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On Carnap's Views on Ontology

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THOUGH no one has influenced my philosophical thought more than Carnap, an issue has persisted between us for years over questions of ontology and analyticity. These questions prove to be interrelated; their interrelations come out especially clearly in Carnap's paper "Empiricism, Semantics, and Ontology."¹ I shall devote particular attention to that one paper in an effort to isolate and reduce our divergences.

When I inquire into the *ontological commitments* of a given doctrine or body of theory, I am merely asking what, according to that theory, there is. I might say in passing, though it is no substantial point of disagreement, that Carnap does not much like my terminology here. Now if he had a better use for this fine old word 'ontology,' I should be inclined to cast about for another word for my own meaning. But the fact is, I believe, that he disapproves of my giving meaning to a word which belongs

AUTHOR'S NOTE. This is part of a paper which was presented to the Philosophical Seminar of the University of Chicago, February 1, 1951.

to traditional metaphysics and should therefore be meaningless. Now my ethics of terminology demand, on occasion, the avoidance of a word for given purposes when the word has been pre-empted in a prior meaning; meaningless words, however, are precisely the words which I feel freest to specify meanings for. But actually my adoption of the word 'ontology' for the purpose described is not as arbitrary as I make it sound. Though no champion of traditional metaphysics, I suspect that the sense in which I use this crusty old word has been nuclear to its usage all along.

Let us agree, for the space of my remarks, on the word. The question of the ontological commitments of a theory, then, is the question what, according to that theory, there is. Carnap thinks—and here is a more than terminological issue—that the question what a theory presupposes that there is should be divided into two questions in a certain way; and I disagree. What he thinks the division should be, and why I disagree, will appear soon; but first let us examine the undivided idea a bit.

It has not always been clear how to decide whether or not a given discourse involves commitment to a given alleged entity. When we say that all fish are aquatic, do we commit ourselves to the acceptance of two abstract entities, two classes or properties, named by the words 'fish' and 'aquatic'? When we use the word 'similar,' without defining it in any anterior terms, do we thereby commit ourselves to the acceptance of an abstract entity which is the relation of similarity? Russell has said that we do. But no nominalist would agree.

Every nominalist, every user of language, avails himself freely of general terms such as 'fish' and 'aquatic' and 'similar'; but only antinomialists imagine in such usage any allusion to abstract entities. The nominalist holds that the word 'fish' is true of each concrete fish, but that it does not, in addition, name an abstract fishhood or class of fish; and that the word 'similar' is true of each alligator with respect to each crocodile, and true of each Pontiac with respect to each Pontiac, but that it does not, in addition, name a relation of similarity. Why should 'fish' or 'aquatic' or 'similar' be put on a par with names such as 'Chicago' and 'Truman' and 'Parthenon'? Many words are admissible in significant sentences without claiming to name; witness 'the' and 'of' and 'sake' and 'kilter.' Why not 'fish' and 'aquatic' and 'similar'?

Perhaps we can convict a speaker of commitment to abstract entities not through his general terms, but only through his abstract terms such as 'fishhood,' 'aquaticity,' 'similarity'? But this is no feasible resting place. If you grant the nominalist his general terms, he can excuse his use of abstract terms as picturesque paraphrasing of what could be said in general terms.

All this tolerance of language and waiving of commitments is reasonable enough, but is there no end to it? The words 'Chicago' and 'Truman'

and 'Parthenon' could themselves be excused in the same spirit, as admissible in sentences without claiming to name. There would appear to be no such thing as commitment to entities through discourse.

I think it is true that there is no commitment to entities through use of alleged names of them; other things being equal, we can always deny the allegation that the words in question are names. But still there is certainly commitment to entities through discourse; for we are quite capable of saying in so many words that *there are* black swans, that *there is* a mountain more than 9000 meters high, and that *there are* prime numbers above a hundred. Saying these things, we also say by implication that there are physical objects and abstract entities; for all the black swans are physical objects and all the prime numbers above a hundred are abstract entities.

Thus I consider that the essential commitment to entities of any sort comes through the variables of quantification and not through the use of alleged names. The entities to which a discourse commits us are the entities over which our variables of quantification have to range in order that the statements affirmed in that discourse be true.

Names are a red herring. The use of alleged names, we have seen, is no commitment to corresponding entities. Conversely, through our variables of quantification we are quite capable of committing ourselves to entities which cannot be named individually at all in the resources of our language; witness the real numbers, which, according to classical theory, constitute a larger infinity than does the totality of constructable names in any language. Names, in fact, can be dispensed with altogether in favor of unnamable general terms, plus quantification and other logical devices; the trick of accomplishing this elimination is provided, in its main lines, by Russell's theory of descriptions. Thenceforward the variable of quantification becomes the sole channel of reference. For ontological commitment it is the variable that counts.

If I understand correctly, Carnap accepts my standard for judging whether a given theory accepts given alleged entities. The test is whether the variables of quantification have to include those entities in their range in order to make the theory true. Allow, of course, for a shudder between the word 'ontological' and the word 'commitment.'

Now to determine what entities a given theory presupposes is one thing, and to determine what entities a theory should be allowed to presuppose, what entities there really are, is another. It is especially in the latter connection that Carnap urges the dichotomy which I said I would talk about. On one side of his dichotomy he puts the question of there being black swans, or mountains more than 9000 meters, or prime numbers above a hundred; on the other side the question of there being physical objects or abstract entities. The distinction depends on what he calls a *framework*:

If someone wishes to speak in his language about a new kind of entities, he has to introduce a system of new ways of speaking, subject to new rules; we shall call this procedure the construction of a *framework* for the new entities in question. And now we must distinguish two kinds of questions of existence: first, questions of the existence of certain entities of the new kind *within the framework*; we call them *internal questions*; and second, questions concerning the existence or reality of *the framework itself*, called *external questions*. . . . Let us consider as an example the simplest framework dealt with in the everyday language: the spatio-temporally ordered system of observable things and events. Once we have accepted this thing-language and thereby the framework of things, we can raise and answer internal questions, e. g., 'Is there a white piece of paper on my desk?', 'Did King Arthur actually live?', 'Are unicorns and centaurs real or merely imaginary?', and the like. These questions are to be answered by empirical investigations. . . . From these questions we must distinguish the external question of the reality of the thing world itself. In contrast to the former questions, this question is raised neither by the man in the street nor by scientists, but only by philosophers. . . . Those who raise the question of the reality of the thing world itself have perhaps in mind not a theoretical question as their formulation seems to suggest, but rather a practical question, a matter of a practical decision concerning the structure of our language. We have to make the choice whether or not to accept and use the forms of expression for the framework in question. . . . If someone decides to accept the thing language, there is no objection against saying that he has accepted the world of things. But this must not be interpreted as if it meant his acceptance of a *belief* in the reality of the thing world; there is no such belief or assertion or assumption, because it is not a theoretical question. To accept the thing world means nothing more than to accept a certain form of language.²

Let us recall now my account of wherein the countenancing of entities consists. It consists in the inclusion of them within the range or ranges of the variables of quantification. Accordingly Carnap describes the introduction of a framework as consisting essentially in these two steps:

First, the introduction of a general term, a predicate of higher level, for the new kind of entities, permitting us to say of any particular entity that it belongs to this kind (e. g., 'Red is a *property*', 'Five is a *number*'). Second, the introduction of variables of the new type. The new entities are values of these variables; the constants (and the closed compound expressions, if any) are substitutable for the variables. With the help of the variables, general sentences concerning the new entities can be formulated.³

It begins to appear, then, that Carnap's dichotomy of questions of existence is a dichotomy between questions of the form "Are there so-and-sos?" where the so-and-sos purport to exhaust the range of a particular style of bound variables, and questions of the form "Are there so-and-sos?" where

the so-and-sos do not purport to exhaust the range of a particular style of bound variables. Let me call the former questions category questions, and the latter ones subclass questions. I need this new terminology because Carnap's terms 'external' and 'internal' draw a somewhat different distinction which is derivative from the distinction between category questions and subclass questions. The external questions are the category questions conceived as propounded before the adoption of a given language; and they are, Carnap holds, properly to be construed as questions of the desirability of a given language form. The internal questions comprise the subclass questions and, in addition, the category questions when these are construed as treated within an adopted language as questions having trivially analytic or contradictory answers.⁴

But now I want to examine the dichotomy which, as we see, underlies Carnap's distinction of external and internal, and which I am phrasing as the distinction between category questions and subclass questions. It is evident that the question whether there are numbers will be a category question only with respect to languages which appropriate a separate style of variables for the exclusive purpose of referring to numbers. If our language refers to numbers through variables which also take classes other than numbers as values, then the question whether there are numbers becomes a subclass question, on a par with the question whether there are primes over a hundred. This will be the situation in the language of *Principia Mathematica* and in the languages of all the other familiar set theories.

Even the question whether there are classes, or whether there are physical objects, becomes a subclass question if our language uses a single style of variables to range over both sorts of entities. Whether the statement that there are physical objects and the statement that there are black swans should be put on the same side of the dichotomy, or on opposite sides, comes to depend on the rather trivial consideration of whether we use one style of variables or two for physical objects and classes.

I must now explain why I call this a rather trivial consideration. The use of different styles of variables for different ranges is common in mathematics, but can usually be explained as a casual and eliminable shorthand: instead of prefacing various of our statements with the words 'If x is a real number between 0 and 1, then,' we may find it convenient for the space of a chapter or a book of probability theory to reserve special letters 'p,' 'q,' 'r' are to the real numbers between 0 and 1. The difference between using the explicit hypothesis ' x is a real number between 0 and 1' and introducing the restricted variables is so negligible that at the level of ordinary mathematical writing it cannot usually be detected; nor is there any reason why it should be detected.

But Carnap does not have just this trivial distinction in mind. He is thinking of languages which contain fundamentally segregated styles of variables before any definitional abbreviations; and he is thinking of styles of variables which are sealed off from one another so utterly that it is commonly ungrammatical to use a variable of one style where a variable of another style would be grammatical. A language which exploits this sort of basic compartmentalization of variables is that of Russell's theory of logical types. However, I think many of us overstress the theory of types to the neglect of its coeval alternative, Zermelo's set theory, and its descendants. In a notation of the latter tradition, carrying no distinctions in styles of variables, all questions regarding the acceptance not only of numbers in general but of abstract entities in general, or of physical objects in general, would become subclass questions—just as genuinely so as the question of there being black swans and prime numbers above a hundred. Thus Carnap's distinction between internal and external, based as it is upon a distinction between category questions and subclass questions, is of little concern to us apart from the adoption of something like the theory of types. I am one of those who have tended for many years not to adopt the theory of types.

Actually the case is a little worse than I have thus far represented it. Even if we adopt the theory of types we remain free to adopt the course which Russell himself adopted under the name of *typical ambiguity*—thus abandoning the use of a distinctive style of variables for every type. Russell uses his device in moderation, but we can go farther and use just a single style of variables for all types. The theory of types remains in force in this way: only those formulas are admitted as grammatical which *could*, by a one-to-one rewriting of variables, be turned into meaningful formulas of explicit type theory with distinctive styles of variables for all types.

This sort of indirect conformity to the theory of types, on the part of formulas written with a single style of variables, is a feature which I have called *stratification*; and it can be defined also directly, without any appeal to a supposedly more fundamental notation involving distinctive styles of variables. Stratification is simply freedom, on the part of the variables in a formula, from certain repetition-patterns in connection with the symbol of class-membership.

Next we can even abandon Russell's notion of a hierarchical universe of entities disposed into logical types; nothing remains of type theory except an ultimate grammatical restriction on the sorts of repetition-patterns which variables are allowed to exhibit in formulas. Yet formally our logic, refurbished as described, is indistinguishable from Russell's theory of types plus Russell's convention of typical ambiguity. Now the point of this logical digression is that even under the theory of types the use of dis-

tinctive styles of variables, explicitly or even implicitly, is the most casual editorial detail.

I argued before that the distinction between category questions and subclass questions is of little concern apart from the adoption of something like the theory of types. But what I now think to have shown is that it is of little concern even under the theory of types. It is a distinction which is not invariant under logically irrelevant changes of typography.

I have doubly warranted hopes of persuading Carnap to abandon this particular distinction. First, as argued, I find it ill grounded. But second, also, I think it is a distinction which he can perfectly well discard compatibly with the philosophical purpose of the paper under discussion. No more than the distinction between *analytic* and *synthetic* is needed in support of Carnap's doctrine that the statements commonly thought of as ontological, viz. statements such as 'There are physical objects,' 'There are classes,' 'There are numbers,' are analytic or contradictory given the language. No more than the distinction between analytic and synthetic is needed in support of his doctrine that the statements commonly thought of as ontological are proper matters of contention only in the form of linguistic proposals. The contrast which he wants between those ontological statements and empirical existence statements such as 'There are black swans' is clinched by the distinction of analytic and synthetic. True, there is in these terms no contrast between analytic statements of an ontological kind and other analytic statements of existence such as 'There are prime numbers above a hundred'; but I don't see why he should care about this.

However, this is not an end of my dissent. On the contrary, the basic point of contention has just emerged: the distinction between analytic and synthetic itself. Carnap correctly states in a footnote:

Quine does not acknowledge the distinction which I emphasize above [viz. the distinction between ontological questions and factual questions of existence], because according to his general conception there are no sharp boundary lines between logical and factual truth, between questions of meaning and questions of fact, between the acceptance of a language structure and the acceptance of an assertion formulated in the language.

I have set down my misgivings regarding the distinction between analytic and synthetic in a recent paper, "Two Dogmas of Empiricism,"⁵ and will not retrace those steps here. Let me merely stress the consequence: if there is no proper distinction between analytic and synthetic, then no basis at all remains for the contrast which Carnap urges between ontological statements and empirical statements of existence. Ontological questions then end up on a par with questions of natural science.

Within natural science there is a continuum of gradations, from the

statements which report observations to those which reflect basic features say of quantum theory or the theory of relativity. The view which I end up with, in the paper last cited, is that statements of ontology or even of mathematics and logic form a continuation of this continuum, a continuation which is perhaps yet more remote from observation than are the central principles of quantum theory or relativity. The differences here are in my view differences only in degree and not in kind. Science is a unified structure, and in principle it is the structure as a whole, and not its component statements one by one, that experience confirms or shows to be imperfect. Carnap maintains that ontological questions, and likewise questions of logical or mathematical principle, are questions not of fact but of choosing a convenient conceptual scheme or framework for science; and with this I agree only if the same be conceded for every scientific hypothesis.⁶

NOTES

¹ *Revue Internationale de Philosophie*, 11:20-40 (1950).

² *Ibid.*, pp. 21-23.

³ *Ibid.*, p. 30.

⁴ This is clearly intended on p. 24 of the *Revue*.

⁵ *Philosophical Review*, 60:20-43 (1951).

⁶ Editor's note: Professor Carnap will reply to this article in an early issue.

A Note on State-Descriptions

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IN HIS latest publications, Carnap defines the central terms of semantics like 'L-true,' 'L-implies,' etc., on the base of the concept of *range* which in its turn is defined by means of the concept of *state-description*. This procedure is very simple, intuitive, and elegant but it has also its drawbacks, some of which, but not all, were noticed by Carnap himself and discussed by him. My remarks will refer, without loss of relevance, to section 18 of *Logical Foundations of Probability* (University of Chicago Press, 1950).

Carnap defines *state-description* with respect to a language L containing only a finite number of individual constants and a finite number of first-level primitive predicates (omitting certain niceties of no importance for our discussion) as a conjunction which contains as components, for every