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Essays in Positive Economics
Part I - The Methodology of Positive Economics *
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CAN A HYPOTHESIS BE TESTED BY THE REALISM OF ITS ASSUMPTIONS?

We may start with a simple physical example, the law of falling bodies. It is an accepted hypothesis that the acceleration of a body dropped in a vacuum is a constant - g, or approximately 32 feet per second per second on the earth - and is independent of the shape of the body, the manner of dropping it, etc. This implies that the distance traveled by a falling body in any specified time is given by the formula $s = ½ gt2$, where s is the distance traveled in feet and t is time in seconds. The application of this formula to a compact ball dropped from the roof of a building is equivalent to saying that a ball so dropped behaves *as if* it were falling in a vacuum. Testing this hypothesis by its assumptions presumably means measuring the actual air pressure and deciding whether it is close enough to zero. At sea level the air pressure is about 15 pounds per square inch. Is 15 sufficiently close to zero for the difference to be judged insignificant? Apparently it is, since the actual time taken by a compact ball to fall from the roof of a building to the ground is very close to the time given by the formula. Suppose, however, that a feather is dropped instead of a compact ball. The formula then gives wildly inaccurate results. Apparently, 15 pounds per square inch is significantly different from zero for a feather but not for a ball. Or, again, suppose the formula is applied to a ball dropped from an airplane at an altitude of 30,000 feet. The air pressure at this altitude is decidedly less than 15 pounds per square inch. Yet, the actual time of fall from 30,000 feet to 20,000 feet, at which point the air pressure is still much less than at sea level, will differ noticeably from the time predicted by the formula - much more noticeably than the time taken by a compact ball to fall from the roof of a building to the ground. According to the formula, the velocity of the ball should be *gt* and should therefore increase steadily. In fact, a ball dropped at 30,000 feet will reach its top velocity well before it hits the ground. And similarly with other implications of the formula.

The initial question whether 15 is sufficiently close to zero for the difference to be judged insignificant is clearly a foolish question by itself. Fifteen pounds per square inch is 2,160 pounds per square foot, or 0.0075 ton per square inch. There is no possible basis for calling these numbers “small” or “large” without some external standard of comparison.

And the only relevant standard of comparison is the air pressure for which the formula does or does not work under a given set of circumstances. But this raises the same problem at a second level. What is the meaning of "does or does not work"? Even if we could eliminate errors of measurement, the measured time of fall would seldom if ever be precisely equal to the computed time of fall. How large must the difference between the two be to justify saying that the theory "does not work"? Here there are two important external standards of comparison. One is the accuracy achievable by an alternative theory with which this theory is being compared and which is equally acceptable on all other grounds. The other arises when there exists a theory that is known to yield better predictions but only at a greater cost. The gains from greater accuracy, which depend on the purpose in mind, must then be balanced against *the* costs of achieving it.

This example illustrates both the impossibility of testing a theory by its assumptions and also the ambiguity of the concept "the assumptions of a theory." The formula $s = ½\ gt2$ is valid for bodies falling in a vacuum and can be derived by analyzing the behavior of such bodies. It can therefore be stated: under a wide range of circumstances, bodies that fall in the actual atmosphere behave *as if* they were falling in a vacuum. In the language so common in economics this would be rapidly translated into: the formula assumes a vacuum. Yet it clearly does no such thing. What it does say is that in many cases the existence of air pressure, the shape of the body, the name of the person dropping the body, the kind of mechanism used to drop the body, and a host of other attendant circumstances have no appreciable effect on the distance the body falls in a specified time. The hypothesis can readily be rephrased to omit all mention of a vacuum: under a wide range of circumstances, the distance a body falls in a specified time is given by the formula $s = ½\ gt2$. The history of this formula and its associated physical theory aside, is it meaningful to say that it assumes a vacuum? For all I know there may be other sets of assumptions that would yield the same formula. The formula is accepted because it works, not because we live in an approximate vacuum - whatever that means.

The important problem in connection with the hypothesis Is to specify the circumstances under which the formula works or, more precisely, the general magnitude of the error in its predictions under various circumstances. Indeed, as is implicit in the above rephrasing of the hypothesis, such a specification is not one thing and the hypothesis another. The specification is itself an essential part of the hypothesis, and it is a part that is peculiarly likely to be revised and extended as experience accumulates.

In the particular case of falling bodies a more general, though still incomplete, theory is available, largely as a result of attempts to explain the errors of the simple theory, from which the influence of some of the possible disturbing factors can be calculated and of which the simple theory is a special case. However, it does not always pay to use the more general theory because the extra accuracy it yields may not justify the extra cost of using it, so the question under what circumstances the simpler theory works "well enough" remains important. Air pressure is one, but only one, of the variables that define these circumstances; the shape of the body, the velocity attained, and still other variables are relevant as well. One way of interpreting the variables other than air pressure is to regard them as determining whether a particular departure from the "assumption" of a vacuum is or is not significant. For example, the difference in shape of the body can be said to make 15 pounds per square inch significantly different from zero for a feather but not for a compact ball dropped a moderate distance. Such a statement must, however, be sharply distinguished from the very different statement that the theory does not work for a feather because its assumptions are false. The relevant relation runs the other way: the assumptions are false for a feather because the theory does not work. This point needs emphasis, because the entirely valid use of "assumptions" in *specifying* the circumstances for which a theory holds is frequently, and erroneously, interpreted to mean that the assumptions can be used to *determine* the circumstances for which a theory holds, and has, in this way, been an important source of the belief that a theory can be tested by its assumptions.

Let us turn now to another example, this time a constructed one designed to be an analogue of many hypotheses in the social sciences. Consider the density of leaves around a tree. I suggest the hypothesis that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the position of its neighbors, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position. 14 Now some of the more obvious implications of this hypothesis are clearly consistent with experience: for example, leaves are in general denser on the south than on the north side of trees but, as the hypothesis implies, less so or not at all on the northern slope of a hill or when the south side of the trees is shaded in some other way. Is the hypothesis rendered unacceptable or invalid because, so far as we know, leaves do not "deliberate" or consciously "seek," have not been to school and learned the relevant laws of science or the mathematics required to calculate the "optimum" position, and cannot move from position to position? Clearly, none of these contradictions of the hypothesis is vitally relevant; the phenomena involved are not within the "class of phenomena the hypothesis is designed to explain"; the hypothesis does not assert that leaves do these things but only that their density is the same *as if* they did. Despite the apparent falsity of the "assumptions" of the hypothesis, it has great plausibility because of the conformity of its implications with observation. We are inclined to "explain" its validity on the ground that sunlight contributes to the growth of leaves and that hence leaves will grow denser or more putative leaves survive where there is more sun, so the result achieved by purely passive adaptation to external circumstances is the same as the result that would be achieved by deliberate accommodation to them. This alternative hypothesis is more attractive than the constructed hypothesis not because its "assumptions" are more "realistic" but rather because it is part of a more general theory that applies to a wider variety of phenomena, of which the position of leaves around a tree is a special case, has more implications capable of being contradicted, and has failed to be contradicted under a wider variety of circumstances. The direct evidence for the growth of leaves is in this way strengthened by the indirect evidence from the other phenomena to which the more general theory applies.

The constructed hypothesis is presumably valid, that is, yields "sufficiently" accurate predictions about the density of leaves, only for a particular class of circumstances. I do not know what these circumstances are or how to define them. It seems obvious, however, that in this example the "assumptions" of the theory will play no part in specifying them: the kind of tree, the character of the soil, etc., are the types of variables that are likely to define its range of validity, not the ability of the leaves to do complicated mathematics or to move from place to place.

A largely parallel example involving human behavior has been used elsewhere by Savage and me. 15 Consider the problem of predicting the shots made by an expert billiard player. It seems not at all unreasonable that excellent predictions would be yielded by the hypothesis that the billiard player made his shots *as if* he knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc., describing the location of the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas. Our confidence in this hypothesis is not based on the belief that billiard players, even expert ones, can or do go through the process described; it derives rather from the belief that, unless in some way or other they were capable of reaching essentially the same result, they would not in fact be *expert* billiard players.

It is only a short step from these examples to the economic hypothesis that under a wide range of circumstances individual firm behave *as if* they were seeking rationally to maximize their expected returns (generally if misleadingly called "profits") 16 and had full knowledge of the data needed to succeed in this attempt; *as if*, that is, they knew the relevant cost and demand functions, calculated marginal cost and marginal revenue from all actions open to them, and pushed each line of action to the point at which the relevant marginal cost and marginal revenue were equal. Now, of course, businessmen do not actually and literally solve the system of simultaneous equations in terms of which the mathematical economist finds it convenient to express this hypothesis, any more than leaves or billiard players explicitly go through complicated mathematical calculations or falling bodies decide to create a vacuum. The billiard player, if asked how he decides where to hit the ball, may say that he "just figures it out" but then also rubs a rabbit's foot just to make sure; and the businessman may well say that he prices at average cost, with of course some minor deviations when the market makes it necessary. The one statement is about as helpful as the other, and neither is a relevant test of the associated hypothesis.

Confidence in the maximization-of-returns hypothesis is justified by evidence of a very different character. This evidence is in part similar to that adduced on behalf of the billiard-player hypothesis - unless the behavior of businessmen in some way or other approximated behavior consistent with the maximization of returns, it seems unlikely that they would remain in business for long. Let the apparent immediate determinant of business behavior be anything at all - habitual reaction, random chance, or whatnot. Whenever this determinant happens to lead to behavior consistent with rational and informed maximization of returns, the business will prosper and acquire resources with which to expand; whenever it does not, the business will tend to lose resources and can be kept in existence only by the addition of resources from outside. The process of "natural selection" thus helps to validate the hypothesis - or, rather, given natural selection, acceptance of the hypothesis can be based largely on the judgment that it summarizes appropriately the conditions for survival.

An even more important body of evidence for the maximization-of-returns hypothesis is experience from countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted. This evidence is extremely hard to document; it is scattered in numerous memo-randums, articles, and monographs concerned primarily with specific concrete problems rather than with submitting the hypothesis to test. Yet the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect testimony to its worth. The evidence *for* a hypothesis always consists of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used, and by its very nature is difficult to document at all comprehensively. It tends to become part of the tradition and folklore of a science revealed in the tenacity with which hypotheses are rather than in any textbook list of instances in which the thesis has failed to be contradicted.