0.5083$

The orbit of the earth having been drawn, and the place of the sun together with the line of equinoxes having been marked; the line of nodes, or in other words, the line of intersection of the planes of the earth's and comet's orbits, must be found by setting off the longitude of the comet's

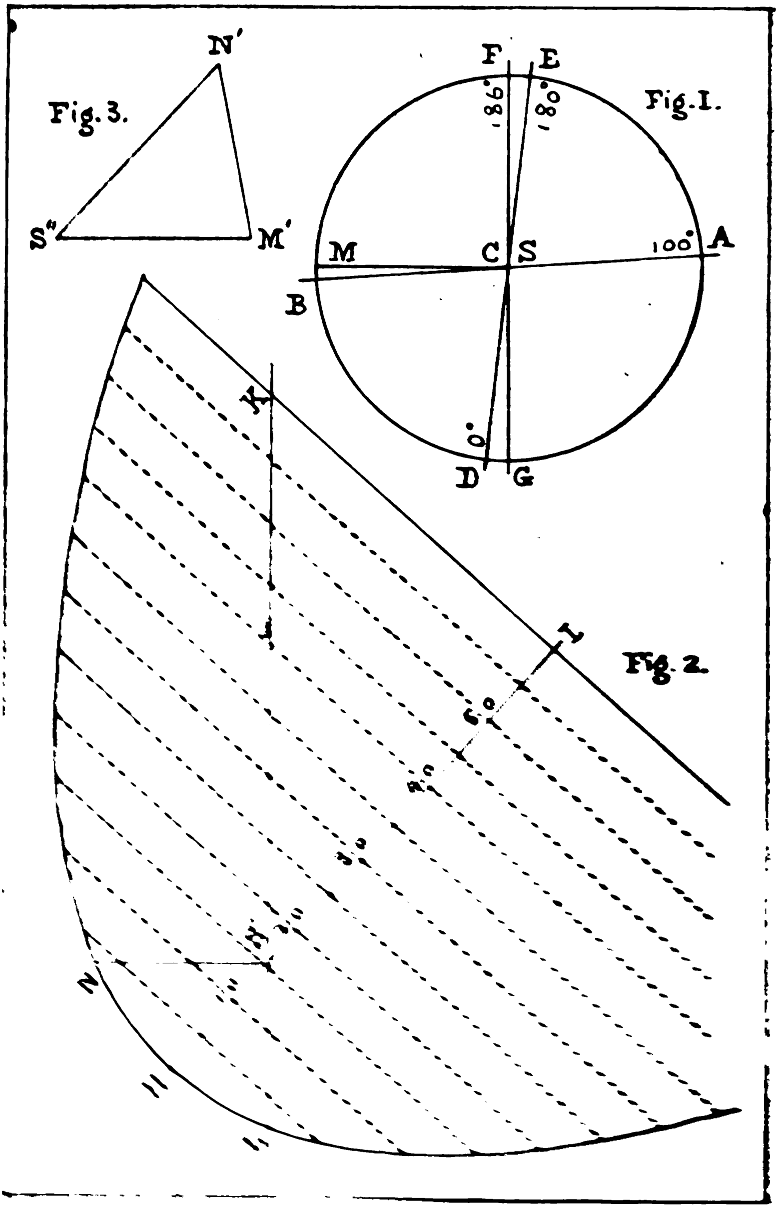

Fig. 3.

Fig. 1.

Fig. 2.

☊ = 281° 58′ counter-clockwise from the vernal equinox; the direction of the earth's motion must be marked as counter-clockwise; and that side of the cardboard upon which the drawing is made must be marked "North Side"—all these operations being performed precisely as in Example I. Next the orbit of the comet together with its axis must be drawn, the place of the sun must be marked, and the line of nodes must be found by setting off the angle ω clockwise from the perihelion. After that the mode of procedure will depend upon the system of elements employed. If the old system is used the direction of the comet's motion must be marked as clockwise, because it is retrograde, and the side of the paper upon which the drawing is made must be marked "North Side;" but on the contrary, if Gauss' system of elements is used the direction of the comet's motion must be marked as counter-clockwise, and the side of the paper upon which the drawing is made must be marked "South-Side." In either case the two disks representing the orbits of the earth and comet must be cut out and put together in the way described in Example I, care being taken that the "North Sides" of the disks face in the same direction, and of course the finished model will be the same whichever system of elements may have been employed.

A most instructive experiment can be made as follows: Let the old system of elements be used to lay down the orbit of the comet, its line of nodes, and the direction of the comet's motion, upon one side of a piece of thin paper; and then let the paper be turned over, and let the lines drawn upon its face be traced upon its back by looking through it. As the front of the paper represents the north side of the orbit, the back necessarily represents the south side, and the lines traced upon the latter will be found to be precisely those required by the Gaussian system of elements. Thus it will be impressed upon the memory that in the case of a comet having retrograde motion the old system of elements represents the orbit as seen from the north side of the ecliptic, while the Gaussian system represents it as seen from the south side of the ecliptic.

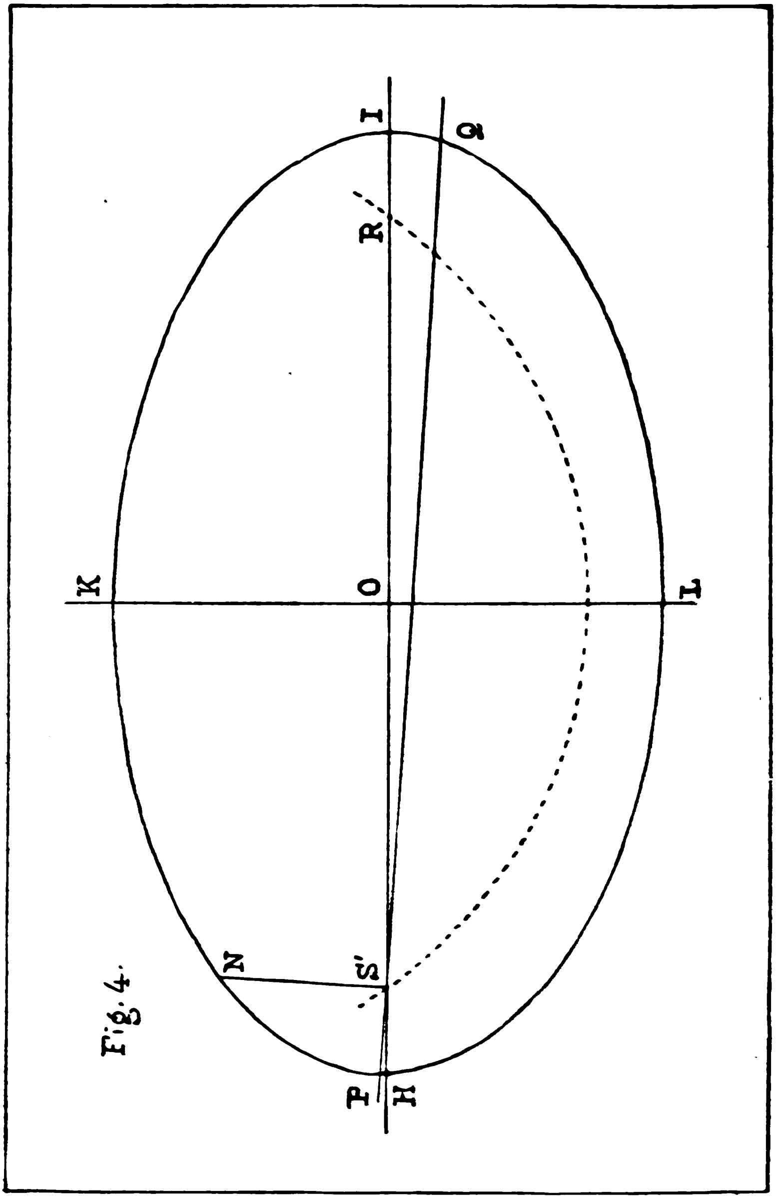

Fig. 4.

EXAMPLE III.—Let it be required to construct a model of the orbit of Encke's comet from the following elliptic elements:

$T = 1871$, Dec. 28d 18h, Greenwich Mean Time.
$\pi = 158^\circ\ 12'$ $q = 0.3329$
$\Omega = 334\ \ 34$ $e = 0.84936$
$i = \ \ 13\ \ 08$ Motion direct

With these elements we form the quantities

$e^2 = 0.7214$ $1 - e^2 = 0.2786$
$1 - e = 0.1506$ $\surd(1 - e^2) = 0.5278$

and then, by means of formulæ (5) and (6), we compute the semi-major and semi-minor axes, thus:

$$A = \frac{0.3329}{0.1506} = 2.210$$

$$B = 2.210 \times 0.5278 = 1.167$$

These results are in terms of the mean distance between the earth and sun as unity, and they show that if that distance is made two inches, the length of the major axis of the comet's orbit will be 8.840 inches, which is a convenient size for our model, and will therefore be adopted.

The construction of the circular cardboard disk of two inches radius to represent the earth's orbit will be precisely as in Figure 1, except that in accordance with formula (7) the angle between the line of apsides and the line of equinoxes will be 100° 43′ instead of 100° 11′, and the longitude of the ☊ will be 334° 34^s instead of 186° 04′.

To construct the disk destined to represent the comet's orbit, select a piece of cardboard of suitable thickness and upon it draw the straight lines HI and KL, Figure 4, intersecting each other at right angles. From their intersection O, set off OK and OL, each equal to the semi-minor axis B, whose length is $2 \times 1.167 = 2.33$ inches. With K as a center and the semi-major axis $A = 2 \times 2.210 = 4.42$ inches as a radius, describe an arc cutting HI in R and S', and these points will be the foci of the eclipse. Stick a pin in each of them. Tie a small loop in a piece of thread; pass the loop over one of the pins, and take a turn around the other with the free end of the thread. The thread will then be fastened to the two pins, and will lie upon the paper stretched between them. Press the point of a pencil sidewise against the thread, and by gently slackening the free end of the latter, let the turn slip until when the pencil is held perpendicularly its point just reaches the end of the minor axis at *K* or *L*, the thread being at the same time quite tight. Then, a finger having been placed upon the free end of the thread to prevent it from slackening further, the ellipse required to represent the comet's orbit may be described by moving the pencil sidewise while its point is kept firmly pressed against the thread.

As either of the two foci may be taken to represent the place of the sun, let *S′* be selected, and with it as a center set off the angle $\omega = \pi - \Omega = 283° 38'$ clockwise from the perihelion, *H*, and through the sun and the point so found draw the line *PQ* to define the intersection of the plane of the comet's orbit with the plane of the earth's orbit. That surface of the cardboard upon which we have been working must be marked "North Side;" and as the motion of the comet is direct, it will move counter-clockwise along the ellipse. The orbit thus laid down must next be cut from the cardboord; a slit must be made in it from *S′* to *P*; a triangle with an angle corresponding to $i = 13° 08'$ must be designed in the same way as Figure 3; and finally, all the parts of the model must be united precisely as described in connection with Example I.

Example IV.—In dealing with comet orbits it is frequently desirable to make a rough model as rapidly as possible, merely to obtain a clear idea of what the elements mean. For that purpose the scale of the model should be such as to give the earth's orbit a radius of about an inch; the paper or cardboard employed may be 0.010 of an inch thick; and there will be needed a set of four or five pattern parabolas, made of cardboard, vulcanite, or sheet brass, in which the distances from the focus to the vertex of the curve are respectively ¼, ½, 1, 1½, and perhaps 2, inches. By their aid the disks representing the orbits of the earth and comet can be made and put together (without the strips of gummed paper) in about ten minutes. As an example of such work let it be required to construct a rough model of comet *f* 1887 (Brooks) from the following parabolic elements:

$T = 1887$, Oct. 6.480, Greenwich Mean Time.
$\omega = 63^\circ\ 18'$ $i = 44^\circ\ 10'$
$\Omega = 84\ \ 33$ $q = 1.2223$

In our previous examples we have adopted a definitive diameter for the earth's orbit and have then constructed the parabola required to represent the comet's orbit; but in order to save time, we must now adopt one of our pattern parabolas to represent the comet's orbit and determine the radius of the corresponding circle required to represent the earth's orbit. As the perihelion distance of the comet is 1.222, and we wish the radius of the earth's orbit to be about an inch, it is evident that we may use either of the two pattern parabolas whose focal distances are respectively 1 and 1½ inches. To find the corresponding radius of the earth's orbit, the focal distance of the parabola must be divided by q, and thus we obtain from the first parabola $1 \div 1.222 = 0.818$ of an inch, and from the second $1.5 \div 1.222 = 1.227$ inches. The latter value seems preferable, and we adopt it. Accordingly, the 1½ inch pattern parabola is laid upon a suitable piece of paper or cardboard, and the comet's orbit is drawn by passing a pencil around the pattern; care being taken at the same time to mark the focus and one other point in the axis of the curve. The pattern is then removed; the axis of the parabola is drawn through the points marked; the angle $\omega = 63^\circ\ 18'$ is set off clockwise from the perihelion end of the axis, and the line defining the intersection of the plane of the comet's orbit with the plane of the earth's orbit is laid down. Next a circle of 1.23 inches radius is described to represent the earth's orbit, and a diameter is traced through it to indicate the line of equinoxes; the slight displacement of the sun from the center of the circle being quite negligible. From the vernal equinox the longitude of the ☊ =84° 33′ is set off counter-clockwise. and the line of intersection of the plane of the earth's orbit with the plane of the comet's orbit is plotted. Finally, the two orbits thus laid down are cut from the cardboard; the necessary slits are made along the line of their mutual intersection; a triangle with $i = 44°$ 10′ is prepared; and the three pieces are put together, either with or without the application of strips of gummed paper. It may be well to add that the above elements show the comet f 1887 to be Olber's comet.

Summary.—The foregoing rules may be summarized in the following form which applies both to the old, and to Gauss' system of elements:

In laying down the earth's orbit—

A. The longitude of the ☊ must be set off counter-clockwise from the vernal equinox.

B. The direction of the earth's motion must be marked as counter-clockwise.

C. That side of the paper or cardboard upon which the drawing is made must be marked "North Side."

In laying down the comet's orbit—

A. The angle ω must be set off clockwise from the comet's perihelion.

B. The direction of the comet's motion must be marked as counter-clockwise; except when the motion is retrograde with i less than 90°, and then it must be marked as clockwise.

C. That side of the paper or cardboard upon which the drawing is made must be marked "North Side;" except when i is greater than 90°, and then it must be marked "South Side."

Caution.—After the parts of a model have been correctly drawn there are several ways in which they may be wrongly put together, and sometimes one or other of the slits in the disks representing the orbits require to be cut in longitudes 180° different from those stated in our examples. The following conditions are imperitive, and must be satisfied by every model, namely:

1. Those surfaces of the two orbits which are marked "North Side" must face in the same direction.

2. The comet must pass *at its ascending node* from the southern to the northern side of the plane of the earth's orbit.

3. The comet's perihelion must lie on the northern or southern side of the plane of the earth's orbit according as $\omega = \pi - \Omega$ is less or greater than 180°.

To prevent mistakes, these conditions should be borne in mind when cutting the slits, and until a careful inspection has shown that they are satisfied, the strips of gummed paper should not be applied.

When i is very small it is difficult to cut the slits in the orbits and insert the cardboard triangle in the way described above, and even if that were successfully done, the larger orbit would conceal much of the smaller. In such cases it is preferable to cut an aperture in the larger orbit equal to half the diameter of the smaller, and to fasten the two together in the proper relative positions by glueing or screwing them to a wooden wedge placed between them; thus dispensing with the slits, and leaving the entire sweep of both orbits visible.

The elements from which a model is made should always be written upon the disk representing the comet's orbit after all the parts of the model are completed, and before they are permanently fastened together.

There yet remain two matters which require notice, namely, the distance to which the disk representing the comet's orbit should extend from the sun; and the method of finding the intersection of the plane of the comet's orbit with the orbits of planets other than the earth.

No comet has ever been seen at a distance from the sun so great as five times the mean radius of the earth's orbit, and they are seldom visible at more than two and a half or three times that radius. The latter limit is therefore sufficient for a model in all ordinary cases. When a periodic comet describes a very elongated ellipse it is both unnecessary and inconvenient to draw the entire curve by the method explained in connection with Example III, and in such cases it will usually be preferable to lay down the part required by tracing it through points whose co-ordinates have been computed by means of formula (4).

In order to show how near a comet may approach to planets other than the earth, it is frequently desirable that the points at which the orbits of these planets intersect the plane of the comet's orbit should be marked upon the disk representing the latter. The readiest way of doing this is to determine for each planet the line in which the plane of its orbit intersects the plane of the comet's orbit, and then to mark the point of intersection of the planet's orbit with that line by setting off the proper distance from the sun. If we put η for the angle upon the plane of the comet's orbit between the intersections of that plane with the planes of the earth's orbit and the planet's orbit; i and i' respectively for the inclinations of the planes of the comet's and planet's orbits to the plane of the ecliptic, both reckoned from 0° to 180° in accordance with Gauss' system; and ☊ and ☊′ respectively for the longitudes of the ascending nodes of the comet and planet; then

$$\cot \eta = \frac{\sin i \cot i' - \cos i \cos (\text{☊} - \text{☊}')}{(\sin \text{☊} - \text{☊}')} \cdot \qquad (16)$$

or, by introducing the auxiliary θ,

$$\tan \theta = \tan i' \cos (\text{☊} - \text{☊}') \qquad (17)$$

$$\cot \eta = \frac{\sin (i - \theta) \cot (\text{☊} - \text{☊}')}{\sin \theta} \qquad (18)$$

The results derived from equations (16) and (18) are freed from ambiguity by the circumstance that η can never be in the second or third quadrant, and must therefore be taken in the first quadrant when $\cot \eta$ is positive, and in the fourth when $\cot \eta$ is negative. As η necessarily lies on the same side of the ecliptic as the planet to which it belongs, and is positive when north and negative when south of that plane, a further check is afforded by remembering that every planet is north of the ecliptic from the longitude of its own ☊ to ☊ + 180°, and south of the ecliptic from ☊ — 180° to ☊.

The planetary elements which may be required for use in equations (16) or (18) are given in Table III.

TABLE III.—PLANETARY ELEMENTS FOR 1850.

Planet.	*i*			☊		
	°	′	″	°	′	″
Mercury	7	00	07.71	46	33	08.6
Venus	3	23	35.01	75	19	53.1
Mars	1	51	02.28	48	23	53.0
Jupiter	1	18	41.37	98	56	16.9

Planets having a less perihelion distance than the comet intersect the plane of the latter's orbit in two opposite points, one of which will in general be north, and the other south of the plane of the ecliptic.

By means of a carefully constructed model having the orbits of the earth and comet graduated to show the positions of these bodies at each instant of time, all problems relating either to the apparent position of the comet in the heavens, or to its position relatively to other bodies of the solar system, can be roughly solved with great rapidity. Hastily constructed models of the kind described in Example IV are of course less useful, but much valuable information may be obtained even from them. Perhaps it is scarcely necessary to add that the position of the comet in the heavens for any given date is taken from a model by laying a straight wire from the earth's place to the comet's place, and then reading off the latitude and longitude corresponding to the direction of the wire. The latitude is the angle of elevation of the wire above or below the plane of the ecliptic; and the longitude is the angle at the sun between the vernal equinox and a line drawn through the sun parallel to the projection of the wire upon the plane of the ecliptic.

Washington, D. C., October 31, 1887.

The phenomena of the planets will find place hereafter regularly in each issue of the MESSENGER.

THE PROGRESS OF ASTRONOMY DURING THE NINETEENTH CENTURY.

Looking back to the year 1800, we are astonished at the change. The comparatively simple science of the heavenly bodies known to our predecessors, almost perfect so far as it went, incurious of what lay beyond its grasp, has developed into a body of manifold powers and parts, each with its separate mode and means of growth, full of strong vitality, but animated by a restless and unsatisfied spirit, haunted by the sense of problems unsolved, and tormented by conscious impotence to sound the immensities it perpetually confronts.

Knowledge might then be said to be bounded by the solar system; but even the solar system presented itself under an aspect strangely different from that it now wears. It consisted of the sun, seven planets, and twice as many satellites, all clinging harmoniously in obedience to an universal law, by the compensating action of which the indefinite stability of their mutual relations was secured. The occasional incursion of a comet, or the periodical presence of a single such wanderer chained by planetary or solar attraction to prevent escape to outer space availed nothing to impair the symmetry of the majestic spectacle.

Now, not alone have the ascertained limits of the system been widened by a thousand millions of miles, with the addition of one more giant planet and six satellites to the ancient classes of its members, but a complexity has been given to its constitution baffling description or thought. Two hundred and seventy circulating planetary bodies bridge the gap between Jupiter and Mars, the complete investigation of the movements of any one of which would overtask the energies of a lifetime. Meteorites, strangers apparently to the fundamental ordering of the solar household, swarm nevertheless, by millions in every cranny of its space, returning at regular intervals like the comets so singularly associated with them, or sweeping across it with hyperbolic velocities, brought perhaps from some distant star. And each of these cosmical grains of dust has a theory far more complex than that of

Jupiter; it bears within it the secret of its origin, and fulfils a function in the universe. The sun itself is no longer a semi-fabulous, fire-girt globe, but the vast scene of the play of forces as yet imperfectly known to us, offering a boundless field for the most arduous and inspiring researches. Amongst the planets, the widest variety in physical habitudes is seen to prevail, and each is recognized as a world apart, inviting inquiries which, to be effective, must necessarily be special and detailed. Even our own moon threatens to break loose from the trammels of calculations, and commits "errors" which sap the very foundations of the lunar theory, and suggest the formidable necessity for its revision. Nay, the steadfast earth has forfeited the implicit confidence placed in it as a time-keeper, and questions relating to the stability of the earth's axis, and the constancy of the earth's rate of rotation, are amongst those which it behooves the future to answer. Everywhere there is multiformity and change, stimulating a curiosity which the rapid development of methods of research offers the possibility of at least partially gratifying.

Outside the solar system, the problems which demand a practical solution are all but infinite in number and extent. And these have all arisen and crowded upon our thoughts within less than a hundred years. For sidereal science became a recognized branch of astronomy only through Herschel's discovery of the revolutions of double stars in 1802. Yet already it may be, and has been called, "the astronomy of the future." So rapidly has the development of a keen and universal interest attended and stimulated the growth of power to investigate this sublime subject. What has been done is little—is scarcely a beginning; yet it is much in comparison with the total blank of a century past. And our knowledge will, we are easily persuaded, appear in turn the merest ignorance to those who come after us. Yet it is not to be despised, since by it we reach up groping fingers to touch the hem of the garment of the Most High.

Our next volume will have new dress and new cover.

A NEW CATALOGUE OF STARS.

The principal work of astronomers, past and present, has been to determine as accurately as possible, with the means which each age has afforded, the position of the stars in the heavens. From comparisons made of the observations at different epochs is determined what is called the proper motion of the fixed stars, and also the movement of our solar system through space. The first observations record roughly the stars most prominent to the eye; and from this beginning of a thousand stars or more, with the invention of astronomical instruments, the number has been extended into the hundreds of thousands. And it is on the positions of these catalogue stars that the determination of all bodies in the solar system depends. Hence the nicest accuracy has been sought for these stars of reference, and redeterminations are constantly being made for various investigations. The verified positions are published for the use of computers of orbits in various astronomical journals and publications.

Dr. C. H. F. Peters, director of the Litchfield observatory at Hamilton College, is one of the most persistent and painstaking observers of the present century, and his observations are universally recognized as of the greatest accuracy. His observations, which are in course of preparation for publication and which will fill many volumes, all depend on stars of reference, a large number of which he has determined himself.

Evidently one of the most valuable aids an observer can have is an accurate catalogue of the reference, or comparison stars, with their position at a given period. Such an aid, a boon to astronomers, has been in process of preparation for several years at the Litchfield observatory. Under Dr. Peters' direction his able assistant, Professor Charles A. Borst, has reduced these stars to an epoch and constructed them into a catalogue, which will be an inestimable benefit to observers and an enduring monument to his own industry and attainments. In the prosecution of his work Professor Borst has gathered the stars from the various astronomical publications for the last half century, and made the computations for the reduction to the epoch of the catalogue from the years in which they were observed. This part of his work was greatly facilitated by access to Dr. Peters' library, which is said to be the most comprehensive and complete in astronomical literature.

Thus has been constructed a catalogue of 30,000 stars, which will be of inestimable utility to astronomers, who observe, and a most valuable acquisition to science. Little can be guessed by one not familiar with such work, of the honest work and painstaking care which this catalogue will represent. Thirty thousand stars, each computed to the present epoch, and each computation verified with the greatest care and accuracy! The computations fill several thousand folios, and have occupied Professor Borst's time during the past six years. The work is now virtually ready for the printer, and it is expected that its publication will be achieved the coming winter.—*Utica Herald, Nov. 5, 1887.*

PHOTOMETRIC OBSERVATIONS OF ASTEROIDS.

HENRY M. PARKHURST.

The variations in the brightness of the stars, are irregular, and affect so large a proportion of the stars, that uniformity of standard can only be secured by employing the means of large numbers. In the Harvard Photometry, Polaris is assumed to be invariable, and is made the standard. Analysis of more than 2000 observations, which would betray variability by causing the other stars to appear to vary simultaneously, proves that there is no change as yet appreciable. It is safe to continue to rely upon it, only because it is so continuously employed that any change which may occur, can be at any time ascertained with precision.

It is hardly to be expected that a star, in the process of combustion, should remain of unchanged brightness. It may be either the flickering light of burning gas, or the gradually waning light of an incandescent body. In the planets, shining by reflected light only, we have a standard as invariable as the illumination of our sun, but subject to certain periodic changes, the extent of which may be ascertained. Of the planets, the asteroids are especially useful as standards of comparison for telescopic stars.

My own photometric observations of the asteroids commenced in April last, when there were four grouped together in the neighborhood of Regulus, and easily identified upon the ecliptic charts, and which could be frequently all observed in one series. I have since had no opportunity to compare asteroids with each other, but have compared several with comparison stars, with the standard of the Harvard Photometry.

In my first reduction I assumed invariable brightness, when reduced to the distance unity. The correction for illuminated surface, amounting to a few hundredths of a magnitude, I soon found to be inappreciable in comparison with much greater changes depending upon phase. This change I have found to vary, with different asteroids, ranging from M .13 for each degree of change in the angle P, the angle at the asteroid, to about M .01. My results, needing correction after the comparison stars have been better determined, have been, M .130, M .116, M .012, M .013, M .007.

The only case in which I have found marked evidence of change probably from rotation, is in the observation of Harmonia. On six evenings the results agreed within a few hundredths of a magnitude; on the other three evenings, the discrepancy was .70, .33, .74, brighter. I am confident that there was no error from misidentification. It is possible that light clouds partially obscured my comparison stars, but at present this seems incredible.

There is one series, which although unfinished, I wish to refer to specially, because it is supplementary to similar observations by Professors Pickering and Harrington; my observations of Vesta. Professor Pickering in 1880 obtained for the brightness, reduced to the distances unity, 3.95. In the following year he made it 3.91; the mean being 3.93. Six years later, comparing with entirely different stars, in a different part of the heavens, and having no dependence whatever upon his previous observations, I brought out in my preliminary reduction, precisely the same value, 3.93. I subsequently found Professor Harrington's observations in 1883, also entirely independent, which gave the value, 4.10 or M .17 greater. These observations had not been corrected for phase.

The agreement between these results proves that our sun has not within the last seven years, varied appreciably in brightness; it proves that whatever irregularities there may be from rotation or other unknown causes, the light of the planet Vesta, in a series of observations, is a reliable standard of comparison; and I think it also proves that it is not yet quite time to "call a halt."

The observations were continued up to Sept. 19. Applying the correction for phase deduced from the observations, M .02 for each degree of the angle at the asteroid; to each of the four series of observations, the brightness, reduced to distances unity and corrected for phase, was as follows:

Pickering,	1880	M 3.59
"	1881	M 3.54
Harrington,	1883	M 3.65
Parkhurst,	1887	M 3.45
Final mean,		3.52

In the final mean, I have given weights to the several series according to the number of comparison stars employed, and independently compared with Polaris. The weight to be allowed for the meridian photometer observaions of Pickering was somewhat arbitrary, but its amount does not perceptibly affect the result.

The mean error of my observations did not appreciably exceed the mean errors in observing the comparison stars.

EDITORIAL NOTES.

This number completes Volume VI of the MESSENGER, and hence nearly all subscriptions for the new volume of 1888 are due. If payment be made in advance, or before Jan. 20, the usual price of two dollars will be charged; if made later $2.50. Subscribers are respectfully asked to notify us promptly if continuance of the MESSENGER is desired.

The Representation of Comet Orbits by Models is the title of an instructive and very carefully written article by Professor William Harkness of the U. S. Naval Observatory. Though unusual space is given to this theme, the details of making models of comet orbits are so fully and plainly stated, that even students of elementary astronomy could do the work well if so inclined. We think it not too much to say that every teacher of astronomy, in any grade of school, would find these home-made models very useful aids in conveying to their students definite ideas of the motions of comets and planets.

Recent Showers of Meteors (*continued from Sidereal Messenger, Sept. and Oct., 1887, p. 288*).—During the three months from August 1 to October 27, 1887, I spent 127½ hours in observation and saw 1144 meteors. On the whole the weather, though not exceptionally good, has proved tolerably favorable for this branch of work. From the many radiant points determined I have selected the following as representing the best streams recently seen here:

Epoch of Shower.	Night of Max.	Radiant Point R. A. Dec.	No. of Meteors.	Appearance,
August 6-25	Aug. 25	334 + 58	10	Rather swift.
August 7-22	Aug. 21	73 + 41	10	Very swift; streaks.
August 7-23	Aug. 23	327 + 48	8	Slow; faint.
August 10-24	Aug. 24	135 + 78	7	Swift.
August 10-24	Aug. 24	349 + 49	11	Rather slow.
August 14-23	Aug. 23	264 + 62	7	Slow: trained; brilliant.
August 20-24	Aug 21	54 + 71½	10	Very swift; streaks.
August 20-24	Aug. 21	347 + 15	7	Slowish.
August 20 25	Aug. 23	43 + 39	9	Swift; streaks.
September 7-24	Sept. 19	5 + 10	15	Slow; short.
September 7-24	Sept. 17	64 + 22	8	Very swift; streaks.
September 7-24	Sept. 8	258 + 60	10	Slowish.
September 12-22	Sept. 18	28 + 72	7	Slowish; short.
September 13-24	Sept 24	7 + 44	7	Slowish.
September 17-19	Sept. 18.	41 + 38	7	Swift; streaks.
September 17-22	Sept. 22	335 + 58	12	Slow; bright.
October 11-14	Oct. 13	192 + 83	10	Slow; bright; trained.
October 11-15	Oct. 14	40 + 29	12	Swift; short.
October 11-21	Oct. 21	29 + 72	13	Swift; small; short.
October 11-24	Oct. 14	40 + 20	45	Rather swift.
October 12-21	Oct 13	312 + 77	8	Swift.
October 14-15	Oct. 15	25 + 44	10	Slow; small; short.
October 14-20	Oct. 14	117 + 47½	8	Very swift; streaks.
October 14-21	Oct. 11	54 + 71	12	Swift.
October 14 21	Oct. 14	105 + 22	12	Very swift; streaks.
October 14-24	Oct. 14	135 + 68	11	Swift.
October 17-21	Oct. 20	47 + 44	8	Swift.
October 20-21	Oct, 21	125 + 43	7	Very swift; streaks.

In addition to these I re-observed those well known showers the Perseids and Orionids. I have already given a table of my results for the Perseids up to the end of July (see MESSEEGER, Sept. and Oct. 1887, p. 288) and now subjoin a list of the radiants for this stream determined here in August.

Date.	Radiant.		No. of Meteors.	Appearance.
1887.	R.A. °	Decl. °		
August 6	42	+ 55	5	The Perseids are swift,bright meteors, leaving phosphorescent streaks.
August 7	43	+ 56	5	
August 8	43	+ 56	6	
August 10	42½	+ 57½	22	
August 11	45	+ 57½	16	
August 14	53	+ 57	8	

The remarkable displacemet to the east as shown by the radiant this year fully confirms my previous observations reported in the SIDEREAL MESSENGER, April, 1886, p. 107.

I watched the shower of Orionids very closely in October, with an endeavor to trace any change also affecting its radiant but the displacement, any, if is too slight for determination. My observations were as follows:

Date.	Radiant.		No. of Meteors.	Appearance.
1887.	R.A. °	Decl. °		
October 11-14	91	+ 17	5	The Orionids are very similar in their visible aspect, to the Perseids.
October 15	91	+ 16	17	
October 17	90	+ 15	3	
October 19	90½	+ 15½	10	
October 20	90	+ 14½	22	
October 21	92	+ 14	23	
October 24	91	+ 16	9	

Allowing for the unavoidable errors of observation the radiating center of the shower seemed slackening at 91° + 15°.

In addition to this annually recurring stream, we have this year been favored with a fine display of meteors from a radiant at 40° + 20° near ε Arietis. I recorded 45 of its meteors in October and have previously referred to this system as a very prominent one at this epoch. (See *Monthly Notices*, Vol. XLIV, p. 24-26.) W. F. DENNING.

Bristol, England, Oct. 27, 1887.

The Supposed Satellite of Venus.—A very interesting and valuable paper entitled "Etude sur le Satellite Enigmatique de Venus," comes to us from the Royal Academy of Belgium, through the courtesy of the author, Mr. Paul Stroobant. It is well known that in a certain number of instances, all previous to the present century, observers have seen in the same field of the telescope with Venus a small object which might have been recognized as a satellite, but for the fact that it was seen only at intervals, sometimes of fifteen or even fifty years, and that it has not been seen at all during the present century, although we possess telescopes of far greater power and better quality.

The author says that his attention was called to the study of this question by an article published by Professor Houzeau in 1884, in which it was suggested that the observations might be explained by supposing the existence of a small planet revolving in an orbit a little exterior to that of Venus. Mr. Stroobant rejects this hypothesis, as well as the seven other hypotheses which have been offered at various times and leads us to the surprising conclusion that in nearly every instance the observers saw nothing but fixed stars which can be identified upon the star charts which we now possess. In several cases it seems astonishing that the observers did not satisfy themselves that they had not observed a fixed star. For instance, in the observation of Roedkioer and Boserup at Copenhagen, August 4, 1761, the observers took χ_4 Orionis (5th mag.) for the satellite, while using χ_3 Orionis (5½ mag.) as a comparison star. Again, the stars observed for the satellite by Roedkioer, July 18, August 7, and August 11, 1761, were *m* Tauri, (5th mag.), 71 Orionis (6th mag.), and ν Geminorum (4½ mag.), and that by Horrebow, Jan. 3, 1768, θ Libræ (4½ mag.). The motion which the last observer ascribed to the satellite during his observation is exactly equal and in the contrary direction to that of Venus.

The author gives first in tabular form a summary of all the observations of the supposed satellite, 33 in number, from 1645 to 1768, then the data for computing the apparent places of Venus, upon the celestial sphere at the moments of observation. He then reviews the various hypotheses which have been advanced in regard to these observations and finally compares the computed places of the satellite with the star charts. In a few instances the identification of the object observed with an existing star is not quite satisfactory, and it may be possible that some one of the brighter minor planets has thus been observed. In one case, Roedkioer, March 4, 1764, it seems very probable that the planet Uranus was observed, the distance betweeen the two planets being then only 16′.

In an appendix the author gives the original text in regard to each of the observations. This appendix alone makes the paper very valuable to astronomers to whom the original texts are difficult of access. There are added three plates giving star charts upon which the positions of Venus and the observed satellite are noted. H. C. W.

N. C. Dunér, Astronomer at Lund Observatory, Sweden, writes Mr. J. A. Brashear recently, a very complimentary letter concerning the Rowland gratings which were supplied to him by Mr. Brashear. Mr. Dunér finds the optical quality of the gratings "most satisfactory," and he says "the brightness of the spectra of the 3d, 4th and 5th orders are really surprising, and in fact greater than that of the 2d, a circumstance which is very favorable for my researches in which great dispersion is desirable." Other foreign observers seeing Mr. Dunér's grating have asked for copies of some as large as the 6-inch concave grating.

Comet Meteor Radiants.—Below will be found the radiants and distances of the new comets of last year. The present list is a continuation of my last, given in SIDEREAL MESSENGER, 1886, p. 152. The first column contains the current number; the second gives the designation of the comet in the order of perihelion passage of the year; the third, the discoverer; the fourth, the day of the earth's passage through plane of comet's orbit; the fifth designates the nearest node; the sixth, the distance of this node from the earth's orbit in the order $R - r$, the earth's distance from the sun being taken as unity; the seventh and last column gives the radiant for the day in question.

Current Number.	Designation of Comet.	Discoverer.	Earth's Passage through Node.	Designation of Node.	Earth's Distance from node.	Radiant.	
						R.A.	Decl.
1	1886 I	Fabry	April 27	Descending	+ 0.11	322°.6	+ 36°.9
2	1886 II	Barnard	May 30	Descending	+ 0.36	350.8	+ 49.0
3	1886 III	Brooks	July 11	Ascending	+ 0.05	20.5	— 40.9
4	1886 IV	Brooks	May 13.	Descending	— 0.36	158.2	+ 54.2
5	1886 V	Brooks	Oct. 6.	Descending	+ 0.72	88.0	+ 78.5
6	1886 VII	Finlay	Nov. 16	Ascending	— 0.14	263.3	— 36.4
7	1886 VIII	Barnard	June 9	Ascending	— 0.56	3.7	— 61.6
8	1886 IX	Barnard	Feb. 6	Ascending	— 0.25	173.5	— 32.7

Harvard College Obs'y, Oct. 22, 1887. O. C. WENDELL.

The Star of Bethlehem.—In your issue of Sept.-Oct. you have an article on the above subject. May I ask your permission to call attention to one point which seems to me to be of vital importance, and which appears to be entirely overlooked? Apart from any question as to the actual occurrence, a scientific consideration of the various hypotheses put forward to account for it would, in the first place, divide these hypotheses into two groups—terrestrial and celestial. If this be done, and we then proceed to consider the Bible account,—that the star "went before till it came and stood over the place where the young child was,"—we shall see at once that none of the celestial phenomena put forward to account for the story are admissible. The "planets" are "wanderers," to be sure; but the perturbations which led to the discovery of Neptune would be nothing to what would have occurred if Jupiter and Saturn or Jupiter and Mars had so far wandered from their ordinary courses as to lead the Magi to imagine that they were following an actually moving body preceding them in their travels. And when the "star" had arrived at its destination, and "stood over" the place—what then? Suppose Jupiter to be be in the zenith, what would the editor give for his chance of finding some buccaneers' buried treasure, because he knew the planet "was over the place" where the treasure was? Of the four heads under which the subject was considered in the article, three I think, must be ruled out at once,—those relating to planets, comets and stars,—and the first left in possession of the field. For, not only does this give an ample explanation of the matter, but all the other suppositions are tainted with the objection that they are extensive interferences with the natural order which do not evade the imputation of miraculousness, which is the only objection to the first head; or that they are merely accidental coincidences in which case the Divine guidance would appear to be entirely lost sight of. I think you will agree with me, that those who are not satisfied with the explanation under the first head, are not likely to be satisfied by any possible hypothesis under any of the others. JAS. T. ELLIS.

Toronto, Ont., Oct. 22, 1887.

Observing Nebulæ at Leander McCormick Observatory.—A method of obtaining the places of new nebulæ, lately introduced at the Leander McCormick Observatory, is given with the hope that its use will insure better places than have been previously, in many cases, given. The method has been only lately rendered convenient for use on southern nebulæ by the publication of the Southern Durchmusterung. The eye-piece on a filar micrometer is used in sweeping; the wires being in the meridian the differences of right ascension is observed, by means of a chronometer, or the beats of the armature of the driving clock, between the nebula and two stars of such magnitudes as to insure their being found in the Durchmusterung. The micrometer is then turned through 90°, the fixed wire placed on the nebula, the telescope is then moved until the star is in the field, and the movable wire placed on it. The reading of the micrometer together with the reading at the coincidence of the wires gives the difference of declination. The position of the telescope is obtained roughly, this with the observed difference of right ascension between the stars serves to identify them almost beyond a doubt. In this way in a few minutes the place of the nebula can be obtained with very little chance of error, and as accurately as is warranted by the star places in the Durchmusterung. If an electric light, the brightness of which is regulated by a switch close at hand, is used to illuminate the wires almost any nebula visible in the telescope can be observed in the above way; and the accurate place of almost any visible nebula with a condensation can be obtained in the usual way with a filar micrometer with such illumination.

A more convenient way of obtaining the places of nebulæ approximately would be to have an eye-piece like that of a square bar micrometer with three of the bars removed and the fourth graduated to, say, ten, or twenty, seconds of arc. This one bar would not interfere much with sweeping, places could be obtained rapidly, and probably as accurately, in the case of diffuse nebulæ, as in any other way.

FRANK MULLER.

The Scientific American, one of the best papers of its kind, is doing astronomy a real service in a popular way. The issue of Sept. 24 contained an instructive article, fully illustrated, on the theme "How Telescopes are Made," showing cuts of the Alvan Clark shops at Cambridgeport, Mass., inside and outside, and describing their methods of work intelligently and somewhat in detail. The group of three sitting at a table, representing the senior Alvan Clark (recently deceased), Alvan G. Clark and George Clark, is true to life.

In the number of October 15 we have another illustrated article entitled "Harvard Observatory and the Henry Draper Memorial." Those who have not visited these places will get definite knowledge of them by these articles.

Charles B. Hill, assistant at the Chabot Observatory, Oakland, California, finds opportunity to make observations such as the MESSENGER has given fequently, outside of the time devoted to public uses of the observatory for which it was built. The instruments are, an 8½-inch Clark equatorial, a 4-inch Fauth Transit and the necessary astronomical time pieces.

Mr. D. Appel, of Cleveland, O., recently returned from Europe. While in Germany he visited the principal cities of the Rhine, thence he made a tour through Italy and Switzerland and back again to Germany, reaching Berlin Aug. 16. He was at Bramberg, Aug. 18, with a view of observing the corona during the total solar eclipse. At the same place were some of the Vienna astronomers, Dr. von Konkoly and von Gothard. Clouded skies defeated observations. Mr. Appel visited several of the leading observatories while abroad.

Report of the Leander McCormick Observatory by the Director, Professor Ormond Stone, for year ending June, 1887, though brief has many points of interest. The discussion of the nebula of Orion, respecting change of brightness, is quite in accord with views recently published by Professor Holden, although the interesting details of structure and probable variation of relative brightness brought out by Professor Stone make the evidence fresh and quite conclusive. The examination and sketching of southern nebulæ have been continued during the last year, resulting in 351 observations of miscellaneous nebulæ, and in the discovery of 270 nebulæ supposed to be new. Considerable work has also been done in the measurement of southern double-stars.

Catalogue of Red Stars.—A working catalogue of Red Stars, prepared by G. F. Chambers, of England, is a compilation of observations extending over a period of seventeen years from 1870. The telescope used in the earlier part of this work by Mr. Chambers, was a 4-inch refractor by Cooke, but all observations made after 1884 were by a 6-inch refractor by Grubb, having a low power eye-piece and a field of 1¼°. Star catalogues covering dates from 1804 to 1876 have contributed red stars to his catalogue, making a total number of 719; This is a very useful compilation in the furtherance of needful study of stellar physics.

The Asteroids.—An essay on the asteroids recently prepared by Professor Daniel Kirkwood, Bloomington, Indiana, and published by Messrs. J. B. Lippincott of Philadelphia, will be issued during the present month. Professor Kirkwood is authority concerning the astronomy of the minor planets.

Warner Observatory.—Dr. Lewis Swift has recently published a series of articles on popular astronomy in the Rochester "Union and Advertiser," entitled "Simple Lessons in Astronomy." They are profitable reading for any one, especially the student, the teacher and the amateur. In a recent letter he says that Dr. Dryer's new general catalogue of nebulæ will soon be ready for distribution. It contains 7840 nebulæ besides those of the postscript.

BOOK NOTICES.

Chauvenet's Treatise of Elementary Geometry, Revised and Abridged; by W. E. Byerly, Professor of Mathematics in Harvard University. Philadelphia: Messrs. J. B. Lippincott Company, Publishers, 1887, pp. 322, 8 vo., cloth, price $1.28.

Most students of mathematics known of the excellent text-books written by Chauvenet. As prepared by himself, we do not know of any better in the English language, generally speaking. Apparently this author could write on themes of pure mathematics, in close, exact and logical phrase, as a ready talker converses with a friend, or a genius speaks his living thoughts. His writings show instructive insight and naturalness. The only trouble that the preparatory schools have found with these books is that they were too comprehensive for common use. To meet this difficulty Professor Byerly has revised and abridged the Geometry, a copy of which has just reached us.

There are three things in this book that are mostly new and excellent. They are

(1) The introduction of *numerous* graded exercises in the body of the book, with suggestions to aid the student in solving the more difficult ones. (2) Stress laid on independent thinking on the part of the student as distinguished from mere memorizing; and (3) A syllabus of most important propositions and corollaries. The first point only needs to be stated; the second point is a wise step, urging teacher and student to adopt, at the earliest time possible, the true modern method in Geometry which is "demonstration at sight" as Latin and Greek are now taught to be read at sight in the best schools. The right use of this book must bring most desirable results.

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Note.—The numbers following the titles of articles are the pages of this volume. Those in parenthesis are the numbers of the Minor Planets.