ber of the solar system, exact data will determine the exact position in orbit at a given time; but here we have neither exact data, nor can we employ trigonometrical methods in the solution. We simply find that the observed proper motions are probably somewhat better reconciled under the hypothesis of an assumed position of the apex of the solar motion. The method of investigation employed by Safford, who has of late years given much attention to this subject, consists in assuming a system of co-ordinates for the pole of the solar motion, from which is determined the direction each star would have if its own proper motions were zero. Comparing this direction with the observed direction as indicated by the observed proper motion, equations of condition are formed from which a correction is found to the assumed position of the apex, by the methods of least squares.

It must always be kept in mind, that the quantities with which we must deal in this investigation are exceedingly minute, and that the accidental errors of observation are at any time liable to lead to illusory results. The weak link in the chain of Mädler's reasoning is to be found here. I think we can assume 0.2″ as the limit of precision in the absolute determination of the co-ordinates of any star, however great the number of observations upon which it depends. Beyond this limit it is impossible to go, in the present date of instrumental astronomy.

It is safe to say, that there is not a single star in the heavens whose co-ordinates are known with certainty within this limit. Do not misunderstand me. Doubtless there are many stars in which the error will at some future time be found to fall within this limit. The law of probabilities requires this, if the maximum limit falls within 1″. But who is prepared to select a particular star, and say that the absolute position of this star in space cannot be more than 0.2″ in error?

e. At present an arbitrary hypothesis is necessary in the discussion of the problem. Airy assumed that the relative distances of the stars are proportional to their magnitudes; and he found slightly different results according to different modes of treatment. Safford assumed that the distances are, at least approximately, in inverse proportion to the magnitude of the proper motions. The general result of his investigations, up to this point, is, that there is some hope of using the solar motion as a base, to advance our knowledge of stellar distances. Later investigations have been made by De Ball, but the details have not yet come to hand. It is understood, however, that his results coincide in a general way with those previously obtained.

It is clear from this brief review, that we have here a field of investigation worthy of the highest powers of the astronomer. The first step has been taken in the survey of the heavens carried on under the auspices of the gesellschaft. It remains for the astronomers of the present generation to solve the difficulties which now environ the problem, and prepare the way for a more perfect scheme of observation in the next century.

PAPERS READ BEFORE SECTION A.

The total solar eclipse of May 6, 1883.

BY EDWARD S. HOLDEN, OF WASHBURN OBSERVATORY, MADISON, WIS.

THIS eclipse had the longest totality of any which has been observed.

An expedition was sent by the National academy of sciences and the U. S. coast-survey jointly, under direction of a committee from the former. Expenses were met by an appropriation of $5,000 by congress and by the National academy of sciences from a fund left by Professor Watson. The navy department also placed the U. S. steamer Hartford at the disposal of the academy, to transport the expedition from Peru to Caroline island, where the eclipse was to be observed, and thence to Honolulu.

The efforts of Mr. Rockwell to provide money by private subscription for this undertaking, though directly unsuccessful, prepared the way by drawing public attention.

Professor Young was the chairman of the committee of the National academy of sciences: it was at one time hoped that he would take charge of the observing-party, but this proved impracticable. The reports of different members of the party are to be submitted to the National academy of sciences in November. Mr. Holden has, however, permission of the academy to present an account of the observation before the American association. It is understood that the present is not by any means a final report. This especially applies to the observations of Dr. Hastings, from which that gentleman concludes that the solar corona is chiefly a phenomenon due to the diffraction of the solar light at the moon's limb. The computations to demonstrate this are not yet at hand, but are to be completed in a few weeks.

The American party consisted of Edward S. Holden, director of Washburn observatory, Madison, Wis.; Charles S. Hastings, professor of physics in the Johns Hopkins university, Baltimore, Md.; Charles H. Rockwell, Tarrytown, N.Y.; E. D. Preston, aid U. S. coast and geodetic survey, Washington, D.C.; Winslow Upton, U.S. signal-office, Washington, D.C.; and Ensign S. J. Brown, U.S.N., U. S. naval observatory, Washington, D.C.

The original six members of the party were joined, on April 20, by four volunteer observers, all officers of the U. S. ship Hartford: these were Lieut. E. F. Qualtrough, U.S.N.; Passed assistant-surgeon W. S. Dixon, U.S.N.; Midshipman W. S. Fletcher, U.S.N.; and Midshipman J. G. Doyle, U.S.N.

On March 11 the party was strengthened by the joining (at Colon) of the two English gentlemen who were sent out by the Royal society of London to make photographic observations of the eclipse, under instructions from J. Norman Lockyer, Esq., F.R.S., and Capt. W. de W. Abney, R.E., of the science and art department of the South Kensington museum. These were H. A. Lawrance, London, Eng., and C. Ray Woods, London, Eng.

During the stay of the party on Caroline island

(April 21 to May 9), ten petty officers and men of the Hartford remained, and rendered very intelligent assistance.

In all, the party on the island consisted of twenty-two persons.

After giving details of the proceedings of the expedition, its arrival, and the preparations for the eclipse, Mr. Holden states, as to the event itself, that the following atmospheric conditions prevailed: The sky proved clear at first contact, cloudy at intervals till near totality, clear during totality except a slight haze in its first minutes, cloudy a few minutes after third contact, and finally clear at fourth contact.

The meteorological observations (for which due credit is given to the members of the party that had them in charge) are noteworthy. In two weeks, April 25 to May 9, twenty showers were recorded; but the rainfall in each was very small, the total in the two weeks being about 8 inches. Half of this fell during the only considerable disturbance of the weather, which took place May 4, when it rained from midnight to 9.50 A.M.

The barometer was notably uniform. Its diurnal movements were plainly marked; the maxima being at 9 A.M. and P.M., the minima at 3 A.M. and P.M. The indications of the thermometer were very constant. The daily range was 9.3°, the highest reading 89.3°, the lowest 72.4°, the daily maximum at noon, the minimum at 6 A.M. The relative humidity ranged from 70 per cent at midday to 84 in early morning, and at no time fell below 61. The island lies in the region of the south-east trades, but the wind (which was very steady) blew constantly between north and east. The average velocity of the wind was 6.05 miles; the largest during twenty-four hours was 212 miles, the least 59 miles; the highest velocity, registered in a squall, was 16 miles per hour.

The botanical and zoölogical observations are not yet ready for publication. During the voyage a series of observations was made by Mr. Upton on southern variable stars. Dr. Hastings and Mr. Holden, while on the island, discovered twenty-three new double stars, a list of which has appeared in SCIENCE.

In preparing for the eclipse, Mr. Holden assigned to each observer a single duty, not requiring him to move from one instrument to another. The excellent photographic apparatus, prepared under the direction of Prof. W. Harkness of the U.S. naval observatory, was not used: the entire field of photography was left to the English party accompanying our own, and to the French party under M. Janssen, who were very successful in photographing the corona.

The combination of polariscope and telescope was used, but not with successful results, the apparatus proving unsuitable. Dr. W. S. Dixon, who attended to a telescopic examination of the details of the inner corona, will report on the same separately, giving a drawing of the corona. With the spectroscope, the chief point of observation was as to the relative lengths of the line 1474 east and west of the sun. At second contact, this line was 12′ longitude east and 3′ west. The length of 1474 east diminished, while 1474 west increased. At mid-totality these were equal. Before the third contact, the appearances were reversed: 1474 west was longer and brighter than 1474 east.

At the beginning of totality, the lines C, D_3, F, and (near G) were seen brilliant but very short. At mid-eclipse the spectrum was deliberately examined. On a continuous spectrum, two lines only were seen: 1474 *bright*, and the D line *dark*. C, E, b, F, were certainly wanting. Near the end of totality, C, D_3, and F appeared again, very short. Five seconds after second contact, four curved lines were seen, — C, D_3, 1474, F. A light cloud passed over the sun; and on its disappearance the spectrum showed a small line, of about one-third the height of the others, between 1474 and F. One hundred seconds after second contact, three coronal rings took the place of the lines: they were red, yellowish-green, and green, and are supposed to be C, D_3, and 1474. Two hundred seconds after second contact, the red ring was decidedly the brightest, and it continued to increase in brightness during sixty seconds. Two hundred and ninety seconds after second contact, the four curved lines, C, D_3, 1474, F, appeared. The reversal of the bright lines at third contact was observed. The change was instantaneous, or nearly so. The reversal of the Fraunhofer lines was not seen. The only bright line seen for the first 190 seconds was 1474. A dark line was seen, which was probably D.

Mr. Rockwell, using a Rutherford grating and a narrow slip tangential to the limb, reported that 1474 K was not seen until a minute and a half had passed. It was followed 4′ or 5′ west of the limb, twice; and it was seen only on the western side of the moon. Two green lines were also seen, each brighter and broader than 1474, but much shorter.

Due credit is given by Mr. Holden to each of the observers of the party. His own observations were confined to a search for the planet Vulcan, reported to exist by Professors Watson and Swift. Mr. Holden's search continued during the whole of totality (five minutes and twenty-five seconds), with a six-inch telescope with a power of 44 and field of 57′ in declination. He saw every star on the map which he had previously published in SCIENCE (Feb. 23, 1883), down to the sixth magnitude, inclusive, except the thirty-sixth magnitude stars nearest to the sun; and he saw only these stars. One of the stars of the map was of the same magnitude as Watson's 'Vulcan.' This was a conspicuous object. No star half so bright as this could possibly have escaped observation. Mr. Holden is therefore confident that Vulcan did not exist within the limits swept over. Mr. Holden also determined the direction of the motion of the diffraction bands before and after totality. This was an observation which he could not make successfully in Colorado in 1878, and which he believes has not been before made.

A new method of investigating the flexure corrections of a meridian circle.

BY PROF. W. A. ROGERS OF CAMBRIDGE, MASS.

THE error due to refraction, the flexure of the circle itself, and the astronomical flexure, the three being functions in themselves, are most prolific errors respecting flexures of a meridian circle.

The theory which suggested itself was arrived at from the use on the telescope of a level of a different construction from any the author had ever seen. He had been a disbeliever in a level, but this device converted him into an advocate of the level. The level tube is attached to a plate, and the plate attached to the cube of the telescope. Then set the telescope at the north point, and reverse it to the south, reading the circle north and south. It would be much better were the point fixed upon a ring so that it can be readily placed at any inclination.

Results of tests with the almacantar, in time and latitude.

BY S. C. CHANDLER, OF CAMBRIDGE, MASS.

THE instrument which has been named the 'almacantar' was described and figured in a paper presented to the association at its meeting in 1880. In its general nature it is an equal altitude instrument. A hollow rectangular trough containing mercury revolves horizontally on an upright central pillar. The trough contains a float which is perfectly free to obtain equilibrium, while it is constrained to revolve with the trough. The float carries a telescope which turns on a horizontal axis, and can be clamped at any desired altitude. When this instrument is revolved on its vertical axis, any given point in the field of view describes a horizontal small circle, or almacantar, in the heavens. The transits of stars over a series of horizontal lines will thus afford means of determining the altitude of the instrument, the error of the clock, the latitude or the declinations of stars, by a proper distribution of the observations in azimuth.

A higher degree of accuracy is attainable by this instrument than by a transit or a zenith telescope of same size. The author's comparison of results is as follows: The probable error of a single star in determining the clock error is only $\pm 0.05^s$ or $\pm 0.06^s$. With a transit instrument of the same size, the quantity is not less than $\pm 0.08^s$. With the almacantar the probable error in determining the latitude of a single star is $\pm 0.55''$, including the error of the star's place. This is about equal to the probable error of a pair of stars by Talcott's method, with the larger telescopes of the United-States coast-survey.

The instrument was a small one, — $1\frac{3}{4}$ inches aperture and 25 inches focus. It was constructed for experiment only, in a provisional way, at a cost of $150. There are obvious defects in design and construction: when these are remedied, the error can be much reduced.

A series of observations with this instrument are given by the author, for the latitude of a pier about 80 feet north of the Harvard-college observatory. The value obtained by averaging these is $0.7''$ less than given by Professor Peirce in his discussion of the prime vertical transit observations taken by the Messrs. Bond, and adopted as the standard value of the latitude of the observatory. The author concludes that Professor Peirce's value is too large by fully three-quarters of a second. By way of proof the author gives a series of observations on the five stars used by Professor Peirce. These are compared with those of Auwers and Boss, and the correction of the hitherto accepted value of the latitude now indicated by the almacantar is thereby confirmed.

The clock errors of two nights selected at random, as given by the almacantar, were exhibited by the author. The results both in time and latitude would be considered satisfactory with an ordinary instrument of two or three times the size. The almacantar can be made much larger than the one under trial, certainly of five or six inches aperture, with corresponding increase of precision along with greater optical power. Its mechanical construction is simple, and reduces the sources of error. Thus in the older instruments there are involved: 1°. The accurate construction of parts, as of pivots, level, graduated circles. 2°. Fixity of mounting, to avoid a shifting of the instrumental plane. 3°. Rigidity of the instrument itself, to secure constancy of collimation and flexure. In the almacantar only the last condition has to be satisfied, and it is by far the easiest of the three to be attained mechanically.

The author regards the principle of flotation adopted as being as delicate an indication of the direction of gravity as is obtained by the spirit-level.

The almacantar gives promise of a new instrumental resource in the higher practical astronomy. It is competent to deal with the most delicate problems. It will evade some of the minute sources of error that still cling to meridian instruments. Especially, it furnishes a method for obviating difficulties, hitherto regarded as almost insuperable, connected with flexure and refraction, in observations with the meridian circle.

Internal contacts in transits of the inferior planets.

BY J. R. EASTMAN, OF WASHINGTON, D.C.

THE author began by reviewing the different values obtained in observing transits of Venus, and by computations thereon since 1761. Eventually it became certain that the differences of these values depended chiefly upon the computer's interpretation of the observer's record. The phenomenon known as the 'black drop' began to be considered as an element in the calculation. Stone regarded it as a necessary phenomenon. He gave an explanation of its origin, and stated that the moment when a dark ligament appears to connect the apparent limbs of the sun and Venus is the time of *real* internal contact. The second phase, when the limbs of Venus and the sun appear in contact, Stone says, is 'the *apparent* internal contact.'

In 1876 M. André, the astronomer in charge of the French expedition to Nouméa, in 1874, announced that "the bridge, black ligament, or black drop, as it is variously called, is a necessary phenomenon under certain circumstances, and not merely accidental." He noticed, however, that "it is always possible to get rid of the ligament, and reduce the phenomenon to geometrical constants, either (*a*) by reducing sufficiently the intensity of the source of light, or augmenting the absorbing power of the dark glass employed; or (*b*) by covering the object-glass with a dark diaphragm composed of rings alternately full and empty, all very thin, and bearing a certain proportion to the focal length of the lens."

These results and opinions of M. André were not generally known at the time of the transit of Mercury in 1878; although his theories were confirmed by his observations at Utah at that date, the results being published by him in 1881. The black drop was seen and recognized in 1878 by many observers of Mercury; some evidently regarding their success in finding it as a proof of accuracy of observation, others apologizing for failing to perceive the phenomenon.

The author of this paper regards it as noteworthy, that every observer, so far as ascertained, who got, by means of shade-glasses, the best definition of the sun's limbs, with an illumination less than the eye could easily bear, did not see any trace of the black drop. Before seeing any account of M. André's experiments, and having given little attention to his deductions announced by Father Perry, the author became independently convinced, after observation of the transit of Mercury in 1878, that the theory of a *necessary* black drop was fallacious.

While, in 1874, many American observers perceived the black drop, none appear to have seen it, among the eight American parties organized by the transit-of-Venus commission of 1882.

The paper winds up with an account of the observations of contact at the transit-of-Venus station at Cedar Keys, Fla., last December. The observation of first contact was prevented by a cloud covering a part of the sun's disk. On the disappearance of the cloud, the illumination was reduced by a sliding shade-glass, till easily endured by the eye. The definition of the sun's limb was perfect. When haze or cirri interfered, a less density of shade-glass was permitted; the steadiness and definition of the limb remaining, and that of Venus being 'all that could be desired,' with no modification, at the edge of the disk, of its dense black color.

Before the second contact, the entire disk of Venus was visible for several minutes. The portion beyond the sun's disk was bordered by a narrow line of light much less bright than the limb of the sun, and of a lighter tint. About one minute before contact, the apparent motion of the cusps of the sun, as they closed around the planet, noticeably increased, although the movement was perfectly steady. The cusps swept around the planet in a line of sunlight of the same tint as adjacent parts of the sun. This line was as narrow as could be seen with the power used, — 216 diameters, — and was free from tremors or pulsations. There was no agitation in the limb of either body near the point of contact, no trace of black drop, ligament, or band, no change of tint or color on the limb of Venus, and no indication of any clinging of the limbs. The contact was as easily, and perhaps as accurately, observed as the transit of a star within 8° of the pole, under the best conditions. The uncertainty of noting the time of the visible contact could not have been greater than three-tenths of a second. The phenomena at the third contact were similar to those at the second, but, of course, in a reversed order.

In conclusion, the author urges his belief, founded upon his own experience as well as on study of the work of other observers, that, with a properly arranged telescope and shade-glass, no observer need have trouble from any phase of the 'black drop.' To attain this end satisfactorily, the observer of contacts must have no other purpose in view than such observation. The study of any branch of solar physics, or searching for some new thing, may, and probably will, detract from the accuracy of his work, which should be confined to obtaining the record of a good definition of the sun's limb, as a reference-point in the passage of the limb of the planet.

An improved method of producing a dark-field illumination of lines ruled upon glass.

BY PROF. W. A. ROGERS OF CAMBRIDGE, MASS.

BY repeated and careful tests the author found that by letting the light, which is held at an angle of 45°, into the telescope, and then splitting the rays by means of two opposite mirrors, throwing them on the horizontal line, an almost perfect light is secured. Thereby it becomes practicable to see with distinctness stars of the smaller magnitudes upon a dark field.

Other astronomers present expressed a preference for the use of red light. Professor Rogers claimed that his method was better for minute observation.

Physical phenomena on the planet Jupiter.

BY G. W. HOUGH OF CHICAGO, ILL.

THE rapid motion of revolution of the planet, by changing the positions of the markings on the surface to our line of sight, makes great apparent differences in their shapes and sizes. This has perhaps been the occasion of reports of sudden and great changes upon the surface. The changes are not sudden, but are gradual; and many of the features are permanent. Minor changes are constantly in progress in the equatorial belts. The author recently observed the belt drifting down toward the red spot; but although it partly surrounded it, they did not coalesce, and the spot forced a scallop into the belt, — a very curious phenomenon. The author saw a satellite pass over this red spot, though the satellites are not visible when on the white part of the disk. He had also had a chance to compare shadows of satellites on the disk and on the spot, and both are dark. The red spot has seemingly retrogaded during the past four years; that is to say, the rotation of Jupiter has seemingly increased from 9 h. 55 m. 33 s., to 9 h. 55 m. 38 s. The future observer should attend more carefully to what he sees, and theorize afterward.

French observations on the solar eclipse of May 6, 1883.

BY DR. J. JANSSEN OF PARIS, FRANCE.

A LETTER from the French astronomer Dr. Janssen, who passed through this country on his return from an eclipse expedition, was addressed by him for the use of the association to Professor Eastman, who translated it, and read the translation in Section A. It was thus entered as one of the papers. Dr. Janssen says, —

"The principal object of the observations was the study of the dark rays in the corona. The visibility of these rays depends more on the light-power of the instrument than upon the perfection of the images. At first the ordinary brilliant rays which the corona presents were recognized; but what was new, and more complete than ever expected, was that the background of the coronal spectrum presented the Fraunhofer's spectrum. All the dark rays were theoretically visible. Phenomena were observed, which indicated that there were some portions of the corona which reflected, much more abundantly than others, the light emanating from the solar sphere: this would indicate the existence of cosmic matter circulating around the sun. The rings of Rispighi were not found arranged symmetrically around the sun. The light of the corona was strongly and radially polarized. All these things were associated with the problem of circumsolar cosmic matter. The observations went to show that no important intra-mercurial planet exists."

Some hitherto undeveloped properties of squares.

BY O. S. WESTCOTT OF CHICAGO, ILL.

THE paper began by ascribing due credit to a method for obtaining squares and square roots, described by Samuel Emerson in 1865. The principles and details of that method were briefly summarized. Mr. Westcott then stated the general principles of his own method, which is very expeditious. He first shows that the tens and units figures of all perfect squares of numbers, from 26 to 49 inclusive, are the same as the tens and units figures of perfect squares of numbers from 24 to 1 inclusive. A table is presented as follows:

$$(24)^2 = 576, \text{ add } 100, = 676 = (26)^2$$
$$(23)^2 = 529, \text{ add } 200, = 729 = (27)^2$$
$$(22)^2 = 484, \text{ add } 300, = 784 = (28)^2$$

and so on, to

$$(1)^2 = 1, \text{ add } 2400, = 2401 = (49)^2$$

To determine the square of any number between 25 and 50, find the corresponding number below 25, and augment its square by the number of hundreds indicated by its remoteness from 25. Or, more conveniently, take the excess above 25 as hundreds, and augment by the square of what the number lacks of 50.

$$\text{Thus:}\quad (43)^2 = (43-25)\,.\,100 + (50-43)^2 = 1800 + 49 = 1849$$

Conversely: To obtain the square root of 1764. The root is plainly between 25 and 50. The tens and units figures indicate 8. Therefore the square root of 1764 is $50 - 8 = 42$.

It is further observable, that the tens and units figures of perfect squares of numbers from 51 to 99 inclusive, are the same as the tens and units figures of the squares of numbers from 49 to 1 inclusive. Since $4 \times$ any number of hundreds + 25, 50, or 75, gives an exact number of hundreds, it follows that the tens and units figures of the squares of numbers less than 25 represent all the possible combinations of figures in those orders of units for *all* square numbers. The terminations of all perfect square numbers are 22 in all: viz., 00, 01, 04, 09, 16, 21, 24, 25, 29, 36, 41, 44, 49, 56, 61, 64, 69, 76, 81, 84, 89, 96.

The following rule is then deduced: To square any number from 50 to 100, take twice the excess above 50 as hundreds, and augment by the square of what the number lacks of 100.

$$\text{Thus:}\quad (89)^2 = 200\,(89-50) + (100-89)^2 = 7800 + 121 = 7921$$

Conversely, $\sqrt{3249}$: The root is plainly between 50 and 60; the tens and units figures indicate 7; therefore $\sqrt{3249} = 50 + 7 = 57$.

For greater convenience it is noted, that in such a case as $\sqrt{7921}$ the root is $50 + 39$ or $100 - 11$, and it is easier to use the latter form. That is, if the root is in the fourth quarter of the hundred, subtract the number indicated by the tens and units from 100, and the difference is the root. Thus $\sqrt{8281} = 100 - 9 = 91$.

To square any number from 100 to 200, take four times the excess above 100 as hundreds, and augment by the square of what the number lacks of 200.

To square any number from 125 to 250, take one-half the excess above 125 as thousands, and augment by what the number lacks of 250.

By a series of steps of this character, the author gives methods for squaring higher numbers, and conversely for obtaining their square roots. A choice of methods is also indicated. The facility which was obtained by such means was deftly illustrated on the blackboard by the author, who in a few seconds performed such exploits as raising 5 to the 16th power, and then showed in detail the processes which he had mentally executed. The paper sets forth the reason for each rule, deducing it from the usual binomial theorem, with almost obvious simplicity.

The demonstrations were received by the section with hearty applause. In response to an inquiry, Mr. Westcott stated, that he had been very successful in teaching this method in classes, about a tenth of his pupils becoming rapid experts in the methods of solution, which were especially useful in handling quadratic equations, and determining at a glance whether a given number is or is not a perfect square.