VII.—NEW BOOKS.

The Limitations of Science. By L. T. MORE, Ph.D. Constable & Co. Pp. 268.

The first six chapters of this book consist of a severe criticism of the use of hypothesis (in a certain sense) in physics, with illustrations from modern views about electricity and mechanics. The seventh and last chapter criticises the attempt to make natural science an 'arbiter of ethics'.

Mr. More contrasts the abstractive method, by which he considers that all valuable discoveries in science have been made with the hypothetical method, which he regards as always useless and seldom harmless. The abstractive method consists in analysing what we actually perceive, generalising the regularities which we discover and thus formulating laws which still refer to what we might perceive, and then verifying these laws by extending our observations. The hypothetical method for Mr. More seems to be exemplified by any theory which attempts to explain what we do perceive in terms of what we do not and cannot possibly perceive.

Of course even the abstractive method uses hypothesis as Mr. More would admit. It was an hypothesis of Newton's that all bodies attract each other with a force proportional to their masses and inversely proportional to the square of their distances. But it was not an objectionable hypothesis because it simply extends to all observable bodies what had been found true for some observed bodies. On the other hand the wave-theory of light according to Mr. More is objectionable because it attempts to explain what we do perceive—light, its reflection, refraction, etc.—by the action of what we cannot perceive under any circumstances, *viz.*, waves in a supposed medium. It would seem then that the real difference between sound and objectionable hypotheses rests on whether the entities assumed in them are merely more of the same kind as we can observe or are ones which we could not possibly observe.

Now why does Mr. More object to the latter sort of hypotheses? His argument is not very clear or systematic, but his main objections seem to be these: (1) No hypothesis of this kind has ever led to a discovery; (2) Such hypotheses cause much waste of time and ingenuity. People of great eminence argue with great learning about whether electrons which no one can possibly prove to exist be spheres or discs, and so on. If they devoted the same ingenuity and labour to the abstractive method we should know much more about nature than we do; (3) The decision between rival hypotheses is a purely subjective matter; (4) Some hypotheses (*e.g.*, Einstein's *Theory of Relativity*), though logically flawless and compatible with all observed facts wantonly conflict with commonsense, or postulate what is simply not realisable in imagination.

(1) With regard to the first point Mr. More uses an argument which is certainly fallacious. He says: How could a hypothesis (such as the corpuscular theory of light) have led to the discovery of a fact (*e.g.*,

aberration) when we continue to believe in the fact after we have ceased to believe in the hypothesis? This however is a complete *non sequitur*. If p implies q and I believe p this may lead me to look for q. If I find q and then later cease to believe p this will not alter the fact that p *does* imply q and that it was my belief in p that made me look for q. I think Mr More makes the following false argument. From (1) Either of several incompatible hypotheses might have caused me to discover the same fact; he concludes (2): This proves that none of them actually led me to discover the fact. As a matter of history I should have thought there were several cases where obscure phenomena were only looked for because they were implied by some hypothesis which was believed at the time. One example is conical refraction, another is the discovery of the small light spot in the middle of the shadow cast by a disc. These were both looked for because they were rather odd and paradoxical results of the wave-theory of light.

(2) We may agree that from a practical point of view very minute researches on the structure of purely hypothetical entities like electrons are rather a waste of ingenuity. But I certainly cannot agree that the working out of hypotheses has interfered with the experimental study of nature. On the contrary it is continually setting problems to the experimenter by suggesting results which ought to follow if the hypothesis be true. *E.g.*, Maxwell's theory led to the important experiment on whether a moving charged body cuts as a current, and the common hypothesis about light and its transmission through the ether led to the celebrated Michelsen-Morley experiment. The one is an example where the experiment supported the hypothesis that suggested it and the other one where the experiment refuted it.

(3) At one point (p. 144) Mr. More goes so far as to say that if a system can be developed from one set of postulates the same result can always be obtained from their contraries. As a matter of pure logic this is mistaken. From the premises P*a* M and M*e* S the conclusion S*e* P can be obtained; from their contraries, P*e* M and M*a* S we cannot derive S*e* P. I do not deny, however, that there are a few fundamentally different ways of viewing the universe (*e.g.*, atomism and hydrodynamical views) from which the same results can be deduced if suitable special postulates be made. The real fact, of course, is that the probability of a hypothesis depends on two factors: (1) How far does it fit the facts? and (2) What is its intrinsic probability? The first is not subjective at all. The second is only partly so. Any hypothesis consists of a number of logically independent assumptions, and the intrinsic probability of each of these is no doubt largely subjective. But, other things being equal, the intrinsic probability of the hypothesis that contains more logically independent assumptions is less than that of the hypothesis that contains fewer. And this, I suppose, is one of the strongest supports for the Theory of Relativity; the Michelsen-Morley experiment could have been explained on the older views by introducing further independent assumptions of little intrinsic probability. The Theory of Relativity enables us to avoid these, and rests its assumptions on experimental facts and on certain genuine but commonly ignored characteristics of our measurements of distances and durations.

(4) Mr. More severely attacks Larmor's theory of electrons as strains in the ether and the modern mechanics which is associated with Einstein, Minkowski and Planck. I must admit that I do not understand what precisely can be meant by a moving strain in a non-mechanical ether. Mr. More seems to admit Einstein's general reflexions about measurement; but he argues that these ought not to affect our commonsense notions of time and space. Now it is perfectly true that Einstein's

theory only *directly* touches our measures of space and time, and that all the results of the Theory of Relativity can be made to square with the most traditional views of space and time. But, as against Mr. More, I should hold that the newer views are an attempt to be *more* empirical not to be more abstract and hypothetical. What is felt is that the space, time and matter of science have a suspiciously 'tidy' and artificial appearance; that they differ greatly from what we can measure and from what we directly experience. Surely it is not unreasonable to try and start with som thing nearer to what we actually experience and measure —a kind of mixture of space, time, and matter—and show how the space, time, and matter of ordinary physics with their tidiness and independence can be logically developed out of it. This seems to me to be the merit of Einstein's work, and still more of that of Messrs. Russell, Whitehead, and Robb.

Mr. More further objects to the attempt to explain mechanical mass in terms of electricity. He says quite truly that we never find electricity apart from matter and that since we can only measure $\frac{e}{m}$ for electrons it is just as open to us to assume the constancy of m and the variability of e as the converse. I imagine that the answer that would be made is that we seem to have no prospect of explaining electrical charge in terms of inertia, whilst the laws of electricity in motion on the bodies of daily life do involve the phenomenon of electro-magnetic inertia. There is thus an obvious simplification in trying to build up a theory in terms of electrical charges and their laws alone.

I must next notice some very odd remarks on physical and mathematical points which occur in various parts of the book. (1) Page 11. 'If ether be discontinuous it must be porous and what becomes of our link between atoms?' I do not see why one atom should not be connected with another by continuous (but crooked) bands of ether even if ether had many holes in it. (2) Page 29. If 'the transmutation of elements' was 'announced as an assured fact' it was not because radium 'gave off energy,' but because it gave off a series of emanations of decreasing atomic weight and varying life which ended with helium. (3) Page 53. Ether must have friction to be set in motion by electrons; it cannot have friction or it would absorb light or heat energy. I do not see why there should not be friction between matter and ether, but no friction between one part of the ether and another. (4) Pages 62-63. It is apparently argued that it is some objection to the electron theory that electrons may be divisible; and it is supposed to be ominous that the chemical atom is now regarded as divisible. But surely no physicist needs to suppose that *nothing* will divide electrons. It is enough to say that there are certain stages in the division of matter—molecules, atoms, electrons—where forces that broke down the larger aggregates cease to be able to break down the smaller ones. (5) Page 86. Descartes's law of the conservation of momentum was the progenitor of the conservation of energy. Surely not. The law of the conservation of momentum still holds, though Descartes's form of it was wrong. (6) Page 148. 'Kant maintained that we were endowed with an innate idea—of pure space and time. These qualities are, however, by themselves inappreciable to our senses. To make them sensible we need a third' (What?), 'which he calls the Ding an Sich'. I hardly think that most people will recognise Kant's theory of time, space and matter in this account. (7) Pages 175-176. This is a curious argument to prove that the velocity of radiant energy is not merely relative. To prove this the author says that energy depends on the square of the velocity and this squared velocity must necessarily be positive, independent of direction. With regard to this astonishing argument I can only say (*a*) that the energy of radiation does *not* depend on the square of the velocity of transmission, which is what Einstein is talking about, but on the square of the amplitude of disturbance. And (*b*) that if it did depend on the square of the velocity of transmission, I fail to see how this could prove that the velocity itself was an absolute one.

In the final chapter Mr. More discusses and rejects the view that science can provide a system of ethics. He seems to base his denial, which is certainly true, largely on the difficulties of eugenics! He fails to see that if eugenics were perfectly satisfactory in theory and in full swing in practice it would still not be *ethics* that is founded on science, but certain practical rules as to the best means to a class of goods.

Frankly I cannot highly recommend this book. Much that is in it is true; but the arguments have little tendency to support the conclusions, the science is often inaccurate, and the philosophy confused.

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Three Lectures on Æsthetic. By BERNARD BOSANQUET, D.C.L., LL.D. Macmillan & Co., Ltd., 1915. Pp. vi, 118. 3s. 6d. net.

All those who are acquainted with Mr. Bosanquet's history of Æsthetic will be glad that he has now given us an account in outline of his views on its fundamental problems, so that his main positions can be more readily grasped than in a work which was first and foremost a history. In three lectures, of course, only the most important points can be touched on; yet Mr. Bosanquet has succeeded in saying a great deal within these narrow limits, and has not only expounded his general theory pretty adequately, but has illustrated its application to several of the disputed points in the subject.

Æsthetic is looked on by the ordinary man with even more suspicion than the other departments of philosophy; and it must be confessed that the literature of the subject is rather bewildering. One reason for this is probably that there is no one generally recognised starting-point for the inquiry; one writer begins by examining the nature of beauty, another with the æsthetic feeling, a third with the æsthetic judgment. And although it is no doubt impossible to proceed very far on any one of these roads without striking the others, still these differences add to the difficulties of the student.

Mr. Bosanquet starts from the simplest æsthetic experience—a pleasant feeling of a certain kind; this is probably the best starting-point, since it assumes nothing beyond what is certainly present in our experience, whereas inquiries into the nature of beauty are held up at the very beginning by the question whether beauty is subjective or objective. Lecture I. is occupied with the further definition and analysis of this æsthetic attitude. The author proceeds by successive partial definitions, each carrying the analysis a step further; and the conceptions which are needed are introduced and explained as the analysis proceeds. "Object," "embodiment," "contemplation," "form," "imagination," and "expression" are the chief of these conceptions; and finally we reach the description of the æsthetic attitude as: The pleasant awareness of a feeling embodied in an appearance presented to imagination or imaginative perception, or alternatively: Feeling expressed for expression's sake, or, as it is stated earlier in the lecture: Feeling embodied in "form".

Lecture II. explains how art, which in its primary form consists of such things as simple patterns, is widened and deepened by the bringing in of representation. And it is shown how the representation of a natural