for observation, and there will be very little diminution in its light during the next three or four months. Using recent observations of the position of the comet, M. Schulhof has computed a new orbit. The following positions are extracted from the ephemeris based upon the new elements :—

Ephemeris for Paris Midnight.

1894.	R.A. h. m. s.	Decl. ° ′ ″
Aug. 27	3 53 39·6	+3 32 39
29	3 55 24·5	3 24 13
31	3 57 1·2	3 15 16
Sept. 2	3 58 29·6	3 5 51
4	3 59 49·6	2 55 59
6	4 1 1·1	2 45 41
8	4 2 3·9	2 35 0
10	4 2 58·1	2 23 56
12	4 3 43·3	2 12 31

A NEW VARIABLE STAR.—The Rev. T. E. Espin informs us, through a Wolsingham Observatory *Circular*, that the star DM + 50°·2251, the position of which is R.A. 16h. 1·5m., Decl. + 50° 47′, is variable.

ON THE NEWTONIAN CONSTANT OF GRAVITATION.[1]

III.

FIG. 8 is a view of the apparatus with the optical compass in position, and with the microscopes focussed upon the wires. They are then ready to be withdrawn by the focussing slide, so as to transfer the distances directly to the small glass scale, as already described.

When this is completed the proper windows are put in position, the screen tubes, the octagon house, and the felt screens are all placed ready for operation 10, in which the deflections are measured, and the period with the balls is determined. As this is the operation in which variations of temperature produce so serious an effect, I prefer to leave everything undisturbed for three days, to quiet down. A few hours are quite useless for the purpose.

In operation 11 the period with the counter-weight in the place of the gold balls is measured; also the deflection, if any, due to the lid and lead balls upon the mirror alone. This is only 1/10 division, but its existence is certain. In the later operations the deflections, if any, due to the lid alone on the mirror alone, and to the lid alone on the mirror and gold balls, are separately determined. Neither of these can be detected. The actual elongation of the fibre may also be observed at this stage, but this is of interest only as bearing on the elastic properties of quartz fibres under longitudinal strain.

Before I come to the treatment of the observations, I should like to refer shortly to the kind of perfection of conditions which by the employment of every practicable refinement that I could devise, I have succeeded in obtaining. Taking experiment 8 as an example, favourable in that the conditions were good, *i.e.* I was not badly disturbed by trains, wind, or earth tremors, I give the worst and the best sets of four points of rest obtained from six elongations. They were :—

Worst set + position	Best set − position
24491	20795·4
24493	20795·7
24493·5	20795·5
24492	20795·5
(24491·7)[2]	
24492·4 mean.	20795·5 mean.

Taking all the mean points of rest, as determined above, in groups of three to eliminate slow shifting, if any, of the points of rest, the series of deflections were :—

3696·0
3696·3
3696·0
3696·8

(Interval of one hour, in which oscillations of large amplitude were observed for period.)

(3697·7)
3696·0

Immediately after the oscillations of large amplitude, which in this case at the end were rather badly disturbed by trains or otherwise, a rather different deflection was observed, but not seriously different. As examination of the figures shows only one anomalous point of rest immediately after the large amplitude disturbance, I feel justified in rejecting the only discordant figure, and in taking the mean of the rest as the true deflection. The unit in this case is 1/10 division. It corresponds to an angular movement of 1/280000, *i.e.* about three-quarters of a second of arc. Now a calculation of the angular twist due to a rotation of the air based upon the period, the moment of inertia, and the logarithmic decrement, shows that if the air in the tube were made to whirl round at the rate of one turn in six weeks, so that the air would blow past the gold balls at the rate of

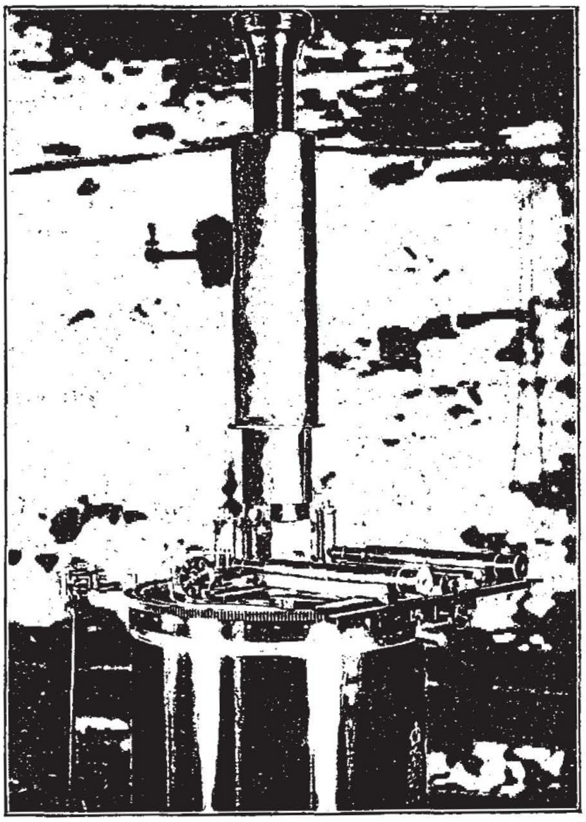

FIG. 8.

one inch in a fortnight, the deflection produced would be 1/10 division, an amount which is greater than the uncertainty of the deflection on this particular night. Again, an examination of the points of rest through the night in the positive and in the negative positions shows a very small steady creep, the same in each case. Creepage of this sort has been, I believe, mentioned as a defect of quartz fibres. When it gives trouble it is due to draughts, as already explained, or to imperfect attachment of the fibres.[1] In the present instance the creepage observed corresponds to a surface rate of movement on the fibre of a millionth of an inch a month.

An examination of the mobile system of the beam and suspended gold balls, of which I exhibit a greatly enlarged and working model, at once shows that all the parts are capable of independent movement to an apparently perplexing degree. This in the theory of the instrument I have

[1] Continued from p. 363.

[2] Disturbed by trains.

[1] See my paper on "Attachment of Quartz Fibres," *Phil. Mag.*, May 1894.

treated provisionally as a rigid system moving all as one piece, which it certainly does not seem to be. For instance, the lead balls, by their attraction of the gold balls, pull them out of the perpendicular, so that their distance from the axis is greater than that given by measurement by the optical compass. The error amounts, in the case of the lower ball, when the lead is at its nearest point, to 1/10,000,000 inch, and I have not taken any notice of it. When the beam is oscillating through so great an angle as 100,000 units the centrifugal force only makes the gold ball move out four times as much, and I have taken no notice of that. Again, when the mirror is under acceleration by the fibre, the gold balls, hanging 5 and 11 inches below, do not follow absolutely; they must lag behind, and so affect the period. Now in this case the gold balls are suspended in a manner which is dynamically equivalent to being at the end of a pendulum 5¾ miles long, the shortest equivalent pendulum that has ever been employed in work of this kind; but short as it is, I have not thought it worth while to be perturbed by an uncertainty of a few inches. There is one point which in some of the experiments only has amounted to a quantity which I do not like to ignore. It is due to the torsional mobility of the separate fibres, about which each gold ball hangs, allowing them in their rotation to slightly lag behind the mirror. As I did not see how to allow for it, I applied to Prof. Greenhill, who immediately told me what to do, and who, with Prof. Minchin, spent a day or two in the country, covering many sheets of paper with logarithms, in finding and solving for me the resulting cubic equation. The correction on this account is 1/7850 on the stiffness of the torsion fibre.

There are four remaining corrections depending on the fact that besides the gravitating spheres there are the ball-holders and supporting wires and fibres, all of which produce small but definite disturbances in the gravitation. These are all calculated and allowed for. They are:—

Disturbances due to brass-holders of lead balls...	1/7320
,, ,, copper ,, gold ,, ...	1/265,000
Attraction of lead balls for quartz fibres	+1/200,000
,, gold ,, phosphor-bronze wires	−1/115,000

Then in experiment 9 gold cylinders were employed. Mr. Edser, of the Royal College of Science, calculated for me the correction to be applied if they were treated as spheres; this amounted to 1/3300.

I have already mentioned that experiment 8 was made under more than usually quiet conditions. Such extreme quiet is desirable, that I manage to reserve Sunday nights, from midnight to six or eight in the morning, for observations of deflection and period. All the other operations can be carried on in the day-time. Sunday is the only night that is suitable, as the railway companies spend every other night shunting and making up trains about a mile away, and this causes such a continuous clatter and vibration, that hours of work may be lost. A passing train does not seem so injurious; but, fortunately for me, most of the observations were made during the coal strike, and fewer trains than usual were running. However, though I may escape from the rattling traffic of St. Giles by working at night, and on Sunday nights am not so badly affected by the trains, I am still not sure of quiet even when there is no wind. For instance, at a quarter to four on Monday morning, Sept. 10, 1893, I was recording chronographically the passage of every ten divisions. Everything was quite quiet, and at the particular moment the marks on the drum recurred at intervals of about three seconds. Suddenly there was a violent non-vibrating lurch of fifteen divisions, or 150 units, which is enormously greater than anything that either trains or traffic could produce; of course, I could make no further record. The time of the last mark was, allowing for the known error of the clock, 15h. 44m. 14·3s. This was entered the same day in my note-book as an earthquake, and in Tuesday's *Standard* I read an account of a violent earthquake in Roumania at about the same time. I have not yet particulars from Vienna, for which Mr. Horace Darwin has written; but though the shock recorded in the newspaper seem to have been too late, preliminary shocks are by no means unknown, and I cannot help thinking that what I observed was the dying out and distant effect of one of these. Of this, however, I am sure, that it was an earthquake that I observed, and not any disturbance due to human origin.

Owing to the viscosity of the air, which limits the time during which an observation for period can be made to about 40 minutes, on account of the resistance that the slowly moving mirror and gold balls experience in their passage through it, I made one experiment, with the view of reducing this difficulty, by the use of an atmosphere of pure dry hydrogen gas, which possesses a viscosity only half that of air. I did find that on this account a great advantage could be gained; but this was more than counterbalanced by the difficulty of getting up a sufficient swing in the gas, and of efficiently controlling the mirror. At the same time, I think that if I had had time to provide means for feeding the gas into the tube without entering the corner, and at the same time were to prevent diffusion at the lower screw, that a little trouble in this direction would be well rewarded. Meantime I found within the limits of error, which were greater than without the hydrogen, that the deflection and the period corrected for the diminished damping were the same. The chief interest of this experiment lies in the fact that it revealed an action unknown to me, and I believe to others, that a thin plane glass mirror, silvered and lacquered on one side, definitely bends to a small extent, becoming slightly convex on the glass side when in hydrogen, and instantly recovers its form when surrounded by air again. This happened many times, producing a change of focus in the telescope of about five-eighths of an inch. I do not offer any explanation of the fact.

There is an observation which should be of interest to elasticians. In experiments 4 to 8 the torsion fibre carried the beam mirror and the ·25 inch gold balls, weighing, with their hooks and fibres, 5·312 grammes. In experiment 9, gold cylinders were substituted, weighing, with their hooks and fibres, 7·976 grammes. The weight of the mirror was ·844 gramme. In consequence of the small increase of load the torsional rigidity of the fibre fell more than 4 per cent., an amount far too great to be accounted for by the change of dimensions, even if Poisson's ratio were as great as ½. There is no doubt about the great reduction in stiffness, for this figure is one of the factors in the final expression for G, which does not show a change of more than 1 part in 1570.

It will not be possible at this late hour to explain how the observations are treated so as to obtain the value of G. It is sufficient to state that in one of these clips all the observed deflections and corrected periods are collected. In the second all the geometrical observations are collected and reduced, so as to obtain what I call the geometrical factor, *i.e.* a number which, when multiplied by the unknown G, gives the torsion on the fibre. In the third, the moments of inertia and periods are made use of to find the actual stiffeness of the fibre in the several experiments, and in the fourth these are combined so as to find G. From G the density of the earth Δ immediately follows.

The annexed table contains the important particulars of each experiment. From this it will be seen that the lead balls were twisted and interchanged in every way, so as to show any want of gravitational symmetry if it should exist. For instance, after experiment 7 the ball that was high was made low, the side that was outwards was turned inwards, and their distance apart was reduced by 1/50 inch, but the change in the result was only 1 part in 2764. The experiments 7, 8, 9, 10 were made under widely different circumstances. After experiment 8 the gold balls were changed for heavier gold cylinders, which, as has already been stated, reduced the torsion of the fibre by 4 per cent., but the result is practically the same as that of experiment 7. I then broke the end of the torsion fibre. After keeping it in London three months, I broke the other end. I then resoldered each end and put the fibre back in its place, and after making every observation afresh, found with the new shorter and stiffer fibre a result differing from that of experiment 8 by only 1 part in 27,635. These four experiments were all made under favourable circumstances, and on this account I feel more able to rely upon them than on the earlier ones, which were subject to greater uncertainty. The last experiment was made under most unfavourable conditions. The periods and deflections were taken in the first four hours after midnight, then, after a few hours' sleep, and far too soon for the temperature to have quieted down, I took the period with the counterweight, but was only able to give ten minutes, as I had to catch a train in order to be able to give my mid-day lecture at South Kensington. It is not surprising that under such conditions a difference of 1 part in 600 should arise. There is a difference of about the same order of magnitude between the earlier experiments and the favourable four. There is one point about the figures that I should like to mention. No results were calculated till long after the completion of the last experiment. Had I known how the figures were coming out, it would have been impossible to have been biassed in taking the periods and deflections. Even the calculating boys could not have discovered whether the observed elongations were such as would give a definite point of rest. I made my observations, and the figures were copied at once in ink into the books, where afterwards they left my hands and were ground out by the calculating machine. The agreement, such as it is, between my results is therefore in no way the effect of bias, for I had no notion till last May what they would be.

No. of Exp.	Lead balls			Gold balls		Neutral lid reading	Date	Deflection	Geometrical factor	Stiffness of fibre.	Result	
	Arch side	Wall side	Shellac spots	Arch side	Wall side						G	Δ
				1·3 grammes each		°	1892			·00	·0000000	
3	2 low	1 high	Inwards	4 low	3 high	267	Oct. 1–30,	5637·3	6089·89	245483	66645	5·5213
				Gold balls of double weight			1893					
4	2 low	1 high	Inwards	4 low	3 high	267	Aug. 15–Sept. 3				66702	5·5167
5	1 high	2 low	Inwards	3 high	4 low	86·5	Sept. 4-11	3667·6	12423·8	772200	66711	5·5159
6	2 low	1 high	Inwards	4 low	3 high	265·9	Sept. 12-14	3667·7	12422·3	Same as No. 8	66675	5·5189
7	2 low	1 high	Outwards	4 low	3 high	265·9	Sept. 15	3664·0	12432·8	Same as No. 8	66551	5·5291
8	1 low	2 high	Inwards	4 low	3 high	265·9	Sept. 16-18	3695·2	12534·2	771664	66575	5·5271
				Gold cylinders								
9	1 low	2 high	Inwards	3 low	1 high	86	Sept. 27–Oct. 3	5775·5	18800·5	739988	66533	5·5306
							1894					
10	1 low	2 high	Inwards	4 low	3 high	85·25	Jan. 1–13	3515·4	12531·8	811011	66578	5·5269
11	1 low	2 high	Inwards	4 low	3 high	85·25	Jan. 14		Hydrogen experiment			
12	2 high	1 low	Inwards	3 high	4 low	265·2	Jan. 17–21	3520·5	12533·7	811385	66695	5·5172
									Adopted result ...		66576	5·5270

My conclusion is that the force with which two spheres weighing a gramme each, with their centres 1 centimetre apart, attract one another, is $6{\cdot}6576 \times 10^{-8}$ dynes, and that the mean density of the earth is 5·5270 times that of water.

It is evident, from what I have already said, that this work is of more than one-man power. Of necessity I am under obligations in many quarters. In the first place, the Department of Science and Art have made it possible for me to carry out the experiment by enabling me to make use of apparatus of my own design. This belongs to the Science Museum, where I hope in time to set it up so that visitors who are interested may observe for themselves the gravitational attraction between small masses. Prof. Clifton, as I have already stated, has given me undisturbed possession of his best observing room, his only good underground room, for the last four years. The late Prof. Pritchard lent me an astronomical clock. Prof. Viriamu Jones enabled me to calibrate the small glass scale on his Whitworth measuring machine; and Mr. Chaney did the same for my weights. I would especially refer to the pains that were taken by Mr. Pye, of the Cambridge Scientific Instrument Company, to carry out every detail as I wished it, and to the highly skilled work of Mr. Colebrook, to which I have already referred. Finally, I am under great obligations to Mr. Starling, of the Royal College of Science, who performed the necessarily tedious calculations.

In conclusion, I have only to say that while I have during the last five years steadily and persistently pursued this one object with the fixed determination to carry it through at any cost, in spite of any opposition of circumstance, knowing that by my discovery of the value of the quartz fibre, and my development of the design of this apparatus, I had, for the first time, made it possible to obtain the value of Newton's Constant with a degree of accuracy as great as that with which electrical and magnetic units are known, though I have up to the present succeeded to an extent which is greater, I believe, than was expected of me, I am not yet entirely satisfied. I hope to make one more effort this autumn, but the conditions under which I have to work are too difficult; I cannot make the prolonged series of experiments in a spot remote from railways or human disturbance; I cannot escape from that perpetual command to come back to my work in London; so I must then leave it, feeling sure that the next step can only be made by my methods, but by some one more blest in this world than myself.

SCIENCE IN THE MAGAZINES.

IN the August magazines received by us, science is but poorly represented. A brief mention of the more important articles will therefore be sufficient this month.

Mr. Benjamin Kidd's work on "Social Evolution" has furnished material for much criticism. In the *National Review* Mr. Francis Galton, F.R.S., discusses the part of religion in human evolution as set down in the book; and Mr. Kidd adds a short note on the opinions expressed in the article. The same magazine contains a paper on "Sleeplessness" by Mr. A. Symons Eccles, and one on "Colliery Explosions and Coal Dust," by Mr. W. N. Atkinson. An experience of fifteen years in investigating explosions in coal-mines has led him to believe that "coal-dust has been the chief, or only, agent in all recent widespread colliery explosions." It is regretted that "no experiments have been made on a scale large enough to yield visual demonstration of the effect of an explosion of coal-dust, under conditions approximating as closely as possible to those existing in mines. The nearest approach to such experiments in this country were those recently made by Mr. H. Hall, H.M. Inspector of Mines, in an old pit shaft fifty yards deep. The length of such a shaft is insufficient to develop the whole force of a coal-dust explosion, and the conditions under which the explosions or ignitions took place were necessarily different from those obtaining in the practical working of mines. These experiments, however, are valuable, as demonstrating that the dust ordinarily existing in a great number of mines (not particular exceptional coal-dusts) are capable of propagating flame to the full limits admitted by the conditions of the experiments."

A psychological paper, entitled "How We Think of Tones and Music," is contributed to the *Contemporary* by Mr. R. Wallaschek. Mr. Andrew Lang tilts at Prof. Huxley's treatment of the Bible story of Saul and the Witch of Endor "as a piece of evidence bearing on an important anthropological problem," and treats the matter from a less scientific point of view.

Eight recent books on Iceland furnish the subject of an interesting account of the island in the *Quarterly Review* (No. 357). The same publication contains a long article on "Forestry," in the course of which the author says that the three great faults noticeable in the treatment of woods in Great Britain are: (1) Discrimination has seldom been shown with regard to the choice of the kinds of trees for given soils and