

encounter were not independent, and our assumptions (*however improbable*) would be therefore entirely based on our previous experience with the direct motion. Without such assumptions we should have inferred, by the ordinary laws of probability, that H would be likely to decrease. This is what I intended to imply in my previous letter; but as I had used accented and unaccented letters in my statement, I failed to make my meaning clear to Mr. Culverwell, who evidently found it difficult to understand a proof involving their use.

G. H. BRYAN.

The Unit of Heat.

I WAS glad to read Prof. Joly's communication in your issue of May 2, for I have made many efforts to call attention to the unsatisfactory nature of our present system of calorimetric measurements, and now that a more powerful voice than mine has been raised in favour of a change, I have some hopes of progress.

The indifference with which, as it appears to me, our physicists regard this matter is probably due to several causes. They ignore the fact that the science of calorimetry has recently made great strides, and that an ambiguity as to the unit, which formerly was of little consequence, has now become almost the only bar to further progress; also, as Prof. Joly has pointed out, our system of calorimetric measurements has been so wedded to the method of mixtures, that the union has (wrongly) come to be regarded as essential.

As to Prof. Joly's proposal, there is much to be said in its favour. It is practical and definite. At the same time the change would be so radical, that I should not feel justified in counting myself as his disciple in this matter without serious consideration.

My own inclination is rather in the direction of a C.G.S., or absolute unit, and the course adopted by Prof. Schuster and Mr. Gannon, in entitling their recent important communication to the Royal Society "The Specific Heat of Water," rather than the "Mechanical Equivalent of Heat," shows that a step has already been taken in this direction.

When we reflect on the attention and the labour which have been devoted to the establishment of our present system of electrical units, it is a cause for wonder that so important a unit as that of heat should have been left ill-defined and unregarded.

I would propose that at the forthcoming meeting of the British Association, the attention of Section A should be particularly directed to this matter; and it would prepare the way for such action if those who have definite proposals to make would, in the meantime, communicate them to your columns.

Cambridge.

E. H. GRIFFITHS.

REFERRING to Dr. Joly's letter last week, would it not be well definitely to adopt the "Joule" as the only fundamental unit of heat, and to realise distinctly that researches such as those of Mr. Griffiths, Prof. Rowland, and Dr. Joly are determinations of the specific heat of water and of the latent heat of steam in terms of it?

OLIVER J. LODGE.

The Examination Curve.

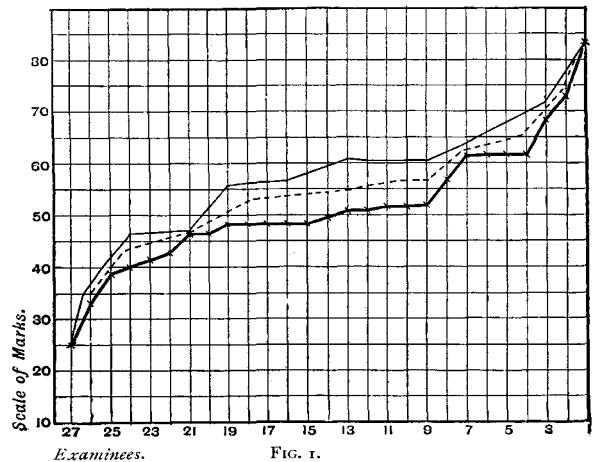
THE extremely interesting article, by Prof. Lloyd Morgan (vol. li. pp. 617-619), on the graphic representation of the marks given in an examination, and of their great use to an examiner, leads me to ask whether even this method may not be developed further with advantage to all concerned, for, as Lloyd Morgan says—"If, after an extensive set of papers has been looked over and carefully marked, an interval of time be allowed to elapse, and then the papers are gone over again, the result of this re-examination is that the head and tail remain practically unchanged, but that there is not a little redistribution among the mediocrities." In other words, the personal equation of the examiner varies, showing itself mostly in the middle of the curve.

The first thing to strike me on looking at Fig 2 (vol. li. p. 618), was the great similarity of the two halves of the curves, and on tracing it, and then turning the tracing half round so that the upper end of the traced curve became superimposed upon the lower end of the original, and *vice versa*, the similarity was so marked as to make one think, that had a larger number of papers been examined and as carefully marked as the first set, the traced curve would have covered the other.

If such be the case, why should not the examiner, after plotting the marks he thinks best, make a tracing of this curve, then

reverse it, superimposing the two ends as before, and sketch it in alongside his first curve (easily done by means of oil-paper), then, if they differed, draw a fresh curve midway between the two; subsequently re-marking his examination papers from this smoothed mean curve? An illustration may be of use; let it be founded on Fig. 1, as it contains the less smooth curve. The dark line is that of the marks first adjudged; the light line, the same curve reversed; and the dotted line, the smoothed mean curve of the two from which his papers are finally marked.

Granting that the plus variations and the minus variations on the two sides of the mean nearly balance, the question would appear to be—Would one be justified in smoothing them in accordance with the generalised results of many such series? It involves some forcing of the examiner's marking into the general mould, but would this be more than sufficient to correct



his personal equation? On the other hand, the two halves—say from paucity of examiners—might be so dissimilar, that the mean curve would differ very much from the original form. In this case, would it be possible to give any general rule whereby one could be guided whether to adopt the mean curve, or to remain satisfied with the original marks given?

In Herbert Spencer's "Principles of Sociology," (vol. i. p. 88) are many references to the fact that "the children of Australians, of Negroes in the United States, of Negroes on the Nile, of Andamanese, of New Zealanders, of Sandwich Islanders [and others], are quicker than European children in acquiring simple ideas, but presently stop short from inability to grasp the complex ideas readily grasped by European children, when they arrive at them."

F. HOWARD COLLINS.

April 29.

Teaching Young Pheasants to Peck.

IT may interest Prof. Lloyd Morgan and others to know that when Asamese find newly hatched chicks in the jungles, they have a system of teaching the little ones to peck and pick up food, without which, I am told, many of them would die.

Walking down a road one morning with a neighbour, we suddenly noticed a little ball of fluff between my feet, and I could hardly avoid stepping on it, as it stuck close to me; almost immediately another appeared at my friend's feet, and we saw they were newly-hatched pheasants, the mother probably carried off by some wild cat.

As it was difficult to walk with these little things running so close and in the way, we lifted them into the short grass alongside, and hurried on some fifty yards.

On returning we had forgotten them, but one ran out, and so pertinaciously stuck to my boots, that to save it I put it into my pocket, and on our arrival at the bungalow tried to feed it with small fragments of hard-boiled egg, rice, and white ants. Of all these it took no notice.

Next morning the other chick was found at the foot of the bungalow steps, having probably followed us unnoticed the day before. I then called my "Babu," as I could not get them to eat, and he said "they must be taught."

He put the gauze wire cover they were under, and the crushed