DO FALLING BODIES MOVE SOUTH?

By Edwin H. Hall.

Part I., Historical.

THE question whether a sphere falling from rest through a few hundred feet of still air swerves perceptibly toward the south, from the vertical path indicated by the plumb-line, is not, perhaps, one of the largest or most urgent problems of physics; but it has the dignity of venerable age and the charm of mystery. It was familiar to Newton; it has been answered in the negative, on theoretical grounds, by Gauss and by Laplace, and in the positive, on experimental grounds, by nearly every one of the investigators who have from time to time through more than two centuries made the actual trial.

If there is any significant feature of this problem, in its purely mechanical aspects, which Gauss and Laplace failed to perceive or could not adequately discuss, the discovery of this feature and its function is a worthy task for any mathematician of the present day. If there is any explanation in electric or magnetic action for a southerly deviation of perceptible magnitude, under the conditions of experiment which have prevailed, this explanation is yet to be offered by any physicist, though several have made the attempt. If the well-nigh universal agreement of experimental evidence as to the reality of such an effect is the result of a long succession of accidental errors in one direction, we have a striking exception to the ordinary course of chance events. If the whole mystery is the consequence of mental bias in the experimenters, the proof and explanation of this bias would have, at least, the merit of psychological interest.

Accordingly, it seems worth while to go over carefully all that is known in regard to the experiments in question, with a view to estimating, not merely the degree of skill and care shown in each, but also the mental attitude of each experimenter with regard to the issue of his work. In *Science* for November 29, 1901, Professor Cajori published an admirable summary of the history of our problem, describing briefly the results of all notable investigations upon it, whether experimental or theoretical. But for our purpose there is need of details which Professor Cajori did not give. Much of what immediately follows in regard to English investigators is taken from Mr. W. W. R. Ball's *Essay on Newton's Principia.*

In November, 1679, Robert Hooke wrote to Isaac Newton proposing a philosophical correspondence. The latter replied, declaring that he had "shook hands with philosophy," had "long grutched the time spent in that study," and was now busy with other affairs, but proposing a scrutiny of the course of falling bodies as a means of demonstrating the revolution of the earth. "Let *A* be a heavy body suspended in the air, and moving round with the earth so as perpetually to hang over the same point thereof *B*. Then imagine this body . . . let fall, and its gravity will give it a new motion toward the center of the earth without diminishing the old one from west to east. Whence the motion of this body from west to east, by reason that before it fell it was more distant from the center of the earth than the parts of the earth at which it arrives in its fall, will be greater than the motion from west to east of the parts of the earth at which the body arrives in its fall; and therefore it will not descend the perpendicular . . ., but outrunning the parts of the earth will shoot forward to the east side of the perpendicular, describing in its fall a spiral line . . ., quite contrary to the opinion of the vulgar who think that, if the earth moved, heavy bodies in falling would be outrun by its parts and fall on the west side of the perpendicular." Newton added some very interesting suggestions as to the method of trying such an experiment. He would use a pistol bullet, on a calm day, would have the bob of the plumb-line "setled in water so as to cease from swinging," and would by preference work in "a high church or wide steeple, the windows being first well stopped; for in a narrow well the bullet possibly may be apt to receive a ply [push ?] from the straightened [compressed ?] air neare the sides of the well, if in its fall it come nearer to one side than to another."

On December 4 of the same year, Hooke laid Newton's proposal before the Royal Society, and an interesting discussion followed, in the course of which "Sir Christopher Wren supposed, that there might be something of this kind tried by shooting a bullet upward at a certain angle from the perpendicular round every way, thereby to see whether the bullets so shot would all fall in a perfect circle round the place where the barrell was placed."

On December 11 Hooke read to the society his answer to Newton's letter, maintaining that the course of the falling body "would not be a spiral[1] line, as Mr. Newton seemed to suppose," and "that the fall of the heavy body would not be directly east, as Mr. Newton supposed; but to the southeast, and more to the south than the east."

January 6, 1680, Hooke wrote to Newton: "In the meantime I must acquaint you that I have (with as much care as I could) made three tryalls of the experiment of the falling body, in every one of which the ball fell towards the southeast of the perpendicular, and that very considerably, the least being above a quarter of an inch, but because they were not all the same I know not which was true. What the reason of the variation was I know not, whether the unequal spherical figure of the iron ball, or the motion of the air, for they were made without doors, or the insensible vibration of the ball suspended by the thread before it was cut."

A little later Hooke answered Newton that "by two tryalls since made in two severall places within doors it [the same experiment] succeeded also," and on January 22, 1680, before the Royal Society, "Mr. Hooke showed the ball, that had been let fall from the hight of 27 feet, and fell into a box full of tobacco pipe-clay, sticking in the clay, upon the surface of which were made lines crossing each other: which showed the true perpendicular point indicated by the ball, when it hung suspended by a thread from the top, and how much the ball had varied from that perpendicular in its descent towards the South and East: and he explained the manner, how the same was performed in all particulars. It was desired, that this experiment might be made before a number of the Society, who

[1] This criticism stimulated Newton to a renewed study of the general law of gravitation, which he had not yet demonstrated, and years afterward became one of the chief points of discussion in the controversy as to how much assistance Newton had received from Hooke in the discovery or development of this law.

might be witnesses of it before the next meeting. The time appointed was the Monday following at three in the afternoon." This apparently ends the account of Hooke's observations. He was Secretary of the Royal Society; and if his experiment had been successfully repeated before the witnesses proposed, it is altogether probable that he would have recorded the fact in some public form.

In what has been quoted Hooke is entirely positive and somewhat circumstantial. Why is he not conclusive? Partly because his trials of the fall were so few, apparently only five in all, partly because the deviations which he recorded were so large in proportion to the very moderate height which he appears to have used. Moreover, Hooke, a brilliant genius but a somewhat uncertain character, had committed himself in the most open way to the opinion that experiment would reveal a southerly deviation. In a man of his reputation such a bias is not to be overlooked; and yet it is hard to believe that he deliberately lied to his associates of the Royal Society. His evidence is not to be altogether ignored.

Left in this dubious state by Hooke, our problem appears to have remained untouched for more than a century, until, in 1791, Guglielmini at Bologna took it in hand. He published a book in regard to the matter, but this book, if it exists still, is extremely rare, and few details of his work are now generally available. It was doubtless better than that of Hooke. He used a much greater fall, about 78 m., and appears to have made as many as sixteen trials. From these he deduced an average easterly deviation of about 1.9 cm. and an average southerly deviation of about 1.2 cm. He had a theory, whether formed before or after his experimenting the writer does not know, which attributed the southerly deviation to the resistance of the air. Indeed, his theory called for a somewhat smaller easterly deviation and a somewhat larger southerly deviation than his experiments produced.

But here, too, there were doubts. According to Benzenberg,[1] writing some twelve years later, "Guglielmini's experiments were made with great accuracy, but he first verified his perpendicular line six months after the experiments." In a footnote Benzenberg

[1] Gilbert's Annalen der Physik, Vol. XII.

quotes Lalande as follows: "Guglielmini writes me that he is now convinced that Laplace is right, and that the theory gives no deviation toward the south. That which he has found toward the east agrees very well with the theory; but this is now no longer proof of the motion of the earth, since the other deflection, toward the south, does not at all agree."

In 1802 Benzenberg was much occupied with falling balls in the tower of St. Michael's at Hamburg. Most of his experiments were on the resistance offered by the air to such bodies, this resistance being deduced from the length of time occupied in a given fall as compared with the time which would have been required in a vacuum. He measured the duration of the fall by means of a watch at the top of the tower, and the paucity of experimental resources in those ante-electrical days is strikingly shown by the fact that he could not get this time for a height greater than a certain number of feet, perhaps 250, because, beyond this limit, he could not at the top of the tower hear the thud of the ball when it struck on a block of wood at the bottom. After these experiments on resistance he made, during the same year and in the same place, observations on the easterly and southerly deviations. For this purpose he dropped 32 balls, finding an average easterly movement of 0.90 cm. and a corresponding southerly movement of 0.34 cm. Was Benzenberg in undertaking this research prepossessed in favor of a southerly deviation? Possibly, for he knew of the work of Hooke and of Guglielmini, and he had not yet received from Gauss those letters which finally convinced him of the improbability of such a phenomenon. That he took the evidence of his own experiments seriously is shown by the fact that he did not at once yield to the authority of Gauss, but questioned whether some feature of the theory had not been overlooked. Later he appears to have received without opposition the suggestion, from Olbers, that air currents had affected the course of the balls dropped in the Hamburg tower. So far as the writer knows, this suggestion was entirely without proof.

Discussion of our problem was at its height just a century ago. Gauss in 1803 and Laplace about the same time, each in his own way, developed the theory of a sphere falling from rest through still air under the influence of gravity alone, that is, without effect from magnetic, electric, or other obscure forces. Each pronounced the southerly deviation inappreciable, for experimental heights. It is not unlikely, therefore, that Benzenberg, when in 1804 he resumed his experiments on falling bodies, was affected by a strong prepossession against the existence of a perceptible southerly movement.

He worked now in the vertical shaft of a coal mine at Schlebusch, with a fall of 84.4 m. His preparations and arrangements were made with care and intelligence. To prevent air currents he covered the top of the shaft and stuffed the passage at the bottom with straw. He suspended each ball before its release by means of a horse hair gripped between the jaws of a vise, and, lest there should be a disturbing bulge in the hair just above the part flattened by the vise, he flattened the hair in advance by drawing it between two hot irons. "The sphere hangs by a flattened horse-hair in a closed space and beneath is an opening through which it falls at release. Together two crossed microscopes are brought to bear, in whose common focus the horse-hair plays. . . . It takes always more than an hour, before a sphere comes quite to rest." "When the sphere is absolutely still, a light pressure opens the finely polished jaws of the vise, and the sphere falls." "Below lies a plank of wood [*Packholz*], which has in the middle a small hole through which the plumb-line of the point of suspension of the ball goes." Across the middle of the hole ran two threads, one of which marked the meridian, the other the parallel of latitude of the place. The ball made a sharply defined dent in the wood, and the distance of the middle point of this impression from both the meridian and the parallel was taken. Benzenberg remarks at one point that he has provided forty spheres for the research, and as these were probably made, like those he had previously used, of an alloy of the softer metals, it is unlikely that he could use any one of them more than once; so that, presumably, he made not more than forty trials in this later work.

Details of the observations at Schlebusch the writer has never seen. Benzenberg stated his conclusion briefly thus: "In still, uniformly heated, air no such [southerly] deviation takes place, and the balls swerve only toward the east from the perpendicular." Cajori, who has apparently found some fuller account of this work than has come under the eye of the writer, says that "on selecting from the total number those experiments which, in his [Benzenberg's] judgment, were made under the most favorable conditions, there seemed to be no indication of a S. D."

The honesty of Benzenberg is not to be questioned. But we may well ask how far he may have been influenced, in "selecting" his evidence from the whole body of data, by the knowledge that the authority of Gauss and of Laplace was dead against the southerly deviation.

Whatever answer we may find for this question, the general belief among scientific men after 1804 must have been in accord with the opinion of the mathematicians, confirmed as it seemed to be by the work of Benzenberg. Accordingly, when Reich, in 1831, took up the problem which had been left undisturbed for nearly a generation, he can hardly, from previous evidence, have expected to find a deviation toward the south. He too worked carefully, profiting by the experience of his predecessors and making some changes of method which were possibly improvements. For example, he dropped his spheres, an alloy of tin, bismuth and lead, through a wooden spout erected especially for the purpose. The idea was good, as such a spout would prevent general horizontal draughts of air; but as we are told nothing about the size of the spout or the care taken to have the line of fall coincide with its axis, one may well be in doubt whether Newton's "ply from the straitened air neare the sides" was not in action here. Reich used sometimes the same method of release that Benzenberg had described and sometimes a curious method of his own "by means of an exactly horizontally placed ring, which was of such size that the balls when hot did not fall through it but when cold did fall through." He heated the ball in hot water, dried it, placed it on the ring and left it to fall through in its own good time. The defence of this unpromising device is that it seems to have worked better, causing less individual variation in the course of the balls, than the other. At the bottom of the fall Reich's arrangements were much like those of Benzenberg, already described. The height he used was 158.5 m., in the shaft of a mine at Freiburg. The following is a summary of his observations as at first recorded, though later the correction of an error in the direction of his meridian line reduced the mean southerly deviation about 0.07 cm.

Series.	No. of Balls.	S. D.	Probable Error.
1	22	+0.6686 cm.	0.992 cm.
2	12	+2.3050 "	1.654 "
3	12	−0.1358 "	1.572 "
4	18	+1.2492 "	1.524 "
5	21	−0.7881 "	0.606 "
6	21	−1.6017 "	1.413 "
	106	+0.5061 cm. [?]	0.27 cm.

Even with the correction for error of meridian Reich found a very considerable S. D., more than 0.4 cm. He had made a more extensive study of the problem, had dropped more balls, than any of his predecessors. Was the evidence he had found conclusive? No. If we were to omit his series 2, the one having the smallest number of balls and the largest probable error, we should have from the other five series not a southerly, but a northerly, deviation. The margin is too narrow.

Oersted, writing to Sir John Herschel a letter which was printed in the B. A. Report for 1846, makes interesting comments on the work of Guglielmini, Benzenberg, and Reich. He regards the work of the last as the best, but says that Reich's observations "differed much among themselves, though not so much as those of Dr. Benzenberg." He concludes: "After all this there can be no doubt that our knowledge of this subject is imperfect, and that new experiments are to be desired." He urges English men of science especially to this research, because of the superior facilities and resources for such work in England.

The only practical result of this appeal known to the writer was the experiment made by Rundell, who dropped balls and various other objects about 400 m. down the shaft of a mine in Cornwall. Rundell appears to have had no knowledge of the details of the devices used by his predecessors, and his own methods were crude, as the following account of them, given by himself in the *Mechanic's Magazine*, Vol. 48, 1848, will show:

"A strong rectangular frame was constructed, having a shelf or stage inside it, capable of turning freely upon an axis, supported by pointed centers, fixed in the sides of the frame. This frame was placed in a horizontal position over the shaft; and when the moment arrived for dropping the bullet, its support was suddenly removed by turning the stage round its axis.

"This plan, it is conceived, ensures the dropping of the bullet, without an appreciable tendency to any particular direction arising from the method employed. It may, perhaps, be objected that the cohesion between the shelf and the bullet would impart to the latter a motion in the direction in which the shelf moved. This is the case when the shelf is made to move very slowly, but when it is turned suddenly on its axis, even if it be some degrees from the truly horizontal position, no deviation arises from this source, as was clearly proved by preceding and subsequent experiments [not described].

"Besides the bullets, iron and steel plummets were used, the latter being magnetised. In form these were truncated cones, the lower and larger ends being rounded. These were suspended by short threads inside a cylinder, to prevent draughts of air affecting them, and when they appeared free from oscillation, the threads were let go. The number of bullets used was forty-eight and there were some of each of the following metals, iron, copper, lead, tin, zinc, antimony, and bismuth.

"A plumb-line was suspended at each end of the frame and east and west of each other; to these were attached heavy plummets, the lower ends pointed. After they had been hanging for some hours in the shaft, a line joining their points was taken as a datum line from which to measure the deflection.

"The whole of the bullets and plummets dropped south of this datum line, and so much to the south that only four of the bullets fell upon the platform placed to receive them, the others, with the plummets, falling on the steps of the man-machine, on the south side of the shaft, in situations which precluded exact measurements of the distances being taken. The bullets which fell on the platform were from 10 to 20 ins., south of the plumb-line.

"The deflection being much greater than I had anticipated could arise from any cause which appeared likely to produce a deviation, I feared the whole experiment was a failure, but more recent considerations have induced me to again test the method employed, [and] I feel confident that the deflection is not due to errors arising from the method of dropping the bullets, and that it is not at all likely that draughts of air in the shaft had any important influence on the result, but that there is a real deflection to the *south of the plumb line*, and that in a fall of a quarter of mile it is of no small amount."

A deviation of ten or twenty inches at the least, and nobody knows how much more, is large enough to be interesting. Unfortunately it is, as Rundell himself felt, hardly credible. Some grave fault of method or conditions is probable here. The manner of release of the bullets, by suddenly tipping the shelf on which they have been resting, is open to criticism, in spite of the confidence which he expresses in it. Indeed, this rather naïve confidence is the opposite of reassuring, as it seems likely that the sudden movement of the shelf would cause a disturbing current of air, if not some more direct mechanical action affecting the course of the bullets; and Rundell describes no experiments to justify his method in this particular. The fact, however, that the "plummets," which were released in a radically different way, all went far to the south, makes it probable, though not certain, that the main cause of the universal southerly movement in these experiments is not to be found in the method of release. The possibility of disturbing draughts of air is disposed of much too lightly by Rundell.

Moreover, we are told too little about the plumb lines, a criticism which applies to all the researches which have been reviewed in this paper. The lower end of a steel or iron wire would be deflected more or less, according to the weight of the plummet supported by it, from the vertical toward the north. Benzenberg probably used a silver wire; Reich used a copper wire. We are not told what kind of wire Rundell used, or Guglielmini, or Hooke.

Curious things may well occur when a plumb-line is suspended or a ball let drop in deep mine shafts not especially prepared for the experiment. Recent observations in the Tamarack, with a depth of more than 4,000 feet, showed two steel wires, carrying each a fifty-pound weight, to be farther apart at the bottom than at the top.

Balls let fall in the same shaft, which is many feet wide, have, the writer is informed, apparently failed to reach the bottom, lodging somewhere on the way down. Air currents are now believed to explain the divergence of the plumb-lines, and air currents possibly may be responsible for the fate of the balls. The fact that a plumb-line in the shaft of an important mine may be at fault gives to our problem a suggestion of utilitarian interest.

In 1902 the writer made, in the inclosed tower of the Jefferson Physical Laboratory of Harvard University, by far the most elaborate and extensive research of which we have record on the course of falling spheres. Details of the method used will be given in another paper. The distance available was small, about 23 m., but the convenience of working in a well-equipped laboratory made possible a very large number of trials. After dropping some hundreds of balls in preliminary practice and development of details of procedure, the writer dropped 948 balls and took the mean deviations, to the east and to the south, for these cases. The result was, for the southerly deviation, 0.005 cm., about the five-hundredth part of an inch, with a "probable error" not much smaller.

If this result stood alone, it might well be considered as practically negative, discouraging belief in a southerly deviation. If the work of any other investigator in this field stood alone, it might well be disregarded, as raising no sufficient presumption in favor of the effect looked for to warrant the undertaking of further experiments. But the well-nigh invariable occurrence of an apparent movement toward the south,[1] the only exception being in the second research of Benzenberg, is a fact not properly accounted for by the history of our problem. Granting that Hooke was prejudiced and may even have merely pretended to find a southerly movement,

Observer.	Time.	Place.	Fall.	S. D.	E. D.	S. D. ÷ E. D.
Hooke.	1680	London.	8.2 m.	+	+	?
Guglielmini.	1791	Bologna.	78.3	1.19 cm.	1.89 cm.	0.63
Benzenberg.	1802	Hamburg.	76.3	0.34	0.90	0.38
"	1804	Schlebusch.	84.4	0.	+	0.
Reich.	1831	Freiburg.	158.5	0.44	2.84	0.16
Rundell.	1848	Cornwall.	400.	25–50	?	?
Hall.	1902	Cambridge.	23.	0.005	0.149	0.03

[1] A summary of results is here given, S. D. standing for southerly deviation, and E. D. for easterly deviation :

granting that every experimenter probably wanted to find some deviation, a positive result in such a research being far more interesting than a negative one, granting that in a case like this, which presents great difficulties and uncertainties, a prejudice in favor of this or that result may lead the experimenter to look farther, so long as his expectation is not fulfilled, and to stop when his expectation is fulfilled — granting all this, the writer finds himself unable to remain quite content with the theory that these conditions have created out of nothing the general evidence in favor of a southerly deviation. Further investigation, under the best possible conditions, appears to the writer worth while, if only to establish a satisfactory negative, so that the question before us may vex no mortal more.

Where can these best possible conditions be found? Apparently in the great monument[1] at Washington. For something like a sheer five hundred feet this great pile has neither window nor crevice opening to the outer air. It can, therefore, be almost hermetically sealed against interior disturbance from winds. There is between the stairway and the elevator, on both the north and the south side, a clear space some thirty inches wide all the way from the "deck" at the top to the base.

But is not the top of so tall and slender a shaft all the time in a state of quiver which would give to the balls a lateral motion before their release, and thus cause them to wander widely in their long descent? Apparently not, at least when there is no wind. During a visit last January to the monument, the writer was surprised to find a mercury surface at the top much less disturbed than it commonly would be in the Physical Laboratory at Cambridge. The mud of the Potomac flats, in which the unfinished shaft once threatened to sink, has at least the merit of being an admirable insulator from the jar of traffic in the nearest streets, which, indeed, are a good distance away. No doubt, in a high wind the top of the monument would quiver, and dropping balls in it in such a case, would be worse than labor wasted, for our present purpose. But, given a year, one might undertake there a research which should be conclusive, yielding a final answer to the question which stands at the head of this paper, a question which has been asked so many times in vain.

CAMBRIDGE, MASS.

[1] Cajori has suggested this place.