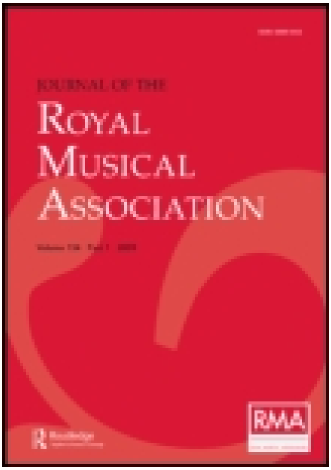

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Deferred Discussion on Mr. Blaiklet's Paper

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R. H. M. BOSANQUET, ESQ., IN THE CHAIR.

DEFERRED DISCUSSION ON MR. BLAIKLEY'S PAPER.

THE CHAIRMAN said, Mr. Bullen, who had intended to read a paper that evening, was through serious illness unable to come, and therefore the proceedings would commence with a discussion on the paper read last month by Mr. Blaikley. He would first ask Mr. Blaikley to give a *résumé* of the substance of his paper.

Mr. BLAIKLEY having done so,

The CHAIRMAN said that, although not practically acquainted with brass instruments, yet he had for some time been occupied with these questions, and he naturally looked on this investigation with great interest. It seemed to him that the most striking novelty in the paper, which was absolutely new and he thought extremely valuable, was the mode of determining nodes of columns of air of any form by the immersion of the tube in water. Assuming that the water rose inside the tube to the same height as it did outside, you could determine the node with great accuracy, and mark off the column of air required. But there was one point which would have to be attended to, and which could be easily allowed for, and that was that the water did not stand at exactly the same height inside the tube as it did outside. In order to make a correction for this it would be only necessary to have a glass tube of approximately the same size and shape, in which you could see the exact position of the water; even that would not be perfectly accurate, but there were means by which this could be corrected.

Mr. DE PONTIGNY asked if this was owing to capillary attraction.

The CHAIRMAN said, Yes. As a practical method he was convinced this would supersede every other. The old method, whereby a membrane was let down into the tube with sand upon it, and you were supposed to find out the node by the cessation of vibrations, was unsatisfactory in many respects. The presence of the membrane certainly altered the relations of the portions of air which lay on either side of it, and he did not know that you could assume that the position of things was even approximately the same when the membrane was there as when it was absent. There was also another law which Mr. Blaikley had ennunciated, which seemed to him very important. He had for some time been doubtful of its truth, and it was only, as he conceived for the first time, that he succeeded in settling it to his own satisfaction, last summer, in the case of the oboe and clarionet. He had found that in those instruments which had a mouth mechanism and where all the notes were produced by the same mouth mechanism, the pitch used for the musical note was always exactly the same as the pitch of the vibrating column of air. Now that was not at all to be assumed as a matter of course; in fact, in organ reed pipes, the resonance of the column of air was nowhere near the note in pitch. They were generally a 4th or 5th apart, and in some classes of pipes almost an octave apart. The mathematical bearings of these relations would, he believed, be of great interest, but, at all events, it was something to be assured by so competent a connoisseur of brass instruments as Mr. Blaikley that this law holds for them, as was amply demonstrated last time. So far as he knew, it had not been formally demonstrated before. In the case of flue organ pipes, which were somewhat analogous to these instruments, having adaptable mouth-pieces, he had formerly been under the impression that the spoken note of the flue pipe was a little higher than the note of principal resonance, although he knew it could be made lower. If you took a stopped diapason pipe and altered the form of blowing you could make it speak a 4th or 5th below. Lord Rayleigh and himself ultimately found that the law in question prevailed in the case of organ pipes; viz. that the pitch was always the pitch of the resonating chamber. He had also ascertained that this law held in an accurate manner in the case of the oboe and clarionet.

Dr. Stone regretted very much that he had not been present at the last meeting, but he had acquired some information from the few words of *résumé* which Mr. Blaikley had given. He might say that he had made some experiments on conical tubes himself, and it was quite correct to say that the tube of a bassoon ought to be a true cone, and when he made a contrafagott he began with that datum. He drew it out on a long board, and with that method got a correct harmonic scale. He could not do so, however, with a bassoon, because when the holes were bored in the correct positions they were quite unreachable. It might perhaps be done, but he had not succeeded in doing so, and the only way he could get an ordinary bassoon bored so as to bring the harmonic series correct was by making it of three cones, one on the top of the other. There must be a conicality which spread out and then shrank down again three times in the bore, something like the dioptric lenses made by Fresnel. He succeeded pretty well with the contrafagott, first taking the cone and then putting the holes in the right positions, and so he presumed the law held correctly, but in the bassoon he could not do so.

Mr. BLAIKLEY said he believed Dr. Stone had particularly investigated the bassoon; he had no doubt that the side holes being of considerable length in proportion to the diameter would have a great effect in altering the pitch.

Dr. STONE said that was the great point. Some time ago, he read a paper before the Physical Society, in which he showed that if the side holes were of considerable length compared with the main bore down which the vibrations were passing, there ceased almost to be an opening to the pipe, and there came a time when secondary vibration was set up in the side pipe and very little air got out. He had made numbers of experiments, and wasted a great deal of time upon this subject, but could never succeed in making a bassoon speak true with short diaphragmatical holes; they all required to be of considerable length. There was great friction in these holes, and very little air got through, but of course it made a weakness in the main tube, and thereby no doubt determined the note spoken. He had made a bassoon of a single cone with the holes in the right place, but it was quite hopeless as a practical instrument. Then he had tried again and again, and at last he found by intentionally falsifying the cone, until it became approximately a triple cone, the notes came right. The best bassoon which he had had made abroad was not at all a true cone, but really consisted of three interlapping cones. He knew as matter of fact also that the only maker who had ever succeeded in making what could be called a perfect instrument, old Savery, whose bassoons would fetch any money, had twenty-one different boring-bits for the tenor joint alone.

Mr. BLAIKLEY said the form of the reed would have some influence.

The CHAIRMAN wished to point out that there was an influence not usually taken account of by those who calculated the lengths on rough principles, which must be taken into account if there was to be any accurate reasoning on these subjects at all. The instance of it with which they were most familiar was the correction for the open end of an organ pipe. The organ pipe did not behave as if it were of its true length, but as if it were increased by a certain quantity. The theory of this increase of length was very complex, and it would be useless to attempt to enter into it, but one could imagine that the currents of air did not spread out instantaneously from a hole or from the end of a pipe; there was no discontinuous connection between the outer air and the pipe. But the currents might be imagined to flow in curved lines, and the effect was the same as if the tube were a little longer than it actually was. The amount of this addition for an ordinary circular hole forming the end of a tube was a trifle more than half the radius of the hole. He took it to be ·55 of the radius of the open end, which you must add to the length of the pipe to make a fictitious pipe which would give the real note. If on the contrary you had a resonator in which the air was separated from the outer air by a plate and the hole was in the plate, then the following effect takes place. On both sides there are currents, converging on the one side and diverging on the other, and the total correction in that case is twice what he had just given, or a little more than the radius of the hole, so that if the holes in the side of an instrument were in a thin plate $\frac{1}{100}$th of an inch thick, and if you had a hole half an inch in diameter, the correction would be equivalent to a tube just a little more than half an inch long. You could never make an instrument in true conical form by calculating the position of the holes, theoretically, without taking account of these quantities which had to be added on to the various lengths, and when you came to take account of them it probably became very different from the ideal you started with, so that the failure to satisfy theoretical conditions need not arise from the failure of the cone, but very likely from something connected with this theory of the holes, which was at present very little understood.

Dr. STONE remarked that the contrafagott was really 16 feet 4 inches, instead of being 16 feet as it should be theoretically.

Mr. HERMANN SMITH thought the nodal points arrived at by Mr. Blaikley were not necessarily those found in the tube when it was blown through. He had tested the tubes simply as resonating bodies, but when the current of air was passing through them he questioned whether the nodal points would be the same as he had determined by means of a tuning fork.

Mr. BLAIKLEY, in reply, said the correction the Chairman had spoken of was no doubt very necessary, and it had been made in his experimental bugle, although he had not gone into details with regard to it. If those corrections were not made the bugle would be altogether out of tune and each of the segments would be very much flatter than was intended. That correction Helmholtz gave as the radius into $\frac{\pi}{4}$, and he had found by experiment that it agreed very closely with a tube of equal section either cylindrical or square, but with conical tubes it did not agree at all. He had made a great many observations for the purpose of establishing a rough practical rule, and probably when the small end of the tube was very much less than the wide end, when, in fact, you were dealing with a conical vessel with a small bit cut off the end, there was no approximation at all. So that when you departed from a cylindrical tube the correction became valueless; the contraction of the orifice seemed to enter into the question much more largely. With regard to the difference between the nodal points, in a still column of air, and a tube which was being blown, there was practically, he believed, no difference at all. The nodal points and centres of ventral segments were determined on the bugle by resonance, but when you put the lips to it, and obtained the vibration of a wind instrument, you got exactly the same results. No doubt there were little corrections to be made: for instance, there was a varying temperature; at the mouthpiece end it was always higher than at the bell end; and there was another cause of discrepancy from the theoretical view, and that was the skin friction of the air against the sides of the tube. That had a decided influence. He had measured it on the trombone, where there was a long length of cylindrical tube of small diameter, and it apparently altered the wave length about one per cent. The question with regard to the tube controlling the reed or the reed controlling the tube had been touched upon by the Chairman, and he might remark that Helmholtz, in his work on the 'Sensations of Tone' spoke rather vaguely on this point. He spoke of the action of the lips as 'tissues heavily laden with watery matter,' and as if the instrument controlled the vibration of the lips. But this could not be the case, because if it were so it would be impossible to play a scale on a trumpet; you could play the difference of a semitone with the lips instantly, so that you certainly controlled the tube by the lips. The lips did not vibrate through the whole length; with a small instrument to get high notes you needed a small mouthpiece, which had the effect of making the vibrating portion of the lips much less than when playing a bass instrument with a large mouthpiece. As far as he understood the action of the larynx in singing, he believed the action of the lips in a wind instrument was exactly the same. As you blew the higher notes the vibrating portion of the lips got shorter and shorter, though no doubt there was also a muscular tension which altered the rate of vibration; although the mouthpiece fixed the length to a certain extent, yet when high notes were being played, even with the same mouthpiece, the portion of the lips which vibrated was much less than when lower notes were produced.

The CHAIRMAN referred to Helmholtz's correction for the open end of a pipe, viz. $\frac{\pi}{4}R$. Helmholtz obtained it by means of the hypothesis of hemi-spherical divergence. Lord Rayleigh and himself had gone fully into the matter, and came to the conclusion that this correction was much less than Helmholtz supposed. Lord Rayleigh adopted the figure ·6 of the radius, whilst he himself adopted ·55; so that there was not much difference between them.

Mr. BLAIKLEY added that in dividing large cones to determine the nodal points in water, it was necessary to make this correction in each segment. He commenced by taking Helmholtz's figure, but he then found on building up the cone again that it was considerably longer than the original cone. He then found by experiment that the correction should be about ·55 of the radius, and he believed that would be correct when the tube was conical.

The CHAIRMAN said that both Lord Rayleigh and himself had published papers on the subject in the 'Philosophical Magazine.' He then called upon Dr. Stone to make a communication.

Dr. Stone said he had not been able to complete the experiments necessary for the paper he had promised to read at the next meeting, and should therefore be obliged to defer it. But as he was informed that Mr. Bullen could not be present to-day, he thought it might be interesting to bring before the members what he had done in another direction. In the first place he had a series of tuning forks forming a tonometer, according to Scheibler's plan, and as there had been a great deal of discussion on tonometry, he thought the members would be interested in seeing the apparatus. The tuning forks had been made from Mr. Ellis's designs and calculations. It was a series of 65 tuning forks, beginning with one of 256 vibrations and going up to one of 512. If 64 was multiplied by four, each varied by four vibrations, and you would have the whole of them in the series equidistant. You had here a very delicate test, and a very convenient one, for he had taken forks made on this principle to various public places where music was performed—to the Opera, for instance, and you could accurately catch the pitch of every instrument as it came out, without making yourself a nuisance as you would do by sounding a reed. He believed these 65 forks required a small correction to be applied to them, because they were originally based on an instrument made from a reed, and it appeared now from recent researches that the reed was more liable to error than the tuning forks themselves. It was easy, however, to apply a correction to them. In the second place, Dr. Stone brought forward a couple of clarionets which he had had constructed, in which he had 19 notes to the octave, but without altering the fingering in any way, so that whilst any ordinary clarionet player could play it easily, yet you had as many duplicate notes as were required for bringing the notes into true intonation. For instance, there were two C♯'s, two F♯'s, and so on. The clarionets were made of india-rubber, which was a new material for this purpose. He had also done the same with a bassoon.

Mr. Ellis asked if Dr. Stone had a list of the 19 notes he had made.

Dr. Stone said he had unfortunately not brought the list, but he thought the instrument possessed all the notes required, though the G♯ and A♭ were not yet quite perfect.

The Chairman asked if the notes were tuned to mean tone or just intonation.

Dr. Stone said, to mean tone; he thought it better to begin with that first, as people were more accustomed to it, but he hoped in the end they would get to a just intonation.

Mr. Ellis thought it would be rather awkward if one of these instruments were played in an orchestra with another one not possessing these extra notes; it might lead to rather unpleasant combinations.

Dr. Stone said, he did not think so. There was really more power of adaptation in an orchestra than many people supposed, and he believed the instruments played much more nearly in correct tune than was generally believed. What was wanted, however, was, that some one should go over the scores and mark them where notes should be sharpened or flattened. He did not think the question of suspensions, which had been sometimes referred to, would lead to the difficulty which had been supposed, because a player could go from one note to another with the greatest ease, and nobody would hear it; he had in fact heard and seen it done. The first point seemed to be to give the power of correcting notes, and then to give the performers an indication on their music which of the two notes was to be selected each time.

The CHAIRMAN, in moving a vote of thanks to Dr. Stone, said the principle of Scheibler's tonometer was well known, and it was exhibited at South Kensington in another form some time ago. His own feeling about it always had been that unless you had continuous tones you could not get enough of the beats counted, and if you could, it was no use trying to count them for a long period unless you had a very good pendulum. His impression was that anything short of the pendulum of an astronomical clock was really worth nothing at all in these determinations. With regard to these clarionets, it was very useful to be able to shift the pitch. He had now some little experience in the orchestra, and he thought that every player must feel that it was very hard upon him, when perhaps the pitch of the orchestra was a little different from that usually adopted, that he could not shift the pitch of a wind instrument at all. You could do very little with the lip, particularly if the orchestra went sharp. He thought something which gave you a greater amount of elasticity, so that when you found the note was not in tune you could correct it by your ear, was a most valuable thing. This improvement tended to that result, and would therefore be very useful.

Mr. ELLIS then made some statements with regard to his experiments on tuning-forks and counting beats.

Dr. STONE corroborated what had been said with regard to the necessity of an accurate pendulum Professor MacLeod had found that an error of three seconds a day in a pendulum vitiated the calculations entirely.

The CHAIRMAN said that some time ago he intended to take up this investigation, and the first thing he did was to provide himself with an astronomical clock and set to work to rate it; this was some months ago, but he had not got beyond that stage yet.

Votes of thanks were then passed to Dr. Stone, and to the Chairman for presiding.