festation of the excessive meekness of American botanists. There may well be biological phenomena, the *record* of whose occurrence is more important than their measurement and which should be recorded even if they can not be measured. For example, only a few years ago, 1931, eel grass (*Zostera marina*) was common in the shallow waters of the Atlantic seaboard, from North Carolina to Nova Scotia—now it is rare. Its diminution was so sudden that no opportunity was given for statistical study, even by the quadrat method. Yet obviously the biologists of the future are entitled to the information that a striking phenomenon occurred in our coastal waters at this period, even if we are not able to furnish figures.

This is, of course, an extreme case, but somewhat similar situations arise over and over again in our consideration of the variations in the incidence of plant diseases. For example, it is biologically and economically important to know that bacterial wilt was exceedingly abundant on sweet corn in the Hudson Valley of New York in 1932 and 1933 and very rare in 1934 and 1935, but whether the loss occasioned in the earlier years was 20 or 40 per cent. and whether the loss in the two later years was one half or three fourths of 1 per cent. is of merely academic interest. In 1932 and 1933 the losses were disastrous, and in 1934 and 1935 negligible.

Nothing could be further from my thoughts than to suggest any radical reform such as would be needed to alter our general professional attitude or develop new concepts and methods particularly suited to the study of living things. I certainly cherish no illusions as to the possibility of securing some slowing down of the rate of accumulation of observations or even a little breathing spell during which we might consider what, if anything, these accumulated facts signify. Quite the contrary, I propose merely that we students of living things shall not restrict ourselves to the type of observation or record prescribed by devotees of other branches of science, but shall record as clearly as we may whatever phenomena seem interesting to us, even though we can not measure them with great accuracy. For such unrestrained self-expression, Dr. Sarton has recently furnished an adequate slogan in his book, "The Study of the History of Science"—"No scientist worth his salt has ever abandoned an investigation simply because the attainable precision was too low."

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## "RACES" AND "HOMING" OF SALMON

IN support of the theory of the "homing" of salmon from distant places in the sea, Dr. Willis H. Rich[1] puts forward the argument that the local "races" of the Pacific salmon could not exist if the fish did not return to their own rivers, seeing that large numbers of them travel hundreds of miles in the sea before entering streams. It would appear, however, that the theory of "races" is in somewhat comparable condition to that of "homing" from distant places, in that adequate proof is lacking.

The characters that have been used to distinguish "races" in species of marine fishes, such as herring and cod, are being demonstrated to result from the action of the environment on the individual during its lifetime, so that it seems doubtful whether there are heritable differences between the populations of different districts. Without such differences the use of the term "race" would seem valueless. It would be interesting to know whether the "races" of any species of Pacific salmon have been shown by rigid experiment to have differences that are heritable rather than the effect of the environment.

It has been maintained for the Atlantic salmon not only that the different rivers have more or less peculiar "races," but also that the same river may contain two different "races," one entering early and the other late in the season, although not spawning at different times. This theory has been causing the Canadian Government to spend considerable money in securing the early running fish and in keeping them till spawning time for breeding purposes, since both anglers and commercial fishermen desire the early fish not only because they are available in the fishing season, but also because they tend to be larger than the late-running fish.

As crucial a test as possible[2] was made of this theory of "races" by taking the fry of Restigouche salmon, which characteristically run early and large (ordinary salmon and big salmon) and planting them in a salmonless branch of Apple River at the head of the Bay of Fundy in the middle of a district characterized by the salmon entering the streams only late in the season and almost wholly as grilse (small salmon). The transplantation was made in 1932 by Mr. H. C. White after studying the behavior of the local fish. He followed the result during the following years, marking the Restigouche smolt when they descended to the sea in 1934 and trapping the adults during the seasons of return. The experiment was concluded in 1936. He was unable to detect any difference between the Restigouche fish and the local fish in size (year of return), in season of return or in any other character except rapidity of growth in the stream, for which the conditions were not comparable. While such a result is no proof that races do not exist elsewhere, it is evidently desirable that local populations should not be considered to be racially

[1] SCIENCE, 85: 477–478.

[2] Ann. Rep. Biol. Bd. Can., 1932, 1933, 1934, 1935 and 1936: 43, 43, 10, 8–9, and 10–11, 1933, 1934, 1935, 1936 and 1937.

distinct until heritable differences have been definitely demonstrated.

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## PHILOSOPHY OF PHYSICS

PROFESSOR HOUSTON'S recent article[1] on the philosophy of physics discusses the significance of quantum mechanics for the philosophical problem of the existence of the external world. I believe that physical theory is neutral toward this problem, and in the following I restate a theory[2] of the relation between perception and the physical world, which provides an adequate basis for science but does not commit one to a specific philosophy.

The primary factor in science is perception. Perceptions are found to be correlated. A perception which belongs to a correlated set of actual and possible perceptions is interpreted to be a perception of some physical body. A theory of physical bodies may now be expressed by two principles. The first principle is that a physical body is a center of reference of correlated perceptions. That physical bodies exist is confirmed by the discovery of functional relations between perceptions. The second principle is that the structure of perceptions indicates the structure of bodies. Its precise version in physics is that the coincidence of perceptions for all observers signifies the space-time coincidence of the events perceived. For mathematical exactness an event must be thought of as a space-time point.

The neutrality of the preceding formulation may be exhibited by giving two philosophical interpretations, dualistic realism and phenomenalism. In traditional dualism a physical body is absolutely independent of experience; it produces perceptions by acting on the observer. The structure of bodies is indicated in perceptions because the structure of an effect corresponds to that of the cause. In dualism the physical world is the object of a constructive hypothesis. The phenomenalistic interpretation is that a physical body is a conceptual parameter which serves to correlate perceptions; thus the physical world is the object of a constructive definition. Perception exhibits the structure of physical bodies in virtue of the mode of construction of the latter.

The issue between dualism and phenomenalism is not affected by the quantum mechanical theory of measurement. In this theory measuring instruments, such as a screen with a slit, are macrophysical bodies which are experienced in perception by classical methods. The properties of microphysical entities are determined from their effects upon the measuring instruments. In these determinations principles, such as those of conservation of momentum and energy, are employed to infer the properties of a microphysical entity. Now, the functional relations expressed by physical principles are to be viewed as constituents of physical reality. Hence the microphysical entity has the same kind of physical reality as the measuring instruments. If the latter are conceptual constructs to which possible perceptions are referred, so are the microphysical entities which interact with them. If the measuring instruments are independent realities in the dualistic sense, so are electrons and photons. The choice between these philosophical interpretations falls outside of physics. Indeed, some positivists hold that since the issue can not be decided by experience it is meaningless.

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[1] SCIENCE, n. s., 85: 413, 1937.
[2] *Nature*, 136: 433, 1935.

## FURTHER DISCUSSION ON SUBMERGED CANYONS

IN the April 3, 1936, issue of SCIENCE, MacClintock and the writer advanced a hypothesis that the submerged canyons off the coasts of all continents might be the result of a change in ellipticity of sea-level. F. P. Shepard[1] criticized this hypothesis, claiming to show that it was untenable because a zero line of no change of sea-level should exist at 35° N. and 35° S. latitude. Therefore the hypothesis could not explain valleys at higher altitudes than 35°.

Shepard's reasoning contains a fallacy. Two ellipses of the same area would intersect at 35°, but the two sea-level surfaces such as we suggested would not do so. The reason for this is that there is not enough water between 35° N. and 35° S. latitude to fill the volume up to the new spheroid above 35°; therefore the new sea-level surface would be parallel to the new spheroid but considerably below it. Thus the zero line of no change of sea level might lie at 55° or 60°, as we postulated.

The writer also wishes to take exception to Shepard's statement concerning the accuracy of soundings taken by the S 48. The writer was on the S 48 when these soundings were taken, and believes the accuracy was quite sufficient for the conclusions drawn.

The writer is not at all convinced that the change in ellipticity of sea-level hypothesis is the correct explanation for the origin of the submerged valleys, but he does still consider it a *working hypothesis,* even though it may be an "outrageous" one. If a solution is to be arrived at for this complex problem, all possible hypotheses must be kept in mind and the critical data bearing on all of them collected.

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[1] SCIENCE, June 26, 1936.