
C. V. Boys.

CHARLES VERNON BOYS

1855–1944

Charles Vernon Boys, son of the Rev. Charles Boys, was brought up at the Rectory of Wing in Rutlandshire, where his father was the incumbent. He was at school at Marlborough, where he came under the influence of G. F. Rodwell, the first science master appointed there, who gave him his first glimpse of science. The seed fell on good ground, and his interest and enthusiasm were strongly excited.

His further education was at the Royal School of Mines (1873–1876) where he was taught physics by Frederick Guthrie, and chemistry by Edward Frankland. At that date there was no mathematical department in the college, nor did Guthrie's teaching lean in that direction. Boys does not appear to have been regularly trained in mathematics at all and practically taught himself what he knew of that subject out of books. He told me once that after a struggle with the initial chapters of Todhunter's Integral Calculus he exclaimed, 'Now I see it', and within a short time, I think only a day or two, he had designed and constructed an integrating machine.

This machine was presumably the one described in the *Philosophical Magazine*, **11,** 342 (1881). Its principle is easier to understand than that of Amsler's (polar) planimeter, working as it does in rectangular coordinates and following more closely the ordinary conception of an integral. But as originally described it is not compact or direct reading like Amsler's instrument, and could hardly compete with it in practice. Boys thought that it might be helpful in teaching the integral calculus, but this suggestion does not seem to have been taken up.

If he had had better opportunities in youth, Boys might have developed into a mathematician, which he was always careful to explain that he was not—though his pupils did not altogether accept this view. Boys' preference was for geometrical rather than analytical methods, and his geometry was of the kind useful in the workshop. His space had three dimensions only, and he was impatient of more.

However, to go back. He graduated in mining and metallurgy, and went on for a short time to a colliery. He was brought back, however, by Guthrie, to whom he became private assistant, and who appointed him demonstrator of Physics in 1881. He tells us that he was in effect Guthrie's right-hand man for the last eight or nine years of Guthrie's life.

It was about this time that Boys made some interesting observations on the garden spider,[1] which he was encouraged to publish by T. H. Huxley, with whom he was in touch at South Kensington. The issue of these experiments

[1] *Nature*, **23,** 149–150 (16 December 1880).

in general was to show how easily the spider could be induced to take the vibrations of the fork for those of its insect prey. If the fork was placed at a point on the circumference of the web, the spider, at the centre, would identify the vibrating radial thread and proceed along it to the fork, which it would seize and embrace. Another experiment was to drown a fly in paraffin and put it in a spider's web, and then attract the spider by touching the fly with a vibrating fork. The spider was soon disillusioned, but in spite of this, when the fork was sounded again it tried again, and in this way it was compelled to eat a large portion of the paraffined fly. Boys remarks that the spider never seems to learn by experience that other things may buzz besides its natural food. It may be that these observations have been greatly extended since the time they were made, sixty-four years ago: as to this I am not informed. It seems to be now pretty generally recognized that invertebrates do not easily adapt themselves to unaccustomed experiences. Although Boys' writings contain occasional references to other matters of biological interest, he does not really seem to have concerned himself much with them. Once seeing one of his physics students occupied in dissecting a frog in the biological department, he rather deprecated such an occupation.

The idea of Boys' instrument called the radiomicrometer for detecting minute streams of radiation by their thermal effect is to incorporate a thermo-couple and galvanometer into a single instrument without connecting wires. The thermo-couple forms part of a closed loop suspended in the field of a permanent magnet. This loop was hung up in a very small chamber fashioned in a large block of metal, with only a minimum of possibility for its being exposed to air currents arising from accidental differences of temperature.

Boys attached special importance to the low resistance and small moment of inertia of the circuit, the small capacity for heat of the junction, the quickness and dead beat character of the indication, and its freedom from extraneous influences. He discussed at length the principles guiding the detailed design, and produced an instrument more sensitive than any constructed up till then, with the possible exception of Langley's bolometer. He was, however, naturally disappointed when he found that the main idea had been anticipated by D'Arsonval a year or so earlier. Nevertheless, Boys' instrument appears to have been considerably superior in performance to D'Arsonval's owing to the careful detailed design.

It was necessary to make the suspended couple of bismuth and antimony as light as possible, and the soldering of these fine metal pieces gave Boys a delicate but congenial task. An instrument maker who chanced to see him at work on this represented that it was a waste of Boys' time, and that it should be entrusted to his firm. Boys, to whom the gentleman in question was apparently not a sympathetic personality, agreed to his trying, and asked him a little later how he was getting on. 'I can't think how you do it', was the reply. When we learn that the bars of bismuth and antimony were as little as $\frac{1}{8}$ mm. (!) thick, and that the solder used at the junction did not exceed $\frac{1}{5}$ of a milligram, we need hardly be surprised that other workers found it hard to imitate.[2]

[2] Details of his methods are given in Glazebrook's *Dictionary of Applied Physics*.

Having completed his radiomicrometer, Boys attempted in 1890 to apply it to measure the heat radiation from the stars. The experiments were carried out in the garden of his father's rectory at Wing in Rutland. The results were negative. He concluded that the heat radiated from Arcturus was certainly less than that from a candle at 1·71 miles, and that the earlier positive results of Huggins (1869) and Stone (1870?) were incorrect. Boys later expressed the opinion that the moving iron parts of the telescope mounting, driven by clock-work, had caused a progressive deflection by direct magnetic action on the galvanometer needle. The heat of the stars was ultimately brought into evidence by E. F. Nichols in 1901, using a Crookes' radiometer with torsional control. His instrument was estimated to be twelve times more sensitive than that of Boys, with additional advantage from a larger telescope.

In later investigations on stellar heat, at Mount Wilson, a thermo-couple in vacuo was used with a galvanometer in the ordinary way. Boys' instrument with its large permanent magnet is heavy and moreover requires to be kept level. These circumstances make it inconvenient for attachment to an equatorial telescope. Further, its construction is generally held to require a skill comparable with Boys' own, and this idea may have been discouraging to many would-be users.

It was while searching for a suitable fibre with which to suspend the loop of his radiomicrometer that Boys made his celebrated invention of the quartz fibre. An ideal torsional suspension should have an invariable relation between the angle of torsion and the restoring couple called into play. The suspensions used previous to Boys' work were metal wires or glass or silk fibres. Glass fibres had a marked tendency to take a semi-parmanent set. Silk (unspun) was also very unreliable. Metal wires were better, but did not lend themselves to being made very fine. Boys conceived the idea of melting various siliceous minerals in the oxygen blowpipe, and drawing them out with very fine fibres. Quartz was found to be an ideal material. When fused, it does not solidify in the original crystalline form. The crystalline structure is destroyed and the solidified product is silica glass, which, as Boys found, could be drawn into fibres of extreme fineness, which had the very valuable property that after being twisted and released, they took no permanent set. They were actually stronger than steel wires of the same thickness would be—supposing that such thin steel wires could be made. Several methods were used for drawing the fibres, the chief requirement being to do it quickly, so that the fibre did not solidify prematurely. For this purpose the quartz rod to be drawn out was attached to an arrow shot from a crossbow. The bowstring was released by a pedal. Mr H. E. Hadley, who was present at the first trial of the method, describes the intense excitement felt. Was there, or was there not a fibre between the starting point and the arrow? There was a large window behind the working bench, and by the strong direct light Boys was able to see that the fibre was there. The two ends were captured by means of small pieces of gummed paper, and the fibre was wound on a wooden frame previously prepared for the purpose. Fibres could be drawn so thin as to be below the limit of microscopic resolution. The further technical details are of considerable interest to laboratory workers, but can scarcely find a place here.

Boys was the first to draw attention to the properties of fused quartz—its mechanical strength, its ideal elastic properties, and its small expansion, which makes it free from the tendency to crack with heat, so conspicuous with ordinary glasses. He found that it could be made red hot and quenched in water without cracking. Boys made a few small quartz bulbs but did not pursue the manufacture on a large scale which has since attained the dimensions of a considerable industry. It should, however, be remembered that the first step was due to him. He also discovered the remarkable properties of quartz glass as an insulator.

His discovery of the properties of quartz fibres had a great influence on his subsequent scientific work, and may be said in a sense to have largely guided it. We are thus brought to his outstanding work on the Newtonian constant of gravitation.

The establishment of universal gravitation by Newton was an achievement which could only be fully appreciated by those who had undergone a somewhat extensive intellectual discipline. That any particle attracted another particle, and according to the Newtonian law, was a conclusion which could only be drawn from the large-scale phenomena of nature by elaborate processes of integration, which were in those days and to some extent still are only accessible to a minority. Newton [3] considered the attraction of mountains and showed that a hemispherical mountain 3 miles high would not by its attraction draw the pendulum so much as 2 minutes of arc out of the perpendicular. He concluded that even the effect of mountains would be insensible, and *a fortiori* the gravitation of bodies of laboratory size could not be detected: and in fact few experimental problems more difficult than this one have ever been successfully attacked.

However, the intermediate step towards the gravitation of small bodies was in the event to detect the deflection of the plumb-line by a mountain. In 1772, Dr Nevil Maskelyne wrote, 'It will easily be acknowledged that to find the attraction of any hill from undoubted experiment would be a matter of no small curiosity; would greatly illustrate the theory of gravity, and would make the universal gravitation of matter as it were palpable to every person, and fit to convince those who will yield their assent to nothing but downright experiment. . . .' He was commissioned by the Royal Society to undertake the experiment at the Society's expense, and he showed that the direction of the plumb-line relative to the stars taken north and south of the mountain of Schehallion in Perthshire showed a displacement of the vertical of 11·6″ attributable to the attraction of the mountain.

The next step was to proceed to examine the attraction of a body of laboratory size, and this, if it proved feasible, would be much more satisfactory, because such a body would be of uniform density, and exactly controlled shape, which the mountain was not. The experiment was originally designed by the Rev. John Michell,[4] who, however, did not live to complete it. The torsion balance which he had constructed passed into the hands of Henry Cavendish, who modified it somewhat and carried the matter through (1798).

[3] *System of the World*. 2nd ed. (1740), 40.
[4] See *John Michell*, by Sir Archibald Geikie. Cambridge, 1918.

Cavendish does not enlarge on the interest of examining the gravitation of small bodies for its own sake, but introduces the experiment simply as a determination of the density of the earth. The torsion balance used for the experiment had a light wooden beam 6 feet long. The middle of the beam was supported by a copper wire, and had lead balls of 2 inches diameter hung from the ends. The system was never found in practice to come to rest; its period of ocillation was no less than 7 minutes of time, and the position of the arm was read by a vernier to 0·01 inch, representing about 1 minute of arc. The attracting bodies were lead spheres of 12 inches diameter. These could be brought to one side or the other of the small balls. The mean angular position of the suspended rod carrying the smaller balls thus shifted by reversal of the gravitational deflection. The linear shift of the balls was 6 scale divisions or 6/20 of an inch.

The Cavendish experiment was repeated later by Reich (1837), Bailey (1842) and Cornu and Baille (1870). Boys had a very high opinion of the work of Cavendish, but, it is remembered, he was by no means equally enthusiastic about the later work of Francis Bailey. Cornu, he considered, had made a definite step in the right direction in diminishing somewhat the size of the apparatus.

Gravitational forces cannot be simply compared with magnetic ones, because of the complication introduced by the polarity of a magnet. However, for general illustration, a steel ball of 1 cm. diameter magnetized, will readily pick up a similar ball and to do this must exert a force of 4×10^3 dynes. On the other hand, the attraction between the balls due to their mutual gravitation when close to one another, will be about 10^{-6} dynes. Comparing these, we get a factor of 4×10^9 which should bring home the fact that to detect the gravitation of small bodies is no easy matter.

Shortly after Boys had made quartz fibres and used them on his radio-micrometer, it occurred to him that they might make possible a considerable improvement in the Cavendish experiment. Boys tells us that the same idea had occurred independently to Tyndall. The problem was one to which the genius of Boys was peculiarly adapted. He brought to it not only his experience with quartz fibres, but also his unusual personal dexterity and great insight into the essentials of mechanical design.

Cavendish does not discuss in detail the dimension and design of his apparatus. This is partly explained by the fact that he used what had been prepared by Michell, and adopted its design without fundamental changes. Cavendish had no experience to build upon, there being no previous measures of the gravitation of small bodies. It is quite natural that he proceeded to see what he could do with the means to hand. To criticize him for not having done all that Boys was able to accomplish would of course be absurd. Cavendish was sixty-seven years of age at the time when his work was published, and it is not surprising that Boys, working a century later, and giving some of the best years of his life to the problem, was able to improve greatly upon Cavendish's methods.

The original apparatus of Michell and Cavendish had been made with very large masses and a very long beam, no doubt to obtain the largest possible

linear movement of the smaller suspended balls when the position of the larger balls was reversed. Boys now considered closely the question of how this design should be modified. It clearly could not be made much bigger, the 12-inch lead balls and the 6-foot beam being near the practical limit. But what would be the effect of making it smaller, halving (say) the length of the beam and the diameter of the large and small balls, but keeping the period of vibration the same by making the torsion wire less stiff?

Under these circumstances he showed that the sensitiveness, measured by the angle through which the beam was turned by reversal of the gravitational deflection, was not affected, the various causes tending to diminish it, namely the smaller size of the balls and the diminished moment of the force they exerted being offset by the closer approach of the centre of each small ball to the centre of its large neighbour, and the diminished stiffness of the torsion wire.

In spite of this, there would be an advantage in the diminished size under the conditions mentioned, because a smaller apparatus is more easily kept at a uniform temperature, and is therefore less liable to disturbing air currents. But the question must next be considered whether it is right to maintain geometrical similarity when the diminution takes place. Boys' answer was—No. When we diminish the length of the beam we should *not* diminish the masses. If we halve the length of the beam and leave the masses unaltered we shall halve the deflecting couple, but we shall diminish the moment of inertia four times, and therefore, for the same period we shall get an increased deflection. This process can be continued until it becomes difficult to get a fibre fine enough to carry the weight without diminishing the period. Boys considered it probable that this was fundamentally the reason why all his predecessors had used relatively large apparatus, though they do not discuss in detail the considerations which had guided their designs.

In this discussion Boys assumed that the period of vibration was kept the same while the dimensions were varied. No doubt he did so to keep the discussion within manageable limits. If the control is made weaker or the moving parts more massive, the period is increased. If the control is made stiffer and the moving parts less massive, the period is diminished. The period depends on the ratio mass/stiffness. To measure gravitational forces, large moving masses have the advantage of increasing the deflection, and weak control also increases it. Thus, a quick period is necessarily antagonistic to a large deflection. On the other hand, if the period is made very slow, we are expecting a small force to control the movement of a large mass, and common sense as well as special experience show that there must be a limit to this. It is difficult to specify *a priori* what this limit is. One writer of some authority goes so far as to say—

'I am convinced that there is no piece of physical apparatus of reasonable dimensions that could have a natural period of 60 seconds. Instability due to mechanical imperfections sets in long before this.[5]

This is certainly an overstatement. Cavendish used a period of no less than 7 minutes. This was probably too large, since the uncertainties of the position

[5] G. W. Walker, *Modern Seismology*, 34. London. 1913.

of rest in this case were no less than 10 per cent of the deflection to be measured, and the work was thus more tedious without advantage from the increased sensitivity. Boys chose a period of 3 minutes for his working apparatus, and 80 seconds for his small demonstration model.

To go back to the question of diminishing the length of the beam. Obviously a complication arises when the diminution is carried so far that the distance between the suspended balls is no longer small compared with their own dimensions. In that case a large ball begins sensibly to attract both the small balls, instead of only the nearer one, and the advantage of further shortening the beam begins to be lost. Boys avoided this by hanging the small balls at different levels, and placing the large balls at corresponding levels also. The small balls, which were of gold, moved inside a tubular case, and the large balls were attached to the inside of a much larger tube, concentric with the former.

In this way he constructed an apparatus of the external size and aspect of an ordinary galvanometer, with which the gravitation of small masses could be shown to a (reasonably patient) audience.

In this demonstration apparatus cylinders were used instead of spheres since they give a somewhat larger effect. For his actual gravitational work he chose rather larger dimensions to allow of accurate measurement in determining absolute values, and reverted to spheres to allow of accurate calculation of the attraction.

In this apparatus, the large balls of lead are 4½ inches in diameter. The small balls of gold are 0·25 inch diameter. The torsion rod is 0·9 inch long, and in itself a mirror in which the reflection of a distant scale is viewed through a telescope.[6]

Uninstructed critics will be inclined to judge the performance of an apparatus of this kind by the number of scale divisions when the gravitational force is reversed, just as they attempt to judge the performance of a telescope by its magnifying power. In both cases, the criterion proposed is entirely misleading. It is no use to apply great magnification to a telescope image which is not good enough to bear it. Similarly it is of no use to apply great magnification to the beam of the Cavendish apparatus if the result is to show that its position is wandering about in an erratic manner.

For this reason the long beam of Cavendish's original apparatus (6 feet) gives no special advantage. Boys' short beam (0·9 inch) which was in fact a plane mirror, used to reflect a scale 23 feet distant, gave adequate magnification and resolving power to serve every useful purpose.[7] The use of a mirror in this way was of course originally due to Kelvin, who first applied it to the galvanometer, in connexion with his work on the Atlantic telegraph.

Boys obtained a deflection of 1·5° on reversing the gravitational force. This was 3 times the angular deflection obtained by Cavendish. Notwithstanding this, Boys' period of vibration was only 3 minutes against Cavendish's 7 minutes. Thus his apparatus was more sensitive, quicker in action, and, owing to its small size, far less subject to random movements due to inequalities of tempera-

[6] Boys' apparatus is at the Science Museum, South Kensington. Cavendish's apparatus is at the Royal Institution. Bailey's apparatus is at the Royal Astronomical Society.

[7] On this, see the late Lord Rayleigh's *Scientific Papers*, **2**, 436 (1885). He compares from the point of view of physical optics the relative advantage of mirror reading and pointer reading.

ture. Although Boys' work was completed nearly fifty years ago, no one has yet succeeded in showing how his methods or results could be materially improved upon.

Boys carried out many of the preliminary studies for this work in a long narrow vault under the private road between the museum and the science schools, which gave the best conditions of steadiness and uniformity of temperature that he could command at South Kensington. But these were not good enough for the final work, and he set up the apparatus at Oxford, in the cellar under the Clarendon Laboratory, which was placed at his disposal by Professor Clifton. The readings had to be taken in the middle of the night, owing to the vibration caused by passing traffic at ordinary times. This, and the constant journeys between London and Oxford made the conditions far from ideal, and Boys practically gave up all vacations while it was in progress. The final results given in his Royal Society paper were all obtained on one memorable occasion. No figure was ever altered, no trial calculations were ever made for, as Boys always insisted, no figure must ever be put down, of which there was the faintest suspicion as to its accuracy.

Boys always spoke of determining the Newtonian constant of gravitation. The density of the earth, which is fixed by the same determination was, he said, a matter of comparatively local interest. His result was that two point masses of 1 gram each, 1 cm. apart, attracted one another with a force of $6{\cdot}6576 \times 10^{-8}$ dynes, and this makes the density of the earth 5·5270 times that of water. This result is very generally regarded as the best hitherto obtained. Boys considered that the 4th figure could scarcely be more than 1 or at the most 2 parts in error, in other words, that the result was correct to 1 or 2 parts in 6000. It must, however, be admitted that in the past careful and experienced experimenters have often been too sanguine of the accuracy they had attained.

Boys' research on the gravitation of small bodies was without doubt the high water mark of his career. What he did afterwards was always original and interesting, but it was directed to less ambitious and less exacting tasks. There are now few survivors of his early days as a physics teacher at South Kensington. One of these few, Sir Richard Gregory, has favoured me with some recollections. Boys had charge of a few students only who were doing advanced experimental work. His influence upon them was by example rather than by precept. Every experiment attempted was to be regarded as an investigation, and the student was expected to make the best use of the available resources. All Boys wanted to know was what each man had done and how he did it, and he was impatient of being asked unnecessary questions, though he never failed to suggest a simple means of carrying out operational details whenever his guidance was really needed. He was not inclined to wait for slow people.

Boys' thoughts were never idle, though he would sometimes remain for days without entering the students' laboratory. When he had thought out a plan or experiment to his own satisfaction he was filled with truly boyish enthusiasm. The door of his private room would open suddenly and he would rush down the

long laboratory shouting like a wild Indian and sometimes jumping over the low tables in the middle of the floor in his excitement. He never lost this *joie de vivre* of the scientific explorer, though he seemed unconscious of the inspiring influence it had upon all with whom he came in contact.

Boys' routine lectures appear to have been limited to a few on the design of instruments.

Among Boys' best known investigations was his photography of flying bullets. This was not so novel as has sometimes been assumed by popular writers in this country, for the well-known physicist and philosopher, Ernst Mach of Prague was some years earlier in this field, as Boys was always careful to emphasize. He did, however, introduce important improvements of technique. He used a simple shadow method instead of a camera and field lens, and also modified the method of releasing the spark. He reduced its duration to only $\frac{1}{12,000,000}$ second. The result was that the waves of compressed air round it came out very sharply defined. The main points were to reduce the inductance of the spark circuit to a minimum, and to make a suitable choice of the electrode metal.

Boys gave a semi-popular lecture on this subject at the British Association Meeting at Edinburgh in 1893. On this occasion he was the guest of P. G. Tait, who, he told me, had been most kind in explaining mathematical matters to him. 'The sort of mathematics I could understand' he said. In return he had to listen not very willingly to Tait's candid opinion of Tyndall. Tyndall he said had also been very kind to him, and moreover he did not share Tait's unfavourable estimate.

Boys was elected to the Royal Society in 1888. He was promoted to assistant professor at South Kensington in 1889, and held this post until 1897. Perhaps if he had been able to obtain a full professorship he might have continued in academic work: but in those days such posts were very few indeed, and the biographical notices of even the most distinguished scientific men show how slender was the chance of getting such an appointment. When the Chair of Physics at South Kensington fell vacant in 1886, A. W. Rücker, who was some seven years senior to him, was appointed, and for many years there was no other vacancy of a suitable post in this country. He could indeed have obtained a professorship in the Empire overseas, but that would have meant in those days complete isolation from the scientific world, and his friends advised him against it.

Boys was able to supplement his income to some extent by examining for London University, but his complaints at home made it clear that he found this work very irksome. Another way of earning which he liked better was popular lecturing, of which he did a good deal. Some of the subjects which he had made his own lent themselves very well to it, for instance, his photography of flying bullets, and his experiments on soap bubbles. His audience did not fail to appreciate his manual dexterity—'like a conjuror' as it was often described. The substance of some of these lectures became widely known by his small popular book *Soap Bubbles, their colours and the forces which mould them,*

which, moreover, gives practical hints for carrying out the experiments. Some 13,500 copies of the original book have been sold and it was translated into several languages. Some of the experiments described were new and original, for example, the coalescence of two soap bubbles in contact, by electrical influence. This was, I believe, suggested by the late Lord Rayleigh's experiment with two colliding water jets which were made to unite when a piece of electrified sealing wax was brought near, but Boys may fairly be said to have made a new and very striking thing of it. Some of his other experiments, such as the blowing of one bubble inside another, and the blowing of enormous bubbles by means of an injector pipe, were more in the nature of tricks, but were much appreciated by popular audiences. He sometimes took the simple appliances needed for these experiments on visits to friends, and they helped to make a party 'go'. Apart from the actual experiments on soap bubbles there is a good deal of miscellaneous information in the book about subjects connected with capillarity, which, if not fundamentally novel, are presented in an interesting and original way, with a minimum of technical jargon. Even serious scientific students can learn a great deal from it, and without any headaches.

Somewhat later, he introduced an apparatus called the 'Rainbow' cup, by means of which a flat soap film can be spun and thus thinned at the centre by centrifrugal action. Coloured rings are developed with a black centre, and various other experiments can be carried out. This was patented, and put on the market as a scientific toy.

Some passages in his book on soap bubbles suggest, what was the fact, that Boys was an inveterate practical joker. With most people the inclination for this wears off with youth, but with him it persisted into middle age. Some of his devices were very artistic. Thus, having made a large smoke ring box, and having filled it with sulphuretted hydrogen, he took shots at passing targets in Victoria Street, allowing for movements of vortex ring and man. This did not hurt the victim, but it made him wonder if the ordinary laws of nature were not in abeyance.

In 1897 Boys left the Royal College of Science, and turning to applied science, he took up the post of Metropolitan Gas Referee, for which he was selected by Sir Francis Hopwood, afterwards Lord Southborough. This probably gave him about the same income as the assistant professorship, but the demands on his time were much less, and he had the use, without charge, of the official premises of the Gas Referees at 66 Victoria Street. He hired a small suite of rooms in the same block, where he set up a private workshop and laboratory.

After the passing of the gas regulation act in 1920, the Metropolitan Gas Referees became Gas Referees for the whole country. The payment went up considerably, perhaps fourfold, but the duties remained comparatively light. The Referees were abolished shortly before the war of 1939, and the work was taken over by the Board of Trade, but Boys' services were retained in an advisory capacity until 1942–1943. The duties of the Referees under the act of 1920 were, so far as can be discovered from the act itself, to prescribe the methods of test for the quality of the gas, and generally to supervise the conditions under

which they were carried out by the gas examiners, some eighty in number, to whom the routine work was entrusted. Boys succeeded A. W. Rücker, and was not improbably recommended by him for the appointment. The other gas referees in Boys' time were A. G. Vernon Harcourt, as chemist, succeeded later by W. J. Butterfield, and J. S. Haldane as physiologist.

Originally the use of gas had been mainly for lighting, and its quality was determined exclusively by photometic methods. Boys took a considerable part in introducing the Harcourt pentane lamp as a standard of light, instead of the old fashioned standard candle, and in overcoming the legal difficulties in making this change. The increased use of gas for heating made it necessary to determine the heating power, though at first this was to be determined for purposes of reference only. This was to be done as from 1 January 1906 and it was for the Gas Referees to prescribe the methods of determining the calorific value.

This led Boys to spend much of the energy and enthusiasm of his later years on devising apparatus for the purpose. This is a very different problem from the constant of gravitation, but the same qualities of mind and of mechanical skill were called into play here also. Whether the result was so valuable from a scientific point of view is more doubtful. In any case it is difficult to find a common measure for scientific and technological progress.

Boys began by designing a simple calorimeter, to determine the heat which could be collected from the combustion of a metered volume of gas. This calorimeter was of the flow type, that is to say, water flowed at a determined rate through the instrument, and the resulting steady temperature was measured. There might not appear to be much scope for inventiveness in this, but as a matter of fact, previous instruments of the kind did not give steady temperatures, the thermometer being found to move through a range of $\frac{1}{10}^\circ$ C. or more, owing to hotter and colder streaks in the heated water. Boys was dissatisfied with this and he succeeded in constructing a calorimeter in which the variations were only about $\frac{1}{100}^\circ$ C. and therefore negligible. The principle he adopted was that the gas should have plenty of space to pass gently through the instrument, and that the water should be taken through every channel strictly in series, all alternative parallel flow being prejudicial.

Under the gas regulation act of 1920 tests of illuminating power were no longer required. The essential operation was to determine the heating power of the gas, and for the first time the Gas Referees were required to prescribe a recording calorimeter, which would give a continuous record of the heating power of gas, just as a barograph records the atmospheric pressure. At that time recording calorimeters were in the experimental stage, and the Referees considered that so far as they had got, they were not suitable for official purposes. This led Boys to spend a large fraction of the energy of his later years over the problem, though it was undertaken not as a necessary part of his duties, but as a labour of love, and because the mechanical problems fascinated him. He does not seem to have been particularly encouraged by the Board of Trade or any other superior authority.

His instrument embodies a gas meter which, with its accessory correcting mechanism, doles out the gas at a fixed rate. Further mechanism, driven by the same clock motor, doles out water at a fixed rate. The gas doled out is used to heat the water doled out simultaneously, and the resulting temperature is recorded on a graph, which, properly interpreted, gives the calorific value of a cubic foot of gas as supplied at any given time. The gas meter and recording calorimeter were each of Boys' own special design, but the details of these, however important in practice, are not of so much general interest as the part which he liked to call 'the thinking machine'. Let us consider how the need for this arises.

If the mechanism simply doled out so many cubic feet per hour, we should be ignoring e.g. the diminished calorific value of a cubic foot of the gas when it is expanded by a rise of the general temperature. The gas as delivered from the gas holder is saturated with water vapour, and the additional water vapour taken up when the temperature rises further dilutes it. Corrections are necessary to the simple doling out by volume and Boys' 'thinking machine' applies the correction as the record is made. The construction of the 'thinking machine' is as follows:

A limited portion of air is contained in a bell jar over mercury, with a little water. When the temperature rises (e.g.) the air expands, and takes up more water. Thus the bell jar rises, and in doing so, actuates a mechanism which makes the gas meter rotate faster, the meter rate being thus kept proportional to the volume contained in the bell jar.

The normal rate of the meter is given by a clock motor, but by means of accessory mechanism the clock motor is made to supply an additional or subtractional rotation to the meter. In this mechanism complicated wheel work is used which delighted Boys' heart, but which need not detain us here. The essential part is the disc-globe-and-cylinder integrator of James Thomson, which is in effect a transmission gear of continuously variable ratio from zero in either direction forwards or backwards.[8] The value of this ratio is determined by the position of the globe or ball which transmits the motion between the cylinder and the disc. This ball is movable along a diameter of the disc, and its position is governed by the rise or fall of the bell jar containing moist air over mercury.

To recapitulate—Suppose, e.g. the temperature rises from the normal. The air in the bell jar expands, the bell jar rises, the ball of the integrator is pushed outwards, away from the centre of the revolving disc against which it lies, and the transmission ratio is increased. Thus an additional rate of rotation is given to the meter, and the dilution of the gas by expansion is compensated so that the same effective quantity of gas is passed per hour. We have no space for a more detailed description but what has been said is perhaps enough to show that there is nothing really very mysterious about the matter.

If any reader is disposed to think that it is over-careful to pay much attention to this temperature correction of the recording calorimeter, he may be reminded

[8] See Thomson and Tait's *Natural Philosophy*, **1**, 488 (1879).

that the annual gas bill of the nation is of the order of fifty million pounds, and a 1 per cent correction on this would be a matter of half a million pounds. The usual payment of scientific workers is certainly not regulated on the principle that this is a negligible sum.

Although Boys expressed his pleasure in having invented the 'thinking machine' and said that it had from the first worked perfectly, he was not content to leave well alone, but went on to devise another recording calorimeter. Here also the principle of the gas bell containing a fixed portion of air is retained, but the correction is made in a different way, the water feed to the calorimeter being diminished when the temperature rises instead of the meter rate being increased. This is described in his Guthrie lecture 'My recent progress in gas calorimetry'. The leading feature of the mechanism for doling out the water exactly, he explains, came to him in a dream, and he got up early in the morning and went to his workshop in Victoria Street to make a preliminary trial of it. He does not apparently regard it as superseding the 'thinking machine' and the scientific interest certainly seems less. However, Boys was so gripped by the problem that he virtually gave up his consulting practice from 1924 to devote himself to it. 'The call of the calorimeter', he said, 'was insistent. I could not afford to waste time merely making money for myself.' He was as keenly interested in the simplicity and durability of the instrument as in its accuracy.

Boys often emphasized that the mechanisms in the recording gas calorimeter were 'positive' in their action. It is a little difficult to define this conception. That the gas and water measurements are 'positive' may be admitted. I once queried, in conversation, whether the same could be said of the disc-globe-and-cylinder, depending as it does on a frictional drive. Boys rather implied that any one of correct mechanical instinct would call it so. I had to confess myself unconvinced, but we were able to agree that it was only a word, and that any one could use it as he chose.

He had what might be called a fastidious taste in mechanism, and in delivering his Guthrie lecture he spoke with humorous scorn of any instrument designer who would lower himself to make use of a *cam*. It may, however, be queried whether the acknowledged masters of machine design have always lived up to this standard.

I have also heard him speak with contempt of the once fashionable pursuit of ornamental turning.[9]

Already, before Boys left academic work he had begun to practise as an expert witness in patent cases. His power of clear and simple explanation was very much appreciated in the courts. In the earlier years up to 1905, the retaining fees he charged were usually of the order of £50, but they were probably increased in the later years, and this source of income continued to improve until after the war of 1914–1918.

It is remembered that Boys was insistent on the desirability of expert witnesses

[9] Not all eminent scientific workers have shared this view. Mr Justice Grove used to take a lathe for ornamental turning when he went on circuit, to amuse himself in the evenings.

of standing charging adequate fees, and when he was consulted on the subject by a colleague, A. G. Greenhill, the mathematician, who proposed to charge a small sum (15s. 6d. Boys said, but this was probably a figure of speech) he reacted vigorously and urged him strongly to send in a bill for £50, which as Boys had expected was paid without demur.

For many years Boys was in every technical patent case worth mentioning. The work was heavy, but unlike most of his other pursuits it was lucrative, and constituted his main source of income. It is significant that he was more often for the plaintiff, who would of course be first in the field, and have the prior chance of retaining him. This would not necessarily imply that he would be called. During this time as an expert witness three or four virtually shared all the work.

Probably his first really big patent case was the Dunlop tyre case. He was also a good deal concerned with Marconi's patents on spark telegraphy, and, while uncertain to what extent Marconi had been guided by clear theoretical views, had no doubt that he had arrived by some means at the right combinations.

In a patent case about the cyclone dust collector, Boys produced in court a saucer of tea with tea leaves. On setting the tea swirling the leaves collected in the centre. In a case about the gyro-compass, he gave a clear explanation of the quadrantal error, to help the judge to understand.

Boys was a member of the Soirée Committee of the Royal Society for no less than fifty-five years, and during much of the time he took a prominent part in its organization. He usually had something to show there, and also at the meetings of the Royal Society Club, and at the time of his death he was the senior member.

He was for a time a civilian member of the Ordnance Committee at Woolwich, and collaborated with Captain, afterwards General, Holden in problems of internal ballistics.

I asked him once whether he would not do well to employ a skilled mechanic to embody his inventions in concrete form, and I suggested that W. Duddell, whose problems seemed to me not dissimilar, found it advantageous to do so. Boys said that his ideas only got into shape as the constructional work proceeded and that this work helped the thinking process, and that he would not get on any faster by having it done for him. He did not seem altogether to agree that Duddell's case was comparable. Many of his friends thought, however, that in fact the workshop part of it was what he really liked most, and that without that a great part of his interest would have evaporated. The final apparatus for the constant of gravitation was made to Boys' drawings by the Cambridge Scientific Instrument Company, but with that exception, I believe that practically everything which he designed was made in the first instance by his own hands. He did not spend time in lacquering and french polishing, but his constructions were tidy, and the workmanship good.

In the year 1900, Boys thought of a method of observing the development of a lightning flash so as to know what is happening all the time that it lasts.

Some calculations made on the back of an envelope on the beach at Bognor where he was spending a holiday led him to think that this would succeed, and he characteristically cut short his stay there to go back to London and construct an instrument for this purpose. It consisted in a camera with a fixed plate, and two lenses revolving in a circle at opposite ends of a diameter. The two pictures of the lightning flash thus obtained show distortion due to the motion of the lenses which are in opposite directions. From a comparison of the two pictures and a knowledge of the velocities of the lenses it is possible to deduce the direction and speed of the developing discharge.

Boys waited twenty-eight years before he succeeded in getting a suitable opportunity for using it, so after all not much would have been lost if he had allowed his holiday at Bognor to run its normal course. The opportunity came at last on the occasion of a visit to the laboratory of Mr A. L. Loomis at Tuxedo Park, New York, in 1928. Although the conditions there were far more favourable than in England, he was only able to secure a single pair of photographs during his visit. The comparison of these, though not affording a spectacular result, showed that the method was feasible. Boys concluded that the flash started at the ground and almost immediately after started in the length next the cloud. The flash then travelled from both these parts, and finished in the middle of the course $\frac{1}{7000}$ second later.

The matter was afterwards taken up and developed with great success by B. F. Schonland and his collaborators in South Africa, where opportunities are relatively frequent. Their results were communicated to the Royal Society by Boys who watched the progress they were making with keen interest, but since he took no further part himself except in improving the apparatus, a detailed account of this later work hardly forms part of the present subject. It may, however, be mentioned that in March 1933 Mr Collens, using one of Boys' cameras, obtained photographs which showed the comparatively slow descent from the cloud of a short luminous 'dart' along the track to be followed by the real upward flash which is seen. This latter is perhaps six times as fast as the downward dart, and about a fifth of the speed of light.

Although Boys was not in the ordinary sense an astronomer, yet he interested himself in various subjects with an astromonical bearing. His studies on the heat of the stars, and the constant of gravitation fall under this head. He was fond of using a small transit instrument to determine the time, and seemed rather to regret the introduction of wireless time signals as spoiling this particular kind of sport. He took a great interest in the dipleidoscope, an instrument sold, at one time, by Messrs Dent, which consists of a special kind of hollow prism in which two images of the sun were seen, one by external and the other by two internal reflexions in the sides of a hollow prism. The diurnal motion carries these images in opposite direction across the field of view, and they cross one another when the sun is on the meridian. The dipleidoscope was originally intended for the same kind of casual use as a sundial, but Boys had the idea that it could be developed into an instrument of precision which might actually rival the transit instrument in routine observatory work.

With this object he showed how a special form of solid prism of suitable design could be used instead of the hollow one used in the original instrument to avoid the effects of prismatic dispersion. He urged that his prism could be more satisfactory fixed in its mounting than an astronomic objective could be and there were other points as well. However, he certainly did not succeed in impressing the then Astronomer Royal with this as a practical project, and the matter seems to have dropped out of notice.

Boys also took a great interest in sundials. He designed, made and presented a special pattern of sundial which is now in Kew Gardens and which it may be hoped will long be preserved as an interesting example of his workmanship. He was greatly interested in horology generally, and the writer remembers keen discussion on this subject between him and Arnulph Mallock who shared this taste.

In 1935 Boys came before the scientific world in rather a new light as the author of a pamphlet on *The Natural Logarithm*. He explains in the preface how, in his youth, he had read the account of this subject in Todhunter's *Algebra*, the textbook in general use, and was dissatisfied with its artificiality. He had written to the author, who, he says, was quite apologetic and encouraging, and Boys began to think how he would himself like to introduce the subject. He appears to have pondered over it at intervals, and this book gives the results. It is certainly very different from Todhunter's approach. As might be expected, Boys has nothing to say about the rather mysterious exponential limit. He begins with the rectangular hyperbola, and shows that the problem is in essence to determine the area under this curve.[10]

Boys aims at the kind of treatment which appeals to the experimental turn of mind, and from this point of view it might be thought that the simplest introduction would be to plot the hyperbola on squared paper and find its area between two given ordinates by counting up the squares. A rough result can be got in this way in a few minutes. But Boys' method was much more ambitious in the matter of numerical accuracy, though I cannot help thinking, at the expense of being less simple and natural. He makes use of a parabola as an approximation to the hyperbola. He finds the area of the parabola by the method of exhaustions following Archimedes, and applies certain corrections which amount in effect to choosing a parabola which crosses the hyperbola instead of lying entirely inside it. In this way he gets a very good value, equivalent to the use of eight terms of the usual logarithmic series. But since he has to test his success by the use of this series, it seems a little doubtful whether much has been gained by postponing the use of it, for as Boys admits it is needed for further study. He tells us that if his treatment had been available at the time he himself first studied the subject, he would have read it eagerly. This we must accept, and it shows that the method suits some minds. But, after all, Boys' mind was an exceptional one, of very strong individuality, and it remains doubtful whether what suited him would suit many others.

[10] The mathematicians too have arrived at a similar point of view, and now often introduce the subject virtually in this way. See, for example, Lamb's *Infinitesimal Calculus*, first published in 1897.

Among the more important honours that Boys received were: the Royal Medal of the Royal Society, 1896; the Rumford Medal of the Royal Society, 1924; the Duddell Medal of the Physical Society, 1924; the Elliot Cresson Medal from the Franklin Institute of Philadelphia. He was honorary LL.D. of Edinburgh University: on the institution of Fellowship of the Imperial College (the confederation which included the old Royal College of Science), he was on the first list of those elected. He was President of the Physical Society in 1916–1917.

Boys married in 1892 Marion Amelia, daughter of Henry Pollock. There was one son of the marriage, Mr G. V. Boys, M.I.E.E., who became secretary of the Institution of Naval Architects, and a daughter, Mrs Malcolm Carruthers, who died in 1937. Boys obtained a divorce from his wife in 1910. The co-respondent was Professor A. R. Forsyth, F.R.S., who afterwards married her. It is desirable to mention that mutual friends were not inclined to pass harsh judgments on any of those concerned.

In his later years Boys lived a bachelor life, having rooms at Palace Street, Westminster, and spending his evenings for the most part at the Savile Club. Week-ends from 1913 onwards were spent at his small house in the country, St Mary Bourne, near Andover, Hants. There he had a workshop, and sometimes put in heroic hours to get a job through during a week-end. His life there was very retired, and he took little or no part in the social activities of the district, but he liked to have real friends down for the week-end. After about 1939 when he gave up the position of Gas Referee he resided there almost entirely.

His leisure in the country was partly spent in the garden, where he seems to have been more interested in dealing with weeds than in growing beautiful or rare species of plants. When he was already eighty-two years of age he published a small book embodying his experience in this matter.[11] It is very characteristic, and would betray even to a reader who knew nothing about him that the author was interested and informed in a wide range of the sciences, including chemistry, molecular physics, practical mechanics, geology, botany, plant physiology, pharmacology, entomology. It is not suggested that he had a deep acquaintance with all of these, if indeed any one could have, but he had knowledge which was available for use in every case. In addition his ever youthful sense of fun peeps out in places. Probably no one interested in the upkeep of a garden would fail to gain useful hints from what Boys has to tell. His experience is classified under the various botanical species, but he emphasizes that he is giving a talk and not writing a treatise, and that he is anxious to pool his experience with that of others.

Boys had strong views about various matters of domestic economy, such as how to adapt a coal fire for burning coke, and how to cook vegetables. I rather gathered from the way he spoke that he was strongly critical of the methods of, at all events, the ordinary plain cook, and that he was accustomed to invade the kitchen and take the work out of her hands. What her reaction to this was he did not say.

[11] *Weeds, Weeds, Weeds.* Wightman & Co. London, 1937.

On the occasion of Boys' eightieth birthday the Royal Society Club, of which he was the senior member, and probably the member who had put in the maximum number of attendances, celebrated the occasion with a festival dinner, when he was presented with an album containing the autographs of members of the Club. This gave him great pleasure. The menu card carried an excellent characteristic photograph of Boys, which is here reproduced. In his speech, Boys recalled that, next to Henry Cavendish, he had been one of the most constant diners and that this was another case when he had done on a small scale what the great Cavendish had done on a large scale.[12]

In his last years, he became as many people do, less flexible in opinion than formerly and his hearing deteriorated somewhat. He had only one eye available for use, and that one was operated on for cateract in 1937, and his figure became very bent. In spite of all this, he did not lose his sense of fun and his skill with tools remained until the end. I asked whether he did not find failing eyesight an obstacle, but he replied that the sense of touch was a substitute, and that he was able to turn an accurate cylinder to size as well as ever. The instrument for drawing ellipses, which is described in his last paper in the Physical Society's *Proceedings* was made when he was eighty-eight years of age.

As a preface to his Guthrie lecture Boys put forward what was ostensibly an account of Guthrie's philosophy, but which we shall probably not be far wrong in taking as an exposition of his own.

'The making of specious scientific surmises unsupported by experiment, however amusing it may be, and however loudly it may be advertised, does nothing to advance the certain knowledge of the world. The acid test of experiment is essential. So will you who in years to come will have the management of the [Physical] Society in your hands accept this as a solemn message from the dead. If you would be true to the ideals of Guthrie you will select a Guthrie lecturer from among those who have done things rather than from those who have merely talked. *Nullius in verba* is the motto of the Royal Society.'

If we look through the bibliography of Boys' work we find that after his research on gravitation was completed and he had given up his appointment at South Kensington, he turned his attention in the main to applied science, and his subsequent contributions to pure science were minor ones, dealing almost entirely with details of apparatus and experimental methods. Exception should be made of his well known demonstrations on soap bubbles including the Rainbow Cup. These have fascinated many audiences—juvenile and adult, but they are entirely eclipsed in importance by his determination of the constant of gravitation and it is by this and by his invention of quartz fibres that his name will chiefly be remembered.

RAYLEIGH

[12] The reference was of course to their respective determination of the constant of gravitation.