

how money grows, and writes for younger minds than does the Rev. G. Henslow, who contributes lessons on flowers, where too many technical terms are, we think, introduced, especially in the first chapter. Miss Fenwick Miller's lessons on the human body, and on ventilation, are excellent, and so are Mr. Philip Bevan's on food, and Dr. Mann's on the weather. Altogether, we congratulate the publisher on the subjects selected, and the authors he has chosen: no doubt the remainder of the lessons that are to be issued will confirm the high opinion we have formed of those already before us. W. F. B.

LETTERS TO THE EDITOR

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The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Appunn and Koenig.—Beats in Confined Air

In my letter published in NATURE (vol. xvi. p. 227), I stated that I should re-examine the question of the discrepancy between Appunn and Koenig, and inform you of the result. During the whole month of September I was engaged in very carefully counting and recounting Appunn's tonometer in the South Kensington Museum, the reeds of which had got a little out of order, a circumstance which did not interfere with the ascertainment of pitch, but disposed at once of any errors in Appunn's pendulum. I employed one of Webster's ship chronometers, which was rated to lose one second daily, and counted each set of beats repeatedly through one or two minutes. I ascertained by this means that the objections made by Koenig on the score of false pendulums and false counting were entirely groundless, and that the former determinations of the relative pitch of Koenig's forks and Appunn's reeds, made by Dr. Preyer and myself, were practically correct.

But as Lord Rayleigh pointed out in NATURE (vol. xvii. p. 12) the practical agreement of the results obtained by Professors Mayer and MacLeod, and by his own new method there described, with Koenig's, serves to show that there is a physical phenomenon to be accounted for. Mr. Bosanquet had drawn my attention to the subject several months ago, and my own experiments on the beating of disturbed consonances had led me to the same conclusion. Accordingly I had devised a series of experiments for ascertaining the fact, the nature of which I lately communicated to Lord Rayleigh; but as they required the use of two tonometers excited by separate bellows, there were difficulties in the way of making them, which I did not overcome till this week. To-day I made the first of these experiments, lasting four hours or more, and ascertained—

1. That the beats of the harmonium reeds in Appunn's tonometer are affected by taking place in a confined space of air.
2. That they are *accelerated*, and
3. That the acceleration, being roughly about one per cent., will probably, when completely ascertained, account for the discrepancy observed.

Details have been sent privately to Lord Rayleigh; they are too incomplete for publication. The experiments will require many weeks to complete with the necessary accuracy. But in the meantime I hasten to communicate an important acoustical fact which may bear upon many other phenomena besides the ascertainment of absolute pitch. ALEXANDER J. ELLIS

25, Argyll Road, Kensington, November 3

The Radiometer and its Lessons

As I now learn for the first time what are the grounds on which Prof. G. C. Foster based his inculpation of me, I may ask for a *very* few last words. I fully admit that in giving a sketch of the history of the Radiometer, I intended to attribute to Mr. Crookes that he had in the first instance put a wrong interpretation upon his own results; because I believed that this was a simple fact, well known to everybody who had followed the history of the inquiry. And Prof. Carey Foster has not called in question the correctness of my statement of the general impression which prevailed among scientific men, alike when Mr. Crookes first exhibited his radiometer at the *soirée* of the Royal

Society, and when its phenomena were discussed at the subsequent meeting. Having followed that discussion with the greatest interest, I cannot now recall one word that was not in harmony with the "direct impact" doctrine, or that suggested the idea of "heat reaction" through residual gas. If the question had been then asked, whether the rotation would continue to take place in an *open* vacuum (were such possible), or in a *perfect* vacuum,—so as to eliminate all "reaction," through residual gas, between the vanes and the containing flask,—I believe that the general, if not the unanimous, verdict would have been in the affirmative. Certainly I heard nothing from Mr. Crookes on the other side, he having previously spoken of the dependence of the "Repulsion resulting from Radiation on the presence of residual gas as 'impossible to conceive.'"

It is clear, then, that in referring to this then prevalent view, I no more wished to put Mr. Crookes in the wrong, than I wished to put in the wrong my very excellent friends among the other eminent Physicists who shared it; the special purpose of this part of my paper being to bring out, as strongly as I could, the thoroughly scientific and philosophical method in which Mr. Crookes afterwards worked himself right. If this is not expressed in as much detail as Prof. G. C. Foster would have approved, it surely afforded no adequate ground for his going out of his way to charge me with having "depreciated Mr. Crookes's merits." Yet this is the *only* ground that I can find in the whole of Prof. Carey Foster's statement, for what I could not but regard as a very grave imputation.

On Mr. Crookes's reply I shall make but a single remark, with reference to his perfectly correct citation of the latter part of my conversation with him, on the occasion of his receiving the Royal Medal. If I had not found, after the publication of my Lectures (in which I said nothing but what was respectful to Mr. Crookes), that he had himself been "digging up the hatchet" which I was quite disposed to keep buried, by giving his public attestation to the "spiritualistic" genuineness of what had been proved to be a most barefaced imposture, I should not have again brought his name into the controversy. But I felt that his greatly increased reputation as a Scientific man would do an increasing injury to what I honestly believed to be the cause of reason and common sense, not only in this country but still more in the United States.

Since the death of Prof. Hare, not a single scientific man of note (so far as I am aware) has there joined the Spiritualistic ranks; but the names of the "eminent British scientists," Messrs. Crookes and Wallace, are a "tower of strength" to the various orders of "mediums"—rapping mediums, writing mediums, drawing mediums, materialising mediums, test mediums, photographic mediums, trance mediums, healing mediums, and the like—whose names form many columns of the "Boston Trades' Directory." And the now notorious impostor, Eva Fay, has been able to appeal to the "endorsement" given to her by the "scientific tests" applied to her by "Prof. Crookes and other Fellows of the Royal Society," which had been published (I now find) by Mr. Crookes himself in the *Spiritualist* in March, 1875. Within two months of that date, as Mr. Maskelyne has publicly stated, an offer was made him (I have myself seen copies of the letters) by Eva Fay's manager, that for an adequate sum of money the "medium" should expose the whole affair, scientific tests and all, "complicating at least six big guns, the F.R.S. people," as she was not properly supported by the Spiritualists.

I have therefore felt it incumbent on me to show that in dealing with this subject Messrs. Crookes and Wallace have followed methods which are thoroughly *un-scientific*; and have been led by their "prepossession" to accept with implicit faith a number of statements which ought to be rejected as completely untrustworthy.

My call to take such a part—which I would most gladly lay aside for the scientific investigations which afford me the purest and most undisturbed enjoyment—seems to me the same as is made upon every member of the Profession to which I have the honour to belong, that he should do his utmost to cure or to mitigate *bodily* disease. The training I originally received, and the theoretical and experimental studies of forty years, have given me what I honestly believe (whether rightly or wrongly) to be a rather unusual power of dealing with this subject. Since the appearance of my Lectures I have received a large number of public assurances that they are doing good service in preventing the spread of a noxious *mental* epidemic in this country; and I have been privately informed of several instances, in which persons who had been "bitten" by this malady, have owed their recovery to my treatment. Looking to the danger which threatens us from