tried already to some extent, but unfortunately without sufficient success to justify an expectation of their being able completely to accomplish the desired change.

In considering this question we ought, it appears to me, to search for some distinct difference between our educational practice and that of Germany, some difference great enough to be likely to have an important effect. Such is indeed easy to find, for there exists a difference so great that it might quite readily lead to very important results, and one which, probably because of its obviousness, is generally ignored. This far-reaching difference is simply that in Germany research work is absolutely indispensable for the *ordinary* degree (by "ordinary" I mean that ordinarily taken by students), and it is at least a very reasonable contention that the moment research work becomes essential for our ordinary degree (B.Sc.), with, naturally, any necessary lengthening of the course, so soon shall we have taken the step which will, not to-morrow, but in ten or fifteen or twenty years' time, perhaps place us on something like an equality with Germany in respect to the point at issue.

Two most important results might be expected to follow the introduction of compulsory research into the B.Sc. degree: (1) There would be provided throughout the country a considerable body of young chemists with some experience, say one year, at least, of research work. There is such a thing as a general method in research, and after even only one year's training in it the young chemist would be able to attack, with very much greater confidence than at present, many of the problems which arise in industrial practice, for in research work, emphatically, it is the first step that counts for most, and this first step being a thing that can be taught, it is the duty of the universities to teach it. (2) Sons of manufacturers who go to the university and take a science degree would of necessity carry out some original investigation, and from this particular class—composed of men who, for the most part, are possessed of some means and leisure—there would be likely to emerge a number of really capable chemists, who might indulge in the higher degree of D.Sc., men likely to carry their chemistry intelligently into their businesses. But even in those least interested there would necessarily be acquired some idea of what research means, some notion of how it might be applied to their own particular requirements, and it is probable that in a comparatively short time, say twenty years, the lack of appreciation of research work which is now attributed to the manufacturer would have wholly or largely disappeared.

In fact, the introduction of research into the ordinary degree would be likely to act in several ways. First upon the student, secondly upon the manufacturer, and thirdly upon theoretical chemistry by the achievement of the excellent educational principle, that the science would be the richer in some fact or in some theory for every graduate who had devoted himself to it. Fourthly, it would react upon the teachers.

Although this is, essentially, an exceedingly simple reform, there would doubtless be great difficulties in carrying it out; it would probably be urged that M.Sc. degrees have been instituted with this especial object, but that would be to misunderstand the present suggestion, the essence of which is that there shall be no degree at all, or anything resembling a degree, which does not require research work. At the present time, however, when traditional prejudices of all sorts are going by the board, it would probably be easy for the teachers to bring sufficient pressure to bear upon Parliament, or conversely, for Parliament or a resolute Government to bring sufficient pressure to bear on the teachers, to secure the immediate accomplishment of this desirable improvement.

It must, however, be sorrowfully admitted that such a change is not likely to make any particular financial difference to the young chemist, but in all probability it would give him a better opportunity for advancement once he had established himself in a technical post, and there is little doubt that the advantage to the country would be very great.

T. S. PATTERSON.
University of Glasgow (Organic Chemistry Department), June 8.

Galileo and the Principle of Similitude.

WHEN I said in NATURE (April 22) that Herbert Spencer was the first to apply the principle of similitude to dynamical problems in biology, I spoke in haste. I might have remembered that Borelli had shown, by help of this principle, that a man would never be able to fly by his own muscular power, and why (for instance) small animals are more active and leap higher than big ones. But I was quite ignorant of the fact that Galileo had treated the whole subject on the broadest lines and with the utmost clearness. His discussion will be found in the "Dialogues concerning Two New Sciences," admirably translated by Prof. Henry Crew and Alfonso de Salvio (New York: The Macmillan Co.; London: Macmillan and Co., Ltd., 1914). So numerous and interesting are the subjects dealt with in this wonderful book that the writer of a long and laudatory notice in NATURE (December 24, 1914) had not time or space to mention that the principle of similitude and the subject of animal mechanics are alluded to therein. The following extract (*op. cit.*, p. 130) is but a small part of what Galileo has to say upon the principle of similitude:—

"*Salviati*: 'From what has already been demonstrated, you can plainly see the impossibility of increasing the size of structures to vast dimensions either in art or in nature; likewise, the impossibility of building ships, palaces, or temples of enormous size in such a way that their oars, yards, beams, iron-bolts, and, in short, all their other parts will hold together; nor can nature produce trees of extraordinary size, because the branches would break down under their own weight; so also it would be impossible to build up the bony structures of men, horses, or other animals so as to hold together and perform their normal functions if these animals were to be increased enormously in height, for this increase in height can be accomplished only by employing a material which is harder and stronger than usual, or by enlarging the size of the bones, thus changing their shape until the form and appearance of the animals suggest a monstrosity. To illustrate briefly, I have sketched a bone the natural length of which has been increased three times and the thickness of which has been multiplied until, for a correspondingly large animal, it would perform the same function which the small bone performs for its small animal. From the figures here shown you can see how out of proportion the enlarged bone appears. Clearly, then, if one wishes to maintain in a great giant the same proportion of limb as that found in an ordinary man, he must either find a harder and stronger material for making the bones, or he must admit a diminution of strength in comparison with men of medium stature; for if his height be increased inordinately, he will fall and be crushed under his own weight. Whereas, if the size of a body be diminished, the strength of that body is not diminished in the same proportion; indeed, the smaller the body the greater its relative strength.

Thus a small dog could probably carry on his back two or three dogs of his own size; but I believe that a horse could not carry even one of his own size.'

"*Simplicio* : 'This may be so; but I am led to doubt it on account of the enormous size reached by certain fish, such as the whale, which, I understand, is ten times as large as an elephant; yet they all support themselves.'

"*Salv.* : 'Your question, Simplicio, suggests another principle. . . .'"—And thereupon the two disputants go on to discuss the effect of immersion in water, of how by reason of its density (*corpulenza*), or, "as others would say," its heaviness (*gravitá*), the weight of bodies immersed in it is diminished; and how accordingly the body of the fish is rendered, so to speak, altogether devoid of weight, and is supported without any injury: though if a giant fish, or a great and heavy-laden ship, were drawn ashore, it would be apt to go all to pieces, crushed under its own mass.

Galileo points out that Aristotle had an inkling of the principle in that chapter of his "Mechanics" where he discusses the question, "Why a long beam is weaker than a short one"—even though the long beam be thick and the short one be thin. But at the beginning of his treatise Galileo makes it clear that he regards the general statement as a discovery of his own, and as one of great importance which moved him even to astonishment.

D'ARCY W. THOMPSON.

The Names of Physical Units.

A L'OCCASION de l'aimable analyse consacrée au "Recueil des Constantes physiques" (NATURE, May 13, p. 281), M. J.-A. Harker s'étonne de certaines expressions insérées dans le tableau dont je suis à moitié responsable, et qui sert de préface à tout l'ouvrage. Je dis "à moitié," car, à l'encontre de ceux qui se rapportent à des constantes proprement dites, le tableau des unités a été discuté et approuvé dans sa terminologie par la Commission tout entière; j'ai seulement proposé, la Commission a disposé.

Le terme *stéradian* n'a pas l'approbation de M. Harker. Evidemment, il n'est pas encore consacré par un usage international, et c'est là, peut-être, son plus gros défaut. Les physiciens français toutefois l'emploient couramment pour désigner *l'angle solide découpant, sur la sphère, une superficie égale au carré du rayon, ou l'angle solide égal au quotient de l'espace entier par* 4π. On conviendra que l'une ou l'autre de ces expressions est encombrante, et qu'une contraction était au moins désirable.

Stéradian est logique, puisqu'il résulte de l'association de radian (angle plan unité) et du préfixe impliquant la solidité ou l'espace à trois dimensions. J'ose donc espérer, malgré l'étonnement de M. Harker, voir nos confrères britanniques adopter ce terme. Ce serait une aimable réciprocité à l'hospitalité donnée par les sportsmen continentaux au mot *starter*, grâce auquel ils évitent aujourd'hui la périphrase: *Fonctionnaire chargé de donner, dans une course, le signal du départ;* tout comme le titre qu'ils s'octroient abrège cette autre appellation: *Gentlemen consacrant une partie de leurs loisirs aux exercices musculaires.*

La question du degré carré sera résolue avec celle du stéradian. C'est bien, si je ne me trompe, au moyen de cette unité que les astronomes évaluent, entre autres, l'espace de la sphère céleste que couvre un cliché photographique.

Une autre espèce d'expressions a frappé M. Harker: *masse volumique, volume massique.* Dans le tableau en question, ces expressions sont inscrites entre parenthèses, en subordination, pour ainsi dire, des termes classiques mais bien peu satisfaisants: *Densité absolue* et *volume spécifique.* Si j'avais eu une entière liberté, j'aurais certainement franchi l'étape et renversé l'ordre. Quel qualificatif, en effet, laisse plus de vague à l'esprit que celui de spécifique? On dit masse spécifique: quotient de la masse par un volume; volume spécifique: quotient d'un volume par une masse; spécifique a, ici, les deux acceptions exactement opposées, sans compter, dans d'autres cas, une foule de sens divergents. En fait, spécifique signifie tout ce que l'on veut, et par conséquent ne signifie rien du tout. La vieille terminologie laisse encore traîner dans la physique des expressions telles que *chaleur spécifique* (capacité calorifique rapportée à la masse) et *résistance spécifique* (résistance rapportée aux dimensions) et tant d'autres, pour l'intelligence desquelles le physicien est chaque fois obligé de faire appel à sa mémoire, sans aucune certitude d'être d'accord avec un confrère dans le sens à attribuer à une même expression.

Il fallait rompre un jour avec ces errements; la plupart des physiciens français, sur la proposition d'Hospitalier, ont accepté depuis des années les expressions que j'ai insérées dans le tableau, comme les mécaniciens français ont adopté, dans la technologie, des termes tels que *puissance massique,* auxquels le lecteur non prévenu ne peut se tromper, tant ils font image.

Ne pouvons-nous, au contraire, regretter de voir nos confrères britanniques conserver des expressions telles que *specific gravity,* où specific est vague et où gravity n'a rien à voir? Je soumets le cas aux méditations de M. J.-A. Harker, avec la certitude qu'il m'approuvera, car il est métrologiste, c'est à dire homme de pensée claire et concise.

CH. ED. GUILLAUME.

Pavillon de Breteuil, Sèvres (Seine et Oise), le 17 Mai.

IT would appear from the interesting letter of the Director of the International Bureau of Weights and Measures that he has a little misunderstood my reference to the new expressions he employs in the preface to the "Recueil de Constantes Physiques." If he will refer again to the review to which he takes exception, he will see that, on the matter of nomenclature, all I wrote was:—

"Some eccentricities appear in the initial table on units; few physicists are familiar with such terms as 'volume massique' and 'masse volumique,' 'degré carré' and 'stéradian.'"

I expressed no opinion as to the suitability of any of the terms in question, but only pointed out that in my view they were as yet far from familiar to the average physicist.

I have taken an opportunity of testing the accuracy of this opinion by consulting six of my colleagues. Not one of these had a clear and definite idea of the meaning of all four of the terms in question.

The introduction of a new name for a unit or an alteration in nomenclature should be a matter for the most careful consideration, particularly if it is intended for general international use; more harm than good may easily be done by an injudicious choice, even if supported by a great authority.

"Stéradian," and the other terms too, may be logical, but it is unpractical to attempt to build a language simply upon logic.

Dr. Guillaume will remember that some time after the use of the term *micron,* with its corresponding symbol, the overworked letter μ, had been introduced into metrology, as the name for the millionth part of a metre—I believe I am correct in saying, largely through the influence of Dr. Benoit—Lord Kelvin,