Study of Insects," gives a short and popular account of entomology generally, by taking a series of types from amongst the best-known North American insects, and describing them in detail. We should have liked to find some of the descriptions rather more explicit, as they might have been, without any alteration in the size of the volume, if some of the illustrations had not been so frequently repeated. In a work like Euclid there is no doubt considerable advantage in having the figures so placed that it is not necessary to turn over the pages in referring to them, especially when it has to be read by boys; but when space is short and the subject of such general interest, we cannot help feeling that their repetition, three times in more than a single instance, is quite uncalled for. The author's own work at the development of Insecta, which he has published in the "Memoirs of the Peabody Academy of Science," enables him to take a larger view of his subject than that held by most. This is particularly indicated in the very suggestive chapter entitled "Hints on the Ancestry of Insects," in which the researches of Ganin, Lubbock, Brauer, Haeckel, and Müller are all brought to bear on such questions as the relation of the *Zoca* form of the embryonic Crustacean to the similarly undeveloped and generalised, here termed *Leptus*, form of Insecta, in which the configuration is ovate, the head is large, bearing from two to four pairs of mouth-organs resembling legs, and the thorax is merged with the abdomen; this general embryonic form characterising the larvæ of the Arachnida, the Myriapods, and the true Insects. The elaborate observations of the first-named of these authors on the development of *Platygaster error*, an ichneumon parasite, in the author's mind tend to confirm the theory held by him that the ancestry of all the Insects, including the Arachnids and Myriapods, should be traced directly to the worms. We recommend this small book to all interested in the progress of this branch of invertebrate zoology.

The Transactions of the Academy of Science of St. Louis, vol. iii. No. 1. (St. Louis, U.S., 1873.)

THIS volume contains a journal of the proceedings of the Society from March 1868 to January 1873, and a few papers *in extenso*. The latter are:—Notes on the Genus *Yucca*, by G. Englemann; On the new Genus in the Lepidopterous Family Tineidæ, with Remarks on the Fertilisation of the *Yucca;* and Supplementary Notes on *Pronuba yuccasella*, by C. V. Riley; Descriptions of North American *Hymenoptera*, by B. D. Walsh; Atmospheric Electricity, by Dr. A. Wislizenus, being the yearly report of atmospheric electricity, temperature, and humidity, from observations made at St. Louis; Catalogues of Earthquakes for 1871, by R. Hayes; and On the Occurrence of Iron Ores in Missouri, by J. R. Gage. Mr. Hayes, on the basis of the recorded earthquakes from 1739 to 1842 has found that the "largest maxima occurred in the years of the heliocentric conjunction and opposition of Jupiter and Saturn, with but three exceptions, and in these cases the increase began in those years, but the maximum was not reached till the following year." He suggests that "these planets induce electric currents which *call into action* those forces to which the causes of seismic phenomena are usually ascribed."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Herbert Spencer and *à priori* Axioms

MR. HERBERT SPENCER (vol. ix. p. 461) has "ended what he has to say on the vexed question of the origin of physical axioms" by laying down—

(1) That "the perceptions and inferences of the physicist cannot stand without *preconceptions which are the products of simpler experiences than those yielded by consciously-made experiments.*"

(2) That "the preconception which immediately concerns us is the exact quantitative relation between cause and effect."

(3) That "if definite quantitative relations between causes and effects be assumed *à priori*, the Second Law of Motion is an immediate corollary."

By speaking of it as an "*immediate corollary*," I presume that Mr. Spencer means that Newton's Second Law of Motion is the proposition obtained by substituting for the general term, *cause*, the particular term, *force*, and for the general term, *effect*, the particular term, *motion generated;* so that, according to Mr. Spencer, this law simply asserts "a definite quantitative relation between a force and the motion generated by that force." But surely the quantitative relation asserted by Newton is not only *definite*, but is further the *special* relation of *proportionality;* so that, if the law is an immediate corollary of an *à priori* assumption, the assumption must be that "the exact quantitative relation between cause and effect is that of direct proportionality," or in more familiar words, that "effects are proportional to their causes." Perhaps this is what Mr. Spencer meant to assert. At any rate let us admit it as a definite basis for reasoning, and endeavour to deduce some consequences from it.

"The cause of a stone falling when left to itself is its weight; but 'the greater the cause, the greater the effect,' therefore the greater the weight of the stone the more quickly will it fall, and thus of two stones let fall from the same height, the heavier will reach the ground sooner than the other." Something of this kind, it may be presumed, was the argument of Aristotle and his followers before the age of Galileo: and how on *à priori* principles is it to be refuted? Of course it is disposed of at once by the simple observation that the same force does not produce the same motion in different masses: but independently of some such observation or experiment, it seems to me impossible to deny that it *may be* true, though even an *à priori* philosopher might show that, as other alternatives are conceivable, it is not *necessarily* true. As a matter of historical fact, Galileo refuted it once for all by the "consciously-made experiment" of letting two different weights fall simultaneously from the leaning tower of Pisa.

But it may be said that the above argument is hardly "*definitely* quantitative." Let us then examine Newton's Second Law of Motion as an "immediate corollary" of our *à priori* assumption. Here the cause is "the motive force impressed," and the effect "the alteration of motion." But then the question arises—how are the quantities of this cause and effect to be measured? Newton carefully defines quantity of motion as proportional to *mass* and *velocity* jointly; that is, he measures it by *momentum*. From another point of view it would have been correct to measure quantity of motion by *kinetic energy* or *vis viva*, that is, as proportional to mass and the *square* of the velocity jointly. Further the "alteration of motion" might be measured either with respect to a given time or to a given space. Newton implies the former, and consequently the explicit statement of his second law is that "the *momentum* generated *in a given time* by an impressed force is proportional to that force." Substitute for this "the momentum generated in moving through a *given space*," or "the *kinetic energy* generated in a given time," and the law becomes untrue. Substitute "the *kinetic energy* generated in moving through a *given space*," and we have a law which is true, but not that which Newton asserted as his second law. Now among these four alternatives how is our *à priori* philosopher to decide? He might perhaps analyse them further and show that some of them are inconsistent with the others, and I believe he might reduce the questions to be decided to still simpler ones; but I fail to see (in common, I believe, with everyone who has thoroughly grasped the fundamental principles of rational mechanics) how, without recourse to consciously-made observations or experiments, he could arrive at a certain conclusion.

May we not say then that these great *à priori* principles, whatever value they may have in a "System of Philosophy," are of little avail in any special science, and that the "axioms" of such science, however much they may involve these principles, are not mere "immediate corollaries" therefrom?

If not intruding too much on your space, I am tempted to apply to Mr. Spencer's great principle of the "Persistence of Force" the same mode of treatment as I have applied above to the principle that "effects are proportional to their causes."