June 17, 1880.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Prof. Charles Niven and Dr. William A. Tilden were admitted into the Society.

The Right Hon. Alexander James Beresford Hope, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Notes of Observations on Musical Beats." By ALEXANDER J. ELLIS, B.A., F.R.S., F.S.A. Received May 28, 1880.

During the last three years I have been greatly occupied with observing and counting musical beats, for the purpose of discovering the cause and amount of error in Appunn's reed tonometer, and of measuring the number of vibrations made in a second by tuning-forks and organs, as materials for my "History of Musical Pitch."* The following are brief notes of some of the observations then made:—

When two musical notes nearly but not accurately form a consonance, or are in unison, they beat. Under ordinary circumstances the number of beats in a second of a disturbed unison is equal to the difference of the number of double vibrations in a second made by each note. It is not so always, as will be shown later on. If x and y be the "pitch" or number of vibrations in a second, made by two musical tones, of which y is the sharper; then, if $my-nx=0$, the tones form what I have termed a *considence*, that is, the nth partial of x falls on the same rank or seat as the mth partial of y. Considences are not always consonances, because other partials of the notes may beat roughly, as when $m:n=8:9$ or $9:10$ or $15:16$, which are well known dissonances, but give appreciable considences. But if the pitch of either x or y be slightly altered, so that $my-nx=\pm b$, the two consident partials become what I have termed *dissident*, or placed on different ranks or seats, and b beats in a second are heard, being called "sharp" when positive, that is when $my>nx$, and "flat" when negative, that is when $my<nx$. This includes the unison for which

* A paper read before the Society of Arts on March 3, 1880, and printed in their Journal for March 5, with an appendix on April 2, 1880.

$m=n$. *Hence all beats heard are beats of simple partial tones*, however compound may be the tones which contain them. This agrees thoroughly with my observations.

Tuning-forks are comparatively simple but always possess an audible second partial or octave, and sometimes higher partials still, capable of being so reinforced by resonance jars properly tuned to them, that beats can be separately obtained from them and counted. This is a matter of great importance in the construction of a tuning-fork tonometer. When the tone is very compound, as in the case of bass reeds (especially those of Appunn's tonometer, furnished with a bellows giving, when properly managed, a perfectly steady blast for an indefinite length of time), beats can be obtained and counted from the 20th to the 30th and even the 40th partial, without any reinforcement by a resonance jar.

Taking tuning-forks first, I find it advantageous to hold the beating forks over one or two resonance jars, tuned, by pouring in water, to the pitch of the partial to be observed, whether it be the prime of both or the prime of one and the second (or octave) of the other. There may be small differences, but I have not found any difference appreciable by my methods of observation in the number of beats in a second, whether the resonance jar is the same or different for the two forks, and whether it is exactly or very indifferently tuned to each fork, but a tolerably accurate tuning much improves the tone and length of the beat. In that case the resonance jar practically quenches all other partial tones, and the beats are distinctly heard as loudnesses separated by silences. If no jar is used, the other partials are heard. In the case of the octave, the low prime becomes a drone and fills up the silences. In the case of beating primes, the octaves, which are beating twice as fast, tend to confuse the ear. Sometimes the second partial of a fork is so much stronger than the prime, that when the fork is applied to a sounding-board, only the octave is heard, which is inconvenient. This is entirely avoided by the resonance jar. Beats being a case of interference, the amplitude of the beating partials should be equalised as much as possible. With two forks of very different size and power, it is easy to regulate the amplitude by holding the louder fork further from the jar. Otherwise the beats become blurred and indistinct. For powerful reeds or organ pipes, beating with forks, it is best to go to a considerable distance from the reed or pipe and hold the fork close to the ear or over a jar. I find 30 or 40 feet necessary for organs ; in Durham Cathedral, where the pressure of wind was strong and my forks weak, I found 60 or 70 feet distance much better. As I was not able latterly to go to a distance from Appunn's reed tonometer, having to pump it myself, I found it impossible to count the primes of the upper reeds by the octaves of my forks, which were completely drowned.

I find beats of all kinds most easy to count (by the seconds hand of a chronometer) when about 4 in a second. They can be counted well from 2 to 5 in a second. Above 5 they are too rapid for accuracy; below 2 and certainly below 1, they are too slow, so that it is extremely difficult to tell from what part of the swell of sound the beat should be reckoned. Partly from this reason, perhaps, I have found great variety in counting successive sets of such slow beats. I never use beats of less than one in a second, if I can avoid it. When the beats are slow it is difficult to discover by ear which of the two beating tones is the sharper; and even fine ears are often deceived. It is easy to discover, however, by putting one of the forks under the arm for a minute. This heats and flattens it by 2 or 3 beats in 10 seconds. Hence if the beats with the heated fork are slower, it was sharper, because it has been brought nearer the other; if faster, it was flatter and has been brought further away. Count for 10, 20, or 40 seconds, according to the fork. Up to 20 or 30 beats in 10 seconds it is easy to count in ones, but from 30 to 50 it is best to count in twos, as one-ee, two-ee, &c., beginning with *one*, and hence throwing off one at the end. When counting for 20 seconds I always count in twos, and for 40 seconds in fours, as one-ee-ah-tee, two-ee-ah-tee, &c., because I have to divide the result by 20 or 40; and this division is avoided by the count itself. Owing to difficulties in beginning and ending the count, I find the possible error per second to be 2 divided by the number of seconds through which the count extends; and that it is best to take a mean of 5 to 10 counts for each set of beats.

Temperature plays an important part. Forks should not be touched with the unprotected hand; they otherwise easily flatten by 2 beats in 10 seconds. Interpose folds of paper. I use two folds of brown paper stitched between two pieces of wash-leather. Large forks are generally on resonance boxes and need not be touched, otherwise the same precautions should be used, as they are very sensitive, and retain the heat longer than small forks. Scheibler's forks are fitted with wooden handles. In tuning, the file heats and flattens; the result, therefore, can seldom be known for a day or two, when the forks have cooled and "settled," as they will be sure to "jump up." I find it best to leave off filing when the forks are two or three tenths of a vibration too flat. In sharpening there is, therefore, great danger of doing too much, as the fork remains apparently at the same pitch, the flattening by heat balancing the sharpening by filing. Hence all copies should be compared some days after, by means of a third fork about four vibrations flatter or sharper than each, to avoid the slow beats of approximate unisons. The filing also seems to interfere with the molecular arrangement of the forks.

The thermometer should be always consulted when beats are taken. But if the beats are between two forks, of which the pitch of one at a

given temperature is known, and both forks may be assumed to be altered in the same ratio by heat, then the temperature need not be observed; but the unknown fork may be presumed to be as many vibrations sharper (or flatter) than the measured fork at the temperature at which the latter was measured, as beats in a second were observed to take place. This is because the alteration is very small, and would be quite inappreciable for the few vibrations between them. But for tonometrical purposes an allowance must be made.

The coefficient of temperature has not been satisfactorily determined. It varies from ·00004 to ·00006 for each vibration and each degree Fahrenheit. Possibly the mean ·00005 is the best number to take, but I have used 1÷21000=·0000476. The flattening seems to be chiefly due to the effect of temperature on elasticity. A large fork of about 435·44 vibrations at 59° F., grew sharper and beat more and more brightly as the temperature descended to 10° or 15° F., being easily counted for 20 seconds and more. At 104° F. it could scarcely be counted for 20 seconds, at 112° F. scarcely for 10 seconds, and at 164° F. I could not count it at all, the sound not lasting more than 2 or 3 seconds, and the beats varying during that time. The effect also seems to vary with the metal and make of the fork and its size, and the coefficient to be greater for high than for low temperatures. For a fork of about 256 vibrations at 59° F., the coefficient from 14° to 59° F. was about ·0000305 or 1÷32,760, but from 59° F. to 175° F. about ·0000548 or 1÷18,280. These experiments, which I made by dipping the forks in freezing mixtures and hot water, and beating them with a fork at mean temperature, are unsatisfactory. In the same way M. Aristide Cavaillé-Coll, the well-known organ-builder of Paris (private letter), experimenting on two forks of about 435 vibrations, one of Scheibler's and one a large-sized diapason normal, found—

	Coeff. for 1° F. and 1 vib.
1st fork, from 59° F. to 194° F. ..	·0000567=1÷17,650
2nd „ „ 60°·8 F. to 194° F. ..	·0000589=1÷16,970

Scheibler ("Tonmesser," p. 50) himself found for a rise of 45° F.—

	Coeff. for 1° F. and 1 vib.
1st fork, about 440 vibrations ..	·00006=1÷16,670
2nd „ „ 220 „ ..	·00005=1÷20,000

Kayser, for two large forks, furnished with mirrors, &c. ("Wied. Ann.," 1879, p. 444)—

	Coeff. for 1° F. and 1 vib.
1st fork, about 72 vibrations ..	·0000494=1÷20,250
2nd „ „ 85 „ ..	·0000566=1÷18,000

Koenig's recent careful experiments ("Wied. Ann.," 1880, p. 413), not made by freezing mixtures and hot water, give two different co-

efficients, deduced originally from a fork of 64 vibrations and extended to one of 256.

	For 1° C. and 1 vib.		For 1° F. and 1 vib.
1st or general coefficient ...	1÷8943	....	·00006212=1÷16,097
2nd or particular „ ...	1÷8951	....	·00006207=1÷16,112

These are practically the same, but it is the latter which he uses in his reductions. Professor McLeod by his machine ("Proceedings," 1879, vol. xxviii, p. 291, and "Phil. Trans.," 1880, p. 1) at first obtained a result nearly identical with Koenig's, but afterwards, when experimenting on one of my forks of about 440 vibrations, he found (private letter) the coefficient to be ·00004882=1÷20,490. While Professor Alfred Mayer (private letter) by repeated experiments on Koenig's and other forks, exposed to the cold of American nights, and counted by beats with forks at mean temperature, obtained the coefficient ·00004545= 1÷22,000, and I adopted 1÷21,000 as a mean of those of Professor McLeod and Professor Mayer. The error must certainly be very small for all Scheibler's forks.

For organ pipes where great exactness does not seem to be possible, I find the coefficient ·00104 for 1° F. and 1 vibration quite sufficiently accurate and to give concordant results generally. In some organs where the air for blowing is cooler than the air of the room, this is too large, according to the observations of Mr. A. J. Hipkins, at St. James' Hall, the coefficient is nearer ·0005 (private letter). In taking the pitch of organ pipes by measured forks, I usually neglect the small alterations of the pitch of the fork, and allow fully for the change of pitch in the pipe. It is probable that this coefficient does not answer for the stopped and fancy pipes. I have applied it only to open, and generally metal cylindrical pipes.

To construct the fork tonometer, invented by J. Heinrich Scheibler (*b.* 1777, *d.* 1837), a silk manufacturer at Crefeld, obtain a set of about 70 good forks with parallel prongs, and of a tolerably large size; tune the lowest to about the C (or B for English high pitch) between the bass and treble staves of any organ or piano, and tune the rest roughly each about four beats in a second sharper than the preceding. Then fit them with wooden collars or handles, and allow them to rest for three months, if possible in the same temperature at which they will be counted. This was not the process adopted by Scheibler, but is much simpler. Then count the beats between each set most carefully, at a temperature which remains as uniform as possible. It may be necessary to use a high temperature; thus Scheibler's was from 15° R. to 18° R.=65·75 to 72°·5 F., which I reckon at 69° F. as a mean; and Koenig now works at 20° C.=68° F., but announces that his former 256 vibrations was only correct at 26°·2 C.=79°·16 F. ("Wied. Ann.," 1880, p. 413.) Count on one day the beats between forks 1 and 2, 3 and 4, &c., and on

another between forks 2 and 3, 4 and 5, &c., so that the same fork is not used for two counts on the same day. Excite by striking with a soft ball of fine flannel wound round the end of a piece of whalebone, as a bow is not convenient unless the forks are tightly fixed. Each blow or bowing heats, and hence flattens, and this tells if the experiments on any one fork are long continued. Count each set of beats for 40 seconds if possible, and many times over, registering the temperature and the beats, and take the mean. Scheibler counted by a graduated metronome, set constantly to an astronomical clock, when the weight was at 60, to eliminate the effect of temperature, and he altered the position of the weight (finally by a micrometer screw) so that there were always four beats to each swing of the pendulum. He seems to have attained extraordinary accuracy. Having counted all, observe those forks which are near the octave of the lowest fork. Find two such, beating with the octave (that is, the second partial tone) of the lowest fork less than they beat with each other. Then the sum of all the beats from the lowest fork to the lower of the two forks, added to the beats of the octave (that is, the second partial tone) with that fork, is the pitch of the lowest fork. Hence the pitch of all the forks is known. The extra high forks are for verifying by the octaves of several low forks, and for the purpose of subsequently measuring. From such a tonometer any other can be made, and the value of each fork at another temperature calculated.

Scheibler made a 52-fork tonometer with infinite trouble, on another plan, and counted it with marvellous accuracy. This tonometer, which I have made many efforts to find, has absolutely disappeared and his family knows nothing of it. But he left behind him a 56-fork tonometer, believed to proceed from 220 to 440 vibrations, and through the kindness of Herr Amels, an old friend of the Scheibler family, who obtained it from Scheibler's grandson, I have had the use of it for a year. I had to count it as well as I could, just as if it had been a set of forks such as I have described, and I found it was not what was thought, but that only 32 sets of beats were 4 in a second, and the other 23 sets varied from 38 to 42 in 10 seconds. I found also that the extremes were probably of the same pitch as in the original 52-fork tonometer. After then counting it as well as I could, and obtaining 219·27 vibrations in place of 219·67, at 69° F., I distributed the error of 4 beats in 10 seconds, as 2 in 100 seconds, among 20 of the 23 sets which were not exactly 4 beats in 10 seconds, leaving the first 3 sets, which I had repeatedly counted and felt sure of, unaltered. Then I reduced all the values from 69° to 59° F. Finally to verify my result I measured by beats with Scheibler's forks as thus determined; first 5 large forks of various pitches, which I had had made for me in Paris, and then 4 forks of Koenig's belonging to Professor McLeod. Professor McLeod himself kindly measured all of them, also, by his

machine, and Professor Mayer, of the Stevens Institute, Hoboken, New Jersey, U.S., kindly measured the first 5 forks by his electrographic method, both with the greatest care and precaution. The three sets of measurements agreed to less than 1 beat in 10 seconds, and more often less than 1 beat in 20 seconds, when reduced to the same temperature. Thus the value of the tonometrical measurement by beats only, and the possibility of counting a tonometer sufficiently, was fully established. Koenig's measurements of his own forks reduced to 59° F., and of the actual Diapason Normal at the Conservatoire, Paris, intended to be used at the same temperature, also agree with mine within less than the same limits. By these forks I have counted 75 standard forks of Messrs. Valantine and Carr, music smiths, 76, Milton Street, Sheffield, who are thus in a position to make small copies, probably not more than half a vibration wrong at most, at a cheap rate.

When forks are counted without a resonance jar, they should not be applied to a sounding board, or held one to one ear and one to the other, but should both be held about six inches from the same ear, and their strengths should be equalised by holding the weaker fork closer to the ear than the stronger.

When the forks are screwed on and off a sounding board or resonance box, there is great danger of wrenching the prongs, unless they are held below the bend, but I have constantly seen this precaution neglected. A wrench immediately affects the pitch and duration of sound of a fork, and renders it comparatively worthless. Such cases have come within my observation. The next enemy to be guarded against is rust. Forks should be kept dry, and occasionally oiled with gun-lock oil. Rust towards the tip affects the fork much less than rust at the bend. My observations and experiments show that errors from rust can scarcely exceed a flattening of 1 vibration in 250, and are generally very much less. But as the amount is uncertain, rust spoils a fork for accurate tonometrical purposes.

My observations on reed tones are confined to those in Appunn's tonometers at the South Kensington Museum, the Museum of King's College, London, and Lord Rayleigh's, where there are copies, all of which I have counted. They consist of oblong boxes containing the reeds placed side by side. The wind pumped into a large reservoir, is driven from it by a spring, and received into this box, the heavy lid of which, separated from the body by the usual bellows-folds of leather, presses on the wind and drives it on the reeds with a very constant pressure. Below each reed is a pallet which, when pulled out by a valve to its full extent, allows the reed to sound at its highest pitch to which it is tuned. If the pallet be slid in somewhat, the pitch can be flattened by as much as two vibrations without much affecting the quality of tone, and even as much as three vibrations with considerable

loss in quality. This power of reducing the pitch slightly and instantly restoring it, is of great service in experiments upon considence and dissidence, which the relations of the pitch of the reeds allow of being tried in a very large number of cases. It also enables considences to be rendered perfect when the instrument, as is necessary generally the case, is slightly out of tune, as the intervals can be made closer by flattening the upper, and wider by flattening the lower reed.

The nominal values of the reeds are as follows:

Bass tonometer, 57 reeds, numbered by their nominal value in double vibrations :—8, 9, 10, 11, 12, 13, 14, 15—16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31—32, 34, 36, 38, 40, 42, 44, 46, 48, 50, 52, 54, 56, 58, 60, 62—64, 68, 72, 76, 80, 84, 88, 92, 96, 100, 104, 108, 112, 116, 120, 124, 128.

Tenor tonometer, 33 reeds, numbered 0 to 32, nominal value $=4\times$ number $+128=128$, 132, 136, 140, 144, 148, 152, 156, 160, 164, 168, 172, 176, 180, 184, 188, 192, 196, 200, 204, 208, 212, 216, 220, 224, 228, 232, 236, 240, 244, 248, 252, 256.

Treble tonometer, 65 reeds, numbered 0 to 64, nominal value $=4\times$ number $+256=256$, 260, 264, 268, 272, 276, 280, 284, 288, 292, 296, 300, 304, 308, 312, 316, 320, 324, 328, 332, 336, 340, 344, 348, 352, 356, 360, 364, 368, 372, 376, 380, 384, 388, 392, 396, 400, 404, 408, 412, 416, 420, 424, 428, 432, 436, 440, 444, 448, 452, 456, 460, 464, 468, 472, 476, 480, 484, 488, 492, 496, 500, 504, 508, 512.

The correctness of these numbers had to be proved in the first place by counting the beats. The sum of the various sets of beats in the treble tonometer should be 256, in the tenor tonometer 128, in the bass tonometer, from 8 to 16, 8 beats; from 16 to 32, 16 beats; from 32 to 64, 32 beats; from 64 to 128, 64 beats. I counted the beats in the treble tonometer at South Kensington several times. On 27th October, 1876, each set of beats being counted for 20 seconds, I obtained 256 exactly. From 5th to 24th September, 1877, I counted each set of beats for a minute, and many times over, and, owing to alterations in the pitch of the reeds, the beats varied from 3·85 to 4·27 in a second, their sums being 256·27. On 25th and 28th September, 1877, I again counted each set for one minute, and obtained 256·28 as the sum. On the 10th and 12th September, 1877, I counted the beats on Lord Rayleigh's copy, and found the sum 256·38. My count of the King's College copy, 13th November to 20th December, 1877, at two minutes for each set, gave 254·75, and was certainly erroneous. The beats $\pm b$, of a disturbed considence, as $y:x$, which is supposed to be near $n:m$, joined with the sum of the beats d, between y and x, when b, d, m, n are known, give y and x by the equations—

$$my-nx=\pm b, \quad y-x=d,$$

and hence the value of l, the lowest note, is known, when $y-l$ is

counted. Of thirty determinations of l thus made for the South Kensington instrument, the highest was 256·65, the lowest, 253·05, and the mean, 255·85. Hence the value of l could apparently be not far from 256, its nominal value. But when compared with an Ut_3 of Koenig's, the reed beat 2·4 flat. Supposing Koenig's fork to have been 256 truly, this made Appunn's $l=253{\cdot}6$, far less than any of my counts and most of my calculations. Lord Rayleigh and Mr. R. H. M. Bosanquet had already suggested to me that the confined air in the box of the tonometer, and the vibration of the whole instrument during the beats, "drew" the notes. I believe they thought that it altered the pitch of the notes, and hence the number of the beats. I began to entertain the same opinion, and devised the following experiment, the Lords of the Committee of Council on Education allowing me to remove the treble and tenor tonometers from South Kensington to the Museum of King's College, where Professor W. G. Adams, F.R.S., allowed me to compare these instruments with those in the Museum for many weeks. Suppose L, M, N, are adjacent reeds on one instrument, and L′, M′, N′ reeds of nominally the same pitch on the other. Practically, they were not quite of the same pitch, a circumstance which showed inaccuracy of construction. Then I took the beats between L and M, M and N, and thus by addition obtained the *internal* beats between L and N, that is, those which occurred within the box of the tonometer. When I took the beats between L and M′, M′ and N, and thus obtained the *external* beats between L and N, that is, beats which were formed in the uncompressed atmosphere of the museum external to the tonometers, *the number of the internal beats always exceeded that of the external.* On taking a mean of my observations, which extended to every set of beats, I found that I could reduce the internal to the external by diminishing the number of the internal beats by 76 in 10,000. I was not completely satisfied with the accuracy of my observations, or with what I considered the rather hazardous mean, but was unable to repeat the very long course of observations. In November, 1879, however, I was able to examine every reed in all the tonometers by means of Scheibler's forks, and I found that the nominal values of the reeds in the treble tonometer could be reduced to those of the forks by a mean subtraction of 76 in 10,000; in the tenor tonometer (which was altogether flatter), by a mean subtraction of 83 in 10,000; in the bass tonometer, octave 64 to 128, by a mean subtraction of 76 in 10,000; octave 32 to 64, of 64 in 10,000; octave 16 to 32, of 57 in 10,000; the octave 8 to 16 was too uncertain to deduce a mean. These results singularly well confirm the former. The nominal values had been deduced from observed internal beats of reed with reed; the external beats agreed whether they were taken between reed and reed, or reed and fork. It is certain, therefore, that the internal beats were accelerated, whether the pitch of the

reeds themselves was raised or not; I think it was not. But the nominal value, depending on the accelerated beats was, of course, increased, and the experiment of the forks shows that it was increased exactly in the ratio of the internal to the external beats. At any rate, therefore, if the pitch of the tones was increased, it was not caused by the confined air acting upon a single speaking reed, but was occasioned by the joint action of two speaking reeds.

This joint action was very powerful. It shook the whole instrument violently. When reeds 256 and 260 (I cite them always by their nominal value) were sounded together, at first only a crash was audible, but after about a second the ear could distinguish the beating of the two primes, closely resembling the beating of two tuning-forks held over resonance jars, but accompanied throughout by a great crash, which made the simple beats difficult to keep well in the ear. This was totally different from the effect when a fork was substituted for one of the reeds. The simple beats remained, but the crash disappeared. Moving of the head caused considerable difference in the loudness of the simple beats, due perhaps, to placing the ear at or near a node of the sound wave, and removing it again. It was useless to attempt to count the beats till these simple beats were well recognised. As I ascended the scale, the crash became less. It was much less for reeds 272 and 276, and the contrast of these beats with those of 256 and 260 on the one hand, and 288 and 292 on the other, was very striking. For reeds 320 and 324 the crash was comparatively faint. I could not distinguish the beats of the second partials or octaves from the general crash arising from the beats of all the other partials. The beats of the primes were quite separate, slow (4 in a second) and distinct, and they seemed to give the time to the other beats. For reeds 376 and 380, the beat of the primes overpowered the crash, which became comparatively light, and after this point, the beats of the primes were always easy to find. From reeds 412 and 416, the beats of the primes were the principal phenomena, and after 492 and 496, the beats were practically simple.

In the tenor and bass tonometers, the beats were still more distressing to the ear, for even for single notes the upper partials beat clearly and slowly enough, after reed 64, to be distinctly perceived as beats; and the beats of two reeds sounded almost like a continually reiterated *feu de joie*. Below reed 16, the primes could not be heard at all, but down to 8 the beating upper partials could be heard.

The effect of the external beats on the ear was distinctly different from the effects of the internal beats. The *surge* of the other partials was not so strong, and the beats were much easier to count. At one time I placed the two tonometers fully 50 feet apart, and stood half way between them to count the beats, which were remarkably clear, the surge becoming indistinct, very like that of the distant waves on

a sea-shore, and forming a running accompaniment, totally dissimilar from the bell-like beat of the partials.

The pitch of the reeds on the treble tonometer furnishes not only numerous cases of considences, but numerous cases of disturbed considences, beating four times in a second, when the reeds are in order. Thus $3\times256=2\times384$, and hence 256 and 384 are a perfect considence. But $3\times260-2\times388=+4$, so that 260 and 388 form a disturbed Fifth, beating 4 times in a second, the upper note being too flat; while $3\times260-2\times392=-4$, another disturbed Fifth, also beating 4 times in a second, the upper note being too sharp. The fact of the upper note being too flat or too sharp is shown immediately by flattening it, as previously described; the first beats are then made more rapid and the second more slow, but it is impossible to destroy them entirely, as the upper note cannot be sufficiently flattened. It is delightful, however, to take what should be a perfect considence, as all were when I first examined the instrument, and throw it out of tune by flattening either the upper or lower note, or both unequally, producing the dissident beats, and then to sharpen these notes gradually, and listen to the beats growing slower and slower till they finally entirely disappear, and then reappear as the sharpening is carried too far. The nature of considence and dissidence is thus distinctly felt, and the delimitation of a considence is determined by the possibility of hearing these dissident beats when one of the extreme notes is flattened. The beats are clear, distinct, and simple, and can be made very slow; their pitch is also exactly what has to be expected by the number of the partials. The other partials of the two notes in the meantime beat roughly, strongly, and very much faster than the dissident beats. Thus for the Fourth 4 : 3, we may take reeds 264 and 352, then the partials will be—

(1)	(2)		(3)	(4)	(5)		
264	528		792	1,056	1,320, &c.		
352		704		1,056		1,408, &c.	
(1)		(2)		(3)		(4)	

Even when the 1,056 is consident, the 88 beats of 704 and 792, and of 1,320 and 1,408 are easily heard, producing the well-known "roughness" of the Fourth, while the 264, 352, and 528 boom along loudly and independently. But if the upper note is flattened, the rate of the two first beats is altered, one becoming faster and the other slower, while slow beats of an entirely different character are introduced at the high pitch of about 1,056, by the tearing apart of these formerly coincident partials. I have watched the phenomenon over and over again for different considences, and cannot imagine a better demonstration of Helmholtz's theories.

The following, among numerous other considences, have all been clearly delimitated by me in the way mentioned, and most of the

forms of dissidences which should beat 4 in a second have also been investigated: Fifth 3 : 2, Fourth 4 : 3, Major Third 5 : 4, Minor Third 6 : 5, Major Sixth 5 : 3, Sub-Fifth 7 : 5, Super-Fourth 10 : 7, Super-major Third 9 : 7, Sub-minor Sixth 14 : 9, Sub-minor Third 7 : 6, Super-major Sixth 12 : 7, Sub-minor or Harmonic Seventh 7 : 4, Super-major Second 8 : 7, Major Tone 9 : 8, Minor Tone 10 : 9, Small Major Seventh 9 : 5, and Diatomic Semitone 16 : 15. The latter was most difficult, on account of the great roughness of the intervening beats, and succeeded best in the highest case, reeds 480 and 512. The Major Sevenths 16 : 9 and 15 : 8, however, baffled me, from the excessive roughness of the other beats. I have frequently shown these effects to others, and as the instruments are accessible at the South Kensington Museum, they can easily be repeated.*

In counting the beats of the reeds with forks, I was unable to use the octaves of the forks, as they were entirely drowned by the primes of the reeds, and hence above reed 440 I was obliged to use other forks which had been previously counted with the octaves of Scheibler's forks; but below reed 220 I always counted by the partials of the reeds. By this means I was able to determine the pitch as far as 12 vib. in a sec., with tolerable certainty. Occasionally I determined the pitch of a single reed by means of several partials, beating, of course, with the primes of different forks. The following table gives the nominal numbers of some of these low reeds, with the partials used, the mean pitch determined, and the decimals of a vibration determined from the different partials, showing the close agreement of the several determinations.

Nominal number of vibrations.	Mean number of vibrations in the prime by Scheibler's forks.	Partials used, of which the pitch was determined.	Decimals of the numbers of vibrations of the prime as calculated from the pitch of the partials.
88	87·34	3, 4, 5	·34, ·33, ·36
72	71·46	4, 5, 6	·45, ·45, ·47
64	63·52	4, 5, 6	·52, ·52, ·53
48	47·68	5, 6, 7, 8, 9	·67, ·68, ·67, ·69, ·71
40	39·92	6, 7, 8, 9, 10, 11	·91, ·93, ·90, ·92, ·93, ·93
36	35·74	7, 8, 9, 10, 11, 12	·73, ·73, ·73, ·735, ·745, ·74
32	31·47	7, 8, 8, 9, 10, 11, 12, 13	·47, ·48, ·46, ·47, ·49, ·45, ·47, ·45
23	22·88	13, 19	·88, ·88
16	15·94	25, 27, 25	·94, ·94, ·92
13	12·90	20, 25	·89, ·91
12	11·90	20, 28	·88, ·91

* Since this paper was sent in to the Royal Society, I have handed to the Secretary of the Science and Art Department for the use of the South Kensington Museum, a detailed account of the method of making these experiments, with tables showing how to bring the proper reeds into action.—June 15, 1880.

After reed 12 the results were very uncertain. After reed 32 the primes were scarcely audible, and after reed 15 they were utterly inaudible. All that could be distinguished was the thumping of the beats of the upper partials, and these became gradually fainter and fainter, but were always audible even for reed 8.

The mode of finding the proper forks for any partials of reeds was simple, since the approximate pitch of the reed and the actual pitch of the fork were known. Thus for reed 12, the 20th partial would be nearly $20 \times 12 = 240$, and hence would lie between the forks 239·66 and 235·69. On trial I found the beats to be respectively 2·00 and 1·96 (the last a mean of several counts). Then $239{\cdot}66 - 20 \times$ reed $12 = 2{\cdot}00$, and $20 \times$ reed $12 - 235{\cdot}69 = 1{\cdot}96$. These give $20 \times$ reed $12 = 237{\cdot}66$ and 237·65 respectively, and consequently reed $12 = 11{\cdot}88$ vibrations. The ease and certainty with which the partials could thus be picked out was delightful to observe. As no resonance boxes or jars were used for the reeds, the objection sometimes made, that such partials are created by multiple resonances within the resonance cavity itself, falls to the ground, and the practical objective existence of the partials is established. The practical coincidence of the values of the prime from several distinct partials shows that there was no error in assigning the pitch to the proper partial. In the case of the four last reeds, 11, 10, 9, 8 only, where successive partials are so very close, did I feel any uncertainty, and hence I have not cited these results. It was for these cases extremely difficult to hear any beats at all, as distinct from the beats of the partials of the single notes themselves, as the partials that had to be used were very high and very weak. From and after reed 32 there were no musical sounds at all; indeed, even reed 64 scarcely deserved the name of a musical sound, so strong were the beats of the upper partials.

As the coefficient of temperature for reeds is unknown, a suspicion of error to a small amount attaches to all these determinations of pitch, which were made at artificial temperatures varying from 45° to 55° F. This want of correction for temperature, and liability to lose pitch from unknown circumstances, militate against the use of the reed tonometer for scientific purposes, but on account of its mumerous partials it is admirably adapted for many purposes which the stabler tuning-fork, with its small number of available partials, cannot subserve. How stable tuning-forks are, it is difficult to say. The lowest and highest forks of Scheibler's tonometer do not seem to have varied, by so much as the twentieth of a vibration since 1837, judging by my own measurements and by Professor McLeod's measurements of a fork in absolute union with the highest. A good fork, marked 438 simple vibrations (that is, 219 double vibrations) in Scheibler's own handwriting, probably about fifty years ago, is now considerably rusty, but I measure it as 218·77 double vibrations,

hence it cannot have altered more than 0·23 vibration, for which the rust fully accounts. At the same time five other forks, of a large size and very different make, after having journeyed to America and back to be measured by Professor Mayer, have, according to Professor McLeod's measures, lost ·0015, ·165, ·0205, ·0285, and ·014 vibration respectively. The second fork was not so good as the rest, and may possibly have been slightly wrenched, as it had to be screwed in and out of a wooden holder. The other losses scarcely exceed errors of observation and differences of estimation of the effects of temperature.

The points to which I wish to draw attention are, the establishment of the acceleration of beats which take place in confined spaces, and the corroboration of Helmholtz's theory of the objective existence of partial tones, by means of beats of these partial tones, either with one another or with those of other compound tones.

II. "On the Lowering of the Freezing-Point of Water by Pressure." By JAMES DEWAR, M.A., F.R.S., Jacksonian Professor of Natural Experimental Philosophy in the University of Cambridge. Received June 10, 1880.

The Cailletet pump may be conveniently employed to observe the thermal effects of compression on solid and fluid substances. Before engaging in an investigation on this subject, it was necessary to test the apparatus, and especially the manometer. For this purpose it seemed, on theoretical grounds, that observations on the lowering of the freezing-point of water by pressure would be a severe test of the accuracy of the pressure gauge, and the constancy of the records of the thermo-junctions under pressure. I am not aware of any quantitative experiments on this subject having been made under high pressures. Sir William Thomson carried the proof of the accuracy of Professor James Thomson's great theoretical discovery to a pressure of 17 atmospheres.* The experiments of Mousson ("Pogg. Annalen," 1858) were not of a quantitative character, being merely intended to show that ice at a temperature of $-18°$ C. might still be liquefied by the application of an enormous pressure. The following experiments appear to show that a convenient manometer for very high pressures, based on the observation of the freezing-point, may be easily constructed.

In all the following experiments the galvanometer, moving to the negative side, represents a cooling effect on the junction inside the

* "The Effect of Pressure in Lowering the Freezing-Point of Water experimentally demonstrated." "Phil. Mag.," 1850.