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MODERN EVIDENCES FOR DIFFERENTIAL MOVEMENT OF CERTAIN POINTS ON THE EARTH'S SURFACE[1]

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THE subject of our discussion is one of those problems that not only straddles the border line between departments of knowledge, but transgresses far into the adjacent territories of several sciences. I am reminded of an apt warning for which Bulwer-Lytton is responsible, not to fall "into the error of the would-be scholar—namely, quoting second-hand." Making no pose as a geologist, however, I must confess that what I may have to say in regard to physiographic examples of differential displacements in the earth's crust will have to be from such little knowledge as I have been able to borrow from you geologists, and I venture to proffer such second-hand information only on the ground that we have a board of experts among us to whom questionable points may be referred subsequently. My excuse for venturing upon this subject comes from a very vital interest that must concern every student of the earth.

Certain investigations with which I have been more or less engaged for the last fifteen years seem to lend promise that the problem of lateral shifts in the earth's crust may be helped toward solution by the continued observation of the latitude and longitude of certain presumably fixed points on the earth's surface. In that unique organization of students of the earth sciences, the American Geophysical Union, experts from many different, but interrelated fields commingle freely. It would appear that it was through such commingling some astronomers became contaminated by some geologists perhaps with a fond hope that a little contamination of astronomy with geology might be of mutual benefit. In the time at our disposal I hope I

[1] An address presented to the Geological Section of the New York Academy of Sciences on April 3, 1944.

may be able to treat a very interesting geophysical problem with sufficient decorum so as neither to offend, on the one hand, any astronomers present who may wish their observatories to stay put, nor on the other hand unduly encourage those geologists who would like to have whole continents moved.

The most dramatic evidences for translational displacements in the earth's crust are still to be found in the geologists' picture album, and I am indebted to our colleague, Professor Thom, for the loan of certain photographs that portray graphically displacements of many feet along fault lines familiar to every geologist. Not all these displacements are sudden occurrences such as accompany great seismic disturbances. The slow crawling of the crust in the neighborhood of the San Andreas fault is well known to the petroleum industries whose pipe lines have bent and buckled and often broken under the stresses and strains of the earth's crust. I have inquired of my geological friends for evidence in other parts of the country of equally striking displacements—so far without very encouraging results. It would appear that California, in keeping with her other superlatives, may continue to boast, as the state par excellence of Hollywood Stars, gigantic telescopes and of maximum terrestrial displacements.

Of course, looking back into geologic history, we have a panorama presented of a long series of depressions and upheavals of overthrusts and underthrusts which have caused the characteristic formations of the orogenic zones. Dr. Bucher has stressed the role of thrust-sheets in orogenic deformation and would cite as a specific example the Cumberland thrust-block lying across the tri-state corner of Kentucky, Virginia and Tennessee.[2] Here a slice of rock over 120 miles long, some 20 miles wide and a mile in thickness has been sheared off, closely parallel with the bedding and pushed along a distance of at least 7 miles. In addition, the whole slice has apparently been rotated, producing cross wrinkles.

The presence of great rift or shift zones in continental shield-areas was graphically depicted by Dr. A. W. Jollife at a recent symposium.[3] Quoting Dr. Jollife,

> Many faults in the Canadian Shield are known to follow essentially rectilinear vertical fractures that extend for distances up to several hundreds of miles. None of them is known to be marked by major vertical displacements whereas a number show strike-slip movements amounting to several miles. From this it is inferred that the fault movements were dominantly horizontal of the same general nature as those along the San Andreas Rift. Movements along many of the Shield faults were recurrent, the most recent being late Pre-Cambrian or even younger in age.

While geologists generally recognize that large differential horizontal displacements have taken place in the past, probably many still believe that such movements between crustal segments are negligible at the present time. Professor Thom,[4] in a recent report of his special committee on the Geophysical and Geological Study of Continents, stresses that "Evidence has now accumulated to a point where it is reasonably clear that an appreciable differential crustal shift is still going on in different parts of the world. And it therefore seems highly expedient that adequate data as to nature, magnitude and disposition of these relative movements should be obtained forthwith because of the critical bearing of these data upon over-all geodetic and cartographic programs and operations."

That a certain recurrence at more or less definite intervals, or a cyclical rhythm, in large crustal displacements has taken place in geologic time seems to be beyond contention. Accumulating evidence contiues to point out that such movements must still be taking place, but when large masses of terrain are involved, especially in regions unscarred by fault lines there is a difficult problem in determining either the direction or the amount of such movements as may be taking place currently. It is for this reason that the geologists look to the astronomers to see what evidence there is, if any, that displacements have taken place in very recent times. To interrelate more closely the determination of the crustal movements with the astronomical method of finding changes in position, we perhaps should remove ourselves sufficiently far from the earth itself to get the cosmic picture of the planet with which we are concerned and its immediate environment.

To use an analogy, those of us who inhabit the earth find ourselves aboard a ship on the high seas of space. Contrary to the opinions of the earlier astronomers, this spherical ship is far from fixed. At this very moment we are being carried eastwards with the earth's daily rotation at a velocity in this latitude of about 600 miles an hour. Meanwhile, the good Ship Earth is steaming ahead in its orbit about the sun with a velocity of 20 miles a second. The sun, however, which is the flagship of our fleet of planets, is steaming ahead on its course among the nearer stars at a velocity of some 12 miles a second, or 40,000 miles an hour. The sun in company with other suns comprising a still larger squadron is in turn sailing through space at a speed of several hundred miles a second towards the lights of other celestial ships seen at the

[2] W. H. Bucher, "Deformation of the Earth's Crust," p. 244. Princeton University Press. 1933. *Trans. Amer. Geophys. Union*, p. 697. 1934.

[3] A. W. Jollife, *Trans. Am. Geophys. Union*, pp. 699–707, 1942.

[4] W. T. Thom, Jr., *Trans. Am. Geophys. Union*, p. 304, 1943.

limit of the horizon of our greatest telescopes. Meanwhile as passengers aboard the earth, all quite unconsciously we are being tossed to and fro due to certain peculiar wobblings of the earth's axis, and now even the deck of the ship on which we stand—the crust of the earth—proves itself unstable as it trembles now and then due to internal catastrophes within the ship's hold.

As passengers aboard our frail celestial craft we are subject to all the cosmic forces known and unknown that play about in the mysterious ocean of space. Only as we examine our environment can we hope to come to anything like a complete knowledge of some of the disturbing forces that are operating upon the earth. It is to astronomers, as the navigators upon the bridge of this strange ship, that our fellow passengers look to tell the ship's position, define her course, and speed. Only by observations of those lights exterior to the earth—the distant stars—can we hope to evaluate not only the motions of the earth, but movements that may be taking place in the earth's crust. While in the last analysis, we can never hope to define the absolute motion of ourselves among a vast system of moving stars, we can take courage at the degree of precision which may be attained from astronomical observations. For those not intimately acquainted with the applications of practical astronomy, it comes perhaps as something of a surprise when we consider that it is possible from looking at the stars to determine one's position on the surface of the earth with an uncertainty of but a very few feet. More specifically, from a series of careful observations we can determine where we are north and south—latitude—within one foot or two; and in the east and west direction—longitude—within an uncertainty of perhaps four or five times that amount.

The problem of the determination of latitude is fundamentally very simple since an observer is as far north of the earth's equator as his zenith appears from the celestial equator, the zenith being determined by the direction of the plumb line at a given point. One needs only to attach a telescope in a sufficiently elegant manner to a level bubble, and the zenith can be located among the stars whose positions have been catalogued with respect to the celestial equator. Unfortunately, however, the positions of the stars can be determined only if the latitude of the observatory is known so that it is only through a series of successive approximations of a large number of cooperating observatories that the positions of the stars can be known so that the latitude of any station can be absolutely determined.

Variations in Latitude

For finding small variations in latitude, at a single station the true value of the star positions is not necessary, provided the star has the courtesy to remain stationary during any series of latitude observations in which we are interested. From a long series of observations made at a selected number of stations operated through international cooperation along the 39th parallel it has been possible to evaluate with great accuracy small variations in latitude. It has been determined that the pole of rotation of the earth is constantly shifting within the earth itself. This wandering of the pole first announced by Chandler, and later by Kustner of Berlin, is of a somewhat cyclical nature, having a period of approximately 428 days with a superimposed annual component. The migration of the earth's pole of rotation about the pole of its figure has never yet been observed to exceed a radius of 30 feet. An inter-comparison, however, from time to time of the latitudes of the several stations involved, namely, Gaithersburg, Carloforte, Chardjui and Mizusawa, have revealed certain discrepancies that may well have indicated crustal displacements. In the reduction of the last International Latitude Survey, Kimura[5] bluntly stated that with the position of the pole determined from all the other latitude stations excluding Mizusawa, the latter station could be considered to have moved southwards by about three meters from the position occupied ten years earlier. On the other hand, certain geodesists are loath to accept such as evidence, preferring rather the alternative interpretation that the discrepancy lies in errors in the adopted positions of the stars used in the reductions of the observations. With a certain daring impishness, however, I venture to remark in paraphrase, "The trouble, dear Brutus, may not be with our stars, but with ourselves, that we are moving."

It should be emphasized that in determining latitude we are concerned with the precise location of the zenith among the stars. Anything that influences the direction of the plumb line, peculiar to the observatory station, will show up as a variation in latitude. A shore station, subject to the loading of the ocean tides, will have its plumb line periodically deflected in the course of a lunar day, by the gravitational attraction of the rising tide of water off shore. The tilting of the land which results from the hydrostatic pressure will be readily manifested on a tilt-meter, but this tilt will not displace the direction of the plumb line or cause a variation in latitude, unless there is a lateral shift along the equipotential surface. The distortion of the plumb line due to the attraction of a mass of ocean water off shore, has been worked out and can not much exceed one one-hundredth of a second of arc. A tilt of the coast line, due to this loading, may amount to a considerable fraction of a second of arc for some distance back of the shore line itself.

[5] H. Kimura, *Proc. Imperial Acad. Tokyo*, 10 No. 10, 1935.

The moon, of course, causes a calculable tide in the solid earth as well as in the oceans. The celebrated Michelson-Gale experiments carried out on the grounds of the Yerkes Observatory in 1913 to 1915 made possible the observation of well-defined earth tides through a tilt-meter consisting of a horizontal pipe 500 feet long half filled with water. The microscopic tides caused in this pipe were measured by an elaborate optical method, and revealed amplitudes in the waves less than those calculable on the assumption of an unyielding earth. The discrepancies between the observed and the calculated values of the tides made possible the evaluation of the amount of yield in the solid earth. From the results it was found that the rigidity of the earth as a whole was perhaps a little greater than that of steel.

It should be pointed out in this connection that such a calculation must of necessity be made for the earth as a whole as a homogeneous body. Were there a different yield in the crust than in the earth's interior there would be no way of determining it by this method. Again it should be remembered that the observations revealed tilts only. Were there an actual rhythmic translation of the crust taking place as the tidal wave passed the instrument, this could not have been detected by the water pipe. On the other hand, any horizontal displacement or movement of the superficial crust along the equipotential surface under the influence of the tidal force would cause the plumb line to shift its position and a variation of latitude would be detected by the zenith telescope.

It was in the interest of trying to discover whether or not such tangential displacements as might take place could be detected by astronomical observation that I spent some years in investigating latitude observations for any variation that might appear to correlate with the moon's position. The results of the investigations indicated what appeared to be a definite lunar correlation in the Gaithersburg series made with a photographic Zenith tube in 1913–1914.[6] Further studies revealed similar correlations for the observation made at Ukiah. The amplitude of the variations found were of the order of several hundredths of a second of arc, corresponding to lateral shifts from two to five feet in the North and South direction.

These results were considerably larger than those that would be anticipated on the assumption of an earth tide calculated theoretically for the earth as a whole. Others who have made a rigid harmonic analysis of latitude observations for a lunar tide found results consistent with a theoretically calculated earth tide. It should be remarked, however, that in my study the form of the curve of latitude with the altitude, or time of lunar day, has by no means repeated itself completely in successive years. Patterns differ both in phase angle and in amplitude. Such results would not be inconsistent with tidal forces encountering resistance to deformation. Such an effect could be completely masked when several years of observation are subjected to a rigid analysis that presupposes symmetric harmonics.

It is of particular geophysical interest to note that the stations conveniently selected by the International Union for determining latitudes happened with one or two exceptions to lie in orogenic zones. The stations originally selected were Gaithersburg, Maryland; Cincinnati, Ohio; Ukiah, California; Mizusawa, Japan; Chardjui in the Himalayas; and Carloforte off Sardinia.

Observations at the Cincinnati observatory were early abandoned because of discrepancies, and large probable errors in the results. The Gaithersburg observatory was next abandoned for a time, but after a lapse of several years has again fortunately been reoccupied. The location of the other stations are, without exception, in extraordinarily interesting sections geologically, and perhaps could not have been better placed if the astronomers had been hunting for discrepancies that could be caused locally through crustal displacements. The astronomers, on the other hand, are naturally reluctant to attribute latitude discrepancies among the several stations in question to differential displacements. The object of the latitude project was patently to observe the motion of the earth's pole of rotation, rather than to try to discover crustal movements in the neighborhood of the telescope, which they presumably believed fixed on masonry piers where they were intended to stay put.

One further comment on this problem of latitude before discussing the east-and-west coordinate of longitude. Some twenty years ago Dr. Lambert, of the Coast Survey, presented an exhaustive discussion of the latitude observations at Ukiah, California,[7] in which it was shown that from 1900 to 1915 the latitude of Ukiah had more or less steadily increased some $0''.15$ during the decade and a half. For some reason it seems easier for astronomers to allow the imaginary earth's pole to slip within the crust than to assume that the actual territory surrounding Ukiah had, during this interval, inconsiderately slipped north 15 feet; so the interpretation of the result at first was that the North Pole of the earth had been slowly migrating affectionately toward North America. This did not appear plausible, so it became more or less the consensus of opinion that the trouble after all was with the stars, that they were drifting. By arbitrarily cor-

[6] H. T. Stetson, *Nature*, 123: 127, 1929; SCIENCE, 69: 17, 1929.

[7] W. D. Lambert, *U. S. Coast and Geodetic Survey* Special publication 80, 1922.

recting the declination of the stars used for observation, one could make the earth's North Pole behave more seemly, and thus not unduly excite the Californians over an unwarranted skid toward the North Pole. That, at least, placed the chief worry in the hands of the astronomers, and relieved the geologists and real estate agents of any cause for alarm. Who, other than the astronomers, care especially whether the stars are drifting or not? We can keep that secret within the family. However, more seriously, even some astronomers in off moments have geological interests, and it is coming to be recognized that while it is difficult to prove, or disprove, some translational movements in the region of the latitude stations, some of us are advisedly considering other methods of observation that do not presuppose a knowledge of the absolute declination of stars, that in turn must of necessity be found only from assuming values for the latitudes of the observatories from which the observations are made. The only apparent method of accomplishing this is the observation of stars above and below the pole. This can be well done in high latitudes. Unfortunately, on account of atmospheric refraction, one can not expect the degree of precision in such a method as is attainable by the adopted Talcott method of zenith observations.

The possibility of comparing latitudes of certain well-established stations after an interval of 70 or 80 years interested Dr. Frank Schlesinger, late director of the Yale Observatory, and I quote from a letter written by him to the director of the Coast and Geodetic Survey on May 5, 1930:

> As you are doubtless aware, the problem of variations of latitude has recently taken on a new aspect of interest, owing to the possibility of a secular change, as well as periodic changes, in the position of the pole. This is one reason why the resumption of observations at Gaithersburg has become so important. An independent way in which considerable light can be thrown upon this question occurred to me several years ago, and I have discussed it with a number of my colleagues, including Doctor Bowie, chief of your division of geodesy. The plan is to reoccupy for one or two nights each, a number of the stations at which observations for latitude were made 20 or more years ago by the Talcott method. The same stars should be observed now as were used then. From the results obtained in this way we may reasonably expect light to be thrown upon the following questions: (1) The actuality of a secular motion of the pole; (2) the possibility of slow continental, or at least general drift; (3) a better determination of the systematic errors in our adopted systems of proper motions of the fixed stars.

As a result of this letter three stations in the United States whose latitudes had been well defined were selected for reoccupation—Mt. Pleasant in Maine, Mt. Tom in Massachusetts and a station in Des Moines, Iowa. The latitude of these stations had been accurately determined in 1851, 1862 and 1869 respectively. In all cases the Talcott method of the zenith telescope had been used. The results of this reoccupation of these stations showed that, using the same stars and the best available star positions from the well-known Boss catalogue, Mt. Tom in Massachusetts had decreased in latitude by $0''.99$; Mt. Pleasant in Maine showed an increase in latitude by $0''.29$; Des Moines, Iowa, showed a decrease in latitude by $1''.49$.[8]

Translated into linear distances the evidence was that in the last three quarters of a century Mt. Tom had slipped about 100 feet south, while Mt. Pleasant in Maine had gone north about 30 feet, and Des Moines, Iowa, indicated a southernly drift of 150 feet with respect to the earlier position.

One of the few major observatories now remaining that has continued positional astronomy is the Lick Observatory, Mt. Hamilton, California. An analysis of the latitude of this observatory based on meridian circle observations from 1893 to 1918 was made some years ago by Professor Lawson and published in the bulletin of the Department of Geology of the University of California.[9] Professor Lawson's discussion exhibited evidence of an increase in latitude of $0''.40$ from 1893 to 1903. In 1903 a conspicuous drop of $0''.63$ in the value of the latitude occurred; an interval coincident with the earthquake of August 2, 1903. The somewhat more scattered observations from 1903 to 1918 showed a subsequent increase in latitude so that by 1918 the position of the observatory was essentially that at the beginning of the series. Dr. Campbell, then director of the Lick Observatory, while considering that small changes in latitude may have taken place at about the time of the earthquake, believed that a certain amount of the change pointed out by Professor Lawson could be accounted for by possible accidental errors. The otherwise apparent steady drift of the Lick Observatory northwards is quite consistent with, though somewhat larger than, the increase in the latitude of Ukiah, and I might also add Gaithersburg, which Dr. Lambert of the Coast Survey has discussed at length. During the earthquake of 1906 when dislocations of six meters were reported no definite proof of a change in the position of Mt. Hamilton was found from Lick Observatory observations. It is surprising that from triangulations made before and after 1903 the report was made that no certain evidence appeared for a shift in the position of the Lick Observatory.

VARIATION IN LONGITUDE

Fortunately, when we come to the consideration of detecting movements of the earth's crust in an east-

[8] F. W. Darling, *U. S. Coast and Geodetic Survey Special Publication 173.*

[9] Lawson, *Univ. of Cal. Pub. Bull. of Dept. of Geology,* 12: No. 7, 1921.

and-west direction by astronomical observations, we do not have to be concerned with the absolute positions of the stars. The difference in longitude between two places is nothing but the equivalent of the amount of time that elapses between the moment when a star is seen on the meridian of an observer at one station and the moment when it is seen on the meridian at a second station, west of the first. If an observer at the Naval Observatory in Washington learns by wireless the exact instant when a star has been observed as it crosses the Meridian of Greenwich, and finds that 5 hours 8 minutes and 15.78 seconds have elapsed before it crosses the Meridian of Washington he knows that the longitude west of Greenwich is 5 hours 8 minutes 15.78 seconds, or an equivalent in arc of $77° \ 0' \ 3''.7$. If from time to time this interval should vary in the last decimal place, he might suspect, if he is mobile minded, that the longitude between Greenwich and Washington varied.

While in the problem of differences of longitude we have delightfully dismissed the problem of accurate knowledge of star positions, we have introduced yet another pernicious uncertainty. This uncertainty is the degree of accuracy with which we can call up London and find the exact moment of the Meridian passage of any given star. Of course, in practice many stars are observed on both sides of the Atlantic, and the best possible values for the corrections for the Greenwich Observatory clocks are made that such observations allow. The exact time is then broadcast at stated intervals by radio which can be picked up by our Washington observatory and compared with the Washington time of transit of the same stars. Meanwhile, however, we have had to rely upon some mechanism that would keep accurate time during the five-hour interval from the moment when the stars are observed in Greenwich, and the time when the same stars can be observed in Washington. Fortunately, we now have rather good timepieces which can be trusted with an astonishing degree of reliability. In the old days of longitude observations time signals had to be passed through the Atlantic cable and large uncertainties in the reception of time signals from England were inherent in the manner of the electric performance of the cable. Time signals can now be exchanged by radio waves which, if one does not know too much about them, may be assumed to travel with the velocity of light. Allowing for all knowable uncertainties, one may state without exaggeration that time can be determined on two sides of the Atlantic and signals exchanged with a probable error of only about ± 0.007 seconds.

A few years ago Mr. Alfred Loomis and myself became interested in what appeared to be unaccountable variations in time observed on the two sides of the Atlantic; variations many times larger than any errors of observation or transmission by radio would seem to allow. Such variations furthermore showed a more or less rhythmic oscillation over periods of days and weeks. At that time we felt that there was fairly good evidence that the position of the moon was definitely related to small shifts in latitude. We therefore undertook to see whether or not the small differences in determination of time on the two sides of the Atlantic, that might represent small variations in longitude, could also be attributable to the moon. The presentation of the first paper on this subject to the American Astronomical Society[10] at a joint meeting with the American Association for the Advancement of Science at Atlantic City in 1932 so startled *The New York Times* that their front page the next morning startled the authors, the astronomers and the rest of the world with the idea that the two English-speaking peoples on the two sides of the Atlantic were sometimes nearer to each other by 60 feet, and sometimes further away from each other by 60 feet, dependent upon what the moon had to say about it. Additional investigations by Mr. Loomis and myself, and later published by the Royal Astronomical Society[11] seemed to collaborate our earlier findings. The variation in time which appeared to correlate with the hour angle of the moon, or the hour of the lunar day, represented recurrent discrepancies of about five hundredths of a second of time. If interpreted as a lineal change of distance in the east and west direction this would correspond to a range of 64 feet. While we were cautious in not forcing any geological interpretation as an explanation for the phenomenon, we could not but speculate as to what hypothesis could be sanely found to explain a to-and-fro movement of the earth's crust, were such really to exist. Much water has gone over the dam since this was first published. As to this hypothesis there have been, geologically speaking, periods of submergence and emergence, of inundation and denudation. I think I can very briefly summarize what has happened since.

I regret that it is not possible here, on account of time, to present more details of the evidence which Mr. Loomis and I presented for a correlation between the position of the moon and the suspected variation of longitude. I should emphasize that we called attention to the fact that our table showed a marked seasonal variation about which it was remarked we should have more to say later in another paper. We did, however, publish date by date the reduction of the

[10] A. L. Loomis and H. T. Stetson, *Publications American Astronomical Society*, 7: 177, 1932.

[11] A. L. Loomis and H. T. Stetson, *Monthly Notices Royal Astronomical Society*, 93: 444, 1933; "Further Investigations of an Apparent Lunar Effect in Time Determinations," 95: 454, 1935, MN-RAS.

time signal observations upon which our findings were based, in the hope that others challenged by the problem presented might be interested to make further contributions.

We did not have long to wait. A Japanese investigator Kawasaki,[12] utilizing our tabulated data, soon took the floor and showed that the reversal in phase of our curve of longitude variations which we had attributed to the change in the declination of the moon from the north to the south side of the equator could be accounted for by an annual term of unknown origin, the annual term being the seasonal variation in the values of longitude variation to which we had called attention.

We now know that Kawasaki was wholly right in showing that under the conditions peculiar to the times when the star observations were made such an annual term could produce the effect observed so far as the reversal of phase in the resulting curves was concerned. He made no attempt, however, to account for the occurrence of the annual term, or the seasonal variation, or to deny that such an effect could be related to the moon.

(*To be concluded*)

OBITUARY

WILLIAM TITUS HORNE

WILLIAM TITUS HORNE, professor of plant pathology in the University of California, died on April 12, 1944, after an illness of a few days. He was born near Kankakee, Ill., on November 8, 1876. Professor Horne received his early education in the public schools of Bennett and Lincoln, Nebr., and then attended the University of Nebraska from which he obtained a bachelor of science degree in 1898. After serving as instructor in the Nebraska Wesleyan University and University of Nebraska Farm School, he was employed in a fish hatchery at Karluk, Alaska, in 1901–1902. He took graduate study at Columbia University as fellow in botany in 1903–1904. At the Cuban Agricultural Experiment Station from 1904 to 1909 he served as assistant and then chief of the department of plant pathology. He came to the University of California at Berkeley as assistant professor of plant pathology in 1909 and was acting head of the division of plant pathology in 1919–1920. He transferred his activities to the Citrus Experiment Station in 1928, where he became associate professor and then professor of plant pathology in 1939. Here he had a long and useful service, especially in the field of avocado and subtropical diseases.

While at Berkeley, many students felt his kindly influence and careful instruction, and numbers of them are now active and prominent in scientific research, teaching and commercial life.

At Riverside he has made notable scientific contributions, especially to the better understanding of avocado disease problems. He made a host of friends by his friendly attitude and gentle, kindly life, not only among his immediate associates but among the growers as well. An excellent example of the appreciation of his work for the avocado industry is a quotation from a scroll presented to him on May 3, 1935:

> The Avocado Department of the Los Angeles County Farm Bureau takes this means of expressing to William Titus Horne, Associate Professor of Plant Pathology at the University of California, its deep appreciation of the years of untiring and unselfish work devoted by him to the problems of the avocado industry. Much of this work has been beyond the requirements of his position. His modest, unassuming manner and deep human interest in the problems of the growers has endeared him to all of us.

Later, in 1938, he was asked by the California Avocado Association to present the medals at Atlixco, Mexico, in recognition of the sending of the Fuerte variety to California. In the same year his colleagues in plant pathology made him president of the Pacific Division of the American Pathological Society. He was a member of the American Association for the Advancement of Science, the American Phytopathological Society, the Mycological Society of America, the California Botanical Society, the Torrey Botanical Club, Sigma Xi and Alpha Zeta.

One of his most important publications since coming to Riverside was his 1934 bulletin on Avocado Diseases. He had ready at the time of his death a completed manuscript on "The Diseases of the Guava," which is being edited for publication by the University of California.

In 1906 he married Mary Tracy Earle, sister of the late Professor F. S. Earle, at Santiago de las Vegas, Cuba. Their beautiful home and garden at Riverside, from which friends received innumerable gifts of flowers and fruits, was an expression of their kindly life and endearing hospitality.

H. S. FAWCETT
L. J. KLOTZ
P. A. MILLER
AGRICULTURAL EXPERIMENT STATION,
RIVERSIDE, CALIF.

RECENT DEATHS

DR. DAVID EUGENE SMITH, professor emeritus of mathematics at Teachers College, Columbia University, died on July 29 at the age of eighty-four years.

DR. JOSEPH CHRISTIE WHITNEY FRAZER, research

[12] S. Kawasaki, *Monthly Notices Royal Astron. Soc.*, 96: 818, 1936.