the rate of 8-13 per hour, of which only a small proportion still were Leonids, and by Mr. Blakeley at Dewsbury, who noted, from 12 to 4½ a.m., with two hours of quite clear sky at last, forty meteors, never appearing faster than fourteen per hour, among which were twelve Leonids with an hourly rate never exceeding six. Four or five of the Leonids were very bright, and their radiant-point was well defined at 150° , $+ 24^{\circ}$; but the whole display fell considerably short in brightness of that observed in 1895.¹

Perhaps the three close-following meteor-showers may all have fallen a little later this year than the figure represents them; but the first few hours of watches on the mornings of November 13 and 14 should at least, as the diagram shows, have been (as they were) both slenderly productive times, while a bright meteor-shower should have prevailed (as it did, rather vividly) on the morning of November 15. On last year's November dates the case was different, a rather bright array of Leonids being seen on the morning of November 14, followed by a smaller one on November 15, while after the latter date a generally clouded state of the sky in England prevented further observations. At Bridgwater, on November 14, Mr. Corder mapped eleven Leonids between 2 and 4½ a.m., and found their radiant point at 152° , $+ 23^{\circ}$. Five or six Leonids were recorded here between 12 and 3 a.m. on that morning, with a radiant-point at 151° , $+ 23^{\circ}$, to which were also traced the paths of two Leonids mapped between 1 and 2½ a.m. on the next morning of November 15, when the shower seemed to be passing off, and when clouds on that and in the next night's watch prevented a complete view of the shower from being obtained at Bridgwater, and generally in England. The spectacle was thus most prominently seen last year on the mornings of November 14 and 15, while it was this year most conspicuous on the morning of November 15; and this agrees with this trial-figure's indication that the early and middle branches of the shower should have occurred at favourable morning hours on November 14 and 15 last year for English observations; but at such late hours this year, as to be only weakly visible in a few slight foretokenings on November 13 and 14, compared with the full brightness of the after-shower brought into view on the morning of the 15th by the same advance in hour together of all the three members of the triple concurrence. If the present positions and durations, then, of these component showers may be assumed to have been depicted in the figure with approximate correctness, a full view of the end and middle portions of the first shower beginning to appear in the next two years, 1897 and 1898 after midnight (a.m.) on November 14, will afford means of comparing in strength and brightness those phases of the leading shower with the middle and early parts respectively of the after-coursing one, then still well visible on November 15, to gauge their relative extents in length and width, and the relative looseness or compactness of their structures, which may perhaps not offer themselves again so favourably for some years.

It will also be very useful in coming years' watches for these detached clouds of fragments from the meteor-comet, to note exactly the hours of the watches kept, the states of sky and moonlight, with the numbers and brightnesses of the Leonids and sporadic meteors seen, to enable a true distinction to be drawn between bordering diffuseness of the streams, and really distinct branch-currents or offshoots from the meteor-cloud; for between 4 and 6 a.m. on November 17, and from 2 to 5 a.m. on November 18, last year, Mr. Corder found true-pathed Leonids almost as numerous (11 out of 22 meteors, and 8 out of 30 meteors) compared with the sporadic shooting-stars, as on November 14 (11 out of 26 meteors), from 2 to 4 a.m.; while on the previous morning of November 13, from 2 to 4½ a.m. only 3 or 4 Leonids were seen among 18 meteors; showing that both gaps and condensations reaching to considerable but as yet not fully determined distances from the main streams exist to either side of them, of which the extents and the changes, or the fixity of distribution would be very interesting particulars of their modes of assemblage to endeavour to trace out by observations.

On the morning of the 27th ult., in clear sky, between 1½ and 3 a.m., seven small meteors were recorded here, two of which, of

1st and 2nd mags., and orange colour, at 2, and 2.42 a.m., between ι and ϕ *Honorum*, radiated with short slow courses from between β and δ , and from near γ *Andromede*, and were evidently fore-shortened Andromedes; but the first of them, at least, was quite as erratic from the true centre, near γ *Andromede* of the Bielán shower, as the tracks through *Cancer*, *Hydra*, and *Leo Minor* of the recent showers of Leonids have been from their native shower's true radiant-point. The horary scarcity of these small Andromedes on their annual date this year, showed that no brisk Biela meteor-shower was then in active progress. On the two evenings immediately preceding and following this short morning watch, the sky here was quite overcast.

A. S. HERSCHEL.

Observatory House, Slough, December 9.

The Force of One Pound.

DR. LODGE has some right to complain of the friendly post-card. I wanted the *Poundal* difficulty to be threshed out in public, and we had just been writing to each other about it, but I quite forgot that my post-card might give him the wrong notion that my general remarks referred to him. Dr. Lodge knows that the real question before us concerns the Poundal; he knows that his advocacy of it has helped to maintain that unit in its academic position, and yet he now leaves its defence to others. He professes his love for all units, and attacks the poundalists and the poundists impartially, for suggesting adherence to any system in particular. This is better than his own maintenance of the Poundal, and I hope that it presages a complete change of front. His maxims are of the best: "Urge clearness of idea and accuracy of speech on all who deal with the junior student. These should not call different things by the same name . . ." But what if they continue to do so? He himself often uses *velocity* when he only means *speed*.

A pound of force, a pound of stuff, the inertia of a pound; here are three very different things all with the same name. When the chemist tells us that there is the same quantity of matter after as before chemical combination, what does he mean? He means that the *weight* of it is the same; the force of attraction by the earth. A certain amount of oxygen is equivalent in a certain property (its weight), to a quantity of hydrogen, and he says that he will call the quantities equal. Certain quite different amounts of them are *equivalent* in another property; he has exactly the same reason for calling these other amounts equal. A ton of iron is equivalent in a certain property to two ounces of gold. Why not call these amounts equal?

A pound of gold is no more the same as a pound of iron, because their weights and inertias are the same, than two chairs are the same as one table because they may be equal in value. I hope that Prof. Fitzgerald may be induced to say something on this head, the "huggermugger" of confounding quantity of matter with inertia; for I think with him that this is what produces far more confusion in the minds of students than the use of many different units for things of the same kind. The practical engineer has uncommon good sense, he hates the Poundal, and I think that Prof. Fitzgerald is right when he says that it is not merely because it is a new unit, but because it is founded on "huggermugger." Let Dr. Lodge read Mr. Jackson's letter which followed his own. He will see that Mr. Jackson cannot comprehend how anybody can avoid using the pound of stuff as a fundamental unit, and how it must be innate perversity which causes engineers to adopt as their unit of inertia (or mass, as they have unfortunately to call it), the inertia of a body to which the unit force gives unit acceleration. This is the fruit of the Poundal; no doubt its inventor thought of inertia. Mr. Jackson thinks of quantity of stuff, which is a conventional or metaphysical idea.

The standard units of time, of force, of inertia; we can only keep them in indirect ways. Assuming that waste is prevented and that the weight and inertia of a certain body measured under the same circumstances at the same place are always the same, any standard body with proper comparing instruments gives us our standards of force and inertia. The instrument for comparing forces is ready to hand, a good weighing balance. There is no instrument which can be relied upon for comparing inertias, so we fall back upon an indirect method, assuming that inertia at any place is proportional to weight. But it is to be noticed that all our practical acquaintance with inertia and with what we call quantity of matter is based upon our measurement of *weight*, of

¹ The *English Mechanic*, November 27, 1896. At Funchal, in Madeira (rh. 8m. W. long, from Greenwich), Leonids were seen falling at the half-hourly rates of 6, 9, 6 in half the sky, from 4½ to 6 a.m. (about 5.40 to 7.10 a.m., Greenwich time), on November 14; the similar rates between 2 and 4½ a.m., in equally clear sky, having been only 3, 1, 1, 2 and 1 in half an hour.—Letter from Mr. W. Anderson, in the *English Mechanic*, December 11, 1896. Note, December 15.—A. S. H.

force. A piece of metal is kept in London; it is defined as having there the weight of 1 pound, but only if weighed in a vacuum. Because it has this weight it is called 1 pound of stuff. Because it has this weight its inertia is said to be that possessed by a body whose weight is 1 pound. Now there are your standards in one piece of metal and in its environment, and in your instruments. You can keep the name 1 pound as the *weight* according to law, or you can call 1 pound the *inertia* of the body, but you cannot call it 1 pound of quantity of any material except of that particular kind of Pt. Which is more convenient? To call it the unit of a kind of Pt is too restrictive. To say that it has 1 pound of inertia is not more scientific than saying that its inertia is $1/32 \cdot 19$. As to convenience, note that we almost never need to speak of the *inertia* of a body except on our way to a dynamical calculation. The claim of *weight* of 1 pound as a unit of force is ever so much greater; for (1) there is the legal unit in existence. It might have been defined as the force required to elongate a certain spring, but this also would have been indirect. (2) All practical men use the unit already, and it is hopeless to try to alter their systems of working. All applied mechanics and engineering books written in the English language, from Rankine's superb treatises to the commonest pocket-book, use this unit; not one such book of repute can be mentioned in which the Poundal is used and in which this unit is not used; Joule used it, and many others of our best experimenters; even physicists never compare their forces in any other way than with the weights of bodies, and the pound is legally the English unit of weight. (3) The unit is so exceedingly easy to understand that years have now to be spent in driving it out of a pupil's mind; there is no part of our universe which we can reach where we might make experiments, where the unit is not easily derivable by a small and often negligible correction from the weight of a body whose weight in London is equal to that of the standard piece of platinum, and such bodies are to be found wherever men buy and sell by weight.

I think that Mr. Jackson is well answered in a quotation, slightly altered, from Dr. Lodge:—

"To identify quantity of stuff and inertia is barbarous, to denote their units by the same name is unwise."

By the bye, I should like Dr. Lodge to point out where I have lost sight of the dimensions of g and treated it as a mere equivalent for $32 \cdot 18$. One might say that it was stupid, or thoughtless, or academic, but I hardly think that Dr. Lodge is happy in his use of the term "illiterate." Anyhow, I did not do it. Engineers do not do things of that kind.

Mr. Cumming thinks that a system of theoretical dynamics can be built upon our basis; he does not seem to be aware that the system is already built; the ordinary dynamical expressions have no more to be altered when our units are employed than when the C.G.S. units are employed, and we do not need even to introduce Dr. Lodge's method of writing, which gives so much trouble in all but the very simplest algebraic expressions, but which certainly enables him to use any units whatever. I am afraid that it is known only to a few people as yet, but it is well worth knowing, being almost a necessity if one uses the Poundal or the Hogshead. It is simply this: every quantity goes about with a label as of a sandwich-board round its neck. Parenthetically I may say that Mr. Cumming is mistaken in thinking that engineers use the C.G.S. units in working with such fundamental equations as

$$V = \left(R + L \frac{d}{dt} + 1/K \frac{d}{dt} \right) C.$$

They use volts, amperes, ohms, secohms, farads, and seconds. Why else were the practical units invented? They were invented by practical engineers who at the same time invented the C.G.S. system, because they found that for the working of engineering problems, accurate answers and not merely academic logic was wanted. Unfortunately the academic 4π trouble was introduced, and till it is removed the practical man feels that life is a burden. These engineers were, as so many of our best engineers have been, trained at Cambridge, and this is one reason why I do not like to hear Dr. Lodge throw so much blame on Cambridge text-books. My opponents are not in agreement among themselves. I wish they were all as catholic in their sympathies as he. He wonders why people object, and who they are who object to the term *centrifugal force*. I think he must know many; anyhow there are certainly some, for they have told me so themselves during the last few weeks. And even he is now disposed to prohibit the use of a word,

never used wrongly by engineers, because the present generation of academic persons have found out that their predecessors had very wrong notions.

I do not see why he should speak so disrespectfully about that most wonderful property of a body, its attraction by and for the earth, which I will call its *weight*. He calls it "a curious and ill-understood deportment," and his dislike for it is due to its having "laid such hold of the engineer's imagination that he has begun to think it the most fundamental property of matter." I am glad that he concedes the engineer an imagination in spite of his not understanding the so easily understood "etherial stress." For my part, I acknowledge great ignorance about it, and I think it the most fundamental property of matter.

His students are very happy; they know all about etherial stress; they know how to use all units, any units, to get along with no units, and, in fact, Dr. Lodge seems to think that what he himself knows, after all his years of study, must also be known to his students because he has told them. I may say that I also delight in using all sorts of units.

I perhaps go further than Dr. Lodge, for I regard algebra as the best of all mediums for translating phenomena into mental processes, and I should not object to the multiplication of a cows + b bridges with c cows + 5 tons of sugar, if I could see any use in giving a meaning to such things as cow $\times$ cow or cow² (curiously enough, Dr. Lodge would object to cow squared as a colloquial reading of cow $\times$ cow), or cow $\times$ bridge. But surely the student described by Dr. Lodge is a very exceptional philosopher, a boy of eighteen, with all Dr. Lodge's knowledge. Is he not assuming that because a student gets marks on an examination paper, he really knows his subject? How easy it is for a student to get full marks for "What is Ohm's law"? But how many years of his life must elapse before he really knows Ohm's law? Or "What are Newton's laws of motion"? How very easy, and yet how exceedingly difficult. Take force = mass $\times$ acceleration. A student thinks he understands perfectly what you tell him, and can work all sorts of exercises on this statement. But let the thing come before him in a new form, and where is his knowledge?

Dr. Lodge *thinks* in all his units, and in my opinion the students of whom he speaks cannot think in them. I want to put students in the way to the mental position which Dr. Lodge postulates as the best. I want them to get into a higher position still, that of the engineer who is able even to think of the same sorts of terms in one equation being in quite different units.

Dr. Lodge would dilate on the ignorance shown by this engineer. Mr. Barrie, in describing two boys in one of his books, says, "Shovel knew everything, but Tommy knew other things."

In my article I refer to the persistent scorn of the academic philosopher for the engineer and the harm that it has done. Surely Dr. Lodge might restrain it a little in a public discussion. All English-speaking engineers use the force of one pound as their unit, and Dr. Lodge sneers at it as the colloquial unit of the shire in which an engineer happens to live. The shire is a very extensive one. It needs a globe to show it all. He sneers at the Heaven-born engineer of whom I spoke, and of his wish to advance in his profession, and the mutilated fragment of science and pocket-book information which serves for commercial purposes. He seems to be very proud of his ignorance of these commercial purposes to which so many students of higher physics mean to devote themselves, and yet he is not backward in expressing his opinions concerning them. He says: "May I tell Prof. Perry what is at the root of the perennial debate between engineers and teachers of mechanics? It is the subject of *acceleration*. An engineer's bodies are nearly always either at rest or in uniform motion, their accelerative stages he is usually able to ignore." If anybody can speak on this subject, surely it is I. I was trained in the shops and at college as an engineer, and I have done a good deal of engineering work, and I teach mechanics. I beg to say that Dr. Lodge is quite wrong in this. When I wrote about professors and engineering students I did not once think of him. I think now that he has written in haste, and that he cannot seriously put forward the view that the very most elementary idea of kinetics is unnecessary to the actual professional work of the engineer! Is it, then, of no use in any practical work of anybody? Has Dr. Lodge no students who think of the forces acting between the parts of reciprocating machinery, of the balancing of engines, of the action of governors, of the effects of centrifugal force? It

is a very grim joke, but we must bear with it and many others of the same kind, and in the meantime we do say that we agree with Dr. Lodge in his notion that our true and only natural foes are ignorance and prejudice. This ignorance of the needs of mere mechanical persons and prejudice against attempts to teach them, are fashionable now among scientific men. At the recent dinner of the Royal Society quite a genteel titter greeted a casual reference to technical education in one of the speeches.

I like algebra myself, but I do not think that it is the only possible conventional way of making an exact statement. However, taking Dr. Lodge's student with his "true from the bottom upwards and entirely true," mariner's formula to find a certain academic distance; is it quite certain that he will think of using it in a practical case? In my experience it is the very last thing that will enter his mind. I should not call him an ass; as Dr. Lodge does; he only follows Plato's maxim, not altogether neglected in English education, that philosophy ought to serve no useful purpose. The Wrangler's naive faith, when he does condescend to a practical problem, is generally shocked in finding that such problems require the addition of a little common sense to the formula. Now I do not object to a man's finding out the usefulness of a rule (call it a mere numerical rule if you like, but it is evidently a very different thing), in actual practical examples before he understands how it is derived, for I am confident that he will then be easily induced to inquire how the rule is arrived at; he will go further and think of the evidence for the roundness of the earth, and indeed it may prove to be the starting-point in his scientific education. I grant you that he will not go far if his instructor makes him begin his studies with the six books of Euclid. Why does the mariner remain so ignorant of mathematics although he uses the results every day? Surely he wants to know the why? Yes, indeed he does, but you have taught him that he cannot know the why unless after years of quasi-philosophic worry.

When I spoke of the practical knowledge needed by the engineer, I meant to include such knowledge of physics as is possessed by Dr. Lodge himself; I mean no mere pocket-book knowledge. Before a student can get to this higher region he needs to be taught to think, and it is in our notions of this preliminary training that I differ from Dr. Lodge.

I do not care much what a man's system of teaching may be; if it is his own, however faddy, he will teach his students better than on a better system, not his own. But we must acknowledge that the average teacher needs a system to be given to him, and this ought to be the best system. Well, I think that the existing system is about the very worst possible. We compel a student to boggle at imaginary difficulties. We worry him for years over four books of Euclid; we have given up the fifth book, and even the supplement to the fifth, but we still worry him with the sixth, and then go on to geometrical conics. Now even the sixth book merely involves ideas which every boy takes in without much difficulty; it is so natural to think of using any unit of length, that Dr. Lodge forgets how the ideas of the sixth book are needed in his simple mariner's rule. But as soon as a student begins his work in physics, he is rushed over difficulties to which the difficulty of thinking about the mere ratio of lines is nothing. I do not say that he ought not to be suddenly surrounded with ideas of the sums and differences and ratios of all sorts of scalar and vector conventions for quantities, and told to sink or swim among them. I think this the very best thing for him; but what of your consistency in mind-training, of the philosophy of your methods? Dr. Lodge compels me to describe my non-academic way of teaching. I thought that everybody knew it, but evidently he does not.

I believe in using the experimental method from the beginning; of squares and ratios of sides of all sorts of right-angled triangles being figured out by the boy of eight years of age, to see how near he gets to tabulated sines and cosines. I believe in his measuring time and lines and forces with the watches and scales and balances which are in common use; in testing the rules of mensuration of areas and volumes, and the finding of weights of bodies by calculation; and it is only when a boy has a good quantitative knowledge from his own experience that I trouble him with the philosophy of mathematics and physics, and then I do it cautiously. I make beginners plot curves on squared paper—curves showing the rate of increase in the price of silk or cotton or the height of the barometer, or the National Debt or other things given in Whitaker's Almanac—in telling them about the slope of a curve and its analogy with velocity and acceleration and dy/dx and ds/dt and a^2s/dt^2 . They "graph" all

sorts of curves; they add and subtract vectors by actual drawing, and their lectures and laboratory work and graphical and numerical exercise work go on simultaneously.

Some of my academic friends not only refuse to let a student use a formula, but they refuse to let him use a table of logarithms until he can calculate logarithms. To be consistent they ought to refuse the use of a watch or of clothes until a student has shown aptitude in the watchmaking and tailoring trades. I let my students use any appliance whatsoever if I think that it will give them a better acquaintance with natural phenomena; anything that will cause them to think. As a student gets on I let him take all sorts of liberties in regard to units; he uses w/g for m ; he speaks of centre of gravity instead of centre of mass or centre of area.

Also, I venture to tell Dr. Lodge that the very best of my students, who know something of Bessel functions and spherical harmonics and elementary St. Venant work on the torsion and bending of prisms, and something about generalised coordinates, are taught to have the very highest respect for the rule-of-thumb practical methods of calculation in use among engineers. They are taught that the engineer has to deal with things that are by no means so simple as the ordinary laboratory phenomena, and that rules arrived at through the trials and errors of generations of practical men are worthy of some respect.

Lastly, I may say that we are tired of the whole academic system which recognises no philosophy or literature or art which is not studied as a dull grind for examination purposes, and I am thankful to say that we have indeed "a sympathetic faith in a much larger training."

JOHN PERRY.

December 17.

The Earthquake of December 17.

IT may interest your readers to know that the recent earthquake of December 17 was shown slightly on the declination curve, and more distinctly on the horizontal force curve, at Kew Observatory. The time of commencement was 5h. 35m. a.m. (± 1 minute) G.M.T. The disturbance on the horizontal force trace approximately equalled what would have been produced by a change of 0.00004 C.G.S. units in that force.

CHARLES CHREE.

Kew Observatory, Richmond, Surrey, December 19.

EARTHQUAKE shocks occurred in Worcester at 3.35 and at 5.31 a.m. on Thursday. The 3.35 shock was feeble, of short duration, and was noticed but by few persons. But the visitation of 5.31 exceeded in violence any previous instance of seismic energy here within the present century. There were in the 5.31 instance two shocks following each other with a bare interval. The shocks consisted of a series of rapid vibrations, too rapid to admit of count. These shocks were preceded by a roar as of thunder. Some describe the roar as that of the noise of a "rushing mighty wind." My house was shaken with appalling violence, displacing roof tiles, and forcing open a closed chamber door. To me the shocks seemed to proceed from north to south. The duration of the shocks lasted between four and five seconds. Some say the shocks lasted fifteen seconds; but if the earthquake had lasted so long, my house would have been down. As it was the house was rocked to its foundation, and the sensation was appalling. Persons whose bedrooms faced the north, saw a great light accompanying the earthquake. This peculiarity is by some attributed to lightning, by others to the effect of a large meteor. Mr. Russell Dirrell, of North Piddle, a place seven miles east of Worcester, saw at the time of the earthquake a great blaze of light low in the northern horizon, continuing for two or three seconds. He was unable to attribute the blaze to a lightning effect. At the homestead of Mr. Walters, of Hallow, three miles north of Worcester, the inmates were thrown out of bed, as was the case in several other instances in the same village. Here the shocks were most severely felt at places on the west of the Severn. In Worcester the shocks created general alarm. Bells were set ringing, shut doors forced open, windows rattled, heavy wardrobes displaced, earthenware scattered about, in some instances broken, but no one was injured. A strong fixed wash-hand basin in a lavatory was split to pieces. The church clock of All Saints, on the east of the Severn, was stopped at 5.15 a.m. As seems to be usual in such cases, poultry and pheasants flew down from their perches and showed signs of distress, birds flew aimlessly about and