

very coarse shingle one turns from the erosion hypothesis, and the slightly pitted nature of the rock surface suggests solution.

ALEX. STEVENS.

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May 6.

The Mountains and their Roots.

MAJOR COWIE'S letter in NATURE of May 8 gives the impression that I had the facts of the observations on the deflection of the plumb-line in India before me, and that I made my assumptions as to relative densities, and the mode of compensation by extension of depressed crust beneath the plains, "suitably adjusted," so as if possible to bring out the desired results. This was not the case. I made the assumptions about relative densities which seemed to be *a priori* probable; and it will be seen from the diagram at p. 184 of my "Physics of the Earth's Crust" that fifteen years before I wrote the paper in the *Phil Mag.* I had suggested that compressed mountains would be partly supported by an extension of the depressed crust beyond them.

Should anyone be inclined to undertake the labour of calculating from my formulæ, introducing fresh constants, or other distances, I would warn him that in the *Phil. Mag.* there is a misprint. In the formula for the plateau, after the first bracket, insert x .

I am much pleased that after so long a time my theories are under discussion, and I hope to come well out of it. I am sending to the *Geological Magazine* a reply to some remarks by Sir T. H. Holland in that journal, and to this I would refer your readers as more fully giving my views on some of the points under discussion.

O. FISHER.

Graveley, Huntingdon, May 9.

An Application of Mathematics to Law.

I HAVE read Mr. Potts's letter in NATURE of April 24, but am at a loss to understand the use to which he would put his equations.

If it be his object to find some equation giving the validity of a patent or foretelling in any way the probability of its being upheld in a court of law, he has clearly failed to do anything of the sort.

If his equation $I = M + i$ is to be of any value, the quantity i must have a fixed value greater than zero. In fact, however, for any given patent, i may have an infinite number of values, including zero, since each person will have his own idea of the amount of ingenuity that must be shown in the particular case by the inventor. Thus the inventor will certainly put a high positive value upon i , while his opponent will as certainly say that the value of i is zero. It is clear that the value of i can only be finally settled when the validity of the patent has been settled by the House of Lords, and at this stage of a patent's career it is scarcely necessary to have an equation to test its validity. So far as the rest of his letter goes, he seems to have chosen a rather complex method of setting out a few of the chief principles of patent law.

R. STAFFORD CRIPPS.

Fulmer, Slough.

I DID not imagine that my letter would be taken as an attempt to supersede the present methods of determining validity. I intended it as a contribution to the theory which underlies the enormous volume of our case-law on the subject. Surely, as in other cases of the progress from empiricism to science, the first step must be in the direction of mathematical or symbolic expression of the facts. The value of

such a symbolism is twofold: first, as an aid to precision of thought; and second, as a preliminary to generalisation. It is a vital principle of English law that all decisions shall harmonise with precedents as much as possible, and on this account alone anything should be of value which assists in formulating generalisations. We admit the value of theory in the physical sciences, apart from immediate practical results: why should an attempt to develop a theory of law be condemned because it does not at once do away with the functions of the judge?

Mr. Cripps's difficulty as to the value of i will not be so great if the actual cases given in my letter are studied. I may add here, however, that it is immaterial what this value is, provided that it is measurably greater than zero. It is settled law that a scintilla of ingenuity is sufficient to support a patent for something new and useful (*cf.* *Thompson v. Amer. Braided Wire Co.*, in the House of Lords, and other cases). I therefore employed this symbol merely to indicate that there had to be some positive difference.

HAROLD E. POTTS.

University Club, Liverpool.

SYNTHETIC BIOLOGY AND THE MECHANISM OF LIFE.

THE presidential address delivered by Prof. Schäfer to the British Association in 1912, and the subsequent independent discussion at a joint sitting of two of the sections, served, as was pointed out by Prof. Armstrong in a paper in *Science Progress* in October last, "as a useful corrective to the wave of vitalism that has passed over society of late years owing to the pervasive eloquence of Bergson and other writers." Probably the majority of those who have studied the phenomena of life from the chemical side will agree with Prof. Schäfer in his dictum that "at the best vitalism explains nothing," and accept his opinion "that we may fairly conclude that all changes in living substance are brought about by ordinary chemical and physical forces." The difficulty, however, lies in obtaining any satisfactory information as to what are the actual chemical or physical changes which occur in the real living cells or tissues. Since this discussion was held Prof. S. Leduc, of the School of Medicine at Nantes, has published a monograph¹ in which he approaches the problem from the novel point of view which now for several years past has guided his experiments and with which readers of his "Mechanism of Life" will be familiar.

It is impossible to do justice to the author's arguments or make clear the proper value of his demonstrations in a short article such as the present, but this will at least serve to direct attention to a few of the very remarkable results that he claims to have achieved, which, if verified, are certainly of the highest significance to the student of the phenomena of life.

The basis of Prof. Leduc's work may be summarised in his own words as follows: "It is in the physico-chemistry of liquids that an explanation of the phenomena of life is to be sought"; and he develops his views largely by studying the nature of diffusion in liquids and the phenomena

¹ "La Biologie Synthétique." By Prof. Stéphane Leduc. Pp. ii + 217 (Paris: A. Poinat, 1912.)