

eous inferences, prejudicial to a correct understanding of what is really taking place and to the setting up of wrong standards in respect to the degree of difference legitimately open to recognition by name.

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PRESIDENT MINOT ON 'THE PROBLEM OF CONSCIOUSNESS IN ITS BIOLOGICAL ASPECTS.'*

SCIENCES, like human beings, are seldom indifferent to the good opinion of others. Even age and great respectability never wholly dull the moral consciousness of a science to the approval and disapproval of its neighbors. This sensitiveness is, however, keenest and most easily wrought upon in the younger sciences, for the reason that these are most frequently challenged to defend their right to exist. Self-consciousness—provided it does not approach morbid embarrassment—is by no means a misfortune to the youthful science. It clears up its concepts, gives self-confidence and helps it to get on with its fellows. Psychology has had, more than most sciences, to give a strict account of itself and of its methods, both because it has had an unusual amount of prejudice to overcome and because it has developed in an unusually critical and criticising period of thought. The social pressure has, however, served its purpose, so far as psychology is concerned; for psychology—even as an experimental science—has passed its majority and knows perfectly well what its task is and how it means to perform it. But, while this is true, and while one science is never, within its own borders, responsible to any other coordinated branch of knowledge, there is, as I have intimated, the temptation to stop and listen when one's character and obligations are discussed in a convocation of the sciences. The temptation is not to be withstood when the discussion turns out to be the authoritative opinion of a near neighbor with whom important and amicable relations have, for some time, been sustained. Professor Minot, in his recent address at Pittsburgh, indicates what he conceives to be the most natural and the most profitable attitude of the biological sciences toward psychology. His outline involves a definition of mental phe-

nomena, a statement of the part that consciousness plays in bionomics, and an appeal to psychology to employ the comparative method. The argument of the address runs as follows:

Consciousness may be regarded either as a real phenomenon in the world or as an epiphenomenon. The 'epiphenomenon hypothesis of consciousness' is, according to the author, 'an empty phrase, a subterfuge.' "Consciousness ought to be regarded as a biological phenomenon, which the biologist has to investigate in order to increase the number of verifiable data concerning it. In that way, rather than by speculative thought, is the problem of consciousness to be solved, and it is precisely because biologists are beginning to study consciousness that it is becoming, as I said in opening, the newest problem of science." * * * "For the present, it is more important to seek additional positive knowledge than to hunt for ultimate interpretations." The 'younger science of experimental psychology' is, therefore, to be welcomed. "It completes the circle of the biological sciences." The most striking peculiarity of consciousness—a peculiarity which is common to biological processes—is that it is teleological. "We do not know what it is, we do not know how it functions, but we do know why it exists." The essential 'function of consciousness is to dislocate in time the reactions from sensations.' The evolution of consciousness is a strong indication of its usefulness to the organism. If it had not been useful it would have disappeared. It is useful because it permits the individual to react on his accumulated experiences. Sensations recur in memory and increase the scope of possible adjustments. Sensations are only symbols of 'objective phenomena.' We 'see' colors, but light—the 'external reality'—is undulations. "Objectively, red, yellow and green do not exist." These symbols are, nevertheless, convenient labels, for by means of them the individual reacts appropriately on every occasion. They are 'bionomically sufficient because they are constant.' 'They enable consciousness to prophesy or foresee the results of the reactions of the organism,' and, hence, to maintain adjustment. Animal conscious-

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ness is a homologue of human consciousness. Its function is the same. Consciousness must be posited at least as far down as sense organs and nervous systems are to be found. These considerations lead to the conclusion that 'the development and improvement of consciousness has been the most important, really the dominant, factor in the evolution of the animal series.' Sensory and motor organs have multiplied for the sake of consciousness; to supply it 'with more possibilities of adjustment to external reality.' Since mind is teleological, it must be primary, and reflexes and instincts derivative. Through habit, consciousness sometimes lapses—for the sake of rapidity in reacting—and reflexes and instincts take its place. If mind has been the most important factor in the evolution of the animal series, 'the necessity of treating consciousness as primarily a problem for biological research to solve' is obvious. The 'psychologists ought now to apply the comparative method on a grand scale.' Psychology is extremely backward; but with the new method we may come 'to the understanding of even consciousness itself.' Consciousness is not a form of energy; it is as ultimate as force, or energy; but it 'has the power to change the form of energy.'

There is little doubt that Professor Minot's plea for a closer alliance between biology and psychology will be seconded, heartily, by many biologists. The advantage promised to the sciences of life is certainly alluring. It is true that the alliance proposed would affect a comparatively small part of the field of biology—that part of zoology which deals with the descent of the higher animal forms—and, likewise, a comparatively small part of the field of psychology. Even if we grant that consciousness has as wide a range as the author maintains (many investigators in both sciences would make the limits much narrower), there is only a portion of one problem in one of the great subdivisions of biology that can hope for direct aid from psychology. Nevertheless, no one can deny either that the problem has enormous proportions or that the promised aid is worth acquiring. As for the other science involved, psychology frankly

recognizes the importance of studying mental development. But she can scarcely consider incidental aid to be rendered another science a sufficient excuse for abandoning her work in general in order to solve a single problem.

But, again, let us see whether the biologist's demand for consciousness is as urgent as it appears to be. Even though he admit mind as a factor in evolution, he is not thereby relieved from considering the development of the nervous system as a likewise important factor. He will hardly deny that a complex and highly differentiated nervous mechanism is an advantage to the organism. If he deny this, what becomes of his argument for the usefulness of surviving organs? If man stands high in the phylogenetic series because he has a good mind, he also—by the same argument—stands high because he has a good brain; a brain that affords him more complicated and appropriate reactions than other animals can compass. And why not go a step further? Since there is no question, either in psychology or in biology, that new mental functions imply new nervous apparatus, or at least new nervous functions, why should the biologist duplicate his factors and posit a double cause for a single effect? If mind 'dislocates' sensations in order to unite the past and the present, the brain—much more literally—preserves a disposition to functionate as it has already functionated, and thereby brings profit to the organism. If consciousness 'lapses' and is replaced by reflexes, in order to insure more rapid adjustment, neural functions cut corners, follow lines of least resistance and become simplified to the same good end. If one sensation 'inhibits' another—a dubious doctrine!—activity in one part of the cerebrum undeniably checks activity in another part of the cerebrum. It is natural that the biologist should make excursions into psychology when he stands in temporary need of links which are missing to his phylogenetic chain of causes; but if he wishes to make consciousness 'the dominant factor in the evolution of the animal series,' he should first show that consciousness contributes something to descent that is not contributed by the physical processes underlying consciousness. When

he has done this and has settled accounts with energetics—for introducing an element which is not energy, but which changes the form of energy—he will be ready to launch his theory of psychophysical causation. He may, even, found a science of psycho-bionomics, which shall stand in precisely the same relation to psychology and biology that psychophysics now stands to psychology and physics.

This last point raises, very naturally, the question of the scope of the biological sciences, a question that has been so often discussed that one is inclined to apologize for raising it. However, Professor Minot's two-fold assumption that 'scientific psychology' is one of the 'great divisions of biology' and that 'the biologist must necessarily become more and more the supreme arbiter of all science and philosophy,' is sufficient excuse. The first part of the assumption is largely a matter of definition and need not distress the psychologist who shrinks from being lost in a vast science of life. If biology can be made to cover all systematic knowledge of the whole—the psychophysical—organism, then it includes psychology; but if it continues to cover the structures, the functions and the histories of organic bodies, then, just as surely, psychology lies outside biology. The choice rests on the likeness or difference of subject matter and the likeness or difference of method. The subject matter of psychology is, as Professor Minot admits, unique. Consciousness is as ultimate as force. As for method, no psychologist with reliable instincts ever does confuse his method with the method of the embryologist or the physiologist, any more than he confuses it with the method of the physicist. He may and does (when it suits his purpose) use—as the author advises—the 'comparative' method, which is 'method' in a narrower sense. So do the historian and the geologist; but they are not, for that reason, accused of writing biologies.

The contention that the biologist must become the 'supreme arbiter' because 'human knowledge is itself a biological function' is a challenge to the epistemologist rather than to the psychologist. The epistemologist will not, I imagine, find it difficult to prick the vulner-

able point in the argument. He may, perhaps, reduce the claim to an absurdity by insisting that it makes biology the universal science as well as the only true philosophy, or he may show that the contention is itself a *petitio*, because it assumes but cannot, so far as it is—as a bit of knowledge—a mere function, stand warrant for its own validity. It is good Baconian doctrine to advise, 'observe more and more and in the end you will know. A generalization is a mountain of observations, from the summit the outlook is broad.' But one does not quite see why it is the biologist—of all the normally functioning organisms in the world!—who is capable of generalization; why 'we must look to biologists for the mighty generalizations to come.' Does, then, the biologist monopolize the function of knowing as well as the study of that function? Or is this only a specific application of the advice, 'know then thyself'? The argument is not quite clear on this point. And, as for the observations, a mountain can neither see itself nor its surroundings. If observations could give their own systematic setting, we should be more inclined to hold them to account when they form a mere heap of dry facts set in a waste of words. As the author says much more truly in another connection, 'our mental wealth * * * consists of the thought into which the data of observation are transmitted [transmuted?]' rather than in the observations themselves. We may take it for granted that Tyndall's 'Tories' in science, who look upon 'facts' as alone having value and who 'regard imagination as a faculty to be feared and avoided rather than employed,' are an extinct class and that even 'deep meditation' is indispensable alike to science and philosophy. On the other hand, the command to 'observe more and more' will scarcely find a heretic to resist it in these days of loyalty to science. I cannot speak for biologists, but I am sure that I can speak for psychologists—the class to whom Professor Minot especially directs his exhortation. Thirty years ago, psychologists left off searching for the ultimate nature of mind and began to clamor for actual knowledge about mental experience. Long since, the tendency to 'observe' has become instinctive,

and for this reason, and this reason only, it is seldom discussed.

The author says that 'the results of experimental psychology are still for the most part future,' though we may even now 'obtain some valuable preliminary notions concerning consciousness from our present biological knowledge.' The statement can be accepted only if one disregard the mass of psychological material that has been collected since Fechner wrote his 'Elemente der Psychophysik,' Helmholtz his 'Physiologische Optik' and 'Tonempfindungen' and Wundt his 'Grundzuege der physiologischen Psychologie.' With a current literature of approximately three thousand titles in the year, a literature that covers every phase of consciousness, with hundreds of trained workers who are making observations in scores of laboratories the year round, it is plain, at least to any one within the science, that disregard of the injunction to observe is not psychology's ruling vice. As for the attitude toward mind that psychology should take—that is, naturally, a problem which the science must solve for herself. For herself, because psychology's first business is to know mind quite apart from any special use that any other discipline—biology, pedagogy, sociology—may wish to make of mental phenomena. A science must choose her own way; a *vis a tergo* from a well-wishing friend can only cause her to stumble.

More specifically, the president's address urges a genetic study of mind because the 'why' of mind, its teleological function, can be investigated with profit while the study of 'what it is' is 'recondite,' metaphysical, and carries us beyond the limits of verifiable human knowledge.' The force of this argument depends entirely upon what one understands by 'why' and 'what.' There is, surely, a scientific 'what' as much as there is a scientific 'why'; and there is as truly a philosophical 'why' as there is a philosophical 'what.' The morphologist and the analytical chemist ask 'what.' They deal with structure. On the other hand, the biologist is answering a 'why' when he explains that mind exists for the sake of the body's 'adjustments to the external conditions' and that the body exists—at least 'a

large part of our anatomical characteristics exist for the purpose of increasing the resources of consciousness.' Mind for body and body for mind! That is a game of teleological 'tag' that is neither 'recondite nor metaphysical.' But should the biologist ask 'why adjustment at all?' 'why evolutionary process?' 'why not being without becoming?' he would find himself as far outside his science as is the hypothetical psychologist who is concerned with the question of the ultimate nature of mind. Surely, observation is, first of all, looking for what is 'there'; 'there' for psychology in one's own consciousness, in the consciousnesses of one's fellows and, later, in the consciousness of the child, the animal, the abnormal, the savage. It is safe to assert that no one can point to a single piece of successful genetic work in psychology which is not based upon a more or less adequate study of 'what consciousness is' in the human adult. Indeed, this must be the case. The development of a thing cannot be described correctly until the thing itself is known. It is, one may admit, not difficult to construct hypothetical consciousnesses for the amoeba, the jelly-fish, the bee and the beaver; consciousnesses which shall explain beautifully the reactions of these animals. But the question arises whether these hypothetical minds really exist. Oftentimes they do not. The recent history of genetic psychology is filled with fictitious minds which are worse than useless to the psychologist, whatever their value may be to the biologist. One proof of their unsatisfactoriness, even as agents of natural selection, is given, I am inclined to believe, in the well-marked tendency within biology to explain reactions of the simpler organisms in terms of 'tropism' and 'taxis' instead of in terms of 'volition' and 'reason.'

The question of the 'epiphenomenal' nature of mind has little interest for the psychologist. 'Epiphenomenalism' or 'automatism' is not a psychological concept. Huxley introduced it into biology to show that biology has no real concern with consciousness, since consciousness—as he affirms—does not react causally upon the body. Psychology rejects the term 'epiphenomenon' not because it denies a

causal relation between mind and organic processes—a majority of psychologists, perhaps, refuse to admit such a relation—but because it implies that there is a great gulf fixed between ‘real’ things in the world, phenomena, and epiphenomena, the conscious ‘foam’ of existence. For the wide use of the term, modern biologists must surely share responsibility with modern monists. It will interest the psychologist, even if it does not instruct him, to hear from biology the authentic statement that ‘consciousness is too familiar to all men to be summarily cast aside and dismissed.’ But, for psychology, mental facts are not a whit more important or more valuable if consciousness turns out to have a survival value. They are important to their own science just because they are a body of facts of experience that are capable of being worked into a system. The argument from the survival value of consciousness—an argument that has had at least twelve years of popularity—gives, it will be generally admitted, some support to the position of the interactionist. But however relevant the argument may appear to biology, it does not persuade the psychologist that the facts of consciousness are one iota more real or more important than he had before considered them to be. Even though he adopt the theory, he will find no reason for making a radical change in his attitude toward mind. Hence, should the very most ‘essential function’ of consciousness prove to be the ‘dislocation’ of reactions, it is biology and not psychology that will need to be apprehensive of the effect upon the organism of so serious a luxation.

There is one further point in President Minot’s address that I shall venture to criticise, although it is more a matter of general methodology and of the science of knowledge than of psychology. In reviving the argument that sensations are symbols, labels, not images; that ‘external reality’ is a ‘series of undulations’ or a series of ‘vibrations of the air’ and not colors and sounds, which have no ‘objective’ existence, the author falls into the ancient fallacy that, somehow, men can be conscious of an external world that is ‘screened from’ consciousness. The fallacy

appears here in an aggravated form. The ‘dislocation’ argument implies that consciousness is made up of sensations, but sensations have no objective reality, and yet we know through sensations—thanks to the ‘biological study of consciousness’—‘that the objective world is real.’ If we grant that the concepts of any single science may be taken as representing the ‘real’ world, we may still ask why the exceptional honor should be done to physics when it is the biologist who ‘must necessarily become more and more the supreme arbiter of all science and philosophy.’ Why should the biologist, when he is casting about for a real world, adopt the ‘doll-idea’ of the physicist? Perhaps it is done in return for the service which physics—by the loan of her ‘real’ undulations—has rendered biology in settling ‘the debate in favor of the view that the objective world is real.’ But the logic of the article seems to require that the most real thing be an organic reaction, or an adjustment, or the evolution of species, and not a disturbance of the air or the ether. However, if—as President Minot urges—all science is, after all, ‘symbolic’ as ‘all sensations are symbols of extreme reality,’ why should we ‘make believe’ in the reality of any of her ideas? If sciences as well as sensations display a ‘peculiar untruthfulness to the objective,’ why deceive ourselves with ‘pseudo-opinions’—why ‘come to fight with shadows and to fall’—when ‘behind in consciousness’ there ‘is the sense of unreality’? The practical advantages of getting on in the world, of ‘prophesying’ the results of reactions, will hardly atone for so gross a self-deception.

Science, as well as popular belief, still cherishes its *pseudodoxia epidemica*. Of these, none is more amazing than the claim of a single science to hold the quintessence of human knowledge; to stand as the ‘supreme arbiter of science and philosophy.’ One expects to find this lack of perspective in the various forms of occultism, but one is inevitably dismayed to find it in science. The various borders of knowledge everywhere overlap. Were this not true, hope of ever knowing the cosmos would be vain. As a consequence, there will always be the possibility of dispute

over the relations of one discipline to another. But this ought not to hinder any science from setting its own limits and doing its own work, while it accepts all the aid it can get from others. No science is more widely indebted than is psychology; but psychology demands, no less than others—such is scientific selfishness!—that she be allowed to work out her own destiny in her own way.

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SHORTER ARTICLES.

THE SALT MARSH MOSQUITO, *CULEX SOLLICITANS* WLK.

IN SCIENCE for January 3, 1902, p. 13, under the caption 'Concerning Certain Mosquitoes' I pointed out that *Culex sollicitans* was the dominant form throughout a large portion of the State of New Jersey. Upon our ability to control this species depended the riddance of the State to any notable extent, and the life cycle of the species became, therefore, a matter of the greatest importance. I suspected even at that time that this species departed materially from the stock history given for *Culex* and assumed for this species; but my observations had been sufficient only to suggest the need of closer study. I showed at that time that, by breeding in salt water and by migrating for long distances the species had distinctive characters. And, bye-the-bye, there is no more perniciously erroneous popular statement than that mosquitoes do not fly far from the place they were bred. It is absolutely untrue of most of the species and not entirely true of any. The only case where it is practically true is where a species is limited in its breeding places, *e. g.*, the species that breeds only in the leaves of the pitcher plant. Of the salt marsh mosquito it is conspicuously incorrect.

In February and March I started a hunt for the adults on the supposition that the female hibernated. My assistant, Mr. Dickerson, searched every nook and cranny that might shelter a mosquito in a seashore locality where, during the summer, the insects had driven out all guests. *Culex pungens* and

Anopheles were found in numbers; but of *sollicitans* not one! I had no better luck when I took up the search myself, and even a reward offered to the natives for every specimen brought to me, failed to produce returns. I concluded, therefore, that the insects did not winter in the adult stage and began a hunt for larvæ. I knew that *Aedes smithii* wintered in the larval stage and that the wigglers would stand repeated freezings. But I failed also to find larvæ in the very regions where they were abundant in 1901, and where I had also seen them in 1900.

A wintering in the egg stage was unknown for *Culex*, but I was driven to that alternative and watched carefully for 'signs.' They came as the water warmed up. First, larvæ were found in pools high up which had been filled by the winter tides. The temperature of the water was distinctly higher than that of the air in the morning and evening and several degrees higher than that of sea water. Area after area became populated and there were millions of larvæ, growing very slowly, before a solitary mosquito was seen. A hibernation in the egg stage seemed obvious; but I ran against the fact that some of the areas swarming with larvæ were dry during the summer and fall of 1901 and became water-filled only during the winter storms. If the eggs hibernated on that ground they must have been laid on dry soil or on the grasses! This then was the point to which I had arrived at the opening of the breeding season. College duties and other matters prevented a resumption of the work until July 7, when Mr. Dickerson and I spent a week at Five Mile beach; I kept him in the field another week alone and rejoined him when the experiments were expected to produce results. Our outfit consisted of a series of seven tubs sunk into the marsh so as to project only a little above the level. In five of them was placed sod from the marsh and two were left bare. Sea water was placed in all save one of the tubs in varying quantities. Two tubs were left open—one with sod, one without; the others were covered with mosquito netting. Conditions along shore at this time were very dry and breeding places were fast disappear-