

or not. Prof. Capellini, of whom I asked the question at the meeting of the British Association at Manchester, could not answer me. *Prima facie* we should certainly expect the Italian evidence to support the French, but this is by no means the conclusion to be drawn from text-books, in which it is generally taken for granted that in Italy the elephant and mastodon have been found at the same horizon.

The question is one of very great interest and importance, and an answer to it would be especially valuable to me. Perhaps some of your readers may have the means of answering it.

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21 Earl's Court Square, February 28.

True Average of Observations?

I HAVE long been dissatisfied with the method of taking the arithmetic mean as the most probable value of a *comparatively few* direct observations of a quantity. This is certainly the legitimate result of the theory of probability, or "method of least squares," when one knows nothing to guide one in giving more weight to one than to another observation.

But without knowing anything of the conditions under which the observations were made, or, otherwise, no choice among them being possible by considering these conditions, still, when one comes to compare the results among themselves, this comparison seems to me to afford means of judging between them. Thus, if all the results are plotted on sectional paper, they are found to be grouped closely together at one place and to be scattered wide apart at others. Now the most probable result (whatever be the right method of finding it) lies certainly somewhere about the place of close grouping; and it seems fair to consider those results that come near this place as the *better ones*, and to allow to them *more weight* than to the others in calculating the mean.

If the observations were extremely numerous, there can be no objection to taking the arithmetic mean as the true probable value. But one has usually to content one's self with a few only, and in order to get a better approximation in this case I have constructed the following formula. I would be glad if some of your correspondents will express their opinions as to its legitimacy. In a case of this kind one ought not to trust entirely to one's own judgment; one should submit one's own judgment to be checked by that of several others.

The method I propose is as follows.

First fix upper and lower limits outside which the true value cannot possibly lie, and reject absolutely all measurements outside these limits. The result will not be appreciably affected by taking these limits a little higher or lower, and it is better to err in taking them too wide apart than *vice versa*. One usually has, or ought to have, a general notion of the quantity sought for, sufficient to determine these limits; but if this be not so, they may be determined by adding to and subtracting from the arithmetic mean what is thought to be the maximum possible error.

Let x_1, x_2, x_3 , &c., be the *excesses* of the various measurements above the lower of the above possible limits. Let x_0 be the *excess* above the same limit of the as yet unknown most probable value as determined by the formula below.

Attach to each x the weight $\left\{ 1 - \left(\frac{x - x_0}{x_0} \right)^2 \right\}$, and take as x_0 the mean of the x 's with these weights attached.

Note that equal weights are given to measurements equally above and below x_0 . Also to an x coinciding with the lower possible limit, a weight zero is given. Zero weight is also given to an x as much above x_0 as the lower possible limit is below it.

The rule results in the following formula:—

$$\text{Weight for } x = 1 - \left(\frac{x - x_0}{x_0} \right)^2 = \frac{2x_0x - x^2}{x_0^2},$$

$$x \times \text{weight} = \frac{2x_0x^2 - x^3}{x_0^2}.$$

Therefore, the mean equals—

$$x_0 = \frac{2x_0\sum x^2 - \sum x^3}{2x_0\sum x - \sum x^2}.$$

This is a quadratic for x_0 , the solution of which is—

$$x_0 = \frac{3\sum x^2}{4\sum x} \left\{ 1 + \sqrt{1 - \frac{8\sum x\sum x^3}{(\sum x)^3}} \right\}.$$

Of course the labour of finding this mean is greater than that of finding the arithmetic mean; it involves summing the first,

second, and third powers. But the method is only intended to be used when the number of values to be dealt with is not large, and with the help of a table of squares, cubes, and square roots, the work is not really very laborious.

It is easy to prove that this result is identical with the arithmetic mean in the following three cases: (1) all the x 's equal; (2) the x 's all equidistant, *i.e.* forming an arithmetic progression; (3) the x 's infinitely numerous.

The practical meaning of the rule may perhaps be made clearer by the annexed table, giving the weights attachable to various values of x where x_0 is taken equal to unity.

x	0	1	2	3	4	5	6	7	8	9
Weight	0	1	0.99	0.96	0.91	0.84	0.75	0.64	0.51	0.36

The following is a numerical example:—

x	x^2	x^3
1.73	2.993	5.178
.89	.792	.705
.42	.176	.074
1.21	1.464	1.7715
1.17	1.369	1.6016

$$\sum x = 5.42 \quad \sum x^2 = 6.794 \quad \sum x^3 = 9.330$$

$$\sqrt{1 - \frac{8\sum x\sum x^3}{9(\sum x)^3}} = .162 \text{ and } x_0 = 1.0925.$$

The arithmetic mean or $\frac{\sum x}{5} = 1.084$.

Mason College, February 4.

ROBERT H. SMITH.

Crepuscular Rays in China.

IMMEDIATELY after sunset enormous rays of light are frequently seen spreading from the part of the horizon where the sun has disappeared, and also—though somewhat fainter—from the opposite part of the horizon. Sometimes the rays stretch right across the sky, and when strongly developed they appear first in the east, and then in the west, and resemble auroral rays, glowing in a yellow or red colour, while the sky between the rays is deep blue or greenish. They appear to be caused by invisible cirro-stratus clouds high up in the air. This phenomenon is never seen in England, or at any rate it is by no means so conspicuous as here. Ancient Greek mariners may have had their imagination impressed by a similar phenomenon, *ῥοδοδάκτυλος ἥως* being so frequently mentioned in Homer.

Crepuscular rays at sunrise or sunset are seen at all seasons in Southern China, but they are most frequent at the height of the typhoon season, and most intense just before typhoons, which latter are indicated beforehand by crepuscular rays as well as by halos.

The following table exhibits the number of evenings when strong crepuscular rays were registered in each month of the past three years, and also the mean monthly frequency of the strongly developed phenomenon:—

	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1885	—	—	3	2	4	3	—	—
1886	—	1	1	1	3	7	—	1
1887	1	—	—	2	3	—	—	—
Mean	0.3	0.3	1.3	1.7	3.3	3.3	0.0	0.3

W. DOBERCK.

Hong Kong Observatory, December 31, 1887.

"An Unusual Rainbow."

I READ with interest a letter with the above heading in NATURE (vol. xxxvi. p. 581) from Mr. S. A. Hill of Allahabad, India, of date September 18, 1887. He describes a brilliant rainbow which he saw after the sun had set, and states that such a phenomenon "must be of rare occurrence," and that he had "never before seen anything similar, nor read anywhere a description of a rainbow after sunset." I had not read his letter when, on the

evening of the 1st inst. I observed a similar rainbow. I saw it first at 7h. 25m. p.m., the registered time of sunset here for that day. It lasted for nearly fifteen minutes. The western horizon was cloudy, and the sunset a fine one. The bow was exceedingly brilliant, and as far as I could judge, a perfect semicircle, the ends of the arc being about 4° above the horizon. There was a secondary bow equally perfect, and of remarkable brightness; the brilliant glow below the primary, and the marked dulness between it and the secondary, added to the beauty of the sight. After reading Mr. Hill's letter, I published my observations in a letter to the *Argus*, that others might confirm or correct them. I have received six replies, all in accord with my observations. One of my correspondents informed me that he had, some years ago, seen a lunar rainbow formed just before the moon had risen.

H. M. ANDREW.

The University, Melbourne, January 26.

The Nest of the Flamingo.

IN an interesting article by Mr. Bowdler Sharpe, entitled "Ornithology at South Kensington," published in the December number of the *English Illustrated Magazine*, there is a description and figure of the flamingo's nest, and an opinion is expressed that the previously-held ideas about the nest being tall, and the female sitting upon it in a straddling manner, might now be considered as exploded.

I have seen numbers of these tall nests in the shallow pans of water—or "vleys," as they are locally called—in Bushmanland, Cape Colony, particularly at Klaver Vley. These quaint nests were built in the water where it was a few inches deep, and at a considerable distance from the shore. They were conical in form, about 18 inches high, and 6 inches in diameter at the top, with a shallow basin-like cavity for the eggs; built, so far as I can recollect, of slimy mud. To perform the office of incubation, the bird must have straddled over the nest. The species no doubt differs from the one described in the article. There should be no difficulty in securing specimens of these nests. Possibly the object aimed at in building the nests in the water is to secure them against some enemy, and the height of the nest, besides conveniencing the long-legged owner, provides for the rising of the water-level.

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Dynamical Units and Nomenclature.

IN his review of Prof. MacGregor's "Kinematics and Dynamics," on page 361, Prof. Greenhill tilts a lance against those whom he terms mathematical precisionists. I do not know this book, and I hold no brief in its defence; but as I owe to these precisionists whatever clear ideas I have on mechanics, I feel bound to enter into the lists on their behalf, little as they need my aid.

Both the precisionists and practical men start with the same two dynamical quantities, which they respectively call *mass* and *force*, *weight* and *force*; of these they select arbitrary units, and respectively name them *pound* and *pound-weight*, *weight-of-a-pound* and *force-of-a-pound* (or *pound-weight* and *pound-force*).

To the single word *pound* the practical man does not, so far as I know, attach any single definite idea, and he cannot, therefore, use this word singly without introducing possible confusion; for it characterizes matter and force equally, and yet is neither. On this view Prof. Greenhill's own expression "the attraction of the earth on a pound," should for accuracy and consistency be "the attraction of the earth on the weight of a pound (or on a pound-weight)."

To the precisionist a pound is a certain mass, just as a foot is a certain length, so that the practical man's "weight of a pound" is simply the "pound" of the precisionist, who would no more dream of 'distinguishing' it as "the mass of a pound" than of distinguishing a foot as "the length of a foot."

The attraction of the earth on a certain amount of matter is called "the force of 10 pounds" by practical men, and "the weight of 10 pounds" by precisionists: these are purely definitions, so that the phrases are absolutely equivalent. If, then, in the specification of a force produced otherwise than by the attraction of the earth a precisionist is required to speak of it as "a force equal to the weight of 10 pounds," the practical man must follow suit with "a force equal to the force of 10 pounds." These expressions stand, or rather fall, together, and the con-

sistent precisionist would specify the force as "10 pounds-weight" merely.

If, however, a *body*, such as a brickbat or the iron block supplied with a balance and called a "pound weight," is to be introduced into the specification, a precisionist would very properly say "a force equal to the weight of 10 brickbats or of 10 pound-weights"; and the *complete* idea hereby conveyed cannot be expressed by the practical man otherwise than by "the attraction of the earth on 10 brickbats or on 10 pound-weights."

In no way, then, is "a force equal to the weight of a mass of 10 pound-weights," the precisionist equivalent of the practical "force of 10 pounds," nor is it even consonant with precisionist nomenclature.

Since, therefore, the precisionist uses *mass*, *force*, *pound*, *pound-weight*, as the exact equivalents of the practical man's *weight*, *force*, *weight-of-a-pound*, *force-of-a-pound*, the advantage does not seem to lie on the side of the latter, more especially when he is untrue to himself in loosely using the word "weight" as often in the sense of "force" as according to his definition.

But so far both practical men and precisionists labour under the immense disadvantage of dealing with a variable force-unit which can be made precise only by a specification of place; and it is greatly to the credit of the latter that they have introduced a simple invariable force-unit by which all forces, whether due to gravitation or other physical action, may be expressed absolutely in a form which allows of direct comparison between them. With this unit *ma* is the correct measure of a force, and when Prof. Greenhill speaks of "the mathematician straining after the equation $F = ma$, when using the gravitation unit of force," I utterly fail to understand what is meant, considering that this expression of a force necessarily implies an *absolute* force-unit; and I further feel strongly tempted to deny that either for this unintelligible operation or for any other the precisionist ever uses *g* pounds as a mass-unit, though, if he ever does use a variable mass-unit in measuring the invariable mass of a body, he is surely countenanced by the practical man who does not hesitate to use a variable force-unit in measuring the invariable force exerted by a given spring compressed to a given extent. I might further add that the precisionist *never* measures the weight of a body in "pounds," even if he denotes it by *w*, and that, if he does sometimes denote this variable force by the same number irrespective of place, it is only when using the practical man's variable force unit.

With regard to confusion arising from the use of the equation $w = mg$ any more than from the use of the equation $w = m$, this would be to me inconceivable, did I not notice that Prof. Greenhill uses the phrase "if the equation $w = mg$ is supposed to be used with absolute units." Does there indeed exist a single man who thinks that this equation can be used with other than absolute units? If such there be, to him certainly will confusion be not only possible, but probable too, and deservedly so; but to others there can surely be no more confusion in expressing a (precisionist) weight as *m* or *mg* indifferently than in expressing an angle as θ or $180\theta/\pi$, it being of course premised that the proper unit—(precisionist) *pound weight* or *poundal*, *radian* or *degree*—is named.

Further, how it can be a solecism to measure pressure in poundals per square foot any more than in pounds-weight per square inch—which latter is the precisionist equivalent of what an engineer would loosely and most inaccurately call "pounds"—I am at a loss to understand, since pressure is the measure of the distribution of force over area, and a poundal is as much a force as "the force of a pound," and very much more definite. And how the expression of the (precisionist) weight of a body in poundals rather than in pounds-weight is a solecism also demands explanation.

Lastly, I must seriously protest against the suggestion that a precisionist should ever ask for, or want to buy, "half a poundal of tea": what he wants is the tea itself, the substance of it and not the earth's action upon it, and very rightly and properly he asks for "half a pound," which the consistent practical man would have to term "the weight of half a pound."

In the above I am not concerned to defend the practice of those mathematicians who select fantastic units of mass or force as a foundation for some puzzling questions of no utility whatever: I have merely attempted to define the position of the physicist or precisionist, and to rebut *seriatim* the charges brought against him in Prof. Greenhill's criticism.

February 27.

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