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THE EQUINOX OF 1950.0[1]

IN every science, in all human progress, there are dividing boundaries, epochs, milestones, marked in no regular fashion nor by equal intervals of time, but by discoveries. Not infrequently it is the life work of some group of men, or the individual contribution of some surpassing intellect, which forms the zero year from which we date a particular epoch in scientific progress. Though the historian of science, a thousand years hence, looking back upon our era may feel sure that the advancement of present science is a continuous function, with few singularities, multiple points or imaginary roots, to those who now take part in this work it frequently seems as though the increase in our knowledge is discontinuous and saltatory. Instead of an evolution progressing by infinitesimal variations in our scientific characters, inherited or acquired, we are ourselves so close to the astounding events of present progress in the physical sciences that we feel rather that we are undergoing an evolution based upon sudden mutations.

Astronomy has similar apparent epochs of sudden mutation, marked by men or processes—the ages of Newton and Bessel, the eras of the meridian circle, of photography, of the spectroscope. Each has marked a tremendous advance, *per saltum,* progress which has seemed to the scientist of each era to be by leaps and bounds rather than by the slow accretions of natural growth, discontinuous rather than continuous. We are prone to forget, in such a view, the minor contributions which have made the road ready, and time has a way of running a relatively smooth curve through the irregularly plotted points. No lapse of time, however, will ever entirely smooth out this always ascending curve; it may always have sinuosities, overtones of the main great harmonic. Perhaps it is sufficient for us, as we contemplate our section of this great graph, to leave to time the smoothing out of the curve of our present progress, while we derive satisfaction, pride and hope from the fact that the curve is definitely ascending.

More than one dreamer, combining mathematics with fancy, has drawn attention to the supposed parallel between the curve of progress and the hyperbola referred to rectangular asymptotes as axes. Sweeping down from infinity, this curve as ordinarily drawn at first makes tremendous and rapid progress

[1] Address of retiring vice-president of Section D, Astronomy, of the American Association for the Advancement of Science, Washington, December, 1924.

toward its goal, the other asymptote. But gradually its rate of approach slows down, and finally aeons must elapse before the curve becomes appreciably closer to its parallel running aim. Such speculations dwell upon the slowness of research and upon the difficulty in making real additions to our sciences where all the really worthwhile discoveries were made when the world was new and all. But, to take only one science which is daily becoming more and more indispensable to my own, who can consider the results in physics secured during the past quarter century as falling on such a curve, as showing any falling off in power or in truly apocalyptic revelation? We must, it seems, rotate this curve ninety degrees accurately to represent our graph of progress. The curve has risen slowly, for a time almost infinitely long; now the rate of progress is increasing faster than the mind of even the specialist can follow; though we shall never reach the asymptote of absolute knowledge, our rate of progress is becoming increasingly rapid. We can forego an actual entrance into heaven, provided only that new discoveries shall infinitely continue to reward our toil!

More fortunate, perhaps, than its sister sciences, astronomy possesses also certain very definite milestones, unlike those epochs which are dated from men or inventions in that they regularly recur. The twenty-five thousand year long rotation of the axis of the giant gyroscope on which we live, which we call precession, and the nineteen-year-long nodding of our pole as it makes this gyration, which we call nutation, and other minor causes, make troublesome changes in the apparent position of that sky which is our field of work. So it has become convenient, and hence the custom, in the astronomy of position, perhaps the most fundamental portion of our science, to refer all charts, maps and positions, not to the slowly shifting equinox of the date when the observation is made, but to the equinox of some fixed epoch at the beginning of a year not too remote in point of time in either direction. Most of our great celestial inventories made in recent years are accordingly referred to an equator and an ecliptic as they were for an instant at the beginning of the year 1900, and any star positions which we publish or quote generally have after them these figures in parentheses, indicating that the positions are referred to the equinox of 1900.0, though we are certain that all the data are different now. Some have suggested the equinox of 1925.0 as a convenient mid-epoch, but it seems more probable that we shall begin now to refer all future data to the equinox of 1950.0 as our next zero year. Considerable thought has been given by astronomers as to the optimum frequency of these quasi-celestial milestones of reference. An interval of a century is certainly too long, and many feel that a quarter of a century multiplies unduly the number of such epochs. Probably the majority of practical astronomers would prefer half-century equinoxes, and it seems highly probable that the equinox of 1950.0 will be set aside as the next reference epoch of the astronomy of precision.

My own work as an astronomer, after a youth not misspent in the classics, started at our last astronomical reference epoch, that of 1900, and the work of many members of our society began a decade or two before that. We have seen a wonderful quarter century of progress in our own and in the allied sciences; many of us will not welcome in the equinox of 1950.0. There are many precedents to the effect that a retiring vice-presidential address should be devoted to a survey of recent progress and achievements; not infrequently the events of but one or two years preceding will give far more than enough material to discuss in the time allotted. But I have preferred to look forward, rather than back. What then are some of the things we may at least hope shall be added to our astronomical inventories of the equinox of 1950.0? Prophets, in science, are generally without honor in all countries as well as in their own, but prophecies must always be based in part upon the past and are, to that extent at least, illuminating.

The equinox of 1925.0 finds the astronomer practically driven off the earth by the meteorologist, the geodesist, the physicist and the seismologist. After the discoveries of the past decade, it would be futile to attempt to prophesy anything for the physicist, and we may safely leave to these fields of science the advances to be made in the coming twenty-five years in the knowledge of our earth. The variation of latitude appears to be a field where all branches may contribute, and to be almost the only part of earth-science where the services of the astronomer are still needed; it seems probable at present that the equinox of 1950.0 will report on this phenomenon as still consistently irregular. We may hope for a somewhat more accurate knowledge of such phenomena as the aurora, for refinements in our theories of earth rigidity, the tides, isostasy and the conditions obtaining in the earth's interior. Our theories of the causation of the ice-ages are now more local than astronomical and may so remain.

I hesitate to function as a prophet when touching the subject of possible advances in instruments or methods. Some new method may change the picture as completely as the spectroscope has differentiated the astronomy of 1925 from that of 1825. To take but one example: let us dream of a dry plate one hundred times as sensitive as the most rapid which we now use, ideal as to "grain" and keeping qualities, and with a uniform range of spectral sensibility from the ultra-violet to the infra-red. This is a wonderful dream; perhaps we may get such a plate in heaven! Such a discovery, or rather advance, would have the same practical effect on astronomy as to replace the 36-inch telescope of the Lick Observatory with one having an object glass thirty feet in diameter! The cycle just past has seen the construction of the Mt. Wilson 100-inch, the Victoria 72-inch, and a number of other noble instruments. Shall we have any larger reflectors by 1950? While many believe that the refracting telescope has reached its maximum efficient size at 40 or perhaps 48 inches, the limit for a reflector is theoretically about sixteen feet. But here, rather than upon the optician's skill, we shall doubtless be more dependent upon the glass-maker's ability, as well as upon the results of income and inheritance taxes in cutting down bequests for scientific research. We may confidently look forward to great extensions in the use of the photo-electric cell and the interferometer, and it will be very strange if the wonderful delicacy of the vacuum tube is not soon utilized in astronomical research.

In extrapolating the curve of astronomical progress for the coming period which will end in 1950 we can, in most fields, do little more than continue the tangent to the present curve. If this has risen but little in the past quarter century, its slope can be expected to increase in the coming epoch only through the introduction of new processes or through discoveries whose nature can not be predicted. Reasoning thus from the past, it seems probable that the equinox of 1950.0 will show but little more than that of 1900.0 in our knowledge of the surface features of the moon and the planets. To clear up the problem of the small outstanding irregularities in the motion of the moon we shall need a century of observation, in all probability, rather than twenty-five years, though five may suffice to compare with the new tables of Brown. In so far as unanimity of astronomical opinion is concerned, it is doubtful whether our knowledge of the surface features of Mars is much more definitely established now than it was in 1900; we may perhaps hope that 1950 will see some reasonable measure of agreement as to the canals of Mars, either for or against.

Were we to select the two most important advances made in the field of cometary research in the past quarter century, our choice would doubtless be, first, the completion of the researches of Strömgren, Fayet, and others, definitely establishing the fact that the comets are bona-fide members of our solar system, being wanderers from outside space in possibly only the rarest of cases. The second selection would doubtless be the advance due to Fowler, in identifying carbon monoxide as an important constituent of cometary tails. We have had no bright comet in the past fifteen years; from the law of averages pertaining to such bodies, it would seem reasonably probable that we may expect one or more bright comets before we have passed our next astronomical milestone, enabling further spectroscopic advances to be made.

1950 will see the number of the asteroids doubled or tripled; the problem of keeping track of the thousand odd we now know has become so serious a one that it may well be that the councils of 1950 will decide that the labor and the rewards are no longer commensurate, that the greater number of the asteroids must be abandoned as soon as possible, leaving only a few of the more interesting objects or types to be systematically followed. The work of discovery will probably continue unabated for a time, as a few asteroids of the Eros type will be worth a thousand of the more mongrel varieties. But this work of discovery can be carried on efficiently only so long as rough ephemerides exist for all asteroids previously discovered, and the time is coming when this task will become too costly and too time-consuming, unless some such graphical perturbation machine as that of Sundman can be constructed to grind out asteroid orbits and their perturbations much as we now grind out tidal tables.

At this midway point from the equinox of 1900.0, there seems to have been secured no more important recent result in the field of space outside our own minute solar system than the confirmation beyond all doubt of the fact that our sun is an average star, roughly at the middle point, so to speak, of our various schemes of stellar evolution, and apparently quite near the general average, also, in its size and in its intrinsic luminosity. As is well known, this deduction works both ways; like most important generalizations, it is Janus-faced. We have in the stars important lines of evidence bearing upon the constitution and upon the evolution of our sun. And in that nearest star which we call the sun, we no longer have to deal with a datum compressed angularly almost into a mathematical point, but we have a star close enough to us to offer us a superficies which, while almost too remote for some of our researches, still permits us to concentrate our studies upon some portion, some detail, of a star's surface.

It is perhaps too much to hope that the equinox of 1950.0 will see the solution of more than a very few of our many at present unsolved solar problems. It now seems evident that in the average star we have a wonderfully complex interweaving of forces and conditions in large part of unknown character, and with this complexity a truly marvelous balance and permanency. Perhaps the best proof of the balance of forces which must exist lies in the fairly well established truth that our sun can not, for any very long period of time continuously, have varied its heat emission by as much as five per cent. during the past two hundred million years. We have come to believe that the life period of a star is of such duration that it may almost receive the adjective eternal; certainly a matter of trillions, rather than millions of years. At our last semi-centennial equinox we should have spoken with all certainty as to the method by which the heat emission of the sun is maintained. We felt that in the contraction theory of the sun's heat we had a rigorous mechanical and dynamical explanation, mathematically exact; following these apparently rigorous proofs we were at that time unwilling to allow more than a mere trifle of one hundred million years or so for the sun's past life, and somewhat less for its future. Increased knowledge frequently brings an uncertainty far less comfortable than the preciseness of ignorance. The astronomer is now willing to grant the geologist and the biologist all the time they need, even all the time they wish. But with this new-found generosity in the time factor, we now are forced to admit our doubts as to the precise mechanism of the output of solar radiation; we conveniently ascribe it to some manifestation of sub-atomic energy; though we know very little about this, we at least feel sure that there is enough of it to last over the most leisurely geological or evolutional cycle upon cycle. The astronomer assumes no undue measure of credit for this deduction, inasmuch as he is forced to adopt it through a rather elementary *reductio ad absurdum*. There seems at present nothing else to explain the sun's tremendous output of energy except sub-atomic forces; therefore, these must be the source.

Omitting eclipses less than two minutes in length, or probably not usable for other reasons, there will be nearly 41 minutes of totally eclipsed sun between now and the equinox of 1950.0. Of course, not all these forty-one minutes will be clear, but when it is remembered that we have had to date, which means practically since the introduction of the photographic dry-plate, only about forty-eight minutes available for eclipse work, it seems evident that the equinox of 1950.0 may see some material advances in our knowledge of the sun's surroundings. Further work is to be expected on the rotation of the corona. The mysterious so-called element coronium still baffles us. There seems no reasonable and logical gap for it among our ninety-two sorts of matter. Very probably it is some as yet unknown spectral manifestation of hydrogen, helium, or calcium; but which? At least three attacks on phases of this problem are planned for the coming eclipse of January 24, by interferometric methods, and with different instrumental equipment, in the attempt to throw some light on the nature of coronium. We remember that helium was first seen and named in the sun, only to be identified terrestrially a quarter of a century later. Perhaps the physicist will outstrip the astronomer in solving the problem of coronium, just as he has lately opened up entire new vistas of research and explained many difficulties in solar and stellar spectra through the investigation of ionization phenomena.

While the next quarter century may not see the full solution of all pending solar problems, it will bring at least the collection of much more extended series of observational data. With this will doubtless come a fuller understanding of such questions as the precise law of the solar rotation, the mechanism of solar convection, electrical concomitants of sunspot activity and of high and low level phenomena. Programs for the more precise determination of solar wavelengths are even now well under way. Whenever, in physics or in astronomy, our results enable us to advance one decimal place to the right, a host of interesting by-products, even complete new fields of research, are at once forced upon our older knowledge. I look for most interesting developments as immediate sequelae of a more exact knowledge of solar wavelengths.

Tennyson put an old truth in beautiful form when he wrote:

> . . . . Yet all experience is an arch,
> Wherethrough gleams that untraveled world,
> Whose margin fades forever and forever as I move.

True for every intellectual advance, this seems doubly true as applied to present tendencies in what may be termed galactic or cosmological astronomy. The scale of our accessible universe has been steadily expanding. Referring to the details of our own Milky Way, we are now using units of a thousand light-years with the ease of familiarity. Similar in form to our own galaxy of stars, many of us believe that the spirals are likewise galaxies of stars, other Milky Ways. Like our own, their diameter is to be measured by tens of thousands of light-years; like our own again, each may contain a billion or more suns; their distances from this local Milky Way of our own are of the order of a million to a hundred million light-years. Astronomy alone of all the sciences can study, in the present tense, manifestations of energy coeval with our remoter geological epochs.

We appear to have, in a vast and all-embracing continuity, a super-system of Milky Ways, and that Milky Way of which our sun and his system forms perhaps but one billionth part is itself but a moiety in the larger scheme. It seems difficult to predict or even to imagine any considerable extension of this mighty concept.

We may hope, however, that the equinox of 1950.0 will see accumulated a vastly increased store of the observational material from which may conceivably be derived much more accurate data as to the scale, shape and structure of our own galaxy. The problem is so vast a one that we shall have scarcely made more than a beginning in the next twenty-five years. For we have in our galaxy a billion or more separate units available for study. Fortunately, there seem to be only four or five main genera of celestial objects, with fewer than one hundred species which we can distinguish at present. Most of our ethics, religion, philosophy and theory of civilization is based upon the average man. In astronomy at present some of our conclusions are based upon correlations made from quite limited numbers of special types of celestial objects. In some cases such methods are the only ones yet available. But it is perhaps reasonable to assume that in the coming astronomical cycle we shall depend more and more upon the average star, that the average celestial citizen, so to speak, will come more and more into his own. For we may hope for relatively vast additions to our data bearing on the motions, distances and intrinsic luminosities of the stars within the next twenty-five years, and our most certain data will inevitably come from the average star. In this respect, I have seen no reason to change a published statement of my own, made four years ago:

> It would appear, also, that galactic dimensions deduced from correlations between large numbers of what we may term average stars must take precedence over values found from small numbers of exceptional objects, and that, where deductions disagree, we have a right to demand that a theory of galactic dimensions based upon the exceptional object or class shall not fail to give an adequate explanation of the usual object or class.

Mutatis mutandis, I feel that the day may come when, for that portion of the greater universe beyond our Milky Way which is now accessible with our largest telescopes, our most reliable data will be based upon what we may term the "average galaxy."

In our collection of the data bearing upon the characteristics of the average star there are still great gaps in the skies of the southern hemisphere. It is now nearly a century since Herschel journeyed to the Cape to continue celestial surveys by the knowledge then available. Later, Harvard continued and extended the great work it had done in the northern hemisphere by establishing its station at Arequipa, in Peru. The beginning of our last semi-centennial equinox period was marked by Campbell's extension of his great radial velocity survey to the southern stars, from the Lick branch station at Santiago, Chile, and soon after came the work of the station of the Carnegie Department of Meridian Astronomy at San Luis, Argentina. A solar physics observatory has just been founded at Canberra, Australia. In this mid-year 1925 Schlesinger and Hussey will go to South Africa, the former for the determination of the distances of southern stars, and the latter to extend to that portion of the celestial sphere his search for double stars. There is still a large gap to be filled in the photography with large reflectors of those nebulae inaccessible from our northern observatories, though a good beginning has been made in this field by Perrine at Cordoba.

At a distance of five hundred light-years the probable error of our best direct parallax determinations is about equal to the distance we are trying to determine. There are still occasional disquieting differences between such direct determinations and the distances determined from the star's spectral type by the ingenious method of Adams. For the comparison and perfecting of the two methods, there is still a large field of work for the direct photographic determination of stellar distances; we look forward to a continuation of our photographic parallax work at Allegheny Observatory for at least ten years, with the probabilities strongly in favor of its continuance until 1950. But certainly before the equinox of 1950.0 we shall be able to determine by the spectroscopic method the average distance of large numbers of faint stars of all spectral types with all desirable certainty and at distances of thousands of light-years. Then, with stellar distances no longer dependent upon the base-line between the earth and the sun, we may perhaps hope for the relegation to limbo of the monstrous and unnecessary hybrid word "parsec."

The astronomy of pure position needs a very generous time element, and the twenty-five year interval between this date and the equinox of 1950.0 is doubtless too short for great additions to the solutions of its various problems. But the work of Albany and San Luis will shortly add very materially to our stock of proper motion data. We may confidently hope, too, that we shall see at last, before 1950, the completion of the great Carte du Ciel, so that we can begin it all over again, and by repeating this tremendous program eventually secure an enormous mass of motion data.

I must relegate to some leader in physics any prophecies with regard to the status of the theory of relativity at the equinox of 1950.0; this in spite of the fact that the three so-called proofs of the theory have been borrowed from the astronomer. Though not accepting the theory as either necessary or inevitable, I may perhaps be permitted to outline what seems to me to be the duty thereto of the astronomer in the quarter century which is before us. The non-Euclidean geometries of Bolyai, Lobatchevski or Riemann are beautiful mathematical structures; they are internally self-consistent and logical. Provided only we assume a mighty enough "curvature," any of these systems can be made to "fit" our physical universe. It has been suggested, though doubtless not very seriously, that the reality and the inevitableness of such non-Euclidean systems could be proved if we could show that the sun of the angles of the triangle formed between our sun and two very distant stars is very slightly different from two right angles. Like the systems named, the theory of relativity is beautiful, internally self-consistent and logical. It would be as futile to argue against it by deducing peculiar consequences or apparent paradoxes in it as it would be to argue against non-Euclidean geometry. Many relativity frameworks are doubtless possible, and any one of these may conceivably be inevitable and necessary, though, following Poincaré, we shall generally choose that system which is simplest. Science, and some would say even government, ethics and religion, is pragmatic. There is but one test of a system; does the system "fit," and can the "fit" be explained only by the system and no other? The line of action thus laid down for us by the theory of relativity is accordingly very simple and direct. We must test the three proofs adduced from astronomical evidence in every possible way. Our criteria will be, first, is the evidence valid, and, secondly, is it impossible to explain it in some apparently simpler way by what we term classical mechanics.

As a by-product of the Allegheny solar wavelength program, Burns and Meggers, by methods which can detect a shift ten times smaller than that predicted by the Einstein theory, find that the shift of the solar lines to the red is far different from the simple and uniform shift postulated by the theory of relativity. They find that amount of shift to the red is a function of the intensity of the solar spectrum line, the finest and sharpest lines being shifted not at all. It seems impossible to find support for the theory of relativity in these results. Others are attacking the same problem, and doubtless programs of investigation are even now under way with regard to the two other proofs, namely, the anomalous motion of the perihelion of Mercury and the observed deflections in star positions photographed near the sun at total solar eclipses. The duty lies upon us of exhaustive investigation of these astronomical apparent evidences. The predicted shift of the spectrum lines to the red is apparently non-existent as a direct result of the theory of relativity; only if future work fails to find explanation for all three proofs shall we need to regard Einstein's theory as both necessary and inevitable. Our course of action seems thus clearly and definitely marked out for us; we must do the work leaving, perhaps, the final summing up and the definite decision as to the validity of this important theory to the astronomer and the physicist at the equinox of 1950.0.

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RESEARCH IN COLLEGES

WE will start with some self-evident propositions, which apply to the colleges.

(1) The *first* function of a college is to teach its students.

(2) Through its faculty, the college has responsibilities to the community, as in making books, public lectures, assisting civic organizations, industries and in religious work.

(3) The college also has a duty to extend the bounds of human knowledge.

These three overlap, for research should be of benefit to teaching and it *is* teaching when students are taught research, and simply the sight of research going on is educational. Moreover, through research we meet our most important obligation to the community.

Is research not better left to industry or to the universities? The object of business is to make money, while the object of our schools of learning is to obtain the truth, which we must have if we are to teach. Research is, therefore, a primary function of our colleges. The university and the college can not be distinguished on the basis of interest in the truth. Formerly the search for the truth was a function of the church, but it has handed it over to the colleges. Colleges past the pioneer stage can have no reason for evading their responsibility in finding out the truth. Since the churches have given over the search for truth, it is all the more important that the colleges, many of them denominational schools, should carry on in order to pay back the debt which they owe to the church from which they draw support; woe to the church which forever remains content with the truth which is now accepted and acceptable!

In no institution will all the faculty be engaged in productive research, still less in scientific research. Some will be primarily teachers and executives, geniuses in committee work; others will "build their homes by the side of the road" and inspire young men with their great personalities, without adding anything to the sum total of human knowledge; still others will find greatest chance for service in making