XXI. Note on the laboratory at St. John's College, Oxford

R.H.M. Bosanquet

To cite this article: R.H.M. Bosanquet (1881) XXI. Note on the laboratory at St. John's College, Oxford, Philosophical Magazine Series 5, 12:74, 178-184, DOI: [10.1080/14786448108627089](https://doi.org/10.1080/14786448108627089)

To link to this article: <http://dx.doi.org/10.1080/14786448108627089>

Published online: 17 May 2010.

Submit your article to this journal [↗](#)

Article views: 3

View related articles [↗](#)

the most confident reliance may be placed on this Table for finding a vulgar fraction whose value shall very nearly correspond to a given value, yet the particular cases must be excepted in which the given value approaches *very* nearly to one of the critical fractions, $\frac{2}{1}, \frac{3}{1}, \&c., \frac{3}{2}, \frac{5}{2}, \&c., \frac{4}{3}, \frac{5}{3}, \frac{7}{3}, \&c.$; and so for others.

This is the point to which, as the leading subject of this paper, I wished to invite the reader's attention. But, as regards the practical use of the Table, in every case (including all these excepted cases) the Table may be made available for exhibiting two vulgar fractions whose product will exhibit the required number with great accuracy, and that in many different ways. Suppose, for instance, the number to be produced is 2·0098, whose logarithm is 0·30315. The nearest logarithms in the Table are 0·30103 and 0·30544, which are far too wide for any accurate purpose. But (among many equally favourable combinations) there is no difficulty in fixing

on the tabular numbers $\frac{91}{61}$ whose logarithm is 0·17371, and $\frac{97}{72}$ whose logarithm is 0·12944. The sum of these logarithms is 0·30315, as was required; and therefore $2\cdot0098 = \frac{91}{61} \times \frac{97}{72}$.

On performing numerically the product of these vulgar fractions, it is found to be 2·00979.

Instead of sum of logarithms of vulgar fractions, difference might be used if it were thought desirable.

Royal Observatory, Greenwich,
1881, August 4.

XXI. *Note on the Laboratory at St. John's College, Oxford.*

By R. H. M. BOSANQUET*.

IN the paper on Beats recently published, the examination of certain simple forms of Beat was described. The then existing arrangements were not free from disturbing noises (engine, shafting, &c.); and it appeared to me to be a waste of time to proceed further with these investigations until some improvement in this respect could be effected.

A small hydraulic engine was procured and attached to the main supply. The pressure available proved, however, insufficient, as I had anticipated; and the attempt was consequently a failure.

Under these circumstances I applied to the College for

* Communicated by the Author.

permission to erect an additional room near the laboratory, in such a position that I could drive the bellows in the new room by a belt from the old shafting, running through the windows. Permission was accorded; and the new room is now in process of erection. When it is finished, I hope to make a more thorough examination of the phenomena in question in the direction indicated in the above paper.

The engine originally employed proved to be defective, in consequence of its excessive consumption of steam. A small 2-horse-power vertical engine has been substituted for it. This is amply sufficient for the work required; and the steam available is more than enough. The engine is now kept going the whole of every working day; and it is consequently always available for any work that may be required.

The bellows suffered materially from the high temperature generally maintained in the laboratory. It has now been remade, and the table on which the slides work has been leathered. It answers well under these conditions, admitting of the employment of a heavy wind for mechanical purposes, such as it would hardly have borne in its original form.

A few words may be not out of place on the question of leathering the table. This practice is never resorted to as a rule by English organ-builders, but always by German builders. English builders consider that they can plane all the surfaces concerned so perfectly that leathering is unnecessary. In actual organ-work small channels are also resorted to, to carry off any wind that may escape between the planed surfaces. However this may have answered with old English organs, in which a very light pressure of wind was employed (generally about 2 inches of water), it certainly fails to give satisfactory results with the heavy pressures now constantly used; and I am of opinion that the tables ought to be leathered much more generally than is the case. In such an instrument as my bellows I consider the leathering of the tables to be essential.

There is another point of importance. When the bellows is loaded beyond a certain point (in my instrument to about 8 inches of water), the action of the escape-valves when the bellows is full gives rise to vibration. It is proposed, in order to get rid of this, to adopt a form of escape-valve made in two pieces, so that a small bit at the end of the valve is lifted first, and then the valve itself later. In this way the sudden shock which causes the vibration will probably be got rid of. But the load of 7 inches is sufficient for all purposes at present; and the improvement in question will be only with a view to the general perfection of the instrument.

A few words may be said about the improvements in the arrangement of the bottle-notes* with which my investigations on beats have been conducted. It was found that the clips used to hold the india-rubber tubes, through which water was admitted to the bottles, were not sufficiently tight, and the arrangement of a small reservoir to each note was not convenient. The corks were also frequently defective; and it was found difficult to procure them sound when of the proper size. A number of india-rubber corks were obtained, two inches in diameter; and the bottles were fitted with these. A number of glass stopcocks were also obtained, such as are used in ordinary chemical apparatus. In this way the difficulty as to the tightness of the water-tubes was completely overcome.

An india-rubber tube was carried from the water-supply of the laboratory, and slipped on to any of the glass-stopcock tubes into which water was to be introduced. An aspirator was similarly employed at first to remove water when required. But it was ultimately found more convenient simply to detach the end of the india-rubber tube from the water-supply, and let the end hang down over the waste. When the india-rubber tube is full of water, this constitutes a simple and efficient aspirator. By these means any of the notes can be tuned with considerable accuracy; also the notes can be made to change slowly and continuously at any desired speed, by running the water slowly in or out. This is an important point in the demonstration of the properties of difference-tones, as some, for instance De Morgan, have maintained that the combination-tones only exist at definite points. By this arrangement the change of the difference-tones can be followed continuously throughout their whole course. The ordinary difference-tone was long ago demonstrated by Tyndall continuously, so that the point is not new; but the present method of demonstration is very convenient.

The bottles do not give good notes much above the middle of the treble staff (about a'). For notes above this, so far as I have examined them, I have used ordinary organ-pipes, both stopped and open, as in this region little depends on the quality of the notes employed. But the particular phenomena I have hitherto worked at are most clear in the lower part of the scale, in which the peculiar purity of the bottle-notes is available for the demonstrations.

During the last severe winter the laboratory was almost uninhabitable, and little was done. An arrangement for heating by gas with ventilation proved a failure; the moisture

* Phil. Mag. 5th ser. vol. viii. p. 292.

condensed in the ventilating-tubes, and injured the machinery. The arrangement has been removed.

The principal work done since that time has been the construction of two disk machines and the clock governor. One rather elaborate disk machine with clock governor attached is finished and ready for experiment. The disks are made of brass, and have thirty radial slots. If the machine is successful, it will enable determinations of tuning-forks to be made with great accuracy by merely striking them and looking at them through the revolving disk.

A good deal of incidental work has been done, on the principle that it is best to provide complete and efficient apparatus for every point as it arises. In particular, considerable additions have been made to the lathe, including an overhead motion (the existing back-motion proving insufficient for some purposes).

In slotting the disks in the lathe, it was found that saws of the ordinary form were unsuitable for the purpose, as they soon choked when imbedded in the brass. This difficulty was overcome according to a principle suggested by Willis. Where choking takes place, the number of cutting-teeth is to be reduced. A small steel circular saw was therefore constructed with four teeth only on the circumference; by means of this the disks were slotted on the lathe without difficulty. A new tool, however, has been recently invented in Oxford, which promises to overcome this difficulty by the application of more power to the ordinary saw. It will also extend greatly the applicability to all forms of circular cutter, whose use has hitherto been limited by the same difficulty.

The first disk machine constructed consisted simply of a solid foundation carrying the spindle on which the disk revolved with a small driving-pulley, and a counting-arrangement. It was driven by a fine string which passed round the driving-pulley and a large pulley on the shafting over head. The string slipped without much friction on the driving-pulley. So long, then, as the speed of the shafting was greater than that corresponding to the required speed of the disk, the disk could readily be kept to any exact speed by the application of any simple form of break. I think this arrangement very promising; but, from various circumstances, it did not seem to be exactly what I was immediately in want of, and I proceeded to construct the second machine, which is of a more elaborate character.

The driving part of the second machine is worked by an endless rope and a weight. The rope passes also through an independent machine by which the winding-up is effected

either by the engine or by hand. In this way a power is secured which is independent of the variations of the ultimate source of power. The arrangement by which the endless rope drives and is wound up are identical, and deserve a moment's attention.

This arrangement is founded on a combination attributed to Sir C. Wren, described in Willis's '*Principles of Mechanism*.' The description, however, is either incomplete or inaccurate; and I had to try some models before I arrived at the form I have adopted.

The arrangement of Wren is figured at p. 431 of Willis. It consists of two parallel cylinders, in each of which four circular grooves are cut. The rope is passed round the cylinders as if it were wound on the two together from outside, being in contact with half a circumference at each point of contact, and thence passing to the other cylinder. I have found the peculiarities on entering and leaving the system best avoided. In Willis's description the machine is supposed to be applied to winding up a weight, and the force is shown as if applied by a handle to one of the cylinders only. I made a model, and found that it would not do, as I suspected; for there are only three half turns of the rope on the cylinder to which the power is applied, and that is not enough to secure adhesion. The second cylinder, with its three or four more half turns, contributes nothing to the application of power. But it seemed to me that if both cylinders were driven by an intermediate toothed wheel, so that they were rigidly connected, the end required would be attained, as there would be then six half turns of the rope, equivalent to three whole turns, in contact with the power, and this would be enough to secure adhesion. And this in fact turned out to be so. I had the cylinders cast with the grooves upon them, and then bored them out in the lathe true to the grooves, so as to avoid removing the rough skin of the cast metal in the grooves. This gives the rope ample hold; and the arrangement works satisfactorily. A separate rope-ratchet is employed in the winding arrangement, which prevents the cylinders from letting the weight down, and works noiselessly.

The pair of driving-barrels drives a train of which the lowest wheels are cast iron and the upper gun-metal. The mode in which the power is transmitted to the disk-spindle is peculiar. A screw of half-inch pitch was cut in the lathe on the half-inch steel spindle; this is driven by a gun-metal wheel about 3 inches in diameter, on which 18 teeth are cut; and the action between these teeth and the screw presents some curious points. The only model I could find for such an action is that of musical

boxes, whose fly-wheels are driven by an arrangement of this kind. In these the teeth of the driving-wheels are cut quite roughly; they are mere slits, and are cut straight, not to the angle of the screw. I first cut the required teeth in the shape of square slots, cut to the angle of the screw, but not rounded or finished in any way. The wheel so cut drove the disk beautifully. I then gave some further cuts to the sharp edges, and rounded them off so as to fit the surface of the screw rather neatly. The wheel then refused to drive the disk at all. It appeared as if the greater surface in contact increased the friction so as to stop the action. Eventually I cut away the greater part of the rounded edges of the teeth again, and the wheel now drives the disk quite well. I believe that straight square slots, in which the surface in contact would be reduced to a minimum, would be the best form of tooth for this purpose.

The differential-velocity machine, which forms the essential part of the clock governor, has been constructed according to the description given in my original paper. No points calling for mention arose in connexion with it.

A number of electro-pneumatic levers have been supplied by Messrs. Bryceson, for the purposes indicated in my original paper. Connected with the bellows, having wind of a pressure equivalent to 7 inches of water, each lever lifts easily a 2-lb. weight through nearly an inch, on passing a current through its circuit. A pair of these levers is applied to drive one side of the differential-velocity governor from the clock, while the other side of the governor is driven by the disk-machine.

When this was all ready, and the electric connexions were tried, it appeared that there was something wrong with the circuits, which consist of two underground cables connecting my rooms with the laboratory. As much with a view to general convenience as for this particular examination, I took the opportunity of forming a sufficient set of resistance-coils and other testing-apparatus. The resistance-coils were copied from standards in my possession. One of the cables proved to have been cut across, forming a fault with an earth-connexion. The Leclanché battery, which had always given trouble, proved also insufficient for maintaining the requisite power for any length of time. A suitable battery has not yet been obtained; but that will not present any difficulty. A clock with wooden seconds' pendulum has been prepared for occasional use in the laboratory itself, that we may not be interrupted by any further failure of the circuits. But it is judged best to keep the standard clock in my rooms, where it is unaffected by the variations of temperature that occur in the laboratory. Some preparation has been made for other

work; but I confine myself here to what has been actually done. It is hoped that the difficulties are now for the most part overcome, and that the attainment of further results is not far distant.

XXII. *Remarks on Dr. Mills's Researches on Thermometry.*—

No. II. *By* T. E. THORPE, *Ph.D.. F.R.S., and* A. W. RÜCKER, *M.A.**

IN a criticism on Dr. Mills's *Researches on Thermometry*, published in the July number of the *Philosophical Magazine*, we stated that we believed that he had committed an error which "upsets all the inferences drawn by him from his formulæ" relating to the movement of the zero of a mercurial thermometer with changes of temperature. In his reply, Dr. Mills informs us that he has "so far failed to follow" the section of our paper in which this remark occurs, "that he is unable to discuss it." It is evident that the question at issue between us cannot thus be left unsettled. We purpose therefore briefly to notice some of the points on which Dr. Mills has replied to us, and to attempt to make still clearer those which he disregarded as unintelligible.

Exposure Correction.—Dr. Mills is wrong in thinking that we questioned the accuracy of his statement that the "correction-factors" for his thermometers were the same at certain temperatures named by him. What we stated (*Phil. Mag.* July 1881, p. 12) was, that his method of presenting his results "tends to exaggerate the importance of the variation of the corrections." To this we adhere—and the more strongly, inasmuch as Dr. Mills's immediately succeeding remarks afford an example of the confusion introduced into the discussion of the subject by his habit of dealing, not with the quantities which it was his object to determine (*e. g.* corrections, positions of the ice-point), but with subsidiary quantities (correction-factors, total remaining ascents), by which they are only indirectly measured.

We stated that, in our opinion, Dr. Mills's experiments prove that the "exposure corrections [expressed by $y = (\alpha + \beta N)(T - t)N$] below 100° C. are in the case of three out of four similar thermometers practically identical;" and we supported our view by an example calculated for 100° C. Dr. Mills retorts that our inference is clearly wrong, because at lower temperatures the differences of the *correction-factors* (*i. e.* of $\alpha + \beta N$) are greater than at 100°. This we fully

* Communicated by the Authors.