fected, the American association dropped no meetings, but, on the contrary, the meetings were well attended. The demands of warfare were a stimulus to chemical and physical science.

Following the Columbus meeting of 1915, a special two-day meeting was held in Washington, in conjunction with the Pan-American Congress. This meeting is not listed with the series of annual meetings.

The delicate relation of the association to the technical societies and the difficult problem of meetings and of functions have been the subject of study by council and executive committee. Without hasty or radical action, but with patience and tolerance, the matter of the mutual relationship has been allowed to develop from year to year, and the present strength and influence of the association, as the general representative of science, and the success of the many societies in their special fields, appear to justify the conciliatory and laissez-faire policy.

The association claims as its field the whole of Pan-America. But it has never held a meeting south of New Orleans. In 1889, Professor Putnam proposed a meeting in Mexico (38: 481), and Brazil was favored by the council in 1913 (65: 464). In 1919, at Chicago, a committee was appointed "to cooperate with such organization as Mexican men of science may form." The Southwestern Division has carried the work to the Mexican border, and the El Paso meeting held a session across the boundary, in Juarez. It is hoped that the political conditions will soon admit of an organization in Mexico, and of association meetings in Mexico and Central America. It would be a happy event for science and for internationalism if a meeting could be arranged for some city in South America.

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(To be continued)

THE GROWTH OF LEGEND ABOUT SIR ISAAC NEWTON

(1) *The usual explanation of Newton's delay of about twenty years in announcing the law of gravitation* involves what appears to be one of the earliest legendary statements concerning Newton. In a publication issued the year after Newton's death, H. Pemberton[1] states that when Newton in 1666 first tested the gravitational hypothesis by applying it to the earth's attraction for the moon, he used too small a value for a degree of latitude on the surface of the earth (60 English miles instead of the more accurate value of 69½ miles obtained later by J. Picard) and found that "his computation did not answer expectation. On this account he laid aside for that time any further thoughts upon this matter." W. Whiston[2] refers to Pemberton's account and adds that Newton was "in some degree disappointed, . . . however, some time afterward," using 69½ such miles, he verified the law of gravitation. These accounts of the computation of 1665 or 1666 are in direct conflict with Newton's own statement[3] found by the astronomer Adams in the Portsmouth Collection of Newtonian manuscripts: "And the same year (1665) I began to think of gravity extending to y^{e} orb of the Moon and . . . I compared the force requisite to keep the Moon in her Orb with the force of gravity at the surface of the earth, and *found them answer pretty nearly*." Newton does not state what value he took for a degree of latitude, but fairly accurate values were known at that time. Measurements of the earth had been made by Eratosthenes[4] and Posidonius[5] in the third century B. C., by the astronomers of Caliph Al-Mamun[6] in the ninth century A. D., by J. Fernel in 1528, W. Snell in 1617, R. Norwood in 1635. Most of these early measurements were in excess of the modern values, some by as much as 13½ per cent. On the other hand, it is true that English seamen used 60 miles to the degree; this was thought sufficiently accurate for their purposes. It was very convenient in computation, for 60 miles per degree of latitude meant one mile per minute. Thus R. Norwood[7] used 60 miles in his "Trigonometrie" of 1631, and again in the edition of 1678, notwithstanding the fact that he himself in 1635 had found the degree to exceed 69 miles. Moreover, Edmund Gunter and William Oughtred[8] call special attention to the inaccuracy of 60 miles. Gunter[9] says in 1624, "I find that we may allow 352000 feet [66 2/3 miles] to the degree." Oughtred[10] (we suspect from what he says,

1 H. Pemberton, "View of Sir Isaac Newton's Philosophy," London, 1728, Preface; W. W. R. Ball, Essay on Newton's "Principia," London, 1893, p. 10; Sir David Brewster, "Memoirs of . . . Sir Isaac Newton," 2 Ed., Edinburgh, 1860, Chap. II, p. 23.

2 Memoirs of the Life of Mr. William Whiston by himself, London, 1749, I, p. 35.

3 W. W. R. Ball, *op. cit.*, p. 7.

4 Sir Th. Heath, "History of Greek Mathematics," Vol. 2, Oxford, 1921, p. 107; Encyklopädie d. Math. Wissensch., Vol. VI, 1, 1907, p. 223.

5 Sir Th. Heath, *op. cit.*, p. 220; Encyklopädie, Vol. VI, 1, 1907, p. 223.

6 Encyklopädie, Vol. VI, 1, 1907, p. 224.

7 Richard Norwood, "Trigonometrie," 1631, p. 102; edition of 1678, p. 147.

8 Newton as a boy studied one of Oughtred's books and later commented favorably on Oughtred's plans for the education of navigators.

9 "Works of Edmund Gunter," 5 Ed., London, 1673, p. 280, 281.

10 W. Oughtred, "The Circles of Proportion," trans. into English by W. Forster, "Addition," London, 1633, p. 21, 27.

that he himself had made crude earth-measurements), in 1633, took 66½ miles. These figures are somewhat below those of Snell and Fernel, but would have yielded fairly accurate results in Newton's computation. In the edition of 1657 of Edward Wright's "Certaine Errors in Navigation," 60 miles are given for 1° of latitude in the body of the book, but in an appendix are given about 66 1/3 miles.[11] It appears that none of the measurements, either ancient or modern (except one of the two estimates made by Posidonius) fell as low as 66 English miles to the degree of latitude. Moreover, Richard Norwood published the results of his measurements (69.5 miles per degree) in his "Seaman's Practice," in 1636, a book whose popularity is attested by the fact that it reached its seventh edition in 1667. It should be noted that Norwood, Wright, Gunter and Oughtred were among the most prominent mathematicians of the first half of the seventeenth century in England.

To claim that Newton took 60 miles in a computation requiring great accuracy is like saying that he would take the value 3, instead of 3.14159, for π, when endeavoring to reach very close approximations to circular areas.

But this is not all. Suppose for the sake of argument, Newton had actually used 60 miles in 1665, he knew in 1672 (as we shall see) that this value was too small. Yet not till about thirteen years after 1672 did he announce his law of gravitation. Why should he have waited that long, if the size of the earth had been the real cause of his difficulties in verifying the law of gravity? We know from at least one source that in 1672 Newton had a knowledge of the best earth-measurements. On January 11 and February 1, 1672, Picard's value[12] (69½ miles to the degree) was mentioned at meetings of the Royal Society. Newton was not present at the first meeting and perhaps not at the second.[13] However, an account of Picard's measurements appeared in the *Philosophical Transactions* for 1675, Vol. 10, p. 261. Again, in 1672, there appeared at Cambridge Newton's own edition of Varenius's "Geographia," which devoted a whole chapter to methods of finding the size of the earth and contained the results reached by Eratosthenes, Posidonius, the Arabic astronomers and Snell.

In 1888 the astronomer J. C. Adams and the mathematician J. W. L. Glaisher came to the conclusion from the study of the Portsmouth Collection of Newton's manuscripts and of his correspondence, that Newton's real difficulty in verifying the law of inverse squares had been not the size of the earth, but the question how a sphere attracts an outside particle.[14] Does the sphere attract as if all its mass were concentrated at its center, or in some other way? In a letter of June 20, 1686, to Halley, Newton said that the previous year (1685) he had been able to clear up this matter. His conclusion is found in his "Principia," Bk. I, Prop. 91.

The investigations of Adams and Glaisher have not received due attention and the legendary account of Pemberton and Whiston is still widely accepted.

(2) *Alleged delay in publication of the "Principia"*: A legend of recent origin is that Halley and Wren held up the publication of Newton's "Principia" three years, because Newton would not give credit to Robert Hooks for his prior discovery of the law of universal gravitation. No authority is given for this statement. It is true that when the manuscript of the first book of the "Principia" was presented to the Royal Society, Hooke entered a claim of priority. It is true that there was a correspondence relating to Hooke's claim, and that Newton finally made an acknowledgment. But that it took three years of effort on the part of Halley and Wren to bring this about is wholly untrue. Halley's troubles were different in character. Fearing further controversies, Newton wrote him once, "The third [book] I now design to suppress."[15] Halley needed all the ingenuity at his command to prevent Newton from withdrawing the third book from publication. The manuscript of the first book of the "Principia" was presented to the Royal Society, April 28, 1686, the last part reached Halley in April, 1687; the "Principia" appeared from the press that same year.[16] Instead of a delay of three years, there was a speed of publication quite exceeding that of recent years.

(3) *Was Newton an inventor of the reflecting telescope?* A noted American critic of mathematical books has asserted recently that Newton did not invent the reflecting telescope but simply constructed such telescopes. As authority for this claim the critic refers to the article "Telescope" in the Encyclopedia Britannica, where emphasis is placed upon the fact that, unlike James Gregory and other theoretical inventors of the reflecting telescope, Newton actually constructed such an instrument. A full and explicit statement is made in the article "Newton," where Newton is credited with the invention. One finds important features of design in Newton's instrument which are contained in none of the previous theoretical designs. Students of history know, of course, that most great inventions and discoveries have been

[11] W. W. R. Ball, *op. cit.*, p. 15, 16.

[12] S. P. Rigaud, "Historical Essay on . . . Sir Isaac Newton's Principia," Oxford, 1838, p. 9.

[13] S. P. Rigaud, *op. cit.*, 1838, p. 7.

[14] *Cambridge Chronicle*, April 20, 1888; W. W. R. Ball, *op. cit.*, p. 16, 17.

[15] Weld's "History of the Royal Society," Vol. I, p. 311.

[16] S. P. Rigaud, *op. cit.*, p. 31, 84.

reached by more than one investigator. A description of Newton's instrument was read before the Royal Society, and it was ordered that a letter should be written by the secretary assuring Newton "that the society would take care that all right should be done him with respect to this invention." The telescope which he presented to the society is carefully preserved and carries the inscription, "The first reflecting telescope invented by Sir Isaac Newton, and made with his own hands."[17] Newton acknowledged that he had been acquainted with the telescope proposed by Gregory, before he contrived his own; nevertheless, certainly no one has greater claim to being called an inventor of a reflecting telescope than Newton.

(4) *Action at a distance:* In the preface to the second edition of the "Principia," 1713, the editor, Roger Cotes, advances the doctrine of "action at a distance." Through lack of discrimination, Cotes's doctrine came to be ascribed to Newton himself, even though Newton nowhere expresses his adherence to this doctrine. On the contrary, in a letter to Bentley, February 25, 1692, Newton says:[18] "That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else . . . is to me so great an absurdity, that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into." In his "Opticks" (Queries, 18, 22) Newton postulated the existence of an ether. In this century a new meaning is attached to the phrase "action at a distance." Instead of being used in the old sense with reference to the non-existence of a medium intervening between attracting masses, it is now used to indicate an instantaneous action at a distance. In place of an agent we now consider the time of action. But even now the view of Newton is misrepresented. Albert Einstein[19] speaks of "Newtonian action at a distance" as "immediate action." Newton, on the other hand, postulates an agent and gives it time to act. To be sure, in his calculations of gravitational attractions, he assumes, as a necessary approximation, that the action is instantaneous, but not so in his talks on gravity. In a letter to Boyle[20] he considers the cause of gravitation between two approaching bodies.

They "make the ether between them begin to rarify." And again,[21] "So may the gravitating attraction of the earth be caused by the continual condensation of some other such like ethereal spirit," . . . in such a way, . . . "as to cause it [this spirit] from above to descend with great celerity for a supply; in which descent it may bear down with it the bodies it pervades, with force proportional to the superficies of all their parts it acts upon."

(5) *Wave hypothesis of light:* The impression is widespread that Newton rejected the wave hypothesis when he advanced his emission hyopthesis. Such is not the case. He showed that the phenomena of colors formed by thin plates might be explained as undulations. With great hesitation did he argue against the wave hypothesis. "'Tis true," he says, "that from my Theory I argue the Corporeity of Light, but I do it without any absolute positiveness, as the word *perhaps* intimates; and make it at most but a very plausible consequence of the Doctrine." And again, "it has a much greater Affinity with his [the objector's] own Hypothesis, than he seems to be aware of; the Vibrations of the Aether being as useful and necessary in this, as in his."[22] Little did Newton suspect that for a whole century his followers would dogmatically insist upon the emission hypothesis and would flatly reject all other explanations, and that even in the twentieth century his study of the possibilities of the wave hypothesis would be overlooked.

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SCIENTIFIC EVENTS

THE PRESIDENT'S COMMISSION ON OIL RESERVES

THE President's Commission on Oil Reserves has organized with George Otis Smith as chairman, and Lt. Commander M. C. Robertson has been assigned by the Secretary of the Navy to serve the commission as its secretary. After calling on President Coolidge the commission issued a statement saying:

> The policy under which the President's Commission on Oil Reserves has been appointed and under which it approaches its task is the definite policy of conservation in aid of national security.
>
> The present is a period of overproduction of oil, but an approaching shortage of American oil can be surely forecast, for consumption is rapidly increasing and already production has begun to drop from the high figures of last year. American wells can not long continue to supply the bulk of the world's needs. Conservation

17 Sir David Brewster, *op. cit.*, Vol. I, 1860, p. 40–46.

18 "Correspondence of R. Bentley," Vol. I, p. 70; Kelvin in *Proceed. of Royal Society of London*, Vol. 54, 1893, p. 381. See also S. P. Rigaud, *op. cit.*, Appendix, p. 62, 69.

19 A. Einstein, "Sidelights on Relativity," transl. by G. B. Jeffery and W. Perrett, London, 1922, p. 4, 5, 17, 18.

20 "Horsley's Newton," Vol. 4, p. 385; S. P. Rigaud, *op. cit.*, App., p. 62–65.

21 S. P. Rigaud, *op. cit.*, App., p. 69, 70.

22 *Philos. Trans. Abr.*, Vol. I, p. 146. Quoted in G. Peacock, "Miscellaneous Works of the Late Thomas Young," Vol. I, p. 145, 146.