

TRUTH, PHILOSOPHY, AND BEHAVIORAL SCIENCE: A REPLY TO HOCUTT

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In his editorial "Some truths about truth," Professor Hocutt (1994) proposed to correct some misconceptions of truth, especially for those behavioral scientists that know little about it. The five points of this editorial were 1) scientists need a notion of truth, 2) truth and knowledge are different, 3) definitions and criteria of truth are not identical, 4) philosophers have adequately defined truth, and 5) the idea of relativism is nonsense. As one of the presenters at the symposium which led to his editorial, I am grateful to Professor Hocutt for this opportunity to respond to his editorial.

I am particularly interested here in defending my statement that behavioral scientists do not need a theory of truth, but only after clarifying what I meant in saying this. In claiming that "there is no such *thing* as Truth to have a theory about," and concluding that behavioral scientists do not need a theory of truth, I was not arguing (as Dr. Hocutt suggested) that there is no use for the word "truth" or that behavioral scientists have no need for a conception of truth. Furthermore, I did not think that I was saying something *true*, thus unwittingly falling into a self-stultifying argument. Rather, I thought I was contributing a statement to a conversation by presenting a pragmatist's conception of truth.

I can restate my claim as follows: Within a pragmatic conception of truth, there is nothing *essential* about truth. No a priori criterion will allow us to separate true from false statements. Therefore, a "theory of truth" which purports to provide a means of evaluating the truth value of a statement is impossible (especially those theories of truth that propose to assess the extent to which a statement corresponds to reality). This is not necessarily a problem for behavioral sciences because the practice of science is not predicated on such a theory of truth. In defense of this argument, I would like to address Professor Hocutt's points in reverse order. I do not intend to refute his points. Instead, I wish to discuss his points in a way that will establish at least the viability of the above argument and highlight some features of the relation between philosophy and behavioral science which cause us some confusion about the term "truth."

Relativism

Relativism is, loosely speaking, the antithesis of a theory of truth. Relativism

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MARKHAM

suggests that truth is relative to subject and time. Stated this way, the disturbing consequence of relativism is that we must view any statement about the world as being just as true as any other statement about the world. Professor Hocutt is quite correct in asserting that this notion of relative truth is a muddle.

The charge of relativism is usually made against those who claim that our ways of evaluating statements are not nearly as systematic or algorithmic as we might hope. However, this claim does not necessarily lead to the conclusion that all statements are equally valid or true. Relativism, in such extreme form, only provides a touchstone to remind us of what we are attempting to avoid - a complete inability to evaluate the relative merit of statements. For science and other forms of inquiry to proceed, we must have ways to evaluate the relative worth of statements. We are often led to believe that the best way to do this is to determine whether statements are true. It is, however, far from clear what it means for a natural language sentence to be true and whether we can actually evaluate the truth of such statements. To address this question, it seems necessary to begin with a definition of truth.

A Definition of Truth

Dr. Hocutt is quite correct in reminding us that there is a satisfactory definition of truth. This definition of truth was given by Tarski (1944, 1956). It is a good one. In fact, it appears to be unassailable. However, three points merit consideration as we work toward making use of such a definition of truth.

First, Tarski (1956) and many who follow him (e.g., Mates, 1971) warn us that his conception of truth is one of many conceptions of truth. Thus, Tarski's definition does not preclude other conceptions of truth. Second, Tarski restricted his definition of truth to a conception of truth within various *formalized languages*. In this context, his definition of truth is certainly satisfactory. However, as he (1956) acknowledged, problems will arise when we take his definition of truth outside the arena of formalized languages. In this perspective then, it is possible that there are other viable conceptions of truth, especially truth in natural languages. In my paper I claimed that *pragmatism* is one such conception of truth.

Finally, it is far from clear that we can usefully apply Tarski's definition of truth to the sort of sentences that comprise scientific discourse, particularly statements about empirical matters. To illustrate this point, let us begin with Tarski's famous example:

"Snow is white" is true if and only if snow is white.

As we move from formalized instances to the sort of sentence that is at issue in scientific discourse, the application of Tarski's definition becomes difficult. Consider these sentences:

"Snow is white"

"The boiling point of water is 100 degrees Celsius"

"There is no such thing as centrifugal force"

"Intelligence is inherited"

Now, it is possible that the problems presented by the latter sentences are a failing of science. One could argue that some sciences, especially behavioral sciences, lack a formalized language such as pure mathematics, or some appropriately formalized

TRUTH KNOWLEDGE AND BELIEF

symbolic language to structure and clarify their inquiries. Even if these disciplines were to adopt a formalized language, Tarski's definition would still not solve the issue of truth for scientists.

This point becomes evident if we treat Tarski's prototypical sentence as empirical rather than semantic or logical. In doing so, we encounter a problem inherent in any attempt to proceed from a *definition* of truth to a *criterion* of truth. That is, even though we know that "*Snow is white*" is true if and only if snow is white, we are left to determine if snow is, in fact, white. We are in need of a criterion for evaluating this statement.

Definitions Versus Criteria Of Truth

How can we determine if snow is actually white? This empirical analysis of Tarski's sentence could proceed by collecting samples of snow and observing whether these samples are white. After collecting a sufficient number of snow samples in which all are white, we could conclude that snow is white. However, we will always be faced with the problem of induction—how many samples of white snow are sufficient?

Alternately, we could follow Popper's scheme by making the conjecture that snow is white then searching for an instance of non-white snow to refute this hypothesis. What if we discover a sample of snow that is not white? Have we then proven this sentence false? One solution would be simply to respond that if the sample is not white, it cannot be snow. However, this response leads us to a situation where the only truths that could be known are those given by definition. Thus, truth would become unattainable in empirical investigations because it can only be derived from definition. Under these conditions, retaining the logical rigor of Tarski's definition of truth would seem to require scientists to abandon empirical investigations.

How then do we include this definition of truth in the practice of science if we cannot arrive at a logically rigorous criterion for deciding which statements fall under this definition of truth? There are at least three ways to do this. The first solution is to make science conform to practices that are logically rigorous, for example the program suggested by Popper (1956, 1974). Popper's program of conjectures and refutations would allow us to retain logical rigor for science and a satisfactory definition of truth. However, some of the finest moments in science (e.g., Galileo and the Copernican Revolution) bear absolutely no resemblance to this model of science and it is far from clear that science can progress under such a model (see e.g., Feyerabend, 1980).

A second possibility is to enlist some criterion of truth for our statements. Criteria of truth generally fall into one of three categories: correspondence criteria, coherence criteria, or successful action criteria. It is correct that none of these criteria of truth provide an adequate definition of truth and, furthermore, that a correspondence theory of truth is not a good criterion of truth. On the other hand, both coherence and successful working criteria of truth provide good tests of truth. However, when we employ these theories as tests of truth, our use of the word "truth" has departed substantially from Tarski's definition of truth. This departure

MARKHAM

is a consequence of attempting to adjudicate truth in a natural language rather than in a formalized language.

A third solution to this problem might be to admit that the goal of science is something other than "truth". Thus we could address our problem by distinguishing truth from knowledge, and acknowledging that science is in the business of determining knowledge, not truth.

Truth And Knowledge

We have now moved from a question of truth to a question of knowledge. Professor Hocutt points out that truth and knowledge are not the same. The given examples were:

(a) *Betelgeuse has planets*

(b) *Betelgeuse does not have planets*

Can we conclude that one of these statements is true? Certainly. How can we be certain which sentence is true? We do not know. As discussed earlier, it is no trivial matter to discover which of these statements is true, and doing so may not be possible.

Given Hocutt's distinction between truth and knowledge, it may seem better to say that science is concerned primarily with knowledge rather than truth and searching instead for ways of evaluating knowledge claims. This enterprise, though, is often confused with searching for ways of adjudicating the truth value of knowledge claims by discovering what is essential about true statements (empirical ones) so that we could formulate a theory of truth that would allow us to evaluate all statements and discover whether they contain this essential quality of truth. I am still as doubtful as ever that such a theory can be found. Alternatively, we could offer a different conception of truth—one that is appropriate for natural language enterprises such as behavioral science.

Science And Truth

It seems doubtful that we can evaluate knowledge claims in the practice of science by determining whether they are *true* (in a logically rigorous sense). That is, although some statements are true and others false, for empirical statements there is no essential feature that distinguishes one from another. Thus a theory of truth that provides a universal a priori "meter-stick of truth" which we use to assess the truth-value of statements is an impossibility. This does not mean that science cannot proceed. Science (and inquiry in general) is not predicated on such a theory of truth. Does this position entail relativism? It does not. I would argue instead that this is simply an acknowledgement of the fact that, once we step outside the arena of logic (and other such formalized languages), adequately rigorous conceptions of truth begin to fade, and determination of the truth of our sentences becomes increasingly difficult and ultimately impossible. Although no "theory of truth" can rescue us from this predicament there are some possible solutions. One is to follow (or at least investigate further) the pragmatist suggestion that the constraints on our statements are conversational rather than epistemological (e.g., Rorty, 1982), thus avoiding

TRUTH KNOWLEDGE AND BELIEF

questions of Truth by reconceptualizing truth (at least in natural language). Another possibility is to attempt to extrapolate from logical or semantic conceptions of "truth" to conceptions of "belief" in the arena of scientific discourse. There are likely to be other adequate solutions available. Nonetheless, I do retain my claim that discovering a theory of *Truth* is not one of them.

Postscript: Basic Versus Applied Philosophy?

I find it interesting that my comments here describe a relation between philosophy and psychology that is, in some respects, analogous to the sometimes strained relation between basic and applied science, especially within the behavioral sciences. Basic scientists sometimes criticize applied researchers for lack of precision while applied scientists criticize basic scientists because basic research bears little contact to the "real world" problems which face applied researchers. One moral seems to be that we gain precision at the expense of applicability and applicability at the expense of precision. Another moral is that it might, in some cases, be useful to view psychology as applied philosophy (I am grateful to Dr. Edward Morris for bringing this view to my attention. In writing this reply I now see what he meant).

I do not doubt that many behavioral scientists know little about truth, especially as they cross into the complex and sophisticated arena of philosophy. I am not ashamed to count myself among those who know little. Behavioral science and philosophy both would claim truth to lie within their respective scopes of inquiry, but in different respects, because these are different disciplines. Although the relation of philosophy and behavioral science is ambiguous at best, often ambivalent, and at worst hostile, these modes of inquiry are inextricably related. In this light, I suggest that the proper course is to continue our conversation.

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