cerned. Some of us have known you personally before, and most of us have long been more or less familiar at second hand with your state and city; and yet, I think, to many of us it is something like a new revelation to see for ourselves what a few years have accomplished. I am not enough of a Latin scholar to quote my Virgil well; but I have been all the time most forcibly reminded of the passage in which Æneas first comes in sight of rising Carthage. Most emphatically the work 'hails' here. We see no drones or sluggards; but every shoulder is at the wheel, and every thing is moving. It may, perhaps, seem to you sometimes, when in our sectional meetings we discuss some question about the stars, or some hypothesis as to the formation of rock-strata, or the structure of some worm or insect, that we are out of the current, and contributing nothing to the advancement of the world. But you know it is not so, and your invitation to hold our meeting here shows that you know it. The world advances, not on one line only, but on many, — on lines material, intellectual, spiritual. To some extent, the movements are indeed independent, but not very far. Any true advance on either line implies corresponding movement on each of the others, if not absolutely simultaneous, yet surely consequent. There is no need to ask you here how much this city owes to modern science, when I see on every side, in your streets and storehouses and mills, the practical application of the highest engineering, mechanical, and electric art; and in the future it is almost certain that science is to contribute still more liberally to business. But not mainly for this reason do I claim your regard to science; but because, made in the image of God as we are, knowledge and understanding are as truly wealth and power as lands and food and money.

I need not add that, as you have invited us here, so we on our part cordially invite you to attend all our meetings, to listen to the papers and their discussion. We cannot promise that every paper will be interesting to all, but each one, I think, will be able to select certain ones he will be glad to hear; and if any of you choose to join us, and enroll yourselves as promoters of the advancement of science, our membership is open on easy terms. Once more, gentlemen, we thank you for the cordial welcome, and address ourselves to our business, in the hope and confidence that our meeting here is to be in the highest degree pleasant and successful.

PROCEEDINGS OF SECTION A. — MATHEMATICS AND ASTRONOMY.

ADDRESS OF WILLIAM A. ROGERS, OF CAMBRIDGE, MASS., VICE-PRESIDENT OF THE SECTION, AUG. 15, 1883.

THE GERMAN SURVEY OF THE NORTHERN HEAVENS.

THE illustrious Argelander was accustomed to say, in the quaint form of speech which he often employed, "The attainable is often not attained if the range of inquiry is extended too far." In no undertaking is there greater need of a judicious application of this sound maxim than in the systematic determination of the exact positions of all the stars in the visible heavens which fall within the reach of telescopes of moderate power.

The first subject which engaged the attention of the Astronomische gesellschaft, at its formation in 1865, was the proposition to determine accurately the co-ordinates of all the stars in the northern heavens down to the ninth magnitude. To this association of astronomers (at first national, but since become largely international, in its character and organization) belongs the credit of arranging a scheme of observations by which, through the co-operation of astronomers in different parts of the world, it has been possible to accomplish the most important piece of astronomical work of modern times. With a feasible plan of operations, undertaken with entire unity of purpose on the part of the observers to whom the several divisions of the labor were assigned, this great work is now approaching completion. While it is yet too early to speak with confidence concerning the definitive results which the discussion of all the observations are expected to show, we may with profit consider the object sought in the undertaking, the general plan of the work, the difficulties which have been encountered, and the probable bearing which the execution of the present work will have upon the solution of a problem concerning which we now know absolutely nothing with certainty, — a problem of which what we call universal gravitation is only one element, if, indeed, it be an element, — a problem which reaches farther than all others into the mysteries of the universe, — the motion of the solar and the sidereal systems in space.

Our first inquiry will be with respect to the condition of the question of stellar positions at the time when this proposal was made by the gesellschaft in 1865. All the observations which had been made up to this time possess one of two distinct characteristics. A portion of them were made without direct reference to any assumed system of stellar co-ordinates as a base, but by far the larger part are differential in their character. This remark holds more especially with reference to right ascensions. Nearly all of the observations of the brighter stars made previous to about 1830 were referred to the origin from which stellar co-ordinates are reckoned by corresponding observations of the sun; but since that date it has been the custom to select a sufficient number of reference stars, symmetrically distributed both in right ascension and declination, and whose co-ordinates were supposed to be well known. The unequalled Pulkova observations for the epoch 1845 form, I believe, the only exception to this statement. From the assumed system of primary stars are derived the clock errors and instrumental constants which are employed

in the reduction of all the other stars observed. The positions of these secondary stars, therefore, partake of all the errors of the assumed fundamental system, in addition to the direct errors of observation.

The following list comprises the most important of the catalogues which have been independently formed; viz., Bessel's Bradley for 1755, the various catalogues of Maskelyne between 1766 and 1805, Gould's D'Agelet for 1783, Piazzi for 1800, Auwer's Cacciatore for 1805, Bessel for 1815, a few of the earlier catalogues of Pond, Brinkley for 1824, Bessel for 1825, Struve for 1825, Bessel for 1827, Struve for 1830, Argelander for 1830, and Pulkova for 1845.

The important catalogues of secondary stars published previous to 1865 are comprised in the following table.

[*Table omitted.*]

An analysis of these catalogues reveals four important facts:—

First, that a large share of the observations relate to bright stars, at least to stars brighter than the eighth magnitude.

Second, that in a large number of cases the same star is found in different catalogues, but that no rule is discoverable in the selection.

Third, that with the exception of the polar catalogues of Fedorenko, Groombridge, Schwerd, and Carrington, the double-star observations of Struve, and the zone observations of Bessel and Argelander, the observations were not arranged with reference to the accomplishment of a definite object.

Fourth, that each catalogue involves a system of errors peculiar to the observers, to the character of the instrument employed, and to the system of primary stars selected, but that thus far there had been no attempt to reduce the results obtained by different observers to a homogeneous system. In estimating the value of these observations it will be necessary to refer to the researches which have been made subsequent to 1865.

The systematic deviations of different catalogues in right ascension *inter se* were noticed at an early date by several astronomers; but the first attempt to determine the law of these variations seems to have been made by Safford in a communication to the monthly notices of the Royal astronomical society in 1861 (xxi. 245), 'On the positions of the Radcliffe catalogue.' I quote the equation derived by Safford, since it appears to be the first published account of a form of investigation almost exclusively followed since that time. It is as follows:—

Diff. of R. A. (Greenw. 12 Year cat. − Rad.) = $-0.38^s. + 0.32^s. \sin(\alpha + 5\text{ h. }32\text{ m.})$. Extending this expression to terms of the second order, it may be put under the form, $\Delta = a \text{ constant} + (m \sin \alpha + n \cos \alpha) + (m' \sin 2\alpha + n' \cos 2\alpha) +$, etc.

Safford also seems to have been the first to notice the connection between the observed residuals, and the errors in position of the primary stars employed. He remarks, "In investigating the causes which would give rise to such systematic discrepancies, I was struck with the fact that the same or nearly the same variations were apparent in the assumed places of the time stars for the years since 1845; that, if the correct positions of the time stars had been assumed, the resulting positions would have been free from these small errors." That the relation given by Safford should have been observed at all, is the more remarkable, since the primary stars upon which the Radcliffe positions depend are nearly the same as those employed at Greenwich. In reality, the systematic errors of both catalogues have since been found to be considerably greater than is here indicated, and the deviation pointed out by Safford is in the nature of a second difference. The speaker has shown (*Proc. Amer. acad.*, 1874, 182) that the weight of the errors of the provisional catalogue assumed, fell between the first and the third quadrants in the Radcliffe observations for 1841–42, on account of the omission of certain clock stars which were used at Greenwich.

Since the discordances which exist between two catalogues may arise from errors in either one or in both, it is clearly impossible either to determine the nature of the erorrs, or to assign their true cause, until a fundamental system has been established which is free both from accidental and from periodic errors, — from accidental errors, since a few abnormal differences may easily invalidate the determination of the errors which are really periodic; from periodic errors, because a relative system can only become an absolute one when one of the elements of which it is composed becomes absolute.

We owe to the researches of Newcomb, published in 1869–70, a homogeneous system of stellar co-ordinates in right ascension, which are probably as nearly absolute in their character as it is possible to obtain from the data at present available. He determined the absolute right ascensions of thirty-two stars of the first, second, and third magnitudes, and comprised between the limits $-30°$ and $+46°$ declination. A comparison of the places of these stars for a given epoch, with the same stars in any catalogue for the same epoch, enables us to determine with considerable precision the system of errors inherent in that catalogue. Several circumstances prevent the exact determination of this relation. Among them may be mentioned the fact that Newcomb's system cannot safely be extended far beyond the limits in declination of the stars composing the system, that the stars are not symmetrically distributed in declination, and that the system of errors derived from bright stars is probably not the same as that derived from stars of less magnitude.

To a certain extent all of these objections have been met in the later discussion by Auwers, to which reference will presently be made. The substantial agreement of these two systems, independently determined, furnishes satisfactory evidence that we have at last obtained a foundation system with which it is safe to make comparisons, from which we may draw conclusions with comparative safety. When the catalogues which were formed between 1825 and 1865 are compared with Newcomb's fundamental system, through the medium of these thirty-two stars, the following facts are revealed.

a. The only catalogues in which there is freedom from both accidental and periodic errors are Argelander's Äbo catalogue for 1830, and the Pulkova catalogue for 1845. One is reminded, in this connecnection, of the remark of Pond, that "we can hardly obtain a better test of our power of predicting the future positions of stars than by trying by the same formula how accurately we can interpolate for the past. In a variety of papers which I have submitted to the Royal society, I have endeavored to show, that, with us, the experiment *entirely* fails."

b. During this interval the constant differences between the earlier catalogues and Newcomb's system vary between $+0.17^s$ for Pond, 1820; and -0.19^s for Pond, 1830: and for later catalogues, between $+0.07^s$ for Cambridge, 1860; and $+.02^s$ for Greenwich, 1860.

c. All the right ascensions determined at English observatories, and especially those which depend upon the positions published by the British Nautical almanac, are too large in the region of five hours, and too small in the region of eighteen hours. The general tendency of the constant part of the deviation from Newcomb's system is to neutralize the periodic errors in the region of five hours, and to augment them in the region of eighteen hours, where, in the case of a few catalogues, the error becomes as great as 0.10^s, — a quantity which can be readily detected from the observations of two or three evenings with an indifferent instrument, if it relates to a single star.

The right ascensions determined at French observatories exhibit systematic errors, which follow nearly the same law as those which characterize English observations.

Distinctively German observations are nearly free from systematic errors. As far as they exist at all, their tendency is to neutralize the errors inherent in distinctively English and French observations.

d. In the case of several catalogues, residual errors of considerable magnitude remain after the systematic errors depending upon the right ascensions have been allowed for. These errors are found to be functions of the declination of the stars observed, and without doubt have some connection with the form of the pivots of the instrument with which the observations were made. This statement holds true, especially with respect to the observations at Paris, Melbourne, and Brussels, between 1858 and 1871; and to the Washington observations between 1858 and 1861.

e. The systematic errors which exist in observations previous to 1865 follow the same law, and have nearly the same magnitude, as the errors of the same class which are inherent in the national ephemerides of the country in which they were made.

The British Nautical almanac and the Connaisance des temps are largely responsible for the perpetuation of this class of errors. For a few years before and after 1860, the ephemerides of the Nautical almanac were based upon the observations of Pond, which contain large periodic errors. It is found that the errors of this system have been transferred without sensible diminution to every catalogue in which the observations depend upon Nautical almanac clock stars. At English observatories it has been the custom to correct the positions of the fundamental stars by the observations of each successive year; but this has produced no sensible effect on the diminution of the periodic errors, which belong to the fundamental system. The periodic errors of the American ephemeris follow nearly the same law as the errors of the Nautical almanac, but their magnitude is somewhat reduced. The error of equinox is also less.

Wolfer's *Tab. reg.*, upon which the Berliner jahrbuch is based, has no well-defined systematic errors; and the correction for equinox is nearly the same in amount as in the American ephemeris, but with the opposite sign. The accidental errors seem to be rather larger than in the system of the American ephemeris.

f. A general estimate may be formed of the relative magnitudes of the errors of secondary catalogues by comparing the average error for each star of the primary catalogue. The numbers given below represent the average deviation for each star, expressed in hundredths of seconds, after the various catalogues have been reduced to a common equinox.

		Average error for each star.
Argelander	1830	1.1
Pulkova	1845	1.1
Greenwich	1845	2.0
Greenwich	1860	2.0
D'Agelet (Gould)	1783	2.2
Cape of Good Hope (Henderson)	1833	2.2
Greenwich	1850	2.2
Greenwich	1871	2.2
Paris	1867	2.4
Washington	1846-52	2.5
Struve	1830	2.5
Cape of Good Hope	1856	2.8
Radcliffe	1860	3.1
Greenwich	1840	3.1
Bessel	1825	3.2
Pond	1830	3.7
Gillis	1840	3.8
Madras (Taylor)	1830	3.9
Cape of Good Hope (Fallows)	1830	3.9
Radcliffe	1845	4.5
Armagh	1840	5.0
Piazzi	1800	5.3
Bessel's Bradley	1755	7.9
Lalande	1800	13.2
Lacaille	1750	24.9

It is obvious from these relations, that previous to about 1825 the magnitude of the accidental errors of observation, combined with the errors of reduction, prevent any definite conclusions with respect to the periodic errors inherent in these early observations. It is probable, also, that early observations of stars of the eighth and ninth magnitudes are subject to a class of errors peculiar to themselves, the nature of which it is now well-nigh impossible to determine.

The systematic errors in declination which belong to the various secondary catalogues named are even more marked than those in right ascension. The experience of Pond in 1833 is the experience of every astronomer who has attempted to compare observations of the same star made at different times, under different circumstances, with different in-

struments, and by different observers. He says, "With all these precautions, we do not find, by comparing the present observations with those of Bradley made eighty years ago under the same roof, and computed by the same table of refractions, that we can obtain by interpolation any intermediate catalogue which shall agree with the observations within the probable limits of error."

We owe to the investigations of Auwers (*Astron. nachr.*, nos. 1532–1536), the first definite system of declinations which is measurably absolute in its character. Yet the deviations of this system from that derived by the same author, but from much additional data in publication xiv. of the gesellschaft, is no less than 1.2″. The present difference outstanding between the Pulkova and the Greenwich systems at 10° south declination is 1.7″.

Within the past five years, the labors of Auwers, of Safford, of Boss, and of Newcomb, have resulted in the establishment of a mean system of declinations from which accidental errors may be considered to be eliminated in the case of a large number of stars; but the different systems still differ systematically *inter se* by quantities which are considerably greater than the probable error of any single position.

When the discussion of the question of a uniform determination of all the stars in the northern heavens to the ninth magnitude was taken up by the gesellschaft at its session in Leipzig in 1865, Argelander, who was then president of the society, appears to have been the only astronomer who had a clear apprehension of the difficulties of the problem. He alone had detected the class of errors whose existence subsequent investigations have definitely established. He alone had found a well-considered plan by which these errors might be eliminated, as far as possible, from future observations.

Argelander, however, always claimed for Bessel the first definite proposal of the proposition under consideration (see *Astron. nachr.*, i. 257). It was in pursuance of this plan that the zones between − 15° and + 15° in declination were observed. These zones were to form the ground-work of the Berlin charts; and Argelander, in the execution of the Bonn Durchmusterung, simply carried out the second part of Bessel's recommendation.

With the exception of the observations of Cooper at Makree observatory, and the charts of Chacornac, these two great works — the second being a continuation of the first, under a better and more feasible plan — are the only ones in existence which give us any knowledge of the general structure of the stellar system.

The observations of stars to the ninth magnitude, found in the catalogues of Bessel, Lalande, and Piazzi, form the ground-work of these charts. The co-ordinates in right ascension and declination of the stars found in these authorities were first reduced to the epoch 1800; the resulting right ascension being given to seconds of time, and the declination to tenths of minutes of arc. With these places as points of reference, all other stars were filled in, down to the ninth magnitude, by observations with equatorial instruments. The work was divided into zones of one hour each. Bremiker undertook five zones; Argelander and Schmidt, two; Wolfers, three; and Harding, two. The remaining zones were undertaken by different astronomers in widely separated localities.

The work seems to have been performed with somewhat unequal thoroughness, some zones containing nearly all the stars to the ninth magnitude, while in others a large number of stars having this limit in magnitude are wanting.

The Durchmusterung undertaken by Argelander at Bonn was a far more serious and well-considered undertaking. This unequalled work consists in the approximate determination of the co-ordinates of 324,198 stars situated between − 2° and + 90° declination. It includes stars to the 9.5 magnitude, the co-ordinates being given to tenths of minutes of time, and the declinations to tenths of minutes of arc.

The first definite proposal of this work undertaken by the gesellschaft, however, appears to have been made by Bruhns. In the course of a report upon the operations of the Leipzig observatory, he stated, that, in his view, the time had come for undertaking a uniform system of determinations of the places of stars to the ninth magnitude in the northern hemisphere by means of meridian circles; but he proposed, at the same time, that the positions of stars fainter than the ninth magnitude should be determined by means of differential observations with equatorial instruments. After explaining certain plans and arrangements relating particularly to his own observatory, he introduced the following resolution: —

"The Astronomische gesellschaft regards it as needful that all the stars to the ninth magnitude, occurring in the Durchmusterung, should be observed with meridian circles, and commissions the council to arrange for the execution of the work."

This proposal occasioned a long and somewhat animated discussion, in which Argelander, Hirsch, Bruhns, Förster, Schönfeld, and Struve took part.

Argelander declared himself surprised at this proposal, which called for the rapid realization of a plan of organization which he had been considering for years with the greatest care, the difficulties of which he had maturely considered, and the execution of which still demanded the most careful deliberation and preparation. One of the necessary preliminary steps was a plan which he had already prepared, published and presented to the society in an informal way, which provided for contemporaneous and corresponding observations of the brighter stars. As president of the society, he felt unequal to undertaking the charge which the acceptance of the resolution proposed would involve; as this procedure seemed to him premature without previous preparation. He would admit, however, that every call to action of this kind tended to stimulate enthusiasm, and should therefore be encouraged; but he felt obliged to ask the society not to require from him the immediate execution of the plan, but to intrust the serious con-

sideration of it, and the preparation for it, to his zealous friends in the council.

Upon the motion of Struve, the society by a rising vote, expressed its confidence in the assurance of the president that he would bring forward his plan at the proper time, as soon as the means for its execution could be assured.

At the meeting held at Bonn in 1867, Argelander again brought up the subject in a communication which appears to have been an exhaustive discussion of the whole problem. This paper is not printed in the proceedings of the gesellschaft; but at its conclusion a committee was appointed to take definite action with respect to the recommendations which it contained. The committee reported at the same session; and their report, which is published in the place of the paper presented by Argelander, is probably identical in substance with it. The plan proposed and adopted was finally published in the form of a programme, in which the details of the work are arranged with considerable minuteness. As this programme has been widely distributed, it seems unnecessary to give any thing more than a general abstract of it. Since it differs in a few minor points from the first report of the committee at the Bonn meeting, the essential features of this report will be given instead of an abstract of the programme itself.

They are as follows: —

a. The limits in declination of the proposed series of observations are — 2° and + 80°. The first limit was chosen on account of the lack of suitable fundamental stars south of the equator. It is probable, also, that Argelander had a suspicion of the fact, since proven, that the uncertainty with respect to the systematic errors of southern stars is, of necessity, considerably greater than for northern stars, and that on this account it would be better to defer this part of the work until further investigations in this direction could be made.

The limit + 80° was chosen because the repetition of Carrington's observations between 81° and 90° was considered superfluous, and Hamburg had already undertaken the extension of Carrington's observations from 81° to 80°.

b. Within these limits, all stars in the Durchmusterung to the ninth magnitude, and, in addition, all stars which have been more exactly observed by Lalande, by Bessel at Koenigsberg, and by Argelander at Bonn, are to be observed.

c. The observations are to be differential. The clock errors are not to be found from the fundamental stars usually chosen for this purpose, and the equator point corrections are not to be derived from observations at upper and lower culminations, but these elements are to be derived from a series of 500 or 600 stars, distributed as uniformly as possible over the northern heavens. The exact co-ordinates of these stars are to be determined at Pulkova, thus securing the unity necessary in order to connect in one system the observations of different zones.

d. Every star is to be observed twice. If the two observations differ by a quantity greater than ought to be expected, a third observation will be necessary.

e. In order to facilitate the work, it will be desirable to use only three or four transit threads, and only one or two microscopes. In order to facilitate the reductions to apparent place, the working-list of stars should be comprised within narrow limits.

f. Before the commencement and after the close of each zone, two or three fundamental stars are to be observed upon the same threads and with the same microscopes as were used in the zone observations. When the seeing is not good, and when for any other cause it seems desirable, one or more fundamental stars may be observed in the course of the zone. The number and selection of the stars will depend upon the character of the instrument employed. If it remains steady for several hours, and has no strongly marked flexure or division errors, or if these errors have been sharply determined, the fundamental stars may be situated ten degrees or fifteen degrees away from the zone limits. However, there must remain many things for which no general rule can be given, and which must be left to the judgment of the observer, aided by an accurate knowledge of his instrument.

g. With a Repsold or a Martin instrument, one microscope will be sufficient, if its position with respect to the whole four can be determined. It will be sufficient, if the change in position during the observations can be interpolated to 0.2″.

h. It will be desirable to divide beforehand the zones into such time intervals that the observations can be easily made.

i. Zones exceeding one and one-half or at the most two hours are not advisable, first, because the zero points will be too far apart, and, second, because a longer duration will involve too much fatigue physically and mentally.

At the conclusion of this report, all the astronomers present who were willing to take part in this work were requested to communicate with the council, stating the region of the heavens which they preferred to select for observation.

At this meeting, Berlin, Bonn, Helsingfors, Leipzig, and Mannheim signified their intention to share in the work. Leiden also expressed its intention of taking part as soon as the work already undertaken should be completed.

When the stars to be observed had been selected from the Durchmusterung, it was found that the number would not vary much from 100,000, requiring rather more than 200,000 observations. Preparations for the work of observation were immediately commenced; and, by the time of the next report in 1869, considerable progress had been made.

In the report for this year, the provisional places of a catalogue of 539 fundamental stars were published. This catalogue is composed of two parts. The list of hauptsterne consists of 336 stars to the fourth magnitude, observed at Pulkova by Wagner with the large transit instrument, and by Gyldén with the Ertel vertical circle. The list of zusat-sterne consists of 203 stars fainter than the fourth magnitude. As the details of the work in the formation of the provisional places of the stars of this list are not given

in the report, it is not quite clear upon what authority they rest. The work assigned to the Pulkova observatory by the zone commission was the exact determination of the places of the stars of this list. The observations were undertaken by Gromadski with the Repsold meridian circle. In accordance with the plan adopted, each star was observed eight times, — four times in each position of the instrument. The observations were differential with respect to the hauptsterne.

The results were published by Struve in 1876; and the places there given were used in the first reduction of the Harvard-college observations for 1874–75, and perhaps in some other cases.

About this time a change seems to have been made in the original plan with respect to the formation of the final catalogue of fundamental stars, of which I have been unable to find a clear account. The original intention was to make the positions depend entirely upon the observations at Pulkova. The zone commission established by the gesellschaft, however, committed the formation of this catalogue to Auwers; and it is to him that we owe the most complete and the most perfect catalogue of fundamental stars yet published. The Pulkova system for 1865 was adopted as the basis; but, in order to obtain greater freedom from accidental errors for individual stars, the final catalogue was obtained by combining with the Pulkova series, the Greenwich observations from 1836 to 1876, the Harvard-college observations for 1871–72, the Leipsic observations, in declination only, between 1866 and 1870, and the Leiden observations in declination between 1864 and 1870. Before this combination was made, however, these observations were all reduced to the Pulkova system.

The following observatories have taken part in the zone observations: —

Observatories.	Limits of zones in declination.	Observatories.	Limits of zones in declination.
Nicolajeff . . .	− 2° to + 1°	Lund	+35° to +40°
Albany	+ 1 " + 5	Bonn	+40 " +50
Leipsic	+ 4 " +10	Harvard college .	+50 " +55
Leipsic	+10 " +15	Helsingfors . .	+55 " +60
Berlin	+15 " +25	Christiana . . .	+65 " +70
Cambridge (Eng.)	+25 " +30	Dorpat	+70 " +75
Leiden	+30 " +35	Kasan	+75 " +80

The zone between −2° and +1° was originally undertaken at Palermo, that between +1° and +4° at Neuchâtel, that between +4° and +10° at Mannheim, and that between +35° and +40° at Chicago.

In the latter case, the great fire at Chicago crippled the resources of the observatory to such an extent, that Safford was compelled to relinquish the work, which was at that time quite far advanced.

The chief items of interest in connection with this work are found in the following tabular statement: —

[*Table omitted.*]

Attention was called, at an early date, to the importance of continuing the survey of the northern heavens beyond the southern limit fixed by Argelander. The preparation necessary for the execution of this work consisted in the extension of the Durchmusterung to the tropic of Capricorn. This was undertaken by Schönfeld at Leipsic.

In the report to the gesellschaft at the meeting held at Stockholm in 1877, he has given an account of this work, in which he stated that it was sufficiently near completion to invite the consideration of the question of the meridian circle determinations of the places of stars to the ninth magnitude. The lack of southern fundamental stars whose positions were well determined was still a hinderance to the immediate commencement of the work. Relatively more stars of this class are required than in the northern observations, in order to eliminate the inequalities due to refraction. Schönfeld stated, that, while the burden of the determination of the places of these southern fundamental stars must rest mainly upon southern observations, it seemed necessary to connect them with the Pulkova system by a connecting link (mittelglied), through observations at some observatory well situated for this purpose. At this meeting Sande Bakhuysen, at Leiden, gave notice of intention to take part in this work. Gyldén urged the importance of securing the co-operation of Melbourne; and Peters suggested the advantage of securing Washington as an additional 'mean term' (V. J. S. 1877, p. 265).

The next reference to this work is contained in the vierteljahrsschrift for 1881, xv. p. 270. A list of 303 southern stars is here given, whose exact places were at that time being determined at Leiden and at the Cape of Good Hope. This list was selected by Schönfeld and Sande Bakhuysen, in a way to meet the requirements referred to in previous discussions.

A final catalogue of 83 southern fundamental stars by Auwers appears in this number of the vierteljahrsschrift. The places depend upon the same authorities as for the northern stars, with the addition of the Cape of Good Hope catalogue for 1860, Williamstown, Melbourne for 1870, and Harvard college (Safford) for 1864. For stars not observed at Pulkova, the general catalogue of Yarnall (1858–1861), and the Washington observations, with the new meridian circle between 1872 and 1875, were employed. As in the case of the northern stars, these observations are all reduced to the Pulkova system for 1865. It is underderstood that the co-ordinates of the list of 303 stars are to depend upon this extension of the general system of publication xiv. to the limits required by the southern Durchmusterung of Schönfeld.

It would be surprising if all the conditions of success were fulfilled in the first execution of a work having the magnitude, and involving the difficulties, of the scheme of observations undertaken under the auspices of the gesellschaft. The extent of the discordances which are to be expected between the results obtained by different observers can only be ascertained when the observations by which the different zones are to be connected have been reduced. Each observer extended the working-list of his own zone 10′ north and south; and it is expected that a sufficient number of observations of this kind has been made to determine the systematic relations

existing between the co-ordinates of each zone with those of its neighbor.

It is probable, however, that the experience of Gill will be repeated on a larger scale. In 1878 he solicited the co-operation of astronomers in the determination of the co-ordinates of twenty-eight stars, which he desired to employ in the reduction of his heliometer observations of the planet Mars for the purpose of obtaining the solar parallax. The results obtained at twelve observatories of the first class are published in vol. xxxix. p. 99, of the monthly notices of the Royal astronomical society. Notwithstanding the fact that the final values obtained at each observatory depend upon several observations, the average difference between the least and the greatest results, obtained by different observers for each star, is 0.24ˢ. in right ascension and 2.3″ in declination. In four cases the difference in right ascension exceeds 3.0, and in four cases the difference in declination exceeds 3.0.″

Even after the results are reduced to a homogeneous system, the following outstanding deviations from a mean system are found: —

Authority.	Δ α	Δ δ	Authority.	Δ α	Δ δ
	s.	″		s.	″
Koenigsberg . .	+.005	−0.71	Leiden	−.053	−0.19
Melbourne . . .	+.026	−0.49	Paris	+.055	+0.01
Pulkova. . . .	+.005	+0.36	Washington . .	−.120	+0.78
Leipsic	+.049	+0.40	Harvard college,	−.072	+0.09
Greenwich. . .	+.009	−0.56	Cordoba . . .	−.032	−0.20
Berlin	+.044	+0.67	Oxford	+.076	+0.21

The observations of a second list of twelve stars, one-half of the number being comparatively bright, and the remaining half faint, showed no marked improvement, either with respect to the magnitude of errors which could be classed as accidental, or in regard to the systematic deviations from a mean system.

This discussion revealed one source of discordance which will doubtless affect the zone observations; viz., the difference between right ascensions determined by the eye-and-ear method, and those determined with the aid of the chronograph.

The programme of the gesellschaft makes no provision for the elimination of errors which depend upon the magnitude of the stars observed; but special observations have been undertaken at several observatories for the purpose of defining the relation between the results for stars of different magnitudes. At Harvard-college observatory, the direct effect of a reduction of the magnitude has been ascertained by reducing the aperture of the telescope by means of diaphragms. Beside this, the observations have been arranged in such a manner that an error depending upon the magnitude can be derived from an investigation of the observations upon two successive nights.

At Leiden, at Albany, and perhaps at other observatories, the effect of magnitude has been determined by observations through wire gauze. But notwithstanding all the precautions which have been taken in the observations, and which may be taken in the reductions, it will undoubtedly be found that the final results obtained will involve errors which cannot be entirely eliminated.

In the experience of the speaker, two other sources of error have been detected. It has been found, that there is a well-defined equation between the observations, which is a function of the amount, and the character of the illumination of the field of the telescope. It has also been found that observations made under very unfavorable atmospheric conditions differ systematically from those made under favorable conditions. When the seeing was noted as very bad, it is found that the observed right ascensions are about .08ˢ too great, and that the observed declinations are about 0.8″ too great.

There are doubtless other sources of error which the discussion of the observations will bring to light. The effect of the discovery of these and other errors will probably be to hasten the repetition of the zone observations under a more perfect scheme, framed in such a manner as to cover all the deficiencies which experience has revealed, or may yet reveal. One would not probably go far astray in naming the year 1900 as the mean epoch of the new survey. If the observations are again repeated in 1950, sufficient data will then have been accumulated for at least an approximate determination of the laws of siderial motion.

What is the present state of our knowledge upon this subject? It can be safely said that it is very limited. First of all, it cannot be affirmed that there is a sidereal system in the sense in which we speak of the solar system. In the case of the solar system, we have a central sun about which the planets and their satellites revolve in obedience to laws which are satisfied by the hypothesis of universal gravitation. Do the same laws pervade the inter-stellar spaces? Is the law of gravitation indeed universal? What physical connection exists between the solar system and the unnumbered and innumerable stars which form the galaxy of the heavens? Do these stars form a system which has its own laws of relative rest and motion? or is the solar system a part of the stupendous whole? Does the solar system receive its laws from the sidereal system? or has Kepler indeed pierced the depths of the universe in the discovery of the laws which gave him immortality? Are we to take the alternative stated by Ball, — either that our sidereal system is not an entirely isolated object, or its bodies must be vastly more numerous or more massive than even our most liberal interpretation of observations would seem to warrant? Are we to conclude, for example, that stars like 1830 Groombridge and α Centauri, "after having travelled from an infinitely great distance on one side of the heavens, are now passing through our system for the first and only time, and that after leaving our system they will retreat again into the depths of space to a distance which, for any thing we can tell, may be practically regarded as infinite"? Can we assert with Newcomb, that in all probability the stars do not

form a stable system in the sense in which we say that the solar system is stable, — that the stars of this system do not revolve around definite attractive centres? Admitting that the solar system is moving through space, can we at the present moment even determine whether that motion is rectilinear, or curved, to say nothing of the laws which govern that motion? How much of truth is there in the conjectures of Wright, Kant, Lambert, and Mitchel, or even in the more serious conclusions of Moedler, that the Alcyone of the Pleiades is the central sun about which the solar system revolves?

These are questions which, if solved at all, must be solved by a critical study of observations of precision accumulated at widely separated epochs of time. The first step in the solution has been taken in the systematic survey of the northern heavens undertaken by the gesellschaft, and in the survey of the southern heavens at Cordoba by Dr. Gould. The year 1875 is the epoch about which are grouped the data which, combined with similar data for an epoch not earlier than 1950, will go far towards clearing up the doubts which now rest upon the question of the direction and the amount of the solar motion in space; and it cannot be doubted that our knowledge of the laws which connect the sidereal with the solar system will be largely increased through this investigation. The basis of this knowledge must be the observed proper motions of a selected list of stars, so exactly determined that the residual mean error shall not affect the results derived; or, failing in this, of groups of stars symmetrically distributed over the visible heavens, sufficient in number to affect an elimination of the accidental errors of observation, without disturbing the equilibrium of the general system.

For an investigation of this kind, a complete system of zone observations, at widely separated intervals, will afford the necessary data, if the following conditions are fulfilled.

First: The proper motions must be derived by a method which does not involve an exact knowledge of the constants of precession. In every investigation with which I am acquainted, the derived proper motions are functions of this element.

Second: The general system of proper motions derived must be free from systematic errors. Errors of this class may be introduced either through the periodic errors inherent in the system of fundamental stars employed in the reduction of the zone observations, or in a change in the constants of precession. It is in this respect that the utmost precaution will be required. If from any cause errors of even small magnitude are introduced into the general system of proper motions at any point, the effect of these errors upon the values of the co-ordinates at any future epoch will be directly proportional to the interval elapsed. We can, therefore, compute the exact amount of the accumulated error for any given time.

When this test is applied to the fundamental stellar systems independently determined by Auwers, Safford, Boss, and Newcomb, we find the following deviations *inter se* at the end of a century.

	Maximum mean deviation in a century.		Maximum systematic deviation in a century.	
	$\Delta\alpha$	$\Delta\delta$		
Auwers *minus* Safford . . .	-0.22^s	$+0.2''$	0.23^s	$1.1''$
Auwers *minus* Boss	–	+0.8	–	2.1
Auwers *minus* Newcomb . .	−0.09	+0.8	0.06	2.2

It is the common impression, that both the direction and the amount of the motion of the solar system in space are now well established. The conclusions of Struve upon this point are stated in such explicit language that it is not surprising that this impression exists. He says, "The motion of the solar system in space is directed to a point in the celestial sphere situated on the right line which joins the two stars measured from π and ω Herculis. The velocity of this motion is such that the sun, with the whole cortége of bodies depending on him, advances annually in the direction indicated, through a space equal to one hundred and fifty-four million miles."

It must be admitted that there is a general agreement in the assignment by different investigators of the co-ordinates of the solar apex. This will be seen from the following tabular values.

Authorities.	Right ascension.	Declination.
Herschel, 1783	257° 00′	+25° 00′
Prevost	230 00	+25 00
Klugel, 1789	260 00	+27 00
Herschel, 1805	245 52	+49 38
Argelander, 1837	257 49	+28 50
Lundahl	252 24	+14 26
Struve	261 22	+37 36
Galloway	260 01	+34 23
Mädler	261 38	+39 54
Airy	256 54 261 29	+34 29 +26 44
Dunkin	261 14 263 44	+32 55 +25 00

In estimating the value which should be attached to these results, several considerations must be taken into account.

(*a*) All of the results except those of Galloway depend practically upon the same authorities at one epoch, viz., upon Brodley.

(*b*) The deviations *inter se* probably result, in a large measure, from the systematic errors inherent in one or both of the fundamental systems from which the proper motions were derived. For example, Lundahl employed Pond as one of his authorities, and it is in Pond's catalogue that the most decided periodic errors exist.

(*c*) Brot in 1812, Bessel in 1818, and Airy in 1860, reached the conclusion that the *certainty* of the movement of the solar system towards a given point in the heavens could not be affirmed.

(*d*) The problem is indirect. In the case of a mem-

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