

Now I think there are several points in the above sentences liable to misconstruction. Mathematics is purely a *form* of reasoning, and, as in the case of all forms of logic, it is merely an instrument, and the product depends upon the material dealt with. This may be the result of observation or of experiment, either of which may or may not be statistical in character. Prof. Howes, in contrasting "statistical, experimental and mathematical tests" with the "observational method," seems to be looking upon mathematical reasoning as something which has more relation to experiment than to observation. I fail to see why as an instrument it is less applicable to the gigantic overthrust of the geologist than to the test-piece in the laboratory, less applicable to an observation on the mottling of birds' eggs than to an experiment on the breeding of mice. It is perfectly true, as Huxley said, that what you get out of the machine depends entirely on what you put into it. Such a platitude in its right context may be a useful reminder. But *without your machine you may be able to get nothing at all out of your material*; and I venture to think that this is the case, not with a few, but with many branches of biological inquiry.

The reason thereof is easy to find. In vital phenomena we are never able to repeatedly observe or to experiment, as we can very closely do in physics, under exactly the same conditions with the same quantities of the same substances. The reader will probably interject, "No, and this is the very reason why mathematics can be applied to the one and not to the other!" On the contrary, because in biological investigation an exact A cannot be associated with an exact B, and an exact C observed (as we can do in physics), biology requires a much more refined logic, much more subtle mathematics than the simplest branches, at any rate, of physical inquiry do. There is nothing more full of pitfalls than "ordinary reasoning" applied to the problems of association. The biologist observes that *some* A is associated with B, and that *some* C is associated with B. But if he wishes to discover whether the relation between A and C is causal, he will need all the refinements of symbolic logic, a mathematical analysis, which is analogous to the geometry of hyperspace, before he can come to a definite logical conclusion on the possible relationship of A and C. He may observe as much as he will, but he will not find out whether the association is confirmable or non-confirmable without this higher logic. It is the all-pervading law of vital phenomena that no two individuals are identical among living forms, that variation exists in every organ and every character, which, so far from disqualifying biological phenomena for mathematical treatment, enforces a need for the most generalised forms of mathematical reasoning. Prof. Howes tells us that the mystery of life can never be solved by mathematical treatment. If he had said that the mystery of life cannot be solved by any treatment whatever, I should have heartily concurred with him. But if he means that observation, rather than observation plus the higher logic, is likely to discover the most comprehensive formula under which the phenomena of life can be described, then I am quite sure he is in error. Observation, for example, has collected a mass of most valuable facts during the past thirty years, but can any one by merely verbal generalising upon these facts venture to assert that evolution by natural selection is more than a probable hypothesis? The very nature of such ideas as variation, whether continuous or discontinuous, as inheritance, whether exclusive or blended, as selection, whether natural or sexual, leads us to the idea of number, of statistics, of frequency, of association, and enforces upon us an appeal to mathematical logic. If we are to feel that evolution by natural selection is as sure a formula as that of gravitation, it will be because mathematics steps in and reasons on the data provided by the Tycho Brahes and Keplers of biological observation.

Prof. Howes must not for a moment suppose I claim biology for the mathematician. I do not even want the mathematician to have a biological training, conscious as I am personally of the disadvantages of its absence. The mathematician who turns physicist is rarely so valuable a discoverer as the born and trained physicist who knows mathematics so far as he needs them. I believe the day must come when the biologist will—without being a mathematician—not hesitate to use mathematical analysis when he requires it. The increasing amount of work being turned out, both in America and Germany, by the younger biologists with a mathematical training, is a sign of the times. In England, I suppose (where, as usual, an Englishman, Mr. Francis Galton, first indicated the great possibilities of a

new method), we shall be left behind, and let other nations gather the fruits of our sowing. Prof. Howes, indeed, leaves a field for mathematical investigation; but it was only a few weeks ago, at a discussion at the Royal Society, that another distinguished biologist asserted that in living forms there was no such thing as number!

Et Verbum interrogabat Vitam: Quod tibi nomen est? Et dicit ei: Legio, id est Numeri, mihi nomen est, quia multi sumus. Et deprecabatur eum multum, ne se expelleret extra regionem.

I doubt whether the demon can now be exorcised conjure Verbum ever so cunningly. KARL PEARSON.

### Education in Science.

SOME discussion has recently arisen as to the methods of teaching mathematics. Euclid has been condemned on the score of its advancement and its antiquity. An infusion of more modern geometry has been recommended, with corresponding arithmetic and algebra. In science, at the same time, there has been a tendency to recognise the historic method. Prof. Perry considers it unnecessary for pupils to traverse the course of their ancestors. But let us ask *why* this course has been recommended. On account of the successive growth of faculties in a historical sequence. Is this a fact or not? It is an undoubted fact, and it is not sufficiently realised by any teachers. Prof. Perry has two saving principles, first to teach by practice, and second to satisfy the pupils' instincts. These being the same reasons which are used by advocates of historical methods secure a certain amount of agreement. We ought to arrive at the same result whether we study the natural methods of pupils, or the methods of primitive peoples. But Mr. Herbert Spencer has well pointed out somewhere that we ought not to go to the Greeks for examples of primitive peoples. They were highly and very specially developed. Hence arises a very great danger in the historic method.

With regard to practical and rational methods, it must have often been noticed by teachers that a great number of pupils have an inherent objection to carrying out rules without some kind of reason for them. It is also to be observed that a very vague, or even a verbal reason, will be more satisfactory than a real one. This is surely in accord with the studies of the history of science. Although it is somewhat misleading to reason from the experience of men of genius, it may be worth while to call to mind the intense satisfaction of Darwin with Euclid's concatenation, and the disgust of Huxley at the irrational rule and rote method of mathematics under which schoolboys grow up. It is the exceptional boy who delights in carrying out enigmatic rules, although all have a temporary taste for that work as sauce to the rest. It is treacherous to reason from one lesson of this kind to a regular course of it.

It is customary to speak of the activity, observation, ingenuity of children. But it is not found, either in the history of children or of primitive peoples, that they are capable of continued mental application, observation or contrivance. We might just as well speak of the great reasoning powers of children on account of their perpetual "why." It is also improper to underestimate the value of this tendency. By it children acquire and cement their knowledge, although a chain of real reasoning will absolutely exhaust them.

From this kind of reasoning we conclude that the time-taught method now pursued in mathematics is a reasonable one, that Euclid with the algebra and arithmetic corresponding is in the main advantageous. But why? Because it is conformable to the instincts of pupils, and also because it is historic. But is it conformable enough? Is it historic enough? I think not. Euclid was a grown man in a grown community of very special bent of mind. Where he does not agree with the reason for his inclusion in school curricula he should be neglected. But instead of supplementing him from more recent geometry, it should be from more antique writers and from study of pupils' methods.

Now we come to the bearing of this on the teaching of science. We are comparatively new at this game. We are finding that we have started in too high a key, and we are being recommended to go back. I have not yet seen a recommendation to exactly imitate the mathematical teachers, and go back to Pliny, Geber, Gilbert and Pallissy. But several have advised Boyle and Black. Along with this advice is an insistence on quantitative work from the very start. It appears to me this is a very grave mistake. The use of a rough balance and rough methods of measurement is all that should be aimed at in

school. For example, one teacher of girls proposes to show by furnace and acid that chalk gives off a definite quantity of gas. This seems to me appropriate for an advanced university student, but is not the thing for schools at all.

Experiments to show the indestructibility of matter have this advantage, that you must begin with some matter, and that you must have some appliances on both of which the inquiring mind may lead. But as to where the matter goes to is another matter, and as to what the measurements are all for, you might as well be noting them during the progress of a pantomime.

The same criticisms apply to physiology and botany. It is said we cannot properly study the stomach without a preliminary of histology. If so, they cannot be approached in schools, for histology is a late science and is vain and empty to pupils. The microbes of false ideas are thick in it. But Harvey knew no microscopical histology, and yet he was not altogether a fool. I find boys and girls of fifteen and sixteen studying the alternation of generations in a phanerogam, and not only the nutrition but the respiration of plants. Surely this is pushing on to modern methods with a vengeance. But is there anything gained in development of faculty? Can they *observe* these things, or do they trace a dim something which they are told are there, and recognise them with the wild delight of an irresponsible original researcher? It is the delight of a child who has jumped six feet high with just a little assistance. An independent mind rejects all this and begs for a little exercise in kinds of knowledge which you will find well represented in Pliny.

When we turn back to the books of study which we read as boys of fifteen and sixteen in the times when the ambition to kick a goal or vault ten feet was so strong and so easily ousted other ideas, how many very important laws we find which we then read and now for the first time *know*. I can remember the time when I tried to wake a class to the importance of Boyle and Charles's laws, and I can also remember the time when I remembered that my own old master vainly tried to wake us to it. The result in neither case was thanks, and it was the teachers who were wrong, not the pupils. We do our best, but we are vastly wrong, and we inflict many injustices by force of punishment just as in the old *régime* they broke the rulers over our fathers' shoulders in teaching them practical prosody. A little study of history will lessen this injustice.

At the same time we must distinguish essential historic progress from mere accidents of time. I should be sorry to exclude hydrogen explosions absolutely. JAMES SUTHERLAND,  
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#### Abbe's Optical Theorems.

IN the article, "Optical Science" (NATURE, p. 203), as well as in the preface to Prof. S. P. Thompson's translation of Lummer there mentioned, regret is expressed at the neglect in English text-books of Abbe's contributions to optical theory.

Will you allow me to remark that statements and proofs of Abbe's theorems will be found in §§ 205*b*-205*f* of the 1899 edition of my "Deschanel, Part iv." They occur in the chapter on "Systems of Lenses," and are based on careful study of the writings of Abbe and Czapski.

Ealing, January 9. J. D. EVERETT.

#### Fireball in Sunshine.

ON Sunday, January 6 last, at oh. 52m. p.m., a brilliant fireball was seen by many observers in Scotland. The sky was clear and the sun shone brightly at the time. The meteor was observed from Whiteinch Park and Great Western Road, Glasgow, flashing across the north-western sky, and resembling a rocket with a long streaming tail. One correspondent at Glasgow says it travelled from the north-east to west, and that in colour it was like reflected sunlight. Another writer describes it as being of considerable size, "the fiery mass being as large as a bowling ball with a glowing red tail attached." At Killearn, N.B., the object passed from N.W. to W.N.W., and was about 12 degrees above the horizon at the time of its disappearance. It traversed a path of about 20 or 25 degrees, during which it fell about 5 degrees. The radiant of the meteor was probably in Auriga, Perseus, or Aries, so that it belonged to a different system from that which furnished the brilliant daylight fireball of January 9, 1900 (NATURE, January 25, 1900).

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#### Air and Disease.

IN these days of fresh-air treatment, some of your readers may be interested in a quotation from Palladius "On Husbandry," an early fifteenth century MS. originally in Colchester Castle.

"The longe-woo," says that writer, "cometh ofte of yvel eire," i.e. lung-woo or consumption comes often of bad air. The whole verse describes the effects by which you may know bad air or water, and is, perhaps, worth quoting in its entirety.

"The longe-woo cometh ofte of yvel eire,  
The stomake eke of eire is overtake,  
Take heede eke yf the dwellers in that leire  
Her wombes, sydes, reynes swell or ake,  
If langoure in thaire bladders ough' awake.  
And if thoue see the people sounde and faire,  
No doubt is in thy water nor thin aire."

Thus we are told that both lungs and stomach are affected by bad air and that, to detect bad air or water we are to see whether the inhabitants have aches in stomachs, &c.

The importance attached so early to air and water may, I think, prove worth mentioning, as it is not what most of us would expect. I came across the passage in turning over the leaves of Lodge's edition of Palladius, published by the Early English Text Society.

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#### RECENT ADVANCES IN THE GEOLOGY OF IGNEOUS ROCKS.

THE closing years of the nineteenth century witnessed a revival of interest in the petrology of igneous rocks, which must be regarded as marking an important stage in the development of that subject. Much detailed work, especially in the laboratories of German universities, during the three or four decades preceding had already accumulated a large body of information; but that work had been confined in great measure to the strictly descriptive side of the science—in short, to what is properly described as petrography—and some of it fell rather into the domain of mineralogy and physical optics than of geology. The value of such a store of material cannot be overestimated; but any tendency which promises to shape it into a connected system must be welcomed by geologists as the breath of life animating the valley of dry bones. Such a movement is undoubtedly felt at the present time, and may perhaps be held to mark the transition in petrology from the stage of observation to that of generalisation. An igneous rock has come to be regarded, more constantly than before, not merely as a mineral-aggregate, but as the product of consolidation of a molten rock-magma; and consideration has been directed to the constitution of such magmas and the conditions governing their consolidation. Recognition of the importance of studying the mode of occurrence of igneous rocks and their relations to one another has led to a closer union of observation in the field with research in the laboratory. Much is being learnt concerning the geographical distribution of the rocks, their connection with crust-movements, and the sequence of eruption of different types at a given centre. The facts thus acquired, and especially the fertile conception of "petrographical provinces," each with its suite of igneous rocks having a community of characters which bespeaks a common origin, have confirmed the conviction that widely diverse rock-types may be evolved from a common parent-magma. Hence arises the problem, to which Brögger and others have boldly addressed themselves, of the processes by which such "differentiation" is effected and the conditions which control them. Hence, too, another problem, a corollary to the former, to frame a natural classification of igneous rocks, based on genetic principles, to supersede the provisional classifications on various artificial or Linnæan schemes which are at present current. The questions involved obviously present great difficulties, and petrologists would be the first to admit that some of