and of nearly 87 per cent. of the original carotenoids, *i.e.,* 97 per cent. of the xanthophylls and about 33 per cent. of the carotenes.

Analyses of a core of ocean mud revealed lipids in concentrations of 0.06 per cent., 0.05 per cent. and 0.04 per cent. by dry weight at the 6-inch, 74-inch and 80-inch levels (estimated to be about 600, 7,000 and 8,000 years in respective ages). All three sections yielded chlorophyll degradation products and carotenoids; but an intermediate section taken from the 44-inch level of the same core (4,000 years) yielded similar greenish cleavage products of chlorophyll but no carotenoids. Instead it contained a pigment with blue-green fluorescence, identical in chromatographic behavior and spectral absorption maxima with a pigment recovered from a sample of California crude petroleum,[5] and similar to a porphyrin isolated by Treibs[3] from a California asphalt. This section of the core yielded tenfold the concentration of lipid-soluble substances found at the other levels.

The general preponderance of carotenes over xanthophylls in muds of the ocean floor may arise from several factors: (1) Xanthophylls are more readily degraded than are carotenes by atmospheric or dissolved oxygen;[25] this is especially true of the prominent marine xanthophyll, fucoxanthin and its isomers, which are also especially susceptible to alteration by heat or alkalies.[23] While the foregoing factors would find less application at the bottom of the sea, two other general processes might well contribute to the situation, both there and in overlying waters, namely: (2) The majority of marine animals so far investigated store chiefly xanthophylls rather than carotenes in their tissues; their storing and partial oxidation of polyene alcohols, with fecal rejection of carotenes could bring about a gradual preponderance of the latter class of carotenoid in bottom sediments. Finally (3) there are indications that some microorganisms living in marine muds may contribute polyene hydrocarbons of their own synthesis, and that other such flora may be capable of reducing xanthophylls to compounds of lower oxygen content, or perhaps even to carotenes.

[25] L. Zechmeister, "Carotinoide," J. Springer, Berlin, 1934.

MODERN EVIDENCES FOR DIFFERENTIAL MOVEMENT OF CERTAIN POINTS ON THE EARTH'S SURFACE. II

By Dr. HARLAN T. STETSON

COSMIC TERRESTRIAL RESEARCH LABORATORY, NEEDHAM, MASS.

Shortly after the publication of Kawasaki's paper, Professor Schlesinger, after correspondence with me, undertook to make the necessary calculations to see if the known variation in the position of the pole that caused the annual term in the variation of latitude could not also introduce an appreciable variation in longitude for geometrical reasons. Professor Schlesinger's deductions were published in 1937 in the *Monthly Notices* of the Royal Astronomical Society,[13] and did indicate that a large part of the variation in longitude that we had attributed to changing positions of the moon could and should be accounted for on the basis of this annual term. Since that time I have corrected all our previously published data by applying this annual term. Though not yet published, the resultant curve still persists in showing a maximum and minimum change in longitude across the Atlantic with the hour angle of the moon. This shows up in the exchange of time signals between the Naval Observatory and both Greenwich and Paris, but not across the English Channel. The magnitude of the variation with the lunar day, however, is reduced in amplitude by the correction to about one half the former value assigned by Mr. Loomis and myself. If the remaining variations therefore are translated into linear values there remains a variation of about 32 feet to be accounted for in the distance across the Atlantic that correlates with the position of the moon.

To add zest to the controversy a publication was shortly forthcoming from the Jesuit Observatory at Zi-ka-wei, China, showing that a study of the intercomparison of time signals between Shanghai and the European stations at Nauan and Bordeaux, revealed periodic variations in longitude between Berlin and Zi-ka-wei, dependent upon the moon's position. The explanation advanced was an earth tide causing a linear variation in distance of 60 feet between central Europe and the east coast of China. The amount of the variation given was several times larger than any probable error of the observations.

Unfortunately after June, 1934, shortly after the publication of our original papers, the daily intercomparisons of the 17.6 Kc. transatlantic time signals between our own Naval Observatory and the observa-

[13] F. Schlesinger, "Note on Annual Change in Longitude." MNRS 97: 696, 1937, 98: 203, 1938.

tory at Greenwich ceased, due to change in the transmitting station at Annapolis. We were obliged, therefore, to try to continue this study through time signals on short wave length, or high frequencies. The short wave transmission, however, across the Atlantic is so unreliable, particularly during the summer, that the project had to be abandoned until the 17.6 kilocycle frequency could be re-established. Through the cooperation of the American Geophysical Union and Captain Hellweg, superintendent of the Naval Observatory, the time signals were resumed on the 17.6 kilocycle frequency in 1939. However, hardly had the interchange of time signals on this reliable wavelength been re-established before the European War broke and we have since been deprived again of this source of data.

Meanwhile an exhaustive study was begun at the Naval Observatory to see if any lunar effect in time observations could be detected from observations made at Washington with the photographic Zenith tube. The result of this series of investigations seemed to show that observations from March, 1934, to February, 1939, showed a small correlation with the moon of only such magnitude as to be consistent with an earth tide calculable on the basis of the accepted constants for the earth's rigidity. If apparent variations in longitude from time comparisons on two sides of the Atlantic were due to earth tide deformation, one would expect that results at either of the two observatories concerned would show a certain dependence of meridian observations upon the hour angle of the moon. However, it should be stated that between the date of publication of the paper by Mr. Loomis and myself and the date at which the observations at the Naval Observatory were studied, our intercomparison of the time signals showed a decadence in the amplitude of the lunar effect.

Since the complete abandonment of the 17.6 kilocycle transmission, no further study could be made. Meanwhile other computers at the Naval Observatory have investigated latitude observations made at Washington, and have found no variations with the moon other than what could be accounted for on the accepted theoretical earth-tide basis. I venture, however, to continue to stress that in any rigid harmonic analysis of a long series of observations, only those terms will persist which show a uniform rhythmic oscillation throughout the whole period of time in which the data were collected. Such a method of analysis does not allow for the possibility of an intermittent lagging effect such as might occur were deformation irregularly resisted, as might well occur in creep phenomena. The last chapter in the discussion of this problem does not yet appear to have been written.

In arriving at our earlier published conclusions, Mr. Loomis and I pointed out that in interpreting the variation in time signals exchanged across the Atlantic we had tacitly to assume that the speed of the radio waves carrying the signal was the same in passing from west to east as in passing in the opposite direction; and that the velocity of the wave does not substantially change during the interval covering each day's exchange of signals, an assumption which should be reasonable, but which can not indefinitely pass unchallenged. Experiments for the verification of such an assumption have already been planned,[14] and should be carried out after the termination of the war.

May I now call your attention to such evidence as exists for two of the most astonishingly large changes in geographical coordinates that have ever been published from the study of the intercomparison of astronomical determinations. One of these concerns Greenland; the other, South America.

In 1932 a Norwegian, Hans Jelstrup,[15] conducted an expedition in the summer of that year to Sabine Island, in the Melville Bay region, off the west coast of Greenland. The purpose of his expedition was to re-occupy a station whose latitude and longitude had been determined by Börgen and Copeland in 1867 and 1870. Jelstrup identified the identical pier used by the observers in 1870. The results of the new determination gave the position of this pier as 615 meters west of the position given by Börgen and Copeland in the 1870 observations. The comparison of the longitude determined by Jelstrup with that given by Börgen and Copeland in 1870 has been questioned on the grounds of inaccuracy of the earlier observations; and Jelstrup's conclusion that in 1932 the station was 615 meters west of the earlier position has been warmly challenged.

It is true that Börgen and Copeland did not have the advantage of the modern radio for time comparison, and did not even have the use of a cable for receiving Greenwich time. They were therefore obliged to depend upon lunar culminations and observations of occultations for determining Greenwich time. These methods are well recognized as quite unsatisfactory and lacking in precision compared with the telegraphic or radio method. It is of interest, however, to find just what degree of reliability could be placed upon those early determinations. For this reason I have taken the trouble to examine with considerable care the original detailed publications of the 1870 expedition.[16] Their probable error was about plus or minus

[14] H. T. Stetson, *Trans. of Amer. Geophys. Union*, 1940, p. 822.

[15] H. S. Jelstrup, "Determination Astronomique A Sabine-Øya Au Groenland Oriental. Skrifter Om Svalbard Og Ishavet." Nr. 58, 1933.

[16] K. Koldewey, "Die Zweite Deutsche Nordpolarfahrt in den Jarhen 1869–1870," Leipzig, 1874, Vol. 2: 710–724.

1.1 seconds for either method. The longitude determinations from the four-star occultations agree within 1.5 seconds of the longitude found from the sixteen lunar transits; as good an agreement as could be expected in the case of the methods used. Observations were also made by Green in 1870 to determine the corrections to the moon's position, thus minimizing uncertainty due to this possible source of error.

The uncertainty of Börgen and Copeland's observations of longitude expressed linearly amounts to about 80 meters, while the modern observations of Jelstrup in 1932 could represent an uncertainty of not more than 20 meters. Now if we allow for the worst possible combination of these estimated errors it would appear that the results in 1932 could be expected to agree with the 1870 observations within about 100 or at least 200 meters, whereas the position actually found by Jelstrup was indicated to be 615 meters west of the position determined from the earlier measurements. Consequently, it would appear that in the 62 years interval Sabine Island had moved west four or five hundred meters.

Critics of this interpretation should bear in mind that there is also evidence of movement from the comparison of the latitude determinations made in 1870, with those made in 1932. Jelstrup's value for the latitude was $3''.2$ less than that found by Börgen and Copeland in 1870, an amount which indicates that the pier as was occupied had moved 100 meters south of the 1870 position. Whatever doubt may be cast upon the interpretation of the longitude differences it should be emphasized that the latitude determinations were entirely independent of the exchange of time signals, and that the order of accuracy attained for latitude determinations in 1870 was probably nearly as good as in 1932. The uncertainty involved would appear to rest chiefly upon the question of the positions of the stars used. The conclusion of the Börgen-Copeland and Jelstrup expeditions would seem to indicate that Sabine Island had drifted south and west at least some seven hundred to eight hundred feet during the 60 years elapsed between the expeditions.

Now for the other startling incident. In 1931 a redetermination of the longitude between the Argentine observatory at Cordoba and the Royal Observatory at Greenwich was made and the results compared with the position of the observatory made under the direction of Dr. Benjamin A. Gould in 1871.[17] Every astronomer recognized Benjamin Gould as one of the most exacting and painstaking of observers and mathematicians. It would appear that he would have used every precaution to secure the highest attainable degree of precision possible in his time. Dr. Gould based his position upon two very careful longitude determinations carried out by the U. S. Naval Astronomical Expeditions. These expeditions derived independently the longitude of Buenos Aires from Greenwich and that of Valparaiso from Washington. One of these determinations was made by way of the Atlantic and one by way of the Pacific. The results of the two determinations showed a satisfactory agreement within 0.05 seconds or a linear uncertainty of some 16 meters.

In the determination of 1931 time signals were exchanged by radio in place of cable and the probable error from 35 nights' observations gave an uncertainty of the resulting longitude represented by a probable error of but $\pm$ 0.007 seconds. When, however, the 1931 result was compared with that of 1871 the two determinations differed by 1.11 seconds corresponding to a linear distance of 400 meters or 1,200 feet, an unthinkable difference to be charged to the account of observational errors.

Discussing the results, Astronomer Zimmer, of the Cordoba Observatory, tacitly assuming that the observatory could not have moved, commented on these results as follows: "The remarkable thing about it is that two independent determinations such as those made by Green and Davis, should be in complete accord, and yet be wrong by a whole second. We have to assume that those two determinations were in error by at least a round second." Mr. Zimmer found it impossible to imagine any continental drift to allow for the change that had taken place, especially so since he stated that if one were to imagine any such movement, "the direction was opposite to that predicted by Wegener." One perhaps should be cautious in evaluating such a statement lest it lead to a type of reasoning that may discourage discovery, by inhibiting the imagination on the one hand, and erroneously assuming on the other hand that if a lateral movement had taken place it would have to be in accord with some particular hypothesis. Perhaps it is a pernicious imagination that would suggest that if an orogenic movement had at one time been westward, there might be a reactionary period when a crustal shift could for the time being take place in the opposite direction. This is something for you geologists to ponder about.

In closing this discussion of longitude variations there remains one other observation to mention—the results of a comparison of longitude determinations between San Diego and Washington, made during the campaigns of 1926 and 1933, and published in 1939.[18] A comparison of the observations of longitude reduction over this seven-year interval taken at face value, showed that the longitude determination in 1933 was 0.045 seconds greater in 1933 than in 1926, an amount over six times the probable error of the observations.

[17] M. L. Zimmer, *Astr. Jour.*, 41: 115, 1931.

[18] C. B. Watts, P. Sollenberger and J. E. Willis, *Pub. U. S. Naval Observatory*, Second series 14: 254, 1938.

This amount expressed in linear dimensions would correspond to an increase in the distance between the east and west coast of 55 feet. This discrepancy seemed so much larger than errors in the observations could allow that the Naval Observatory astronomers re-examined the reductions of the 1926 observations and came to the conclusion that a small correction to the reading of the chronograph records by the earlier computers could be made so as to bring the results of the two series more nearly in accord. Even after such corrections were applied, it is found that in 1933 San Diego appears 37 feet further west than the 1926 observations indicated.

One should emphasize here that there has been a vast improvement in the technique of determining longitude in the last fifty years, largely through the advent of frequent time comparisons by radio. In place of being dependent upon cable lines one can receive at will in any field station, time signals broadcast with an accuracy undreamed of a century ago. Nevertheless with every advance in obtaining another decimal place, one is sure to encounter new problems to be solved. In this case we have no exception to the rule. In recent studies we have made on the apparent elapsed time in the propagation of radio signals between distant points, it has been found that it is no longer safe to assume that the velocity of these radio waves are constant and equal to that of the velocity of light, an assumption all too easily made by those who have not faced directly some of the problems of communication by way of the ionosphere.

In a paper published in the *Journal of Terrestrial Magnetism* in 1936[19] I showed that, in deriving the velocity of propagation of time signals employed in the longitude campaign of 1926, the apparent group velocity of the radio waves deduced on the assumption that the waves traveled the shortest possible path between stations, varied in speed all the way from values approximating the velocity of light, to values approximating but a little more than two thirds the velocity of light. Furthermore, there was indication that the speed appeared to depend more or less directly upon the intensity of the earth's magnetic field in the region over which the waves passed.

Even in the case of constant comparisons between two fixed stations such as Greenwich and Washington, it was found that over a period of six years the velocity of the radio time signals could vary appreciably, for reasons not readily explained. The inference of this would seem to be that in attempting to obtain a still higher degree of accuracy in determining longitude we are involved in a study of ionospheric conditions, upon which the peculiarities of radio transmission must depend. For a higher degree of precision in longitude determinations, it would appear desirable that the observing stations involved should be in relatively close proximity to wireless stations which transmit the signals.

VERTICAL DISPLACEMENTS

We may now dismiss completely the matter of astronomical determinations and call your attention for a moment to the possibility of measuring short periodical displacements of the earth's crust in the vertical direction. Thanks to the improvement in gravimetrical instruments within the last few years, it has been possible to measure variations in gravity with a degree of precision never before attained. While the modern gravimeter has been developed ostensibly for commercial purposes, one of these instruments placed for several months in a fixed location has revealed so unmistakably the rise and fall of the earth's crust in response to solar and lunar tides in the earth itself as to render the results unquestionable.

A few years ago I took the opportunity of visiting the Gulf Research Laboratory in Pittsburgh, where, under the direction of Drs. Foote and Eckhardt, a sensitive gravimeter set up on test in the basement of the laboratory for several months had shown consistently both the solar and lunar tides in the solid earth. The tidal curve reduced from the gravimeter observations, when compared with the calculated theoretical tide potentials, showed a definite phase lag of about 50 minutes between the time of the maximum tidal force and the time of the maximum response of the earth's crust.[20] While the experts in charge of this instrument felt that caution should be exercised in determining the amplitude of the curve on account of certain uncertainties in the calibration of the instrument, one could state with certain reservations that the range of movement of the earth's crust in the vertical direction during the lunar day represented a displacement of about two feet. Were there only this single incident, it is quite sufficient to emphasize the importance of the establishment of a number of such gravimeters in selected regions to determine not only such differential displacements, but to discover if possible regions where such displacements may exceed or fall short of the magnitude of the variations recorded at Pittsburgh.

As a possible cosmic cause for differential movements in the earth's crust, one can not forget the gravitational force exerted by the moon. It is always difficult to see how the very feeble stresses set up in the earth's crust by the lunar tidal attraction could have an appreciable effect in moving large continental masses. One perhaps should not, however, forget the

[19] H. T. Stetson, *Terr. Magn. and Atmos. Elec.*, 41: 287, 1936.

[20] P. D. Foote, SCIENCE, 82: Supp. P8, 1935.

possible resultant effect through the continued application of intermittent stresses repeatedly applied in a given direction. Were it possible to think of extended regions of mobile matter within the earth, then perhaps tides set up in this underlying mobile material could produce, through hydrostatic pressure, far-reaching effects in this connection.

A few years ago I undertook a reinvestigation of the frequencies of the occurrence of major seismic disturbances as possibly correlatable with the position of the moon.[21] While the results were not as conspicuously convincing as one might have wished, there was a definite indication that, at least so far as deep focus earthquakes are concerned, the curve of frequency deduced showed two maxima and two minima during the lunar day. Maximum values coincided with positions of the moon four hours and sixteen hours past the meridian. One might say that there are indications here of a trigger action, such that if through accumulated stresses large earthquakes are about to occur, there is a better chance of their occurrence during a time when the horizontal tidal component of the moon's gravitational force is at a maximum.

A few days ago I received a communication from that venerable and esteemed geologist, Professor William Hobbs of Michigan. Said Professor Hobbs to me, "If you have published material proof of the changes of latitude and longitude, could you send reprints, or else references to place of publication." By training and temperament I think any scientist is reluctant to use the word proof, and while the material I have presented may have fallen far short of what any mathematician could desire as proof, I hope I have fairly and without undue prejudice presented such evidences as exist to encourage a degree of open-mindedness among both astronomers and geologists on so fundamental a question as that of differential movements in the earth's crust now going on. If so, then I will have been successful at least to the extent of having presented material consistent with the subject assigned.

[21] H. T. Stetson, *Proceedings Am. Philosophical Society*, 78: 411, 1937.

OBITUARY

RONALD FRASER MACLENNAN

In these days, when full millions of men of pre-middle age must necessarily turn their potential physical and intellectual energies into the kinetic form necessary to winning the war, the death of a single member of that age-group may pass almost unnoticed. However, when such an individual was already a teacher and trainer of, and inspiration to, hundreds of students at the upper educational level, the loss reaches into a long and incalculable future.

For this reason the passing of Ronald Fraser MacLennan on May 27, 1944, of coronary thrombosis, at thirty-seven years of age, has more than commonplace significance. He took his A.B. at Oberlin College, in the town of his birth, in 1928, with honors in zoology; an A.M. in 1930 and the Ph.D. in 1932, both at the University of California (Berkeley). He was at once appointed instructor on the zoological staff of the State College of Washington, there advancing to the associate professor level; in 1940 he was called back to his alma mater. While his teaching was an outstanding activity and success, always carrying a heavy schedule, he was never lacking in eagerness for research; eighteen carefully wrought studies in the field of protozoology appeared during the twelve years following attainment of the doctorate. He was also one of "the biologists who, in our opinion, were the best men,—who could speak with authority, to write these chapters" (quoted from preface); MacLennan was thus chosen to present "Cytoplasmic Inclusions" in the large treatise on "Protozoa in Biological Research" (1941) edited by Calkins and Summers.

Oberlin has a new biological laboratory in the offing, and the confidence placed in his judgment is indicated by the action of the administration in releasing him from other duties for a half year while he cooperated with the architects. Further portrayal of the role he was filling can best be given in summary by quoting from the memorial minute adopted by the faculty on June 6:

> All of us had every reason to assume that his would be a long and brilliant career. Of this there was abundant promise. It showed itself in his already numerous and significant scientific publications, in the enthusiastic and affectionate relationships established alike with colleagues and with students, and in many active services for his church and for the village.
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> To those of us who were closest to him in his daily work, his ready and engaging smile, his unfailing wit and good humor, his quick assumption of his full share and more of any task however arduous, and his careful, critical, yet unassuming scholarship were a constant inspiration.

He is survived by his wife, Mrs. Marie Schulte MacLennan, and a son, Frederick; by his mother and two sisters. Memberships in scientific organizations included the American Association for the Advancement of Science, American Microscopical Society, American Society of Zoologists and the corporation of the Marine Biological Laboratory.

Robert A. Budington