which occur in the epithelium of the gills and labial tentacles of the Marennes oyster.

I also showed that such cells are present in the common oysters, but that the granules they contain are not green. I further showed that these cells occur abundantly on the *surface* of the gills, crawling about and exhibiting amœboid movement. I also showed that the Marennes oysters are specially fed upon *Navicula ostrearia* which contains a highly refractory blue pigment "Marennin," and I *inferred* that the granular cells of the gills derive their colour from the blue pigment of the naviculæ—since it was shown long ago by Gaillon (in 1824) that the *huitres de Marennes* are purposely placed by the oyster-culturist into tanks containing the *Navicula ostrearia*; that when placed there they have gills of the usual yellow-brown colour, but rapidly acquire the green colour; that they actually feed on the *Navicula ostrearia*, and that when removed from this article of diet, they lose the green colour of gills.

The inference that the "granular cells" are to be regarded as wandering phagocytes, was not first published by me; and, though I have no doubt of its justification, I may point out that it is an interpretation, and not an observation of fact.

Lastly, let me say that I showed by chemical analysis that the green colour of the oyster's gill is *not* due to any metallic base—either copper, iron, or chromium. The statement made by Carazzi that there is "abbondanza di sesqui-ossido di ferro" in the mud of the tanks where the oysters are fed, is therefore doubly futile. Every one knows that such mud contains abundance of iron; but as there is no iron in the green pigment of the oyster, it is useless to draw attention to the iron in the mud.

E. RAY LANKESTER.

Oxford, May 4.

The Origin of the Cultivated Cineraria.

I MADE two objections to Mr. Dyer's account of the history of the Cineraria; the careful reader will observe that his letter meets neither. Mr. Dyer informed us that the cultivated Cinerarias were produced "*by the gradual accumulation of small variations*," *i.e.* without the selection of definite sports. My object in adducing historical evidence of Cineraria sports was to prevent Mr. Dyer's pronouncement from being repeated without further evidence. That purpose I think has been attained; for I notice that in now restating his account Mr. Dyer does not refer to the point, though it was the object of his original exhibition of the Cineraria to the Royal Society. That the Cineraria was an excellent "illustration of the amount of variation which could be brought about under artificial conditions in a limited time" I should be the last to dispute. As I showed in my first letter, there is evidence that the time was very short indeed.

Compared with this point, the second question—that of the hybrid origin of cultivated Cinerarias—is of subordinate interest. For the view that they were originally hybrids, resulting from crosses between *C. cruenta*, *C. lanata*, and other species, I have given the evidence, quoting the explicit statement of contemporaries and the almost universal opinion of practical gardeners, with references to the sources of information. Mr. Dyer, however (with him Mr. Rolfe) declares that they are descended from *C. cruenta* alone. Is this statement a mere inference from the want of likeness between particular cultivated Cinerarias and the wild species, or have Mr. Dyer and Mr. Rolfe evidence of a more substantial character? Of course these authorities may be right, and the rest who have written on the matter may be wrong; but I ask for proof of this, and the request can hardly be thought unreasonable.

Mr. Dyer has referred to a remark I made at the meeting respecting the Camellia. At the risk of diverting attention from the real issues, I feel bound to speak of this, for I was then in the wrong. In justice the circumstances must be stated. Speaking of the Cineraria, Mr. Dyer declared that though the flowers have changed so much, the foliage, which had not been an object of Selection, still resembled that of his wild plant. I replied that though this might be true of the Cineraria, it led to no universal induction, for it is well known that the foliage of many plants selected solely for their flowers or for their fruits had varied greatly. As an illustration taken on the spur of the moment, I said that though the matter had not come within my own observation, there was, I believed, a passage in one of Darwin's books to the effect that the foliage of the several kinds of Camellia differed so much that they could be recognised by it alone. Upon Mr. Dyer interjecting that this was not true, I immediately gave up the illustration as not coming within my own knowledge, and substituted that of the Apple, of which I myself know several kinds to have distinct and characteristic foliage. Such examples may be multiplied indefinitely. Now the passage in Darwin is as follows:—"Verlot mentions a gardener who could distinguish 150 kinds of Camellia when not in flower" ("Animals and Plants," ed. 1885, II. chap. xxii. p. 238); but Darwin takes the case as an illustration of the fact that structures "though appearing to an unpractised eye absolutely undistinguishable, yet really differ." My use of this case was therefore a wrong one, and as Mr. Dyer has thought fit again to refer to the matter, I take the opportunity of withdrawing it once more.

W. BATESON.

St. John's College, Cambridge, May 5.

The Assumptions in Boltzmann's Minimum Theorem.

MR. CULVERWELL'S letter in your issue of April 18 leaves many important points in connection with the reversibility of Boltzmann's Minimum Theorem untouched. On the question as to what different people mean (or think they mean) when they assert that the theorem is true, enough has already been said. What we want to know is what assumptions are involved in the mathematical proofs of the theorem, why they have to be made, and for what systems they are likely to hold. This question has been ably treated by Mr. Burbury, but in view of Prof. Boltzmann's assertion that the theorem is one of probability, it is desirable to examine more fully where probability considerations enter into proofs such as Dr. Watson's, which contain no explicit reference to them.

Dr. Watson starts by assuming two sets of molecules so distributed that the *numbers* having coordinates and momenta within the limits of the corresponding differentials are

$$F(P_1 \ldots Q_m)dP_1 \ldots dQ_m \text{ and } f(p_1 \ldots q_n)dp_1 \ldots dq_n.$$

If, however, the differential elements are taken very small (as when we consider a volume-element comparable with molecular dimensions), these expressions no longer represent numbers of molecules, and it is *assumed* that in this case they represent the *probabilities* of a molecule having coordinates and momenta within the given limits.

It is then necessary to *assume* that *the probabilities for the two kinds of molecules are independent of each other*. This assumption was pointed out to me by Mr. Burbury, and is what I intended to imply in my previous letter when I said that Dr. Watson's assumption was more *natural* than any other. Under these circumstances alone can we assert that the probability of a given combination of coordinates and momenta of *two* molecules is proportional to

$$FdP_1 \ldots dQ_n \times fdp_1 \ldots dq_n$$

To make the proof independent of the choice of coordinates, let $y_1 \ldots y_{m+n}$ be *any other* system of coordinates specifying the *pair* of molecules, so chosen that $y_1 = 0$ at the beginning of an encounter. Then if $x_1 \ldots x_{m+n}$ denote the corresponding momenta, we may employ the theorem proved in my last British Association Report, § 14, to write the above expression in the form

$$Ff/dy_1dy_2 \ldots dy_n dx_1 \ldots dx_{m+n}.$$

and if we write $(dy_1/dt)dt$ for dy_1, the probability of a configuration in which an encounter will take place in the time-element dt becomes

$$Ff/dy_2 \ldots dx_{m+n}(dy_1/dt)dt$$

corresponding to Watson's expression with (dy_1/dt) in place of (dq_n/dt). This step involves the assumption (made above) that dy_1 is small in comparison with the dimensions of a molecule.

From this point on Dr. Watson's proof is easy. But it will be seen that the probabilities for two molecules are not independent of each other *after* a collision between them. The method would fail if the same pair of molecules were likely to collide repeatedly. Thus the Minimum Theorem depends on the free motions of the molecules quite as much as on the collisions themselves, and it only applies to gases whose molecules mix freely among each other between collisions, not to media where they are densely crowded. In such cases, however, we have Mr. Burbury's investigation (*Phil. Mag.* January 1894).

If we were to reverse the motion exactly, we should have one in which the probabilities for two molecules *before* an 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 versâ*, 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

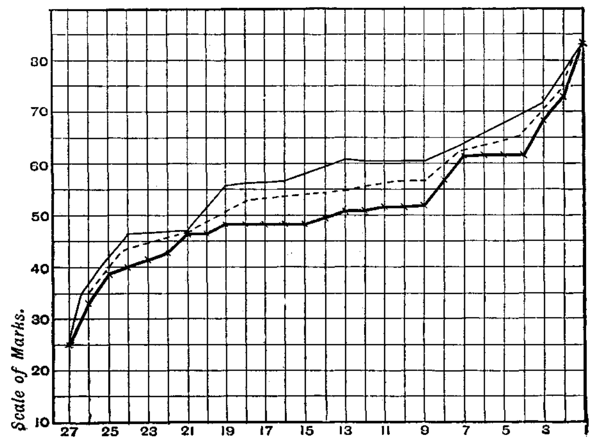

FIG. 1.

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