is 'pulled gently and steadily,' is the reason that the fifty-pound weight acquires a greater velocity, because the weight resists less (if so, then resistance is less than itself), or because the time of application is greater?

In elementary works on physics, the word 'inertia' should be seldom used, lest the pupil acquire the impression that inertia is an entity. Most exact writers, foremost among whom is J. Clerk Maxwell, carefully avoid the use of the word. But if Dr. Hall's quasi-definition, given in the last paragraph of the article under discussion, is to be accepted, then must the word necessarily become one of constant use. It is a pity that Maxwell has not given us a definition of 'an inertia unit.' We shall be pleased to have Dr. Hall supply the desideratum. A. P. GAGE.

In my article on 'Inertia' I was mainly concerned for the distinct recognition of a physical fact. My interest in the word 'inertia' was secondary. Professor Mendenhall and Mr. Gage appear to deny the reality of the 'resistance' of which I spoke in defining inertia. I said, "Matter possesses a property in virtue of which it offers resistance to an agency which *is setting* it in motion." Professor Mendenhall attempts to avoid the idea of a resistance in explaining the fact that force is required to set a body in motion, by speaking of the *work* done. The attempt seems to me entirely unsuccessful, unless he has some unusual definition of the word 'work.' According to Maxwell (Theory of heat, 4th ed., p. 87), 'work is done when resistance is overcome;' and, though he does not say that work is done *only* when resistance is overcome, no reader of Maxwell will deny that he meant that. This, by the way, is the only reply I need make to my critics' use of Maxwell's tea-and-sugar illustration; for certainly Maxwell considered setting a mass in motion to be doing work. With this I leave the question of physical fact, and come to that of the word or words used to denote that property which I have called 'inertia.'

In using the word 'inertia' as I did, I knew perfectly well that I assigned to it a meaning sometimes given to the word 'mass.' I knew that Maxwell, in the very passage of which I quoted a part, and of which Dr. Hastings has quoted the whole, used 'mass' as I have used 'inertia.' It was my belief, however, and it still is, that Maxwell, in that famous chapter, used 'mass' in two senses. He does use it as I have used 'inertia,' and in that case defines it as a '*property* of matter' (the italics are mine). Elsewhere in the same chapter he says, "What is really invariable is the *quantity of matter* in the body, or what is called in scientific language the *mass* of the body," etc. (the italics are mine).

As to Maxwell's use of the word 'inertia,' I was in error. I certainly spoke as if he gave undoubted sanction to the word in the sense in which I have used it. This I had no right to do, for he merely states what others have meant by this word. Any one, by reading the passage which Dr. Hastings has quoted from Maxwell, will see all the excuse I have to offer for my blunder.

Dr. Hastings admits that Thomson and Tait use the word 'inertia' to denote that property of matter for which I have used the same name; but he says that their statement is confused. This criticism is just; but it is irrelevant, unless Dr. Hastings means to imply that Thomson and Tait wrote 'inertia' where, in a clearer moment, they would have written 'mass.' Moreover, his commendation of their definition of the latter word might lead one to infer that Thomson and Tait use 'mass' as Maxwell does in the passage he has quoted. What, then, is their definition of 'mass'? It reads thus: '*The quantity of matter* in a body, or, as we now call it, the *mass* of a body," etc. (art. 208).

And now what is the practice of my critics in the use of the words 'inertia' and 'mass'? In the preface of Mr. Gage's Elements of physics, we read, "Dr. C. S. Hastings of Johns Hopkins university has read the larger portion in manuscript, and the remainder in proof-sheets." On p. 8 of this book I find, "By the *mass of a body* we understand *the quantity of matter in it*," and on p. 20, "The term *mass* is equivalent to the expression *quantity of matter*." Of course, the word 'mass' occurs in many other passages of the book; but I have discovered no case in which it appears to denote any thing but *quantity of matter*.

As to the use of 'inertia' in the same book, on p. 90 I find, "This inability is called *inertia*. Evidently the term ought never to be employed to denote a hindrance to motion or rest." But when we come to the subject of centrifugal force, p. 101, we read, "Centrifugal force has, in reality, no existence: the results that are commonly attributed to it are due entirely to the tendency of moving bodies to move in straight lines in consequence of their inertia."

Now, one of these results is the maintenance of the solar system. Why do not the planets, obeying the law of gravitation, fall into the sun? According to the teachings of this book, we must answer, "Simply because of their 'utter inability' to put themselves in motion, or to stop themselves, although this inability must never be understood as a 'hindrance to motion or rest.'" A little farther on in the book we read, it is true, that "to produce circular motion, the centripetal force must be increased . . . as the mass increases." 'Mass' enters here when the book speaks of numerical relations; but we see, that, when it attempts to *explain* 'centripetal force,' it appeals to 'inertia,' and says nothing whatever of 'mass.'

I think it not too much to claim that 'mass,' used to denote that property of matter which Thomson and Tait call 'inertia,' is comparatively rare, while one can hardly take up a book upon physics without finding 'mass' used in the sense of 'quantity of matter.' That an exceedingly intimate relation exists between inertia as I have defined it, and mass as commonly defined, I am well aware. Thomson and Tait's words are, "This, the *inertia* of matter, is proportional to the quantity of matter in the body." I should prefer to say, bodies of equal inertia (see the last paragraph of my article on 'Inertia') are assumed to contain equal quantities of matter. Quantity of matter, in this sense, is called 'mass.'

If it seems best to use 'mass' to denote also the property of matter which Maxwell undoubtedly does denote by it, let us so use it; and, by all means, let its double meaning be distinctly recognized in the elementary text-books. To me it seems far wiser, however, to use the two words, 'inertia' and 'mass,' substantially as Thomson and Tait use them, and to rigorously exclude from the text-books the comparatively useless 'inability' definition of inertia.

E. H. HALL.

Silk-culture in the colonies.

The term 'silk-balls' was doubtless employed at times to designate cocoons; but that is quite different from 'raw-silk' and 'raw-silk balls,' which, as we stated, might more appropriately apply to the twisted hanks of raw silk which are so doubled and

tied as to suggest such a designation. The choking or drying of the cocoons was in colonial days a part of silk-raising, and not of silk-reeling; and, while reeling-establishments may undertake to choke the cocoons brought in by the raisers in their immediate neighborhood or by agents, the marketing of fresh cocoons must necessarily be limited in time and distance. They cannot bear pressure without injury, and all baled cocoons must needs be choked. One is hardly justified in comparing the methods of colonial times with those in vogue to-day in France, where modern steam filatures and railroads have produced such profound modifications. We cannot see how choked cocoons, which have but one-third to one-fourth the weight of fresh cocoons, can be marketed at the same rates as the fresh cocoons. The term 'green' cocoons is often used in English as the equivalent of fresh cocoons; but, as quoted in the French markets of to-day, the word 'green' (*vert*) refers to those of a green or greenish color. Perhaps this may explain the puzzle. C. V. RILEY.

Thermometer exposure.

In No. 58 of *Science*, Professor Mendenhall calls attention to interesting differences of the minima temperatures on cold, still nights of the winter. I agree with him that a difference of exposure, and proximity to buildings, may explain a difference in reading; but it is impossible to explain by them alone the enormous difference noticed in Columbus (27.3° F.). There must have been, besides, one or another of the following conditions, probably both. When the conditions are favorable to radiation, and the night is still, the lowest strata of the air are mostly cooled by contact with the cold, upper surface of the ground; and more so if there is snow, and a so-called inversion of temperature is produced. The temperature rises from the lowest strata to a certain height. Examples of this can be found in the observations at Pulkova, near St. Petersburg. A thermometer placed at the height of seventy-eight feet was almost constantly higher than one at six feet above ground at eight P.M. In August, on clear days, the mean difference was 2.1° F., and once in September it was 5.2° F. In the months from December to March, when the ground is covered with snow, even at one P.M. the upper thermometer was higher than the lower; the mean difference on clear days of December and January at one P.M. amounting to 1.3° F., and once it amounted to 4.1° F.

The same results were obtained by experiments made at Kew, by direction of the meteorological office. The minima were lower at a height of twenty-one feet above ground than at a hundred and twenty feet; and on one occasion, at nine P.M., during a fog, the latter was higher by 10.8° F. than the former.

Now, most of the signal-service stations must have comparatively high minima, not only because they are mostly located in the interior of cities, but because the thermometers are often placed very high above the ground, at the level of the fifth or sixth story of city buildings. Probably the stations of the Ohio state service are placed lower.

Besides the height of thermometers above the ground, what I call the 'topographical conditions' are of importance. At an equal distance from the level of the ground, under conditions favorable to radiation, there will be much lower minima in valleys than on hills. This is caused by the descent of the coldest and heaviest air to the valley, and also by the fact that in a valley the air is in the vicinity of a greater surface of the ground. During the anticyclone of Dec. 19–30, 1879, the summit of Mont Verdun, near Lyons, France, had a mean temperature of —1.7° C.; and the Parc de la Tête d'Or, in the city, situated four hundred and fifty metres lower, a mean of —7.1° C. The mean minima differed by more than 12°C. Very likely the observations of the state service at Columbus were made on lower ground than those of the signal-service. Where anticyclones in winter are common in high latitudes, with the ground covered with snow, the mean temperatures of the winter months must be considerably colder in valleys than on the surrounding hills and mountain slopes, as the insolation during the day interferes but slightly, and not at all during some days at points beyond the polar circles, with the equilibrium of air strata obtained during the night.

This cold of the nights in valleys, subjecting plants to freezing on nights when those that grow on hills are spared, is well known. Perhaps it is less noticed in the United States, as there low temperatures are oftener accompanied by high winds than in Europe. The olive-cultivators in southern France, and the coffee-growers in the hilly districts of the province of San Paulo, southern Brazil, know this so well that they do not plant their trees in valleys, from fear of frosts. A. WOEIKOF.

St. Petersburg.

Dalmanites in the lower carboniferous rocks.

During a recent geological excursion near this city, one of our party, Mr. Henry Lane, found and pointed out to me a trilobite, which I extracted from the stone myself. The rock on which we were working was the upper part of the Cuyahoga shale of the Waverly group of Ohio, now universally, I believe, referred to the lower carboniferous system. The only genus hitherto reported from these rocks in America is Phillipsia, with the exception of two species of Proetus scarcely distinguishable from Phillipsia. The specimen in question, however, distinctly differs from both of these in the pygidium, the only part yet obtained. Instead of the evenly rounded and margined tail of those genera, it shows the flabellate and fimbriate form of Dalmanites. The occurrence of this genus or of this type of trilobite, so high in the geological series, is both surprising and 'uncanonical.'

E. W. CLAYPOLE.

Buchtel college, Akron, O.,
April 14.

'A curious optical phenomenon.'

Except in one curious point, 'F. J. S.'s' latest experiment (*Science*, No. 63, p. 475) obviously accords with my note (same page). Apparently, the virtual image is three feet *in front* of him, or nine feet from the wires, since the phantom rises when he bows; the slats are seventeen and two-thirds times wider apart than the wires, from centre to centre; and every fourth wire hides every third slat, while the next wire but one hides a slat-shadow. But how can thirty slats and their shadows thus give *twelve* dark phantom lines? With his telescope, 'F. J. S.' may find that two of them, least perfect, are where wires cross the *frame* of the blind.

Two words of mine, three lines from the bottom of the page, require correction. The size of the image is not 'very nearly' as described, but *exactly* so. If this image could become an actual screen, then *its* image, in turn, would be the farther screen; and any line through a wire-crossing in either of the three screens would meet the other two at points *quasi*-homologous to each other.

JAMES EDWARD OLIVER.

Cornell university, April 29.