LETTER, ON THE DEVIATION OF FALLING BODIES FROM THE PERPENDICULAR, TO SIR JOHN HERSCHEL, BART.

FROM PROF. OERSTED

(REPORT OF THE SIXTEENTH MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. 1846. NOTICES P. 2—3. LONDON 1847.)[1]

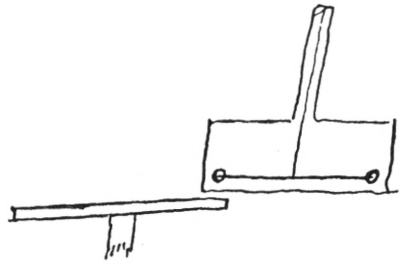
The first experiments of merit upon this subject were made last century, I think in 1793, by Professor *Guglielmini*. He found in a great church an opportunity to make bodies fall from a height of 231 feet. As the earth rotates from west to east, each point in or upon her describes an arc proportional to its distance from the axis, and therefore the falling body has from the beginning of the fall a greater tendency towards east than the point of the surface which is perpendicularly below it; thus it must strike a point lying somewhat easterly from the perpendicular. Still, the difference is so small, that great heights are necessary for giving only a deviation of some tenth-parts of an inch. The experiments of *Guglielmini* gave indeed such a deviation; but at the same time they gave a deviation to the south, which was not in accordance with the mathematical calculations. *De la Place* objected to these experiments, that the author had not immediately verified his perpendicular, but only some months afterwards. In the beginning of this century, Dr. *Benzenberg* undertook new experiments at Hamburg from a height of about 240 feet. The book in which he describes his experiments, contains in an appendix researches and illustrations upon the subject from *Gauss* and *Olbers,* to which several abstracts of older researches are added. The paper of *Gauss* is ill-printed, and therefore difficult to read; but the result is, that the experiments of *Benzenberg* should give a deviation of 3·95 French lines. The mean of his experiments gave 3·99; but they gave a still greater deviation to the south. Though the experiments here quoted seem to be satisfactory in point of the eastern deviation, I cannot consider them to be so in truth; for it is but right to state that these experiments have considerable discrepancies among themselves, and that their mean therefore cannot be of great value. In some other experiments made afterwards in a deep pit, Dr. *Benzenberg*

[1] [The same subject is dealt with in: Amtlicher Bericht über die 24ste Versammlung deutscher Naturforscher und Aerzte 1846 in Kiel. P. 192—93. Kiel 1847.]

obtained only the easterly deviation; but they seem not to deserve more confidence. Greater faith is to be placed in the experiments tried by Professor *Reich* in a pit of 540 feet at Freiberg. Here the easterly deviation was also found in good agreement with the calculated result; but a considerable southern deviation was observed. I am not sure that I remember the numbers obtained; but I must state that they were means of experiments which differed much among themselves, though not in the same degree as those of Dr. *Benzenberg*. Professor *Reich* has published his researches, an abstract of which is to be found in *Poggendorff's* »Annalen der Physik«. After all this there can be no doubt that our knowledge upon this subject is imperfect, and that new experiments are to be desired; but these are so expensive, that it is not probable that they would be performed with all means necessary to their perfection without the concurrence of the British Association. I will here state the reasons which seem to recommend such an undertaking. 1. The art of measurement has made great progress in these later times, and is here exercised in great perfection. 2. All kinds of workmanship can be obtained here in the highest perfection. I think it would not be impossible to have an air-tight cylinder of some hundred feet high made for the purpose. This would indeed be expensive, but it would present the advantage that the experiments could be made in the vacuum and in different gases. 3. With these experiments others could be connected upon the celerity of the fall and the resistance opposed to it by the air and by gases. Professor *Wheatstone's* method for measuring the time would here be of great use. 4. If the southern deviation should be confirmed, experiments could be undertaken in order to discover in how far this could be effected by magnetism in motion. For this purpose balls of different metals might be tried. Very moveable magnetical needles, well-sheltered, but placed sufficiently near to the path of the falling bodies, would indicate magnetical effects induced in them.

[The same is contained in a letter from *Oersted* to Sir *John Herschel* sent from Portswood House $^{14}/_{9}$ 1846 (Universitetsbibliotheket, Copenhagen. Parcel No. 17), which has in addition the following unprinted page:] »I am far from thinking that the experiments in question give a certain result, or even a highly probable one, but I think that its probability is sufficient to call forth new experiments, and to utter some opinion upon the cause of the southern deviation. I think that the southern deviation originates from the action, which a magnetic pole exercices upon a body approaching it. Now in the experiments

here mentioned the falling body approaches with increasing celerity to the northern pole and is therefore repelled from it. Though this could be considered as a consequence of well known experiments, I have tried some ones [once?] more, particularly related to the subject.



The diagramma here joined represents the very simple apparatus. It consists of two parts: a glasscase, containing a balance of torsion, in which two leaden bulbs are fastened at the ends of a wooden rod, and of a magnetic bar fixed on a perpendicular axis, connected with a machinery by which it can be turned round, so that the magnet moves in a plane perpendicular to that represented here by the paper, and very near to the bottom of the glass case. The motion of the magnet produces a considerable deviation of the horizontal rod from its original situation, towards which it returns through oscillations, when the magnet ceases to be moved.‹

ON THE CHANGES WHICH MERCURY SOMETIMES SUFFERS IN GLASS VESSELS HERMETICALLY SEALED

BY PROF. OERSTED

(REPORT OF THE SIXTEENTH MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. 1846. NOTICES. P. 37. LONDON 1847)[1]

It has been frequently noticed that mercury inclosed in glass tubes, even when those tubes were hermetically sealed, undergoes a remarkable change. It first becomes covered by a thin film of a yellow colour, which adheres to the glass, and becomes eventually nearly black. This has been attributed to oxidation, but the oxidation which would arise from the exceedingly small quantity of atmospheric air which could be contained within the bulbs exhibited by Professor *Oersted* was too small to account for the formation of such a quantity of dark and yellow powder as many of them exhibited. Professor *Oersted* referred the change on the mercury to the action of that metal on the glass of which the bulb was formed. It appears that sulphate of soda is frequently employed in the manufacture of glass, and it is thought that a sulphuret of mercury is formed by the decomposition of the glass itself. This is not however satisfactorily proved, and the subject has only been brought forward that attention might be directed to a subject which appeared to involve some remarkable conditions.

[1] [The same subject is dealt with in: Det kgl. danske Videnskabernes Selskabs Oversigter. 1845. P. 11—12. All the essays of ›Videnskabernes Selskabs Oversigter‹ are to be found at the end of this volume.]