Definitions, Explanations, and Theories

1. General Introduction. The ensuing account is not intended to be a full treatment of the topics referred to in the title. A number of specific points that have concerned recent writers on logic and the philosophy of science and which are related to these topics are discussed and an attempt is made to indicate how these are related. These points include a discussion of translation, rules, dispositions, theoretical concepts, correlational concepts, statistical explanation, incomplete explanations, ‘bridge laws,’ unobservable entities, implicit definition, etc. But many other points would have to be dealt with, and those mentioned dealt with in more detail, in order to justify a claim of completeness.\(^1\) It is not even supposed that the present topics are the most important or the most controversial in any treatment of definitions, explanations, and theories, nor that the treatment of these topics is of great originality. But it does represent a different approach from those which have been commonly supported. It is different in method from the approach of the positivist symbolic logicians whose work led us to the major reconsideration of these topics which occurred in the earlier part of this century. And it is different in certain of its results from the approach of the Wittgensteinian school of logicians whose method it shares.

2. Analytic Method. I shall call my interpretation of this method, the method of context analysis, in order to focus attention on its complementarity with traditional and symbolic logic which might be called content analysis.

The differences in approach, which in no way amount to incompatibility, arise from the difference in the success with which formaliza-

\(^1\) I have discussed many further points that arise in the analysis of one of these topics in “Explanations” (unpublished D. Phil. thesis, Oxford, 1956), especially pp 246–420; but others of equal importance I did not discuss at all.
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A context can be employed for these two kinds of analysis. Context analysis is undertaken in the belief that the meaning of terms or concepts or logical problems can only be thoroughly understood if we include a meticulous examination of the circumstances in which they occur, rather than relying on a relatively rapidly extracted formalization of their apparent internal logical features. The context analyst's viewpoint as thus stated has two important corollaries. First, it regards the rigor of symbolic logic as partly spurious; and for the problems of analysis with which we are faced concern already existing concepts and problems, and experience makes very clear that these are not governed by rigorous rules and definitions. This is not to deny that within symbolic logic, taken as a branch of mathematics, the highest standards of rigor obtain; it is only to assert that this rigor is not the same thing as rigor in the analysis of an ordinary logical problem.

Second, it views the utility of symbolic logic as partly illusory; the chief claim for symbolic logic—that it provides a means of avoiding as well as solving problems—is held to rest on the mistaken belief that a language can in general be rich enough to perform the tasks required of it without containing the traps which produce the confusion or puzzlement. Nevertheless, context analysis is frequently facilitated by limited formalization and in certain cases, for example, proof theory and dimension theory, regards a highly formal analysis as the best means for success. But it should be noted that the latter cases do not include such common topics as those mentioned in the title or introduction to this paper—or at the most refer to special instances of them. The context analyst employs a different tool; instead of formalization, he uses comparison. The purpose of these comparisons is to elicit the less apparent significance of the concept (etc.) which is under analysis. It is thought that the process of exhaustive comparison with cases where the meaning of the concepts is clear is the best way of discovering the function of the expression(s) being considered. The function is not itself the meaning but is a guide to the meaning just as the manifest form is not itself the meaning but is a guide to the meaning. So the two approaches could alternatively be described as functional analysis and formal analysis.²


³ By which I mean such problems as giving an analysis of meaning, inference, and necessity.

⁴ I think the most important argument for context analysis depends on the vast amount of information about the meaning of a term that is implicit in a well-described example of its use over and above that which can be summarized in any manageable form by a definition or set of definitions. The present paper in part endeavors to illustrate the truth of this remark.
analysts will and often do agree. It is therefore essential not to suppose there is an irreconcilable difference between the procedures of the two schools of analysis.

In fact, we can readily see that when their positions are so stated that such a difference exists, one of them is actually at fault. For a formal analyst to propose an analysis of theories which he could not support by showing that it refers to and illuminates important examples of what we usually call “theories” would be as irresponsible as it would be for a context analyst to insist that precisification or enlargement of our logical vocabulary is always superfluous.

Despite the impossibility of defending either position when stated in a way that makes it sharply distinguishable from the other, there is certainly a considerable difference (even visually) between the characteristic results of an investigation by representatives of the two schools. In fact it is nearly as common for context analysts to regard their method as illuminating, precise, and indeed responsible for a new revolution in philosophy, as it is for content analysts and non-analytical philosophers to regard it as an insidious, corrupting, decadent, faddish, sloppy, and pointless perversion of the great traditions. It is worth adding that the Minnesota Center for Philosophy of Science has found the interaction of the two viewpoints a source of great stimulation and hybrid vigor and has until now always had staff members whose primary allegiance is to different schools.

3. Definitions: Introduction. We begin with some very simple points. They will all be necessary. We may start by assuming that a definition gives or is supposed to give the meaning of a term or terms. These terms may have already been in use or they may have been coined for a special purpose by the proposer of the definition. There are many ways in which the meaning of a term can be given, some of them rather strikingly different from others. Let us consider some of the particular types generated by these alternatives.

4. Dictionary Definitions. In a traditionally pre-eminent position there is the explicit linguistic definition of a word in current use, such as the following example from Webster’s New Collegiate Dictionary (1953).

exotic. Introduced from a foreign country; extraneous; foreign . . . (4.1)

*For example, compare this paper or Strawson’s paper with the one by Pap in this volume or the one by Carnap in the first volume of Minnesota Studies in the Philosophy of Science. The paper by Sellars in this volume affords an excellent example of a synthesis of the two approaches.

*This does not mean that whenever we understand all the terms involved we can always tell whether a dictionary definition is at fault; for there may be senses of the term given or not given about which we are ignorant. However to understand the term requires at least that we understand one sense of it (usually the main sense, and usually several other senses as is certainly the case with, e.g., “but”), so when we understand all the terms in a definition we can sometimes say that it is definitely incorrect, sometimes that it is definitely incomplete, and in the remaining cases that it is correct with respect to the senses it does discuss.
more than the ability to recognize one's dog in a canine lineup depends on one's ability to describe him in terms which would enable another to do so. The ability to use a term correctly is so enormously different from the ability to correctly formulate its use that the context analyst always relies on the first (which we know how to identify if we know how to speak the language) whenever a great deal hinges on the meaning of a word.

It would be unfair to criticize the definition quoted by pointing out that "extraneous" is not (and probably never was) synonymous with the literal meaning of "foreign". For the dictionary conventions, properly understood, do not imply that the alternatives are exactly equivalent, but rather that in certain contexts each will be a near-synonym for the term defined—and these contexts may differ slightly. The perceptive reader picks up some of the flavor of the term—its refinements of meaning—by studying the range of its synonyms. Of course, unless the contexts are specified in which each substitution is permissible, the definition will not be fully adequate; and it is chiefly for this reason that the Oxford English Dictionary runs to eleven volumes—it actually gives contexts for the different uses of the terms by means of quotations, i.e., by giving examples of the proper use.

6. Dictionaries versus Encyclopedias. There is clearly a certain vagueness about dictionary definitions, and indeed there are very marked differences between those proposed in one dictionary and those proposed in another dictionary, so one cannot place very much reliance on every definition in every dictionary; a considerable amount of research beyond the dictionaries is often required in order to obtain even a limited understanding of the meaning of a term. Conversely, a good deal of information is contained in a dictionary which is not strictly part of the meaning of terms at all, e.g., the family relationships of biblical figures and the fact that mistletoe is the state emblem of Oklahoma. And there is clearly great difficulty about drawing a line between information about a defined class and the definition of that class. Is it part of the definition of "Iroquois" that this Indian confederacy was centered in New York State or that it admitted the Tuscarora in 1722? These facts are given in Webster, but such facts can't all be dismissed as irrelevant to a proper definition, because the distinguishing part of definition consists only of such facts. Of course, we might conclude that the term is not definable other than as "an Indian tribe"; yet some-

one who reads the 'definition' in Webster will actually be in nearly as good a position as anyone else to say that he understands the meaning of the term. Moreover, a confederacy with different members, admitted at different times, centered in Oklahoma rather than New York would not be the Iroquois at all, but the so-called Five Civilized Tribes; so such facts are not wholly independent of the meaning of the term "Iroquois". One doesn't have to know some Iroquois by sight in order to use their tribe's name properly; and the same is true of any individual's name.

7. Definitions as Substitution Rules. Now the usual decision procedure advocated in logic textbooks amounts to taking definitions as giving substitution rules (elimination rules); in deciding whether the property P is part of the definition of the term X, one asks "Could there be any meaning in the description of something as 'an X that isn't P'?" If the answer is "Yes," then P cannot be part of the definition of X since if it is, the description would be self-contradictory. Unfortunately, as the Iroquois example shows, this approach would whittle away virtually every property which could possibly be part of the meaning, leaving us with the alternatives of saying the term has no satisfactory definition or that some other criterion for definition must be employed.

It may be thought that proper names could well be said to have no definition in the sense of a substitution rule (i.e., to have no intra-linguistic equivalent). But "Iroquois" is not a proper name of the kind for which substitution rules cannot be given, if indeed there are any such; its capitalization is logically as irrelevant as that of nouns in German. To make a decisive point against the decision procedure of the last paragraph, however, we need only show its inapplicability to other important kinds of terms.

The term "lemon" as the name of a fruit is defined by Webster as "The acid fruit of a tree (citrus limonia) related to the orange." Suppose that there is a way of identifying the tree without looking at its fruit. We can now ask whether something could possibly still be a lemon if it grew on a special variety of quince tree in place of the quinces. Supposing that it is exactly the same in every respect—shape, taste, color, etc.—we would indeed be surprised but we would surely not deny that it was a lemon. We might call it a 'quince-grown lemon' but the justification for this kind of description (cf. 'corn-fed beef')
usually lies in some difference in quality of the end product which in this case is not present, ex hypothesi. It would simply be a lemon with an odd history. Moreover, if we were able to produce them in a hydroponic farm from a chemical pulp itself synthesized from a purely inorganic mixture, they would surely still be lemons, though the fruit of no tree. Again, if by selective crossbreeding of lemon trees we could produce a fruit which was in every respect except taste identical with what we now call a lemon, but had a curious pungent flavor of its own, not sweet but not really acid either, we would surely no more abandon the term “lemon” for it than we abandon the term “grapefruit” for the sweeter, pink-fleshed products of the lower Rio Grande Valley. Then what sort of fruit is a lemon? We can clearly divide and conquer any concomitance of physical or genetic characteristics in the same way; and yet the term clearly has no distinct meaning over and above such characteristics. The answer must be that a lemon is something with some or most of these properties, e.g., those given by Webster, but that no one of these properties is individually necessary. Hence, the usual decision procedure fails again.

The formal analyst will about this stage propose that such sloppy terms could well be smartened up by adopting an exact definition; and we could then concentrate on the important work for science, which is research into the nature of the world and prediction and control of its future. The functional analyst will raise his two crucial questions: What ‘exact definition’ could we adopt, and what advantages would there really be in doing this? The theme of this paper is that no exact definition could be adopted which would exhibit significant advantages over the present ‘vague’ definitions—not only for commonplace terms like “lemon” but even more certainly for the crucial terms of theoretical and observational science. Yet I still wish to maintain an intensional account of definition; my aim is to retain the notions of synonymy etc., but to recognize their limitations.

The points so far made should lead us to abandon the idea that a good definition contains only terms each of which is necessarily connected with the term being defined. (Of course, I do not at all wish to deny that some definitions are like this, e.g., “sibling” = “brother or sister”.) I said at the beginning of this section that this idea is equivalent to regarding definitions as substitution rules. For two terms to be inter substitutable, it is both necessary and sufficient that the decision procedure we have just examined should be applicable. If the rule applies, then all the properties $P_i$ which pass the test must be present when $X$ is present and vice versa. (Using the rule in reverse we ascertain whether the conditions are jointly sufficient as well as severally necessary.) Hence any sentence where either $X$ or the set $P_i$ occurs will be unchanged in meaning if the other is substituted. Conversely, if $X$ and the set $P_i$ can be substituted for each other without loss of meaning, it cannot be the case that one of the $P_i$, say $P_n$, fails to pass the decision procedure since such a failure would have made the substitution impossible in any context where the compound description “X but not $P_n$” occurs (this would be a self-contradiction if the set $P_n$ is substituted for $X$, but is not a contradiction as it stands, ex hypothesi).

In consequence of these considerations, we may wish to restrict the term “definition” to substitution rules at the expense of saying that large numbers of common words are undefinable. Before doing this let us examine a few other examples of dictionary definitions. The price may turn out to be higher than we can pay, to be in fact an error strictly comparable to the idea that good definitions must capture the ‘real essence’ of a concept from which all other properties could be deduced. In both cases, as we shall see, the only respectable candidates come from mathematics, and mathematics is a poor ideal for a logical analysis of the factual sciences since its truths are mostly analytic and its criteria for existence mostly conceptual.

8. ‘Undefinable’ Terms. In the first place it is completely mistaken to suppose that scientific vocabularies do not contain words with the systematic evasiveness of the examples considered above. Not only do the relatively undeveloped sciences such as meteorology and psychology contain innumerable words with the same logical properties, but even the highly developed sciences such as physics exhibit them, though often in a disguised form. The commonest indication of their real nature is a history of change in the operational criteria. Such terms as “acid” and “temperature” have certainly changed their operational meaning over the last few centuries, and this indicates—what careful study clearly reveals—that no complete analysis of their meaning can be given in terms of a set $P_i$ each necessarily connected with their application in the sense explained above. However, it is quite unnecessary for us to complicate our task by dealing with such concepts over whose definitions a good many disputes have raged and still arise. We shall find it
easier to deal with everyday terms and easy to justify the application of our conclusions to scientific terms. The very few special properties of the latter will be dealt with specifically, e.g. the reference to unobserved entities.

Before coming to that stage of the argument, however, I want to lay the foundations for a constructive account of definition by examination of some other logical types of dictionary definition, for the moment continuing to use the term ‘definition’ in the wider sense indicated at the beginning of section 3.

Language is put together with conjunctions, prepositions, and pronouns which for the lexicographer present in part a similar and in part a different problem from that of defining nouns and adjectives. It is clear that they have meaning, and it is fairly clear—as we shall see—that this meaning usually cannot be given by an intra-linguistic equivalent that is satisfactory for all contexts. This raises the problem of selecting the proper interpretation of an ambiguous term, a problem common enough for any grammatical class. In the definition of “but” as a conjunction connecting coordinate elements (it is defined in eleven other grammatical categories) there are still two alternative meanings given, viz. “with this exception” and “on the contrary; yet; notwithstanding.” We are unable to formulate a single substitution rule which takes account of this residual ambiguity; for the rule cannot be a satisfactory substitution rule if it includes both (since they are different in meaning), and it cannot be adequate if it leaves out either. We can only formulate two rules and select one rather than the other depending on the context. And we cannot do that unless we understand the context so well that we can make some quite subtle discriminations. Not only can no single rule be given here, as is the case with all ambiguous terms, but the actual rule given is not an exact substitution rule. Webster gives as an example of the first use the phrase “whence all but he had fled” and one cannot substitute “with this exception” for “but” in this context and retain the grammatical proprieties. (In this context “except” would do, but in others “except” produces nonsense.)

This incompleteness is not accidental or the result of inefficiency but typical of the approximative process of conveying meaning by definitions. The formalist sees what he takes to be the ideal situation where exact rules, unambiguous forms, and necessary connections are the only ones allowed, and hastens to reform language in that direction; the functionalist hesitates pending further consideration, dubious whether such a pervasive feature can be legislated out of existence, suspicious that if the new language was used to do the tasks for which language is required it might immediately acquire these features. If so, there has been no gain in redefining terms and a great loss of time and energy. It may be better to recognize from the beginning that there are certain limits of precision beyond which definitions in an applied language usually cannot go. We shall argue that the same is true of laws and explanations in science.

The reconstructionist might concede the possibility of such a situation but argue that one does not progress by sitting still in the face of the possibility that we are incapable of movement. But the repairist has a reply. Not only has the reconstructionist been trying for many decades to produce definitions and rules which will eliminate our difficulties, without—he argues—any noticeable success (the pragmatic counterargument), but there are some strong reasons for suspecting that success is impossible and not merely frustrated by apathy (the logical counterargument). These reasons arise from a consideration of the function of language. We may expect, the functionalist argues, that a noun, X,* used to refer to something, P₁, which always in fact occurs together with P₂, will soon come to be applied upon the appearance of P₂ in the expectation that P₁ will also be present. In the course of time we may discover that P₂ is one of a family of predicates which provides a better basis for classifying things like X than does P₁’s family. Eventually, we shall adopt P₂ as the primary meaning of X. Or we may never quite decide. In either event there will be a long intermediate stage during which it would be quite incorrect to assert that X meant P₁ alone or that it meant P₂ alone. Yet during that time the term X is no less useful or less comprehensible than before. Hence the substitution type of definition would hardly apply at all to what may be a central scientific term, since it will apply only when we can categorically distinguish the essential from the accidental properties of X.

To this the formalist’s standard reply is that we must distinguish the context of discovery from the context of justification and analysis. Imprecise terms may suffice for the rough-and-ready needs of the frontier.

*The symbols are here and on some other occasions used as names of words as well as words, since there is no danger of confusion.
of research; but when we wish to say what we have discovered as opposed to what we have defined, then we must be able to distinguish definitions from empirical statements. The functionalist counters by saying that it is dubious whether anyone except the historian, i.e., the student of the context of discovery, needs to be able to distinguish discovery from definition to the degree of precision which the formalist is seeking; and for the rest, it is quite easy to say what we know and what evidence we have for it, and also to explain what terms mean ('define' them, in the usual sense) without having to make any but the most elementary distinctions between necessarily-connected P1's and empirically-connected P2's. Thus the physicist would not permit one to assert that conservative systems were discovered to obey the conservation laws or that canal rays were defined as having a positive charge. But he does not have to answer the question whether it's a matter of fact or definition that neon has the atomic weight 20.2; the relative unimportance of this question is a central tenet of this paper. One's logical reflexes make one fight against this conclusion. But if the distinction is an artificial one, why should one suppose that answers are not also artificial, i.e., do not reflect a real difference? I think the answer must be "The distinction does not apply here": there are truths by definition and empirical truths and there are also truths that are a compound like "Men are less than 120 feet tall." So the functionalist argues that there is a certain class of examples where a predicate can be said to be part of the meaning of a term and yet can also be said to tell us something empirical when it is applied to the term. And the more one reflects on the difference between knowing how to use a language and knowing how to formulate its rules correctly (or identify correct formulations of its rules), the more odd it would seem to insist that there must be a yes-or-no answer to the question "Is 'X is P' true by definition?" Compare "Is it a matter of convention (rather than free choice) that the clan gathers every Easter?" and "Is it by rational consideration (rather than unreasoning acceptance) that a Communist adopts the Moscow 'line'?"

This, then, would be the functionalist position argued on the basis of the noun-adjective type of term. The particular interest of the so-called connectives, i.e., words like "but", "and", "or", and "therefore", lies in their pervasiveness throughout a language; regardless of the boundaries between theory and observation, between scientific terms and the terms of everyday speech. We do not have a scientific defini-

*Not the same as actual vagueness; for the latter is a reflection of indeterminacy of the concept. The general property of non-susceptibility to exact definition is something else, perhaps best regarded as a limitation of the procedures of definition in a given language; it does not follow there is an associated vague concept, e.g., "but". A body under no forces moves with constant velocity.
versa, but this is needless paradox. It is only a little better to say 0 has a property in common with positive numbers and a property in common with negative numbers. So one could say that the first law is definitional because it could very properly be included in or entailed by an adequate explanation of the meaning of the term “force”, and it is empirical because it would be false \(^{39}\) in a Galilean world (where the motion of particles under no forces is ultimately circular). But “being definitional” and “being empirical” are uneasy bedfellows no less than “positive” and “negative”; one should rather admit they are not true contradictions.

9. Inter-categorical Definitions. Now the definitions we have so far solicited from the dictionary exhibit at least one common logical feature. They all provide words that, precisely or imprecisely, necessarily or empirically, tell us about the meaning of a term by providing alternative words that would not be completely out of place if substituted for the term defined—though we might have to do a little grammatical trimming as in the case where “but” was defined as “with this exception”. If they have not been exact substitution rules, they were nevertheless possible substitution rules, i.e., they offered a translation in the same grammatical category as the original.

I wish now to mention some strikingly different cases. Taking another connective of an even more fundamental nature, we find that “and” is defined by Webster in the following terms: “a particle expressing the general relation of connection or addition, used to conjoin word with word, phrase with phrase, clause with clause.” Now this is a grammatical account of the role of the word, not a possible substitute for it. It does not appear to express an equivalence which might have enabled us to first employ the term when we learned the language as a native, it tells us something about the word from the viewpoint of the grammarian. It does this partly because there is no alternative. One can produce synonyms for “but” if one is prepared to separate out enough contexts. “And” is too fundamental. So Webster abandons the search for synonyms completely and instead talks about function. I want to say two things about this definition. First, it seems remotely possible that there are circumstances in which it would give someone complete understanding of the meaning of a term, e.g., a foreigner with an understand-

\(^{39}\) This is something of an oversimplification since there are some possible though unlikely escapes from the mentioned disproof.

10. The Definition of “Definition’. Again we must decide whether to draw the line at calling this a definition or not. Webster would allow it not only in practice but in theory (“a formulation of the meaning or meanings of a word; as, dictionary definitions”) unless we object to the term “meaning” in the same way. But his product contains many examples which make his own definition too narrow, e.g., definitions of phrases such as “a definite article” (the adjacent entry to “definition”), and translations of sentences in foreign languages, e.g., “Le roi est mort, vive le roi.” The formalists are well behind us by this time, and only Richard Robinson—with an occasional disciple—is out in front.\(^{11}\) The objections to a restriction of the term “definition” which would exclude the above definition of “and” are, to repeat, that we must then say “and” is indefinable, which makes rather mysterious the fact that it is quite easy to explain what it means, and that no more restrictive analysis appears to hold up under careful scrutiny—perfect synonymy in natural languages being virtually imaginary. The objection to the wider interpretation offered by Robinson is that a flash of (telepathic or other) insight is an odd-looking definition. But there is a way of meeting the objections to a narrower view of definition. We examine with care the difference between indefinability and incomprehensibility, taking “indefinable” to mean simply “such that no explicit definition of it can be formulated” rather than “such that no explanation of its meaning can be given”. Then we abandon the ideal of exact synonymy in favor of the realistic, fully satisfactory concept of practical or conditional synonymy (discussed at length later in this paper). (This is perfectly

\(^{11}\) “... any process, whether verbal or otherwise, by which any individual, whether God or angel or man or beast, brings any individual, whether himself or another, to know the meaning of any elementary symbol, whether a word or other...” Definition, p. 27. Oxford: The Clarendon Press, 1950.
compatible with the rejection of some definitions of a given term as inferior to others on the grounds that they are less adequate formulations of intralinguistic equivalents, i.e., synonymous terms.

If we adopt this position, according to which a definition expresses a substitution possibility under certain conditions and with a workable degree of accuracy, it appears that we would still be able to reject the proffered ‘definition’ of “and”. For surely, as I have said above, “a particle expressing the general relation of connection or addition, etc.” cannot be used as a substitute for “and”, any more than “a noun made up of the three letters ‘i’ ‘n’ and ‘k’ in that order” can be used as a substitute for “ink”. The clues in a crossword puzzle are not always synonyms for the missing words though they are usually unambiguous—but obscure—references to them. Similarly there are many ways in which we can unambiguously—and not obscurely—convey the meaning of a term to someone other than by giving a synonymous term or conjunction of terms. We might add to this reason for differentiating Webster’s definition of “and” from his definition of most common nouns, the further reason that the latter alone can be regarded as reflecting the meaning the terms originally have for us. The definition of a term, it might be said, is the one which serves correctly to introduce it to the language-user previously unfamiliar with it. Robinson is right in stressing the connection of definition with bringing someone to understand a word, and right in stressing that definition refers only to elementary terms.12

We do not talk of the definition of a sentence. But it also seems that a definition has a preferred status among possible explanations of meaning which in part depends on its role as the proper formulation of the meaning of the term for the language-learner. A particular language has a structure of a particular kind according to which some expressions about the meaning of terms are definitions and some are not, even though almost all could be used for bringing someone to know the meaning of a term. Certainly, in a natural language there are usually several slightly different formulas which are all acceptable as expressing ‘the’ definition of a given term; but they are distinguishable from the

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12 Robinson's elementary terms are (my interpretation) terms which contain no proper parts such that the meaning of the term as a whole can be inferred by the usual rules from the meaning of the proper parts. Thus, the term “book end” has proper parts with meaning but its own meaning could not be inferred from their meaning by the usual rules (which would give it a meaning analogous to that of “rope end”).

many other true and complete statements about the meaning which the sophisticated user of the language can produce, and these other statements are such that the individual learning the language could (virtually) never extend his vocabulary by learning them. They would include such analyses as the grammatically slanted one of “and”, as well as others which involve ‘circularity’ or more difficult terms than the one being defined.

The implied functional (or genetic) character of definition I have here been proposing is not the same as the formal one of requiring substitutability. It is possible for a substitution rule to fail the functional requirement—Dr. Johnson’s celebrated ‘definition’ of “net” is a case in point, as is the definition of “sibling” as “male sibling or female sibling.” 13 And conversely, a functionally satisfactory definition may not be a substitution rule—ignoring ostensive ‘definition’ for the moment—we may by way of an example mention the ‘recursive definitions’ of number theory.14

If this functional condition is allowed, we see a further reason for our disquiet about the definition of “and”. Even though there are conceivable circumstances in which it might be used to introduce someone to the meaning of the term, they are so queer as almost to evade description: how could someone have understood the quite sophisticated grammatical vocabulary and the meaning of “with”, “or”, “connection”, and “addition” without understanding the word “and”? If some doubts are entertained whether the functional condition is allowable, then they may be allayed by asking whether it would be perfectly satisfactory if all definitions were couched in terms more abstract and complicated than that which is being defined? Or again if they were all circular, directly or at first or second remove? It seems clear that part of the function of definitions is the explanation of the meaning of the terms defined, in the language as it stands; and this is not possible or is extremely difficult if they are substitution rules of a kind in which the

13 If one alters the substitution-rule requirement to an elimination-rule requirement (and the distinction is not usually made, though its importance is here apparent) the genetic condition is satisfied more or less adequately. For to require eliminability is to require that none of the defining terms be such that they could only be understood via an understanding of the defined term, i.e., it is almost the genetic condition.

14 See, e.g., R. L. Wilder, Introduction to the Foundations of Mathematics, p. 253. New York: Wiley, 1952. A simpler example than the one he gives would be the definition of the square of a positive integer by the two assertions (a) the square of 1 is 1; (b) the square of (n + 1) is greater than the square of n by (2n + 1).
proffered substitute is harder to understand than, or impossible to understand without understanding, the defined term.

11. The Misleading Analogy with Mathematical Definition; The Actual Limitations on Redefinition. Before exploring the further consequences of this analysis of definition for certain traditional beliefs about definition, it is important to examine one possible objection. It might be said that an elementary understanding of formal systems reveals the possibility of defining terms in a large number of different ways, providing that corresponding changes are made in the status (but not the statement) of other definitions and propositions. To use the example given above we may define the square of an integer in terms of addition and subtraction in the recursive fashion indicated; in which case we will be able to prove that the square is equal to the self-product of the integer. Or, alternatively, we may define the square as the self-product; in which case we can readily prove that the differences between successive squares are the successive odd numbers (the formula on which the recursive definition is based). In either case, it would be argued, there is no change at all in the set of true propositions about numbers, in which we are to believe. Hence the functional condition, which would single out one of these as a correct definition and reject the other, is too restrictive.

But the functional condition would not have this consequence. It only requires that in a given formalization we be consistent about our descriptions of the various formulas. The ones which are used to express theorems are not usable to express definitions in the same system because one can't give proofs for definitions and one has to be able to give them for theorems. Mathematicians are very used to dealing with alternative formalizations and the comment that we can define the function "square of an integer" recursively as indicated does not mean that we can (in the ordinary way) assert that this is the definition of that function. It is not, since the accepted formalization introduces it as the self-product. But, and here is the crucial point, even the degree of arbitrariness which is permissible in the formalization of a deductive system, is not generally possible for a highly interpreted language. Essentially this is because the sentences expressing laws in one formalization—which would be adopted as definitions in another—are empirically true, not necessarily true as are most mathematical propositions. (One must add the qualification "most" in view of the Axiom of Choice, etc.)

DEFINITIONS, EXPLANATIONS, AND THEORIES

In the first place, of course, one cannot 'reformalize' English plus its scientific extensions, hence its present structure places definite limitations on what can be a definition. Second, in spite of the limitations on the program of formalizing Cantorian mathematics imposed by the Skolem paradox, truth-in-all-normal-interpretations remains an important criterion for judging the desirability of a mathematical axiom, whereas it counts against an axiom for a formalization of a physical theory (though some with this property are permissible). Hence a switch from one definition to another which was previously a law involves a risk which does not (generally) arise in mathematics, viz. discovery that this world is one in which the assumed interpretation does not give true statements. Since there are many cases (it would be wrong to say all) where the meaning of a term is impervious to new discoveries, this indicates that such a reformalization involves a change in the meaning. For example, the traits 'dominant' and 'autonomous' correlate very highly but to incorporate this correlation into the meaning of the terms by a 'reformalization' would be a plain error, roughly because the terms have other commitments of much greater importance in determining their meaning which require that this connection be dispensable by the facts in certain circumstances.

This point is elementary enough, but its significance has been obscured by the long search for extensional analyses (i.e., analyses in which necessity is nothing more than truth without existing exceptions) in the positivist logical tradition, since on an extensional analysis the distinction cannot be sustained. Thus Hempel is prone to saying "... if we wish, we may then imagine the theory-plus-interpretation at hand to be thrown into the form of a deductive system in which (7.5) becomes a definitional truth, and (7.3) assumes the character of a set of empirical statements ..." (p. 56, "The Theoretician's Dilemma"). According to what I am now saying, this could be done only at the expense of giving the terms in (7.5) a new meaning. The remark then becomes somewhat unexciting since it then expresses the fact that we can always adopt a new meaning for a given symbol.

Hempel's idea that a law can be adopted as a definition (presumably without change of meaning) is derived from his extensional requirements for a definition, viz. a set of empirically necessary and sufficient conditions for the application of a term. This is unsatisfactory according to both the formal and the functional criteria. It is not a formally
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satisfactory substitution rule since it makes nonsense out of many perfectly sensible subjunctive statements; and it is not functionally satisfactory because it elevates any accidental but exceptionless concomitants of a given condition or entity to the status of defining characteristics, i.e. characteristics which could be used to introduce and adequately express the meaning of the term. The further errors into which it seems to me this leads him in connection with, e.g., the hypotheses of reducibility will be considered below; for the moment, let us spell out the two simple reasons mentioned against the extensional account of definition.

Suppose all faculty members at Harvard live in a special city housing project, the Bainbridge Development. They do this by free choice, because they prefer the company of their colleagues and the rents are paid by the estate of Jonathan Bainbridge ('03); the city gave them first refusal on the space and no one else lives there. Clearly this will continue to be the case! Then this residence characteristic is an empirically necessary and sufficient condition for the application of the term "faculty member at Harvard". If we regard this as a justification for defining it in that way, the conditional statement, "If they increase the size of the faculty by another 850, some faculty members at Harvard will not be living in Bainbridge Development," (S) becomes self-contradictory. Now consider Hempel's suggestion about the possibility of rearranging the deductive system. We adopt the residence predicate as defintory of "Harvard faculty member" and we observe the empirical fact that all Harvard faculty members have jobs on the teaching staff at the largest private university in Massachusetts. The analogy with mathematics appears to hold in the sense that the same two statements about the Harvard faculty are held to be true, although true for different reasons. Is this not a perfectly satisfactory alternative method of defining the term?

It is not, since the meaning of the term "Harvard faculty member" is wholly independent of possible housing shortages in the city of Cambridge, Massachusetts, and the second formulation makes it wholly dependent on this. The corresponding situation cannot arise—empirically or even logically—in mathematics because no future contingency will falsify the theorem which we adopt as a definition in the second formalization. It is an essential part of the meaning of "Harvard faculty member" that sentences such as S are sensible rather than nonsensical, that we can use the term to describe changes in the residence of the group to which it refers, just as it is an essential part of the meaning of "resident of the Bainbridge Development" that it be independent of the occupation of those to whom it applies. In fifty years, it may be a radioactive slum where a few dying Martians live; the term will not have altered its meaning in the literal sense, even if the associations may have soured for those who know what has happened. Hence there often is a literal meaning for a term, i.e. a shifting boundary beyond which only misuse and metaphor lie.

Similarly, and in this case consequently, we should be doing a poor service to the language-learner were we to suggest that "Harvard faculty member" could be defined as "one who lives in the Bainbridge Development" since he would then suppose, for example, that when the Bainbridge Development is wiped out by a cobalt bomb, the term no longer has any application—which would simply be an error in his understanding of its meaning. An error of a different kind would result when he claimed to have discovered the occupational similarity of Harvard faculty members.

It is therefore not possible to reformalize interpreted languages and theories without changing the meaning—in the only useful sense—of certain of the terms, a point to which I shall make further reference in the later parts of this paper. The special features of mathematics make it a dangerous ideal for the logician concerned with the analysis of theoretical language, but we observed that it is even true in mathematics that the functional requirement applies to a given formalization.

12. Definitions of Logical Constants in Symbolic Logic. A nice example of the borderland arises in connection with the definability of the 'logical constants' in terms of each other. There is an understandable sense in which we can say that "and" (as a statement connective, only!) is definable in terms of "not" and "or" according to the rule

\[ p \land q: \text{It is not the case that; either } p \text{ is not the case or } q \text{ is not the case or neither } p \text{ nor } q \text{ are the case}. \]

This almost satisfies the formal requirement (the exceptions make it a poor but not an absurd definition), but one can see the improbability of supposing that it could satisfy the genetic requirement in any ordinary language used for the usual purposes. But let that point go and examine
the claimed possibility of defining “not” in terms of the Sheffer stroke function which we introduce according to the rule
p/q: either p or q is not the case.
The suggestion is that we use the definition
p is not the case: p/p.
This clearly violates the functional condition since the notion being defined is not only linguistically prior to that doing the defining in terms of feasible language training, but is directly involved in it and hence could not conceivably be learnt from this definition. (At least, not unless it is possible to acquire the apparently compound notion of a disjunction of negations without first acquiring that of a negation. This is not a problem of experimental psychology; for it can be answered by—and only answered by—a logical analysis which provides an account of what it would be like for someone to do this. (In formal terms this amounts to giving a model of a behavior-descriptive system which is (1) adequate for ordinary language-using situations and (2) in which the description in question is true.) If it can be shown to be logically possible, the point I am making would have to be modified, without any necessity for research into what is physically possible for the human organism.) This point is well put by Strawson in the following words: “The stroke-formula has an especial charm in that it illustrates with peculiar clarity the remoteness of the conception of definition within a formal system from the ordinary conception of verbal elucidation of meaning.” The actual account of definition that Strawson adopts, however, is equivalent to the straight synonym approach.

It is important not to forget that even the mathematical concept of definition is tied to the functional criterion within the system; if we define f(x₁) as g(x₁) · h(x₁), we cannot also define g(x₁) · h(x₁) as f(x₁). This is because we normally wish our definitions to serve as elimination rules, not merely substitution rules, i.e., the substitution has a particular point. The condition which ensures that this point is attainable is the functional condition, the requirement that the series of definitions should be such that the terms used in each definition are comprehensible independently of the term being defined. The special peculiarity of defining the negation of a proposition in terms of the stroke opera-

tor is that it violates the functional condition in interpretation but not in the uninterpreted calculus. How can one be said to understand a term in an uninterpreted calculus? When one can give its permissible equivalences and transformations, etc. Thus, there is nothing at all wrong with using the stroke operator, defined by an uninterpreted truth table to define the uninterpreted “¬p”. But one cannot define the logical concept of negation in terms of the logical concept of double negation, in the ordinary sense of definition, because one cannot introduce the latter without using and understanding the former; and if one has to use it and understand it, one is operating according to some kind of a definition already. This might of course be something unformalizable and not a definition in the usual sense; but in the propositional calculus it is in fact formalizable, e.g. in truth table form. Hence, the definition does not serve to introduce the concept; and hence, more crucially, it cannot serve to eliminate it. So the mathematician’s ideal of a definition as an elimination rule is attained only formally (the sign “¬p” can be eliminated) but not semantically (the concept of monadic negation is not eliminated). This is different from the situation in a calculus where we define “and” in terms of “not” and “or”. Apart from the literal weaknesses of the truth-functional formalization, semantic elimination is in this case perfectly possible, since the concept of conjunction does not enter into the concept of negation and disjunction. I think this is a reason for preferring the axiomatization of Principia Mathematica or Hilbert and Ackermann to the hyper-formalized performances by, e.g., Church in Introduction to Mathematical Logic, unless systemic elegance is regarded as more important than interpretable logical analysis. Of course, after all the connectives have been introduced, there is no difference between the systems; but their means of introducing the connectives makes one of them a reasonable analysis of the connectives we use in non-formal logic, while the other is not.

13. Definitions as Analyses. To say that a definition of a term already in use should be an analysis is, I think, another way of asserting the functional condition. If we are appraising a formal system supposed to represent the usual connectives, we regard it as a defect if they are represented as defined in terms of concepts which are incomprehensible.

17 Strawson, op. cit., pp. 78–79.
prior to the comprehension of the defined term, partly because they cannot be an analysis of what is meant by the defined term. Certainly Moore had something like this in mind when he said “we cannot define anything except by an analysis,” in which he later elucidates by saying that a concept has been analysed when “nobody can know that the analysandum applies to an object without knowing that the analysans applies to it.” This would eliminate such definitions as “brother” = “male sibling” and would not eliminate “sibling” = “a brother or sister”; it would eliminate the grammatical definition of “and” and the stroke-operator definition of “not”; but allow the definition of the statement connective “and” in terms of “not” and “or”, etc. It is well not to suppose that Moore imagines that someone who knows the analysandum can properly be applied in a certain case, thereby knows what the analysis of it is. Robinson misinterprets Moore in this way and criticizes him for talking of his “peculiar view that analysis is always easy and obvious.” He knows that it applies, if someone produces it; but he has not necessarily got it in mind as the analysis. In somewhat the same way, the man who knows how to swim an eight-beat crawl knows how to coordinate his arm and leg movements in the ratio of one cycle to eight cycles; but he may not even be aware that this is what he is doing in the sense of being able to produce this fact. However, he can very readily be brought to see that this is what he knows how to do (slow-motion in photography, etc.).

14. Definability and Reducibility. There is a type of putative definition of considerable importance closely related to the Moorean type of analytical definition on the one hand, and to the intercategorical definition, e.g., the grammatical definition of “and”, on the other. This is exemplified by the definition of “(visual) light” as “electromagnetic waves from such-and-such a frequency range”, and by the definition of “thirst” as “the state of an organism, after such-and-such a period of deprivation, which has a tendency to emit such-and-such response patterns”. It is not the case that someone using the defined term must know that the defining terms apply in the same range of cases, hence this is not a Moorean analysis and does not satisfy the functional condition. Nor, on the other hand, would its use as a substitution rule produce a nonsensical result, so it is not an inter-categorical definition.

The reductionists are those who have supported programs which involve ‘reducing’ optics and chemistry to the physics of waves, fields and particles, or psychology to the study of observable behavior, or biology to chemistry, or history and ethics to psychology, or ethics to decision theory, etc. As Hempel observes, “one component of this problem is the question whether the terms of the first discipline can be defined by means of those of the latter” (this volume, p. 56). The preceding discussion should alert us to the possibility of two different interpretations of this question. Does it mean “can be defined” in this way now, or does it mean “can be defined” in this way in some future reconstruction of our language? I think most discussions of reduction have been inconclusive due to a failure to specify which of these alternatives is under consideration. If the first, we shall certainly have to abandon the functional condition on definition and even—as we shall see—modify the formal condition, which makes a definition possible only in a peripheral sense. If the second, then the issue becomes either trivial—since one can always introduce a term in any way one likes—or involves a hidden condition to the effect that the meaning of the redefined term must not be too different from the original meaning of the term; in which case the first alternative question must be answered.

I shall assert that the reductionist thesis, in so far as it involves definability is untenable, in so far as it involves redefinability is pointless, and in so far as it involves anything else amounts to the assertion that there’s an explanation for everything.

First, then, to answer the question whether one can properly define the term to be reduced in terms of the reduction base, we notice immediately that the functional condition has to be abandoned since with respect to the English language as it now stands (which is all we can consider under the first alternative), we can certainly understand “light” without in any way understanding or recognizing the correctness of an analysis in terms of “electromagnetic”. There may well be said to be a technical usage of the term according to which it is defined in terms of “electromagnetic”; but to consider that alone is...
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comparable to considering a language in which the term has been re-defined in that way, i.e., amounts to considering the second alternative. Of course, the functional condition, although implicitly accepted by most logicians who actually propose only the formal condition—as we have seen—may be abandoned experimentally in order to see how strong a sense of definition is left for the reductionist.

Unfortunately, the formal condition cannot survive either. For suppose that what we might call a temporal Döppler shift is discovered to be operating, i.e., the wave lengths of visible light are steadily changing, but without any effect on the ordinary phenomena of vision. (It is not only that we find ourselves able to see by what was previously identified as ultraviolet light according to its wave length and unable to see what was previously in the wave band of red light; the same objects at the same temperature and under the same conditions still have the same visual appearance, but the measured wave length of what we see as yellow light has increased quite noticeably and gives every appearance of reaching the point where it would have been called red prior to the beginning of the shift.) Suppose also that no biological explanation gives any (independent) support to the claim that our retinal sensitivities have altered, and that the wave length of the sodium lines, etc. also changes. Then we would have the sort of situation which produces fundamental changes in theoretical physics; but it would certainly not produce any change, fundamental or otherwise, in our ordinary use of the term “light”. Why should it? The same things have the same colors, the same devices shed light, darkness comes at the same time, etc. Yet the proposed ‘definition’ of light no longer applies, i.e. the description just given is nonsensical. Hence it cannot be a substitution rule; hence, the formal criterion is violated. “Light” does not mean what the definition asserted it does mean. Nor is this case like the definition of “lemon” where we might be able to say that an approximately correct substitution rule has been proposed (a move which has its own serious difficulties—it will be discussed later); for there is every possibility that the present definition will be as far wrong as we care to specify, given time.

This conclusion leaves as the only possibility for saving the reductionist position the adoption of a weaker sense of definition. The immediate candidate is Hempel’s ‘set of empirically necessary and sufficient conditions.’ But this has the crucial difficulty of failing to distinguish between an empirically well-confirmed correlation and what we would now regard as a definitional connection, as a basis for definition, a point made above in terms of the description “Harvard faculty member”. It eliminates forever the question, “Is this true by definition or is it a matter of fact?” a question which it is indeed mistaken to suppose can always be usefully asked or answered (see section 8 above), but equally mistaken to suppose need never be answered. It eliminates as meaningless the consideration of certain hypotheses and conditional statements which are of great importance in the discussions of theoretical physics. It should be clear that if we can only support the definability aspect of reductionism by using such a weak account of definition, then the definability thesis had better be called something else since all it now amounts to is the requirement that the original term be applicable in the same cases as the ‘reduction’ of it, assuming our beliefs remain the same, and regardless of differences of meaning such as those between “featherless, tailless biped” and “human”. To suppose that “human” can be defined in such a way is to suppose that the meaning of the term makes it perfectly applicable to a plucked chicken, and this is simply to display ignorance of its meaning.

It is important to make two extensions of the above remarks. In the first place, it is entirely incidental that the example chosen—the definition of “light”—happens to be of a term in ordinary language. We could have equally well chosen a chemical term such as “aqua regia” or a psychological term such as “drive”, and the corresponding reductions in terms of atomic physics and neurophysiology respectively.

In the second place, even in the Hempelian sense of “definable”, the thesis does not hold owing to the imprecision of the terms involved. That is, no exact set of necessary and sufficient conditions in the reduction vocabulary could be specified for the important terms from the discipline which is to be reduced. It may seem odd that such a seemingly practical limitation should be thought to really count against an abstract logical thesis. But the limitation is not one of careless practice; it is rather an essential feature of theoretical terms that they are not reducible by definition or empirical correlation to an observation base and a fortiori not reducible to the theoretical terms of another discipline. This point and its effect on the ‘deductive ideal’ for explanations will be further examined below.

We should now turn to the second alternative, that of interpreting the definability thesis as applying to a possible, not an actual language.
Of this I want to say that it can be done only at the expense of the applicability of the language. The reason is very simple; the fact that a term is introduced by means of a certain definition does not guarantee that the term will always be correctly defined in that way. Thus, if we construct a new language within which the usual terms of chemistry are replaced by their ‘reduction’ in terms of atomic physics, I am suggesting that the terms will not retain these meanings if the language is used as a language for chemists in chemical laboratories. If it is so used, I am suggesting that the terms would quickly metamorphose in such a way as to reacquire their original unreduced meanings. This has nothing to do with the linguistic habits of chemists making them recalcitrant about altering their theoretical vocabulary: it reflects the logical point that the observation vocabulary and procedures of chemistry differ from those of atomic physics. Not that theoretical terms are explicitly definable in terms of the observational vocabulary; but without doubt their meaning is in part or wholly a function of their relation to the observation language. Chemists use, not bubble chambers and bevatrons, but flame tests and litmus solutions; psychologists use, not neurone probes and electroencephalographs, but T-mazes and Skinner boxes; naturalists use binoculars and counting devices rather than the electron microscope, etc.

Naturally the reductionist believes that even the observation base of chemistry can be described in terms of the reduction base of atomic physics. But the description is one which depends upon the correctness of atomic physics whereas the meaning of chemical terms does not—in general. The example of the definition of “light”, discussed at some length above, shows the way in which a redefinition in terms of the reduction base gives new hostages to fortune in a way which greatly reduces the utility of the redefined term and the viability of theories phrased in terms of it. Such a handicap is offset by no apparent gain. The unity of science can be exhibited in other ways; the utility of terms whose meaning is directly related to the observable phenomena is far greater than if some other domain must be consulted before definite proof of their applicability can be obtained. In short, the precision is bought at the expense of the utility.

15. The Concept of ‘Completed Science.’ The formalist at this stage is likely to resuscitate the ‘context of discovery vs. context of justification’ distinction; to suggest that for ‘completed science’ the redefinition will be advantageous. To do this is to interpret the definability thesis in a yet more utopian way. The very notion of ‘completed science’ reflects a misunderstanding of the context of scientific research; it depends on the metaphysical supposition of a determinate list of possible questions within a given field. There is no such list. Not only are there an infinite number of logically distinguishable questions—which would not in itself preclude the possibility of answering them all—but there is an infinite number of possible new types of question, and there is at least one type of question which permits no end to its answers.

These points can even be made at the observation level. There is no such thing as a ‘complete description’ of the contents of a laboratory. Not because there are an infinite number of atoms in the room, but because there is an indefinite number of things in the room and an indefinite number of relations between them and always an indefinite number of answers to the question, “What is the best description?” Of course, in any given context we can often talk of a complete description, e.g., for the police files, for the public health records, for the bank’s inventory, for the interior decorator, for the time-and-motion investigator, and for the biologist. In the same way we can come to the end of a particular type of inquiry in a scientific field; and in certain cases, we can even define the field in terms of a type of inquiry rather than a type of subject matter. But physics, chemistry, astronomy, and psychology are fields which are not restricted by the interests of one type of enquirer, e.g., the man using only optical instruments and only interested in finding linear relations.

So, first, the idea of the ‘completed science’ is a myth; but second, the redefinability thesis seems to lose most of its little remaining point in such circumstances. If all the questions have been answered, then we already know the relation of the concepts in the sciences at both levels, and this relation is already formalized. What virtue is there in converting these empirical relations into matters of definition? One disadvantage would be the impossibility of teaching the history of science.

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24 Of course, there are physical chemists, neuropsychologists like Hebb, and laboratory biologists. But the existence of shades of grey does not lessen the difference between black and white. I shall in fact argue for the necessity of shades of grey below; here I argue for the necessity of black and white (differences of meaning between the terms used by those whose observational vocabularies are different). Different semantic rules imply different concepts.

25 I owe this point to Stuart Hampshire.
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without the most appalling confusion since all the terms used in the contemporary science would have meanings different from that used in history, and every conceptual problem would have to be radically and confusingly reformulated. Another would be a development of my last point: even if the science is complete, it is presumably not completely dead, i.e., it is still applied, if not to 'really new' situations at least to situations new to those who confront them. In that case the criteria of application will rapidly come to supplant the reduction-base criteria, as before, because the theory is still supposed to be an account of the original field for the worker in that field, not for the worker in the reduction-base field. One can redefine the chemical vocabulary in terms of atomic physics, but as long as people are dealing with the kind of practical problems the applied chemist deals with, e.g., making analyses of samples, so long will the terms he uses have their meaning connected with the reactions of his samples to chemical tests, rather than to atomic physics.

Perhaps, however, one should go even further and ask whether the chemist as such might not become superfluous to the atomic physicist armed with a computer and various devices for producing beams of test particles. The answer to this is that one can hardly deny it as a logical possibility though for both theoretical and economic reasons it is highly improbable. But such an event would not save the definability thesis of reductionism. It would merely show that the chemist's problems can be solved by means of atomic physics better than by specifically chemical techniques. Remembering that the chemist's problems are often solvable with the help of a balance, a gas flame and a few dollars worth of reagents and glassware, rather than computers and beam analyzers, one can see the economic improbability of this occurrence. Remembering that the synthesis of organic compounds for specific purposes shows no signs of being a terminable process since, for one reason, the list of purposes comes from fields as diverse as agriculture and zoology and can always include as desiderata previously uncatalogued properties, one can see the theoretical improbability of the occurrence. There is also the sense in which the completeness of one science is impossible before all sciences are complete.

16. The Acceptable Aspects of Reductionism. Finally, there seem to me to be two further insuperable objections to the definability thesis in any form. In the first place it is unnecessary for the reductionist posi-

tion and in the second place it involves an unjustifiable assumption about the logical form of concepts. I shall not elaborate these points here beyond a few paragraphs, since their substance will be expanded in the later parts of this paper.

The reductionist position contains a certain element of truth which, as far as I can see, does not carry over to the definability thesis. This truth is that we are committed to explaining macro-phenomena in terms of micro-phenomena and thus must believe that the truths of the macro-level (whether observations or statements about theoretical entities) can be accounted for in micro-terms. The levels are not independent; they bear the relationship of explaining level to phenomena level, just as within each level there is also a distinction to be made between the observed phenomena and the phenomena postulated to explain them. Chemical reactions per se are not part of the observed phenomena of atomic physics—which would include, e.g., tracks on photographic plates exposed at high altitudes—but they consist in (can be described in terms of) atomic phenomena and hence must be explicable in atomic terms.

Why? Because the atomic level is the level to which we turn for an explanation of what we accept as basic data at the chemical level, e.g., the periodic table. If we cannot find an account there, we take that to be a deficiency of the micro-theory; it may be remediable there or we may have to invoke a new micro-theory, or we may have to invoke a micro-macro-level, the nuclear level. This procedure is what the search for explanations chiefly amounts to, in our world, with science as it now stands. It is by no means inconceivable that we shall have to accept limits to this procedure and it is often argued that in quantum mechanics we have already done so. At the moment, outside such areas, we have the very best inductive reasons for persevering in many such cases. In others, such as the reduction of psychology to the behavior level, we need to be very clear in what we consider the subject matter to consist; since only in one interpretation can even this form of the reductionist view be maintained.

As I understand him, Nagel (loc. cit.) does not agree with this. To say that we are committed to the existence of a micro-account is by no means to say that we are not sometimes likely to find, or committed to finding non-micro-accounts. There is nothing very mysterious about the former claim; it is mostly an analytic consequence of the identification of a particular level as the micro-level with respect to another.

But the claim that the macro-facts can be given a micro-explanation does not at all involve the claim that the macro-terms can be defined by micro-terms. In fact, reflection indicates how unlikely this will be in general: the Chinese have a perfectly adequate language for ordinary descriptive purposes, but it is far from being the case that we can give explicit definitions of each Chinese character in terms of the English vocabulary. Certainly we can (a) often give good approximations to an explicit (or simple contextual) definition, and (b) always explain—eventually—what the character means, in English. Even this need not be possible as between macro- and micro-languages; but supposing it is, the definability thesis is not thereby supported except in the sense in which it is indistinguishable from the thesis that a micro-explanation can be given of any macro-phenomena. We have seen that in this sense—a matter of fact, not of meaning—the definability thesis is very misleadingly titled.

As a particular application of the above remarks, it should be noted that an identity thesis on the mind-body problem is a case of reduction. For if it merely asserts extensional coexistence, it is not a solution of the mind-body problem but an assertion about the goings on in the world; whereas if it asserts an intensional identity, it runs onto the logical reefs partially charted above. (But Professor Feigl’s noble analysis in this volume suggests, and I think shows, the possibility of a channel through the reefs.)

There remains for many people the intuitive feeling that it must be possible to construct, in the micro-language, a concept which would have the same role as any given macro-concept, if the micro-theory is going to be capable of making the ‘same’ predictions and giving the ‘same’ explanations, etc. as the macro-theory. This view I shall call the isomorphism thesis, a name suggested by Paul Meehl. It seems to me to rest upon a very strong assumption about the ways in which terms may be given meaning. It is quite common in certain branches of applied chemistry and zoology for a distinction to be made between substances on the basis of their origin or history even when this does not coincide with any intrinsic physical differences. To take an example from gemology, the stones known as Cape emeralds (from their origin and color) are physically identical with the hydrated calcium-aluminum silicate usually called “prehnite” and found in the Hautes Alpes region and Connecticut. Now we would not expect this distinction to mani-

fest itself at the micro-level, nor would its failure to do so in any way affect the success of predictions about the behavior of the stone under conditions of heating, scratching, illuminating, etc.

The isomorphist might say that gemmology is partly geography or economics and that this part is not reducible. This move is dangerous since it immediately leads to the question why, e.g., variations in the strength of gravity between Cape Province and Connecticut are not also questions of geography. What we undertake to reduce must not be defined in terms of what we can achieve. But it might be said, by the isomorphist, that his language does contain an isomorphic element for each of the terms “Cape emerald” and “prehnite”, viz. the physico-chemical description of the mineral, qualified by a geographical description of the origin. That is, he imports the geography into the micro-language. Then what does the isomorphist position amount to? It looks very much like “If you can’t beat them, hire them”: it does not provide us with a further characteristic of a micro-language over and above its explanatory ability with respect to the macro-phenomena.

Certainly it is a requirement of a micro-theory that it be able to answer the question, “What do X’s consist of?” for every name, “X”, of a macro-state or macro-entity; and if the X’s figure in macro-laws, then their micro-equivalent must figure in formally similar laws or else we cannot be said to have correctly given the micro-equivalent of X. But our example shows that one cannot always give a purely micro-account of some X’s, e.g., when they are distinguished from Y’s by something which has no micro-reduction. If these examples do not count against isomorphism, then it is no more than a reiteration of the defining condition of any micro-account, that it explain macro-phenomena by analysing macro-concepts.

The question naturally arising from these criticisms of the reductionist program concerns the extent to which similar criticisms can be made of the proposals to reduce theoretical terms to the observation vocabulary. I think very similar criticisms can be made; but it will repay us first to pursue somewhat further our study of the many curious ways of defining which even the limitations of the dictionary encompass. There is a somewhat crude form of the latter program embodied in the operationist criterion of meaning (in its characteristic interpretations), to which the objections can readily be extrapolated from the above account.
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17. Definitions in Terms of Linguistic Function. The limitations of explicit identification with synonyms as a means of definition were discussed in the earlier part of the paper, and it is now time to examine some of its relatives on the other side of the family from those that abandon the formal requirement of meaning equivalence (intensional equivalence) in favor of the empirical requirement of truth equivalence (extensional equivalence). The cases to be examined retain intensional equivalence but abandon the explicit form; or compromise with both, but in a different way from Hempel's candidates.

It will be recalled that the definition of “but” was given as a set of synonym sets, the major divisions being according to grammatical categories and the minor according to context, this being illustrated by examples. The definition of “and” consisted primarily in a direct reference to its grammatical function, “a particle . . . used to conjoin word with word, etc.”, although the context where it could so serve is indicated by reference to the meaning “. . . connection or addition. . . .” This definition thus reverses the methods of context and meaning specification employed in the definition of “but”.

It is not hard to find further cases in which some reference to the function of a word is involved in giving its definition. In fact the conventions of the dictionary disguise the fact that every word which is both a noun and an adjective or verb, etc., e.g., “board” and “hand” has its various definitions distinguished by reference to these grammatical criteria. In what sense is this a defect? It is a defect in the sense that it involves abandonment of the formal criterion since one certainly does not find the part-of-speech label in the context of the unpremeditated term.

It may not violate the functional criterion since, even if the labels are not understood, the mere displaying of various alternative definitions may provide enough assistance for the reader to select the appropriate one, given the context in which the term occurs. But to give several definitions is hardly to give the definition of a term and the conclusion I want to insist on is that in many cases there is no way of giving the definition of a term in a particular context unless some labeling according to function is allowed. What is the definition of “board”? Well, it depends on the context. If we know from the context what its grammatical function is, we may then be able to give the definition. Again, we may not be able to do this, because the term may still be ambiguous, no matter whether it is a verb, an adjective, or, as in this case, a noun.

The sign 1/4/57 in the top right-hand corner of a business letter will be read by an American as January 4, and by an Englishman as April 1, without either having a suspicion that it is ambiguous. The context that decides will here consist in data about the nationality and residence of the author, the addressee, etc.

In view of such cases, why shouldn’t it be allowable to use some other clues in order to identify its relevant sense, such as a reference to the type of situation of those who used the term? Certainly a dictionary does this, with its descriptions “colloquial”, “slang”, “geometry”, “classical”, “obsolete”, “Old Persian”, etc. These are not, as is commonly thought, merely further information of an etymological nature, though indeed for someone who already or otherwise understands the word in question, they serve this purpose. They are also aids to selecting one sense of the word from the others. Yet they could form no part of the “definition” in the sense of “synonymous terms”.

And if these devices fail us, as they commonly do, we have to rely on the partial meaning we are able to abstract from the original context to distinguish among the remaining alternatives. So the definition gives us sets of possible translations, but only incomplete rules of selection. Nor should it be thought that these difficulties afflict only ambiguous terms.

For, in the first place, it is a great problem to decide when a term is ambiguous. Is the word “rational” (or “certain”) ambiguous because there are important differences between what it takes to be rational/certain in different situations, e.g. observing the standards of deductive versus inductive logic? Is the word “ground” ambiguous to the extent of having 23 different definitions as in Webster, or is the smaller number given in other dictionaries a more accurate analysis? Whereas philosophers often proliferate ‘senses’ of words the dictionary defines quite simply, e.g., “rational”, the reverse is even more common. To some extent, this is because the term “ambiguous” itself involves some context reference; what senses are distinguishable depends upon what is to count as a sense, i.e. what differences are significant for the current discussion.

But there remain many cases where it is possible to argue as strongly for regarding a term as ambiguous as against it; and in such cases, it seems a little absurd always to suppose that one point of view is really
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correct, and the other incorrect. ("Number" is unambiguous because it means "the basic arithmetical concept"; ambiguous because it includes integers, fractions, imaginary numbers, etc.) Hence a real difficulty remains for the lexicographer with respect to terms referring to linguistic function. If he can see a difference in linguistic function he is very inclined to distinguish meanings: how could a noun and a verb have the same meaning? But couldn't a conjunction connecting co-ordinate elements have the same meaning as a conjunction introducing a subordinate clause? And exactly what is the difference in meaning between "board" in "room and board" (noun use) as opposed to "You may board here" (verb use) except function? Different synonyms may be relevant because of the different function; but could one really understand one meaning and not be able to understand the other? If not, the ambiguity here is unlike that between "board" meaning "plank" and "board" meaning "group of directors". I want to suggest that distinguishing meanings (and hence defining terms) commonly involves, is facilitated by, and sometimes necessitates reference to linguistic function.

In the second place, there are many occasions when any definition of a term must involve reference to linguistic function—although we may prefer to say the term is indefinable. I have in mind such cases as "here", "him", "today", "this", as well as more complex cases such as the Oxford English Dictionary (but not the Webster) definition of "good" in terms of its role in the procedure of commendation. I do not include for the moment the other standard 'indefinables' such as "red", "itch", "cold", etc. because they are at least further away from the 'ideal' cases of triangles and squares.

Now "here" can be defined, and is by Webster, as "in this place"; but then we find "this" defined as "a demonstrative word referring particularly to what is present or near in place, time, . . . ." So the linguistic function comes in at one remove. It could be avoided even then, at the cost of complete circularity; but the dictionary is intended to serve the purpose of explaining meanings not merely providing synonyms, and hence it, like a logical system, adopts the functional criterion as far as it can.

Once more, as with the word "and", we find ourselves in something of a dilemma because, unlike 'definitions' of "red", "cold", etc. these definitions in terms of linguistic function are (a) complete and cor-
rect; (b) usable, admittedly in odd circumstances; (c) possibly analytic; and (d) about the meaning of the words. Thus we hesitate about saying the terms are indefinable. I wish to bring out the difficulty of a decision here in order to bring out the continuity of these definitions with 'contextual definitions' and 'implicit definitions' as well as with 'ostensive definitions'. But I think there are considerable dangers in overlooking the differences, so that the fact that in this paper I use the term "definition" in all these cases is not to be regarded as evidence that I take them at all to be essentially the same. Certainly I wish to retain a distinction between "defining a term" and "explaining the meaning of a term" and hence do not agree with Robinson's account. Since (a), (b), and (d) have already been discussed or do not need discussing, I shall add a few comments on (c).

18. The Analyticity of Definitions and Statements about Linguistic Function. It would normally be said that the definition of "this" just quoted was a metalinguistic statement, and that the dictionary slips these in among the translation rules because its conventions do not provide for a distinction between use and mention. The term to be defined occurs in heavy type and the definition may say something about it or give a synonym for it.

There is an alternative interpretation. Since the grammatical classification also occurs, along with various etymological facts, etc., it might be better to say that the material given is always about the term and that sometimes it contains other terms which the conventions entitle one to regard as jointly or separately synonymous, while at other times no such terms are given.

These synonyms should be regarded as being asserted to be synonyms by the definition just as the demonstrative properties of "this" are asserted by the definition. Hence it is always the case that dictionary definitions are empirical statements about meaning, linguistic function, origin, etc. This is supported by the extent to which other material is included; Robinson cites as examples judgments on the success of theories (definition of "phlogiston" as "the supposed cause of fire"), on the state of the metal market ("gold" as "the most precious metal" (Oxford English Dictionary), on proper spelling ("-ize" preferred to "-ise" by the Oxford English Dictionary). Other important non-synonymous material includes pictures (often almost essential, e.g., for "cuneiform," "illusion, optical"), definitions involving the use of the frequent
qualifications “usually”, “occasionally”, “excluding”, etc., and ‘incomplete’ definitions of various kinds as, e.g. of “meson”.

Moreover, if we are to be allowed to use our wits to the extent of distinguishing between the various senses offered, there is no reason to suppose us incapable of deriving synonyms from statements of linguistic function. Thus we can see from the definition of “this” that in some contexts we can substitute “the country I am now in” for “this country”. The lexicographer himself faces the choice of giving a multiplicity of senses or what might be called a generating formula for such senses, i.e. a description of linguistic function (cf. the definitions of “but” and “and”). Surely we should not regard one of these processes as defining and the other as something else (cf. recursive definitions in mathematics).

Finally, it could be added that definitions in terms of linguistic function are analytic just as are definitions in terms of synonyms and unlike etymological remarks, so we should not feel we are opening the floodgates when we admit them as definitions.

About this latter point it is necessary to make some qualifications. Consider the following statements.

“Brother” comes from an Anglo-Saxon word. (18.1)
“Brother” has seven letters. (18.2)
“Brother” means the same as “male sibling”. (18.3)
“Brother” means the same as “Brother”. (18.4)
Brothers are male siblings. (18.5)

We may consider 18.1 to be a paradigm of an empirical statement (a synthetic statement) about a term in a language. We discover its truth by an investigation quite independent of the procedure of coming to understand the statement itself. 18.5 is a paradigm of a simple definitonally-true statement (analytic statement); we could not be said to understand the statement without knowing it is true. There are some definitonally true statements of great complexity which we could be said to understand without recognizing their truth, so we extend the account of “analytic” to mean “such that its truth follows necessarily from the meaning (i.e. rules of use) of its terms”, i.e., requires no independent empirical investigation. Now 18.2, 18.3, and 18.4

*The ensuing account owes a very great deal to some discussions with Gavin Alexander. Its relevance to the nature of laws is discussed in his paper in the present volume.

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present a more serious problem. For in an obvious sense they tell us something about a particular sign, and it is clearly a matter of empirical fact that this particular sign has the properties of composition and equivalence, etc. that it has in a certain language. Yet on the other hand, it also appears that an understanding of the meaning of the terms involved is perfectly adequate grounds for deciding the truth of the statements without recourse to further empirical investigation.

True, we do have to do some counting in order to decide about 18.2, even though the capacity to do the counting may be required as part of understanding “seven”. If we wrote down a Chinese phrase, put it in quotes, and added the predicate “consists of three characters”, we see that in an analogous case we might well understand this one Chinese phrase, and understand what a character is in general terms, i.e. the sign unit in Chinese script, and what “three” means, without knowing (or being susceptible to a proof involving no further premises) that the statement was true. Let us then accept 18.2 as synthetic. Now, 18.3 is in the same situation, although it is more complicated. The trick of translating it into another language which sometimes clarifies the logical nature of statements, is unhelpful here since the requirement of knowing the meaning of the terms then involves knowing two languages; someone could certainly make the same statement without knowing that it’s true, but that doesn’t show that if he understood the meaning of all the terms involved, he wouldn’t know it was true.

This brings out the extent to which the term “involved” needs clarification. Is a term that is mentioned by name in a statement involved in the statement? If it is, 18.3 is analytic; if not, not. I think that the analytic-synthetic distinction originates in ‘unmixed’ discourse, i.e., discourse in which terms are used, not mentioned. It naturally extends to cases like 18.1 and probably 18.2. But then, with 18.3, one gets a conflation of the criteria: understanding the terms ‘involved’ and making the empirical investigation required to establish the truth of the statement about them amount to the same thing. Some refinement of the analytic-synthetic distinction is required if the distinction is to be preserved in such cases. (Or one could call them, unhelpful, synthetic a priori.) The natural redefinition would be of “analytic” as “true by virtue of the definitions (and rules of use) of the words and conventions...
used”. Thus, 18.3, which does not use the word “brother”, is synthetic. But 18.4 is surely analytic! The naming convention is merely instantiated, and it is hence directly derivable from that convention (which would presumably be expressed in the meta-metalanguage) in the same way in which “Brothers are brothers” or, more completely, 18.5, is directly derivable from the conventions governing the meaning of its used terms, e.g., 18.3. (Although I use the terms “directly derivable” and “necessarily follows” in the definitions of “analytic” above, these are not to be construed as meaning the converse of entailment, or deducibility; one cannot express rules of use in the object language, hence analytic statements in the object language cannot be deduced from rules of use.)

It is worth noting that certain further difficulties arise in connection with such statements as “This sentence is in English” (18.6) where the sentence itself is mentioned as well as used. It would seem most satisfactory in such cases to admit the limitations of the analytic-synthetic distinction when dealing with mixed-level statements. For if we agree that 18.6 is analytic, then

This sentence contains five words

is very similar but is ‘more synthetic,’ etc. Whereas if we consider taking 18.6 as synthetic, its similarity to 18.4 and thus 18.5 can be enlarged as can also its important differences from

The first sentence quoted in the sentence after that in which 18.7 was quoted, in the paper by Scriven in the volume . . . is in English.

These qualifications being made, we nevertheless can see the considerable strength of the contention that statements of linguistic function are very similar to definitions by synonym, particularly if the latter are exemplified from a dictionary. 18.3 tells us something about the meaning and use of the term “brother” by telling us it is the same as that of “male sibling”. Instead it might have been rephrased in terms of function and context and told us that it should be used in talking of families with children to refer to any of the male children (18.3’).

Once we see that statements expressing the complete meaning of a term in a form usable by the language-learner are not always statements of intra-linguistic equivalence, we are in a position to consider using the term “definition” in the wider sense which would include all such state-ments. I believe this is a viable alternative to the traditional one, and to Robinson’s. While it is naturally extensible to contextual definition, it would not include either ostensive or theory-implicit definition (definition by postulate).

The most important point for our further considerations of the discussion in this subsection, is the possibility of expressing definitions in the metalanguage. In fact definitions can only be expressed in the metalanguage unless some special convention is adopted to distinguish those object-language statements which are definitions from the others. Such a convention is the device of putting “= Df.” between the defined and defining terms. There is no internal evidence which enables us to recognize definitions, i.e., the content analyst has to accept the radical difference in the logical nature of a sentence which different contexts can make possible. In a simple artificial language, however, definitions are of less importance, the defined terms being characteristically eliminable. It is only when interpretation and explication and analysis of terms whose meaning is subtly shifting or highly complex becomes necessary that we turn to and need to study definition. But Church admits the peculiar difficulty of the present point when he refers to and avoids “such puzzling questions” as the nature of the sign “= Df.”

19. Definitions as Rules. The most interesting alternative view of the logical status of definitions amounts to taking them as rules of the language and hence as neither true nor false, but either adopted or ignored, applicable or irrelevant. Thus a rule like “Automobiles must keep to the left of the street” will apply in England, Sweden, and Australia and will not apply in Brazil or the United States. But if someone does not obey it, it will not thereby have been shown to have been false but only to have been ignored. In short, although general in form, a rule does not assert general compliance; it is neither true nor false, although the statement that it applies to a certain country or game or language is true or false. Definitions are simply linguistic rules of equivalence (or function, if the wider sense of “definition” is adopted) and even such errors as we find in dictionaries are actually formulations of rules which are not observed and so neither true nor false, just as the ‘correct’ definitions are actually merely those which are observed.

\[\text{Church, op. cit., Vol. I, p. 76n.}\]

\[\text{Ibid., p. 77.}\]
The notion of ‘error’ or ‘correctness’ derives from the preface or title of the dictionary which itself makes or implies the claim that the attached set of rules does apply to the English-speaking people: this claim is either true or false, but not the definitions themselves.\footnote{One of the problems about sentences like 18.3 is whether the implicit restriction to the English language does not make them analytic. Now “English” is defined by the total set of such rules; but it is similar to a word like “Iroquois” in that it clearly doesn’t depend on every single rule—English changes, but “English” doesn’t change its meaning (cf. “America”, “Thames”, etc.); yet this does not make every rule contingent.}

This position is peculiarly well suited to treatment of so-called stipulative (or legislative or impromptu) definitions. These are to be regarded as rules of the language game which are proposed for future observance; and as everyone appears to agree, whatever their view of dictionary definitions, stipulative definitions are proposals or promises, etc. and hence are definitely not true or false.

The central question in assessing this account of definitions is the relation between the facts about how a language is used and the definitions which are in some way connected with these facts. The usual assertion, contra the rule analysis, is that dictionary definitions are reports of usage and hence true or false. Let us consider this view.

\textbf{20. Dictionary Definitions as Reports of Usage.} I would imagine that an ordinary empirical report of usage would be a statement something like the following:

The Swahili-speaking peoples use the word “hiranu” to refer to male siblings in 89 per cent of the occasions when the term is employed.

Of the 11 per cent remaining cases, 10 per cent were cases where the speaker was judged on other grounds to be somewhat illiterate or relatively immature.

While I do not think we normally refer to inter-linguistic translations as definitions, we can in this case waive the point since an intra-linguistic explanation could be readily substituted and consider the following sentence as closely similar to a definition.

“Hiranu” in Swahili means the same as “brother” in English.

It seems fairly clear that 20.3 is significantly different from statements like 20.1 and 20.2. I would regard it as a hypothesis which is supported by 20.1 and 20.2 but needs a good deal more support. It is of little consequence that 11 per cent of the population do not use “hiranu” as we use “brother”; it is doubtless the case that a similar percentage of our population, e.g. that under the age of four years plus adult immigrants, similarly misuse the term. And anyway it is ambiguous (lay ‘brothers’ can be single children). These facts in themselves do not count against the usual dictionary definition of “brother’. Of course, if 99 per cent of the population used it in another way, we would have a great deal of trouble in sustaining 20.3 or its intra-English analogue. The kind of further information we need in judging whether 20.3 is justified concerns the homogeneity of the usage of the remaining 11 per cent (has the word other senses, i.e., is there a consistent usage around another norm?); the compresence of other relevant properties with male siblinghood, e.g. normal size and pigmentation and their absence in the aberrant cases (Were all dwarfs and albinos among them? i.e., Does “hiranu” involve the property of manliness or normality? etc.). And so on, indefinitely. The trained eye of a language-using creature and particularly that of a multilingual creature picks out these possibilities and discards them with immense speed and efficiency; the analysis of meanings in natural languages to any degree of subtlety is a formidable task to do implicitly but a hundred times more difficult to do explicitly. Homo sapiens alone among the species can do the former, but only an expert in linguistics can approach the latter.

Now, should we say 20.3 is a report of usage of a highly complex kind; or should we rather say that it is a statement about the meanings of words which is related to and supported by reports of usage in a highly complex way? Are statements about right and wrong complex statements about what people want, admire, encourage, etc.; or are they rather ethical statements which are related to what people want, etc.; or something else again? Are statements about the validity of arguments complex statements about the truth and falsity of the premises or are they merely related to such statements? and so on. Insofar as my arguments about reducibility in the earlier sections have weight, they support the second alternatives here. For when we introduce and build up a new vocabulary and approach and new treatment and interests in a subject which has been discussed in other ways before, we can virtually never dispense with the new vocabulary in favor of the old one, whether this is scientific analysis, ethics, or logical analysis.

The suggestion that 20.3 is a report of usage prompts the question,
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“What does it tell us about usage?” The answer to this question is extremely difficult: one can indicate this, suggest that, rule out something else. But one would have to qualify what one said many times because it is so dependent on the other aspects of the language and the group which uses it; and one could never in any circumstances say that a statement of the precision of 20.1 or 20.2 followed from it. Yet 20.3 is not a sloppy summary about usage; it is a precise statement about meaning. How central is this type of point to the whole enterprise of logical analysis, and how difficult it is for the formalist to deal with it adequately! The virtues of a natural language in science, as in philosophy or literature, lie in the eternal struggle to produce understanding by coining or using terms with irreducible, ‘indefinable’ dimensions of meaning, not incomprehensible for this reason but instead more comprehensible—so far is the formalist wrong and reconstructionism stu[1]lifying.\(^\text{33}\)

21. The Truth of Rules. Now 20.3 is not a rule as it stands, but an assertion as to the applicability of a rule, as the “definitions are rules” supporter would quickly point out. The rule would presumably be

“hiranu” means “brother” \(^{(21.1)}\)

This, it would be suggested, is the appropriate rule for Swahili-English translation, but not for Urdu-English translation. But in neither case is it true or false; it is merely applicable or relevant to the one case and not the other.

The position which I prefer to this one would deal with the situation as follows: I would say that 21.1 as it stands is neither a rule nor a statement of any other kind. It is a sentence of grammatically proper structure. In one context it could be a rule. Let us examine the sort of context required by considering the sentence

If a card is faced during the deal, the dealer redeals. \(^{(21.2)}\)

This could be an ‘accidentally true’ statement about a game or it might be a rule. For it to be a rule, violations of it would have to be regarded—when observed—as punishable, inappropriate, or as reprehensible (unsporting); it would have to be generally observed, be related in certain ways to the definition of the game and its governing body if any, etc.

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These conditions are both weaker and stronger than the conditions for it to be an accidentally true statement about the game. Suppose that a set of such conditions do obtain; then 21.2 is a rule of the relevant game. And, in the context of that game, it can be used to make a true statement. Suppose an argument arises as to who redeals after a card is turned up. If a player utters 21.5 with the appropriate intonation, it would then be appropriate for someone to turn to the rulebook, look up the relevant section and then say of the man who uttered 21.5, “He’s absolutely right,” or “It’s perfectly true, the dealer does redeal.” (The use of the present tense in rule language is somewhat unusual; cf. “If Socrates is a man and . . .” uttered today about the dead Greek philosopher.)

It might be asked: how could 21.1 ever be accidentally true—and if not, is 21.2 analogous to it? It is preferable to consider different examples.

The left bower is a higher card than the trump ace. \(^{(21.3)}\)
The penalty for a revoke is two tricks. \(^{(21.4)}\)

These examples have the feature that—so far as I can see—they could not intelligibly be said to be accidentally true. This is roughly because they non-vacuously involve rule-impregnated terms, i.e. they do not admit of a descriptive rather than prescriptive interpretation—whereas 21.2 does. I think that 21.1 is very similar in that “means” ties us straight down to a certain type of verification procedure, just as “higher than” and “penalty” do here, where they are applied to terms whose definition involves them, viz., “bower” and “revoke”, while “redeals” does not since it is not so applied. It seems to me very difficult to deny that if 21.4 was uttered at a bridge table in the context of an argument, it would be categorically true. Certainly as it stands it is neither true nor false, in somewhat the same way as “He loves cinnamon ice cream” is neither true nor false. But given the context in which the reference is clear, then the evaluation words are the same in both cases, viz. “true”, “correct”, etc.

21.3 even has the special interest that the term “left bower” is so defined (in euchre, etc.) that it is analytically true.

Now it is possible to say in reply to all this that when I say 21.4 can be true what I am really saying is that it is true that 21.4 applies to, e.g. bridge. The person at a bridge table who utters it should be taken to be saying, “21.4 is the relevant rule,” etc.
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This appears to me to be a forced interpretation; but one could compromise in the following way. One might say that 21.4 without context is a rule and is neither true nor false. In a certain context it can be used to make a statement which is true or false and which is equivalent to “21.4 applies here” (although not identical, since it is in the object language). This view has the consequence that one must be able to identify rules without examining the activities which they are said to govern. It is apparently easy to identify 21.3 and 21.4 as rules. But 21.2 is not, by itself, definitely a rule or not a rule. It may be in a different logical category, just as “Automobiles must keep to the left of the street” might well be a piece of advice (cf. “Automobiles must be regularly greased”). Many examples are even less committed to rule status, e.g., “The owner gets his property back immediately the stolen goods have been recovered.” How does one tell whether this is an unobserved rule or merely a false statement?

A more satisfactory compromise, then, would involve saying that a rule is an abstraction or legislation about certain aspects of behavior in what is called a “game”, (rather than saying it is a sentence which might be this) and that it is neither true nor false although a statement can be made with the same words which is true or false. The only drawback about this analysis (apart from the fact that it leaves 21.3 in the odd position of being neither true nor false) is its failure to do the required trick for definitions. For we must surely regard the definitions in a particular dictionary as statements made in a certain context rather than non-true non-false candidates for exemplification. When we open a Swahili-English dictionary and find opposite the “hiranu” the word “brother”, this is like opening a supposed book of bridge rules and finding that the list of penalties includes 21.4. The context is provided and the translation of “hiranu”, as also the listing of penalties, is correct or incorrect, this to be decided according to the complex procedure previously mentioned. It appears to me that the relation between the utterances “21.4 applies here” and “21.4” is like that between “The sky is blue” is true” and “The sky is blue.”

There remains to be considered what is perhaps the most powerful argument for the ‘definitions are rules’ view, viz., the analysis of stipulative definitions.

No essential difference arises if we consider a dictionary of the English language; the other kind was employed in order to eliminate the ‘translation move.’

22. Stipulative Definitions. Just as one might propose a variant of golf in which a player is restricted to two clubs, or plays with a hockey stick, so one may propose variants of English in which an old rule is changed or a new one added. These proposals are in both cases rules and in the second case are referred to as stipulative definitions. A typical example would be the introduction of the term “climatic tolerance factor” or “CTF” for an individual’s score on a series of tests done under varying conditions of humidity, air velocity, and temperature. It is surely absurd to debate whether such a definition is true or false. It is a proposed rule. Hence, one might argue, other definitions should be construed as accepted rules: still neither true nor false. But we must lay the context and some comparisons out a little more fully before we can judge the merit of this suggestion.

If a man says, “Let’s start a fashion for wearing odd socks,” he has proposed a pattern of behavior. Certainly his proposal is not true or false per se. But subsequently we can say, “It’s fashionable to wear odd socks” and be right or wrong. If a man says, “Let’s use the term ‘climatic tolerance factor’ for the score on these tests,” his proposal is not true or false, but in a year or two we can say “CTF means ‘score on these tests’” and be right or wrong. For the world is no respecter of word inventors, and the more widespread the term’s acceptance becomes, the more likely it is that a deviant usage will develop and, perhaps, become dominant.

Now at the time the innovator makes his odd-sock proposal, would it be correct to say immediately that it is fashionable to wear odd socks? Clearly not, unless Beau Brummell himself is the original speaker and even then the statement would be anticipatory even if reliably so. A fashion is a fashion only if it catches on. So, just as it would be an error to suppose that the progenitor of a new pattern of behavior is in a wholly privileged position with respect to its future development, so it is an error to suppose that he can always guarantee even a short period of acceptance. When we consider the special case of inventing a new term, however, we are inclined to say that here at least the innovator’s word is law, for a stipulative definition is a fashion which requires only his own acceptance to verify the statement that the term has the meaning he proposed. And in the case of a man introducing a term in the preface to a book which he has already written, he can be fairly certain he is correct about the term’s meaning in that book. (It seems
this case is more like legislating: the legislature’s word is law as soon as it says it is.)

But if he can be correct, what has happened to the idea of the stipulative definition as neither true nor false? The truth seems to be that the exact form of a stipulative definition has never been very clearly specified. According to Copi, they may be proposals (which he expands as commands or invitations), or they may be predictions.\textsuperscript{35} Can a definition really be all these things? Is it not rather that the definition may figure in a proposal or command and be related to a prediction? More exactly, the proposal or command is to the effect that a certain sentence be accepted as a meaning rule, the prediction is to the effect that this sentence will be the correct expression of a meaning rule governing the term in a certain context. As we can see from this formulation, which seems to me more accurate, the sense in which the proposal involves a definition is the weak sense in which a proposal of marriage involves a marriage or the proposal of a truce involves a truce. No actual definition, marriage or truce is being discussed, but a contemplated one is outlined.

But that which is proposed is a definition of the ordinary kind: there is nothing special about it except that it does not yet apply. I suggest, therefore, that we abandon the term “stipulative definition” (or “legislative definition” or “improptu definition”) as a distinct category of definition, retaining it—if at all—merely to refer to the circumstances in which a particular formulation of a definition is found. In this way we avoid the embarrassment of explaining what possible difference there is between the statement “In Modern Elementary Logic, ‘p \iff q’ means ‘p and q are inter-deducible’” when made by the author of that (fictitious) book in the introduction and when made by a student of contemporary notations. The legislation passed by Parliament is not itself true or false, but its passage makes true a statement made in the same words; the definition in the introduction is not quite so immune to judgment, because it is also a statement about the meaning of the word as used thereafter. Hence it is true or false as soon as its employment is created. What we should say of it in that space of time which may exist (or may not, depending on which is written first, the book or the introduction) before its employment is created is discussed below. But here


we should note the analogy would have to be with a legislature which passes, not a law, but an ‘observed law.’ There are circumstances in which we would agree their act does create an observed law (e.g., if the practice already exists, cf. a dictionary definition). But even when a law isn’t observed at all we can still say it is the law, whereas a definition which is totally ignored in the sequel in favor of another usage could hardly be said to be ‘the definition of’ that term as it occurs in the sequel (and the context makes it clear that it is intended to refer to the sequel).

This is not to reduce all stipulative definitions to predictions—the only one of Copi’s alternatives which can be true or false. The definitions he has in mind are not predictions—much less so, in fact, than his ‘lexical’ (dictionary) definitions are ‘empirical reports of word usage.’\textsuperscript{36} But both, being the same, are true or false—in the same way and for the same reasons—even if not analytically so in the standard sense. It may even be agreed that the sentence which is being proposed for a definition is at that very instant (supposing there to be as yet no other usage of the term in the proposed sense) true, because it ‘constitutes its own usage.’ But at that very instant it is not in the full sense a definition: a proposed rule is not a rule; the proposed marriage is not, at the moment of proposal, a success or a failure; though it will turn out to be one or the other just as the sentence will turn out to be a correct or an incorrect formulation of the definition when the usage comes into existence. And just as we may have good grounds for pessimism about the proposed marriage so we may have good objections to the proposed definition. We must realize that “a proposed definition” means “a sentence which it is proposed we should take as a definition” and not “a definition which is expressed in a proposal”\textsuperscript{37}.

I conclude from this discussion that, absurd though it may be to regard proposals as true or false, and mistaken though it may be to regard the definition forms which are the subject of some proposals in certain fleeting contexts as true or false, this can be regarded as due to the fact they are not yet definitions, just as a proposed marriage is not yet a marriage, a contemplated vacation not yet a vacation.

Is it not very dogmatic to say, as I appear to be saying, that the

\textsuperscript{36} \textit{Ibid}.

\textsuperscript{37} It is really very unfortunate that proposals, i.e. propositions, are not propositions, i.e. not true or false.
mathematician who begins a systematization of a new branch of the calculus with what he calls a set of definitions, is misusing the latter term? But I am no more saying this than I am that one would be unjustified in writing and so naming an ‘introduction’ to a book before writing the book. Certainly, if one never wrote the book it would be somewhat odd to refer to what one had written as an introduction: it would be a curiosity, an arrow sign without a head. What could it refer to? To the proposed, but nonexistent book, i.e. it would read as if there were a book attached to it. Again, one sometimes hears the reply made to an enquiry about the progress of someone’s book, “Well, I’ve completed the table of contents.” It isn’t a table of existing contents, but an existing table of contemplated contents. Similarly, a ‘stipulative definition,’ e.g. a proposed abbreviation, is a rule which governs no actual usage—at the moment of its formulation—but can be referred to as a definition simply because it is intended to govern a contemplated usage. And after the usage is created, it is no longer a stipulative definition but just a good or bad definition.

We are here facing another version of the difficulty that the analysis of rules faces over identifying something as a rule without there being any game in existence to which it applies. Independent of all contexts, one could not so identify a rule, unless it contained several terms from rule language used non-vacuously—and we cannot easily decide what a non-vacuous use would be in such a case.38 But in a certain context, say contemplation of a possible new game, we can consider them as rules. The mathematician setting out a formalized system shows us how insignificant the stipulative definition’s moment of existence commonly is. For he does not pad his presentation of the definitions of the system as proposals or predictions; he merely lists them as definitions. By so doing he commits himself to the claim that they are the correct definitions of the terms he is about to use; but are his definitions lexical or stipulative? The answer I have given, by denying the existence of the separate category, is “Neither: for these are not proposals, predictions, or reports.” They are statements about the meaning of certain symbols in a certain context which, when they will be read, already exists (hence

38 Nor indeed can we easily decide whether the terms are from rule language; consider the following sentence: “Only one marriage can be declared at one time.” Is it a statement of impossibility, a rule of prudence, or a moral rule? We cannot say; in fact, we can add to the alternatives by pointing out that it is a rule of the card game gagel (Hoyle, 39th edn., p. 148. Cincinnati: U.S. Playing Card Co., 1941).
but the definition is subsequently either true or false.\footnote{If a stipulative definition were ever a prediction, which I do not think is so, it would be immediately true or false. I would say that the statement “In the sequel ‘cabard’ will be used to mean ‘without virtue’” is a prediction about future usage but not a definition, my argument being the same as that in connection with the proposal interpretation above.} While I have been talking in much of the above as if definitions were a kind of rule, I want finally to qualify this as a precise formulation by saying that I think the term “rule” is commonly used for the non-true, non-false abstraction from rule-governed behavior or contemplated behavior (but cf. 21.3), while I think the term “definition” is more commonly used for statements of the form “‘x’ means ‘y,’” or for the equivalences found in dictionaries of a specific language, and I think these are true or false. One can abstract from these and use the term “definition” for what we would usually call “possible definitions”, but I do not think this is usual. Hence it would be misleading to identify definitions as rules, except when this difference is irrelevant or less important than the undoubted similarities.

It should be noticed, however, that the theory of games provides certain meta-rules which may or may not be correctly stated, e.g. “In zero-sum two-player games, for maximum expectation stakes should be less than half the players’ capital.” Somewhat analogous to these (but I think a little less so than Professor Braithwaite suggests\footnote{The Theory of Games as a Tool for the Moral Philosopher. Cambridge: Cambridge Univ. Press, 1956. Cf. my review in Ethics (forthcoming). There seems to be a pressing need for a companion volume written by some cardsharp mathematician (or perhaps a lifeman) called “The Theory of Games as a Tool for the Immoral Philosopher.”}) are moral rules of which it seems to me some are obviously true, e.g. “Murder is wrong” (cf. “The left bowler is higher than the ace of trumps”); while others are better construed as too context dependent to be true or false as they stand, e.g. “Polygamy is wrong” (cf. “No player may stake more on a hand than he has on the table before the first bid is made”).

24. The Application and Invocation of Rules. A further caution must be added to what has been said in such metaphorical terms about ‘abstracting rules from behavior.’ Someone might watch poker played every night for a year and have no idea whether the ‘royalties’ rule is regarded as applying, i.e. whether holders of royal flushes are paid a fixed sum by everyone in the game, whether they are betting or not in the particular hand. Rules which apply are not necessarily regularly invoked.

DEFINITIONS, EXPLANATIONS, AND THEORIES

But that they apply can be determined by various means of enquiry, and when we talk of rules as abstractions from behavior we have to include potential behavior. To say that a rule applies is to say that in certain circumstances certain procedures are correct. In the case of definitions a similar problem arises over constantly concomitant ‘accidental characteristics.’ Despite the cautions already entered against insisting these must be either purely definitory or purely empirical, there are often distinctions to be made which depend upon (manifest themselves as) the answers to questions about possible states of affairs and might never show up in natural usage. Hence we must regard definitions as abstractions from actual plus potential language practices. As the term “usage” is often taken to include both, this comment does not necessarily amount to a criticism of the view that definitions are abstractions from, or encapsulations of, usage. This point is of central importance for the discussion of definitions with empirical content, presuppositions, or bearing, to which we now return. (And it is virtually unmanageable within the usual categories of designation, denotation, connotation, etc. as is the succeeding discussion of names.)

25. Names of Individuals. Can one define terms such as “Immanuel Kant”, “805 Harvard Avenue, Swarthmore”? It should not be supposed that a categorical distinction exists between such terms as these, and “Iroquois” or “Hindu”; nor between the latter and “native” or “lemon”. Neither is there any merit, so far as I can see, in regarding one group as epistemologically or psychologically prior to another. To do so would be like regarding one of the following groups as epistemologically primary: motorcycles, automobiles, and buses.

Suppose we were to propound the definition

Immanuel Kant: German philosopher, (1724–1804) author of “Critique of Pure Reason,” etc. (25.1)

This might be an entry in a pocket biographical ‘dictionary,’ but—we feel—it can hardly be regarded as a respectable definition. The chief reason would presumably be that the ‘defining terms’ actually provide empirical information rather than defining characteristics. The statement “Kant died in 1804” is not analytic. As it stands, this argument is no different from that which would make a definition of a term such as “lemon” or “copper” impossible. It will be recalled, however, that we produced one remaining type of analytic statement from the usual