

The Analytic and the Synthetic

The techniques employed by philosophers of physics are usually the very ones being employed by philosophers of a less specialized kind (especially empiricist philosophers) at the time. Thus Mill's philosophy of science largely reflects Hume's associationism; Reichenbach's philosophy of science reflects Viennese positivism with its conventionalism, its tendency to identify (or confuse) meaning and evidence, and its sharp dichotomy between "the empirical facts" and "the rules of the language"; and (coming up to the present time) Toulmin's philosophy of science is an attempt to give an account of what scientists do which is consonant with the linguistic philosophy of Wittgenstein. For this reason, errors in general philosophy can have a far-reaching effect on the philosophy of science. The confusion of meaning with evidence is one such error whose effects are well known: it is the contention of the present paper that overworking of the analytic-synthetic distinction is another root of what is most distorted in the writings of conventional philosophers of science.

The present paper is an attempt to give an account of the analytic-synthetic distinction both inside and outside of physical theory. It is hoped that the paper is sufficiently nontechnical to be followed by a reader whose background in science is not extensive; but it has been necessary to consider problems connected with physical science (particularly the definition of 'kinetic energy,' and the conceptual problems connected with geometry) in order to bring out the features of the analytic-synthetic distinction that seem to me to be the most important.

In addition to the danger of overworking the analytic-synthetic distinction, there is the somewhat newer danger of denying its existence altogether. Although, as I shall argue below, this is a less serious error (from the point of view of the scientist or the philosopher interested in the conceptual problems presented by physical theory) than the customary overworking of the distinction, it is, nevertheless, an error. Thus the present

paper fights on two fronts: it tries to "defend" the distinction, while attacking its extensive abuse by philosophers. Fortunately, the two fronts are not too distant from each other; one reason that the analytic-synthetic distinction has seemed so difficult to defend recently is that it has become so bloated!

Replies to Quine. In the spring of 1951 Professor W. V. Quine published a paper entitled "Two Dogmas of Empiricism."¹ This paper provoked a spate of replies, but most of the replies did not match the paper which stimulated them in originality or philosophic significance. Quine denied the existence of the analytic-synthetic distinction altogether. He challenged doctrines which had been dear to the hearts of a great many philosophers and (in spite of the title of his paper) not only philosophers in the empiricist camp. The replies to Quine have played mostly on a relatively small number of stereotyped themes. The tendency has been to "refute" Quine by citing examples. Of course, the analytic-synthetic distinction rests on a certain number of classical examples. We would not have been tempted to draw it or to keep on drawing it for so long if we did not have a stock of familiar examples on which to fall back. But it is clear that the challenge raised by Quine cannot be met either by pointing to the traditional examples or by simply waving one's hand and saying how implausible it is that there should be no distinction at all when there seems to be such a clear one in at least some cases. I do not agree with Quine, as will be clear in the sequel. I am convinced that there is an analytic-synthetic distinction that we can correctly (if not very importantly) draw, and I am inclined to sympathize with those who cite the examples and who stress the implausibility, the tremendous implausibility, of Quine's thesis—the thesis that the distinction which certainly seems to exist does not in fact exist at all.

But to say that Quine is wrong is not in itself very fruitful or very interesting. The important question is How is he wrong? Faced with the battery of Quine's arguments, how can we defend the existence of any genuine analytic-synthetic distinction at all? Philosophers have the right to have intuitions and to believe things on faith; scientists often have no better warrant for many of their beliefs, at least not for a time. But if a philosopher really feels that Quine is wrong and has no statement to make other than the statement that Quine is wrong and that he feels this in his

¹ Reprinted in *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press, 1953), pp. 20–46.

bones, then this is material to be included in that philosopher's autobiography; it does not belong in a technical journal under the pretense of being a reply to Quine. From this criticism I specifically exempt the article by P. F. Strawson and A. P. Grice,² who offer *theoretical* reasons for supposing that the analytic-synthetic distinction does in fact exist, even if they do not very satisfactorily delineate that distinction or shed much real light on its nature. Indeed, the argument used by them to the effect that *where there is agreement on the use of the expressions involved with respect to an open class, there must necessarily be some kind of distinction present*, seems to me correct and important. Perhaps this argument is the only one of any novelty to have appeared since Quine published his paper.

But important as it is to have a theoretical argument supporting the existence of the distinction in question (so that we do not have to appeal simply to "intuition" and "faith"), still the argument offered by Strawson and Grice does not go far toward clarifying the distinction, and this, after all, is Quine's challenge. In other words, we are in the position of *knowing* that there is an analytic-synthetic distinction but of not being able to make it very clear just what the nature of this distinction is.

Of course, in some cases it is not very important that we cannot make clear what the nature of a distinction is, but in the case of the analytic-synthetic distinction it seems that the nature of the distinction is far more important than the few trivial examples that are commonly cited, e.g., 'All bachelors are unmarried' (for the analytic side of the dichotomy) and 'There is a book on this table' (for the synthetic side). To repeat: philosophers who do not agree with Quine have found themselves in the last few years in this position: they *know* that there is an analytic-synthetic distinction but they are unable to give a satisfactory account of its nature.

It is, in the first place, no good to draw the distinction by saying that a man who rejects an analytic sentence is *said* not to understand the language or the relevant part of the language. For this is a comment on the use of the word 'understand' and, as such, not very helpful. There could be an analytic-synthetic distinction even in a language which did not use such words as 'analytic,' 'synthetic,' 'meaning,' and 'understanding.' We do not want, after all, to draw the analytic-synthetic distinction in terms

² In *Defense of a Dogma*, *Philosophical Review*, 65:141-158 (1956).

of dispositions to use the words 'analytic' and 'synthetic' themselves, nor dispositions to use related expressions, e.g., 'have the same meaning' and 'does not understand what he is saying.' What is needed is something quite different: We should be able to indicate the nature and rationale of the analytic-synthetic distinction. What happens to a statement when it is analytic? What do people do with it? Or if one wishes to talk in terms of artificial languages: What point is there to having a separate class of statements called analytic statements? Why mark these off from all the others? What do you do with the statements so marked? It is only in this sort of terms that I think we can go beyond the level of saying, "Of course there are analytic statements. I can give you examples. If someone rejects one of these, we say he doesn't understand the language, etc." The real problem is not to describe the language game we play with words like 'meaning' and 'understanding' but to answer the deeper question, "What is the point of the game?"

The analytic-synthetic distinction in philosophy. It should not be supposed that the axe I have to grind here is that Quine is wrong. That Quine is wrong I have no doubt. This is not a matter of philosophical argument: it seems to me there is as gross a distinction between 'All bachelors are unmarried' and 'There is a book on this table' as between any two things in the world, or, at any rate, between any two linguistic expressions in the world; and no matter how long I might fail in trying to clarify the distinction, I should not be persuaded that it does not exist. In fact, I do not understand what it would mean to say that a distinction between two things *that* different does not exist.

Thus I think that Quine is wrong. There are analytic statements: 'All bachelors are unmarried' is one of them. But in a deeper sense I think that Quine is right; far more right than his critics. I think that there is an analytic-synthetic distinction, but a rather trivial one. And I think that the analytic-synthetic distinction has been so radically overworked that it is less of a philosophic error, although it is an error, to maintain that there is no distinction at all than it is to employ the distinction in the way that it has been employed by some of the leading analytic philosophers of our generation. I think, in other words, that if one proceeds, as Quine does, on the assumption that there is no analytic-synthetic distinction at all, one would be right on far more philosophic issues and one will be led to far more philosophic insights than one will be if one accepts that heady concoction of ideas with which we are all too familiar: the

idea that every statement is either analytic or synthetic; the idea that all logical truths are analytic; the idea that all analytic truth derives its necessity from “linguistic convention.” I would even put the thesis to be defended here more strongly: ignore the analytic-synthetic distinction, and you will not be wrong in connection with any philosophic issues not having to do specifically with the distinction. Attempt to use it as a weapon in philosophical discussion, and you will consistently be wrong.

It is not, of course, an accident that one will consistently be wrong if one attempts to employ the analytic-synthetic distinction in philosophy. ‘Bachelor’ may be synonymous with ‘unmarried man’ but that cuts no philosophic ice. ‘Chair’ may be synonymous with ‘movable seat for one with a back’ but that bakes no philosophic bread and washes no philosophic windows. It is the belief that there are synonymies and analyticities of a deeper nature—synonymies and analyticities that cannot be discovered by the lexicographer or the linguist but only by the philosopher—that is incorrect.³

I don’t happen to believe that there are such objects as “sense data”; so I do not find “sense-datum language” much more interesting than phlogiston language or leprechaun language. But even if sense data did exist and we granted the possibility of constructing sense-datum language, I do not think that the expression ‘chair,’ although it is synonymous with ‘movable seat for one with a back,’ is in the same way synonymous with any expression that one could in principle construct in the sense-datum language. This is an example of the type of “hidden” synonymy or “philosophic” synonymy that some philosophers have claimed to discover and that does not exist.

However, misuse of the analytic-synthetic distinction is not confined to translationists. I have seen it argued by a philosopher of a more contemporary strain that the hypothesis that the earth came into existence five minutes ago (complete with “memory traces,” “causal remains,” etc.) is a *logically* absurd hypothesis. The argument was that the whole use of time words presupposes the existence of the past. If we grant the meaningfulness of this hypothesis, then, it is contended, we must grant the possibility that there is no past at all (the world might have come into existence at *this* instant). Thus, we have an example of a statement which uses time words, but which, if true, destroys the possibility of their use.

³ I do not wish to suggest that linguistic regularities, properly so called, are never of importance in philosophy, but only that analytic statements, properly so called, are not.

This somewhat fuzzily described situation is alleged to be tantamount to the meaninglessness or self-contradictoriness of the hypothesis I described.

Now I agree that the hypothesis in question is more than empirically false. It is empirically false, if by empirically false one means simply that it is false about the world—the world did not come into existence at this instant nor did it come into existence five minutes ago. It is not empirically false if one means by ‘empirically false statement’ a statement which can be confuted by *isolated* experiments. But while it is important to recognize that this is not the sort of hypothesis that can be confuted by isolated experiment, it is not, I think, happy to maintain that the existence of a past is analytic, if one’s paradigm for analyticity is the ‘All bachelors are unmarried’ kind of statement.⁴ And I think that, while few philosophers would explicitly make the kind of mistake I have described, a great many philosophers tend to make it implicitly. The idea that every truth which is not empirical in the second of the senses I mentioned must be a “rule of language” or that all necessity must be traced down to the obligation not to “violate the rules of language” is a pernicious one, and Quine is profoundly right in rejecting it; the reasons he gives are, moreover, the right reasons. What I maintain is that there are no further rules of language beyond the garden variety of rules which a lexicographer or a grammarian might discover, and which only the philosopher can discover.

This is not to say that there are not some things which are very much *like* “rules of language.” There is after all a place for *stipulation* in cognitive inquiry, and truth by stipulation has seemed to some the very model of analyticity. There is also the question of linguistic misuse. Under certain circumstances a man is said not merely to be in error but to be making linguistic mistakes—not to know the meaning of the very words he is employing. Philosophers have thought that by looking at such situations we could reconstruct a codex which might constitute the “implicit rules” of natural language. For instance, they hold that, in many circumstances, to say of a man that he knows that *p* implies that he has, or had at some time, or can produce, or could produce at some time, evidence that *p*—and that such an implication is very much like the implication between being a bachelor and being unmarried. But, as I shall argue be-

⁴ To accept the hypothesis that the world came into existence five minutes ago does not make it necessary to give up any *particular* prediction. But I deny (a) that it “makes no difference to prediction,” and (b) that “it therefore (*sic!*) amounts to a change in our use of language.”

low, there are differences which it is absolutely vital to recognize. It is not that the statements I have mentioned fall into a third category. They fall into many different categories. Over and beyond the clear-cut rules of language, on the one side, and the clear-cut descriptive statements, on the other, are just an enormous number of statements which are not happily classified as either analytic or synthetic.

The case of stipulation is one in point. One must consider the role of the stipulation and whether the truth introduced by stipulation retains its conventional character or whether it later figures in inquiry on a par with other truths, without reference to the way in which it was introduced. We have to consider the question of the arbitrariness versus systematic import of our stipulations. There is one kind of wholly arbitrary stipulation which does indeed produce analytic statements, but we should not be led to infer that, therefore, every stipulation produces analytic statements. The Einstein stipulation that the constancy of the light velocity should be used to “define” simultaneity in a reference system does not, Reichenbach to the contrary, generate an analytic truth of the same order as ‘All bachelors are unmarried.’ And even the case of *knowing* and *having or having had evidence* requires much treatment and involves special difficulties. I shall in the body of this paper try to draw some of the distinctions that I think need to be drawn. For the moment let me only say this: if one wants to have a model of language, it is far better to proceed on the idea that statements fall into three kinds—analytic, synthetic, and lots-of-other-things—than to proceed on the idea that, except for borderline fuzziness, every statement is either analytic or synthetic.

Of course many philosophers are aware that there are statements which are not happily classified as either analytic or synthetic. My point is not that there exist exceptional examples, but that there is a far larger class of such statements than is usually supposed. For example, to ask whether or not the principles of logic are analytic is to ask a bad question. Virtually all the *laws* of natural science are statements with respect to which it is not *happy* to ask the question “Analytic or synthetic? It must be one or the other, mustn’t it?” And with respect to the framework principles that are often discussed by philosophers, the existence of the past or the implication that some time exists between knowing and having had evidence, it is especially a mistake to classify these statements as “rules of language” or “true because of the logic of the concepts involved” or “analytic” or “L-true” or . . . This is not to say that all these principles

have the same nature or that they form a compact new class, e.g., framework principles (as if one were to take seriously the label I have been using). ‘There is a past’ is recognizably closer to the law of conservation of energy than ‘If Jones knows that p, then he must have or have had evidence that p’ (in the cases where the latter inference seems a necessary one); and ‘If Jones knows that p, then he must have or have had evidence that p’ is more like ‘All bachelors are unmarried’ than is ‘There is a past.’ But neither statement is of exactly the same kind as the law of conservation of energy, although that law too is a statement with respect to which it is not happy to say, “Is it analytic or synthetic?” and neither statement is of exactly the same kind as ‘All bachelors are unmarried.’ What these statements reveal are different degrees of something like convention, and different kinds of systematic import. In the case of ‘All bachelors are unmarried,’ we have the highest degree of linguistic convention and the minimum degree of systematic import. In the case of the statement ‘There is a past,’ we have an overwhelming amount of systematic import—so much that we can barely conceive of a conceptual system which did not include the idea of a past. That is to say, such a conceptual system differs so greatly from our present conceptual system that the idea of ever making a transition from one to the other seems fantastic.⁵ In the case of knowing without ever having any reason to believe, still other considerations are involved. We have to ask what we would say if people appeared to be able to answer questions truthfully about a certain subject matter although they had never had any acquaintance with that subject matter as far as we could detect. *Knowing* is something that we do not have much of a theory about. It makes little difference at *present* whether we say that such people would be correctly described as “knowing” the answers to the various questions in the area in which they are able to act as an oracle, or whether we say that they have an “uncanny facility at guessing the correct answer”; although, in the light of a more advanced theory, it might very well make a good deal of difference what we say. The concept of the past, on the other hand, and the concept of time, are deeply integrated into our physical theory, and any tampering with

⁵ For example, we *could* accept the hypothesis that the world came into existence January 1, 1957, without changing the *meaning* of any word; but to do so would have a crippling effect on many sciences, and on much of ordinary life. (Think of the *ad hoc* hypotheses that would have to be invented to account for the “creation.” And consider the role played by data concerning the past in, say, astronomy—not to mention ordinary human relations!)

these concepts would involve a host of revisions if simple consistency is to be maintained. In the sequel I shall try to describe in somewhat more detail the diverse natures of the statements in that vast class with respect to which it is not happy to say “analytic or synthetic.” But on the whole my story will resemble Quine’s. That is to say, I believe that we have a conceptual system with centralities and priorities. I think the statements in that conceptual system—except for the *trivial* examples of analyticity, e.g., ‘All bachelors are unmarried,’ ‘All vixens are foxes’—fall on a continuum, a multidimensional continuum. More or less stipulation enters; more or less systematic import. But any one of these principles might be given up, farfetched though it may seem, and perhaps without altering the meaning of the constituent words. Of course, if we give up a principle that is analytic in the trivial sense (‘All bachelors are unmarried’), then we have clearly changed the meaning of a word. But the revision of a sufficient number of principles, no one of which is by itself analytic in quite the way in which ‘All bachelors are unmarried’ is analytic, may also add up to what we should describe as a change in the meaning of a word. With Quine, I should like to stress the monolithic character of our conceptual system, the idea of our conceptual system as a massive alliance of beliefs which face the tribunal of experience collectively and not independently, the idea that “when trouble strikes” revisions can, with a very few exceptions, come anywhere. I should like, with Quine, to stress the extent to which the meaning of an individual word is a function of its place in the network, and the impossibility of separating, in the actual use of a word, that part of the use which reflects the “meaning” of the word and that part of the use which reflects deeply embedded collateral information.

Linguistic conventionalism. One more point will terminate this rather interminable set of preliminary remarks. The focus of this paper is the analytic-synthetic distinction, not because I think that distinction is of itself of overwhelming importance. In fact, I think it is of overwhelming unimportance. But I believe that the issues raised by Quine go to the very center of philosophy. I think that appreciating the diverse natures of logical truths, of physically necessary truths in the natural sciences, and of what I have for the moment lumped together under the title of framework principles—that clarifying the nature of these diverse kinds of statements is the most important work that a philosopher can do. Not because philosophy is necessarily about language, but because we must become

clear about the roles played in our conceptual systems by these diverse kinds of truths before we can get an adequate global view of the world, of thought, of language, or of anything. In particular, I think we might begin to appreciate the real problems in the domain of formal science once we rid ourselves of the easy answer that formal truth is in some sense “linguistic in origin”; and in any case I think that one’s whole view of the world is deeply affected, if one is a philosopher, by one’s view of what it is to have a view about the world. Someone who identifies conceptualization with linguistic activity and who identifies linguistic activity with response to observable situations in accordance with rules of language which are themselves no more than implicit conventions or implicit stipulations (in the ordinary unphilosophic sense of ‘stipulation’ and ‘convention’) will, it seems to me, have a deeply distorted conception of human knowledge and, indirectly, of some or all objects of human knowledge. We must not fall into the error of supposing that to master the total use of an expression is to master a repertoire of individual uses, that the individual uses are the product of something like implicit stipulation or implicit convention, and that the conventions and stipulations are arbitrary. (The notion of a nonarbitrary *convention* is of course an absurdity—conventions are used precisely to settle questions that are arbitrary.) For someone who uses language in the way that I have just described, there are observable phenomena at the macrolevel and there are conventional responses to these, and this is all of knowledge; one can, of course, say that “there are atoms” and that “science is able to tell us a great deal about atoms,” but *this* turns out to be no more than making noises in response to macrostimuli *in accordance with arbitrary conventions*. I do not think that any philosopher explicitly maintains such a view of knowledge; and if he did it is clear that he would be a sort of mitigated phenomenalist. But I do think that a good many philosophers implicitly hold such a view, or fall into writing as if they held such a view, simply because they tend to think of use as a sum of individual uses and of linguistic use on the model suggested by the phrase ‘rules of language.’

To sum up: I do not agree with Quine, that there is no analytic-synthetic distinction to be drawn at all. But I do believe that his emphasis on the monolithic character of our conceptual system and his negative emphasis on the *silliness* of regarding mathematics as consisting in some sense of “rules of language,” represent exceedingly important theoretical insights in philosophy. I think that what we have to do now is to settle

the relatively trivial question concerning analytic statements properly so called ('All bachelors are unmarried'). We have to take a fresh look at the framework principles so much discussed by philosophers, disabusing ourselves of the idea that they are "rules of language" in any literal or lexicographic sense; and above all, we have to take a fresh look at the nature of logical and mathematical truths. With Quine's contribution, we have to face two choices: We can ignore it and go on talking about the "logic" of individual words. In that direction lies sterility and more, much more, of what we have already read. The other alternative is to face and explore the insight achieved by Quine, trying to reconcile the fact that Quine is overwhelmingly right in his critique of what other philosophers have done with the analytic-synthetic distinction with the fact that Quine is wrong in his literal thesis, namely, that the distinction itself does not exist at all. In the latter direction lies philosophic progress. For philosophic progress is nothing if it is not the discovery of new areas for dialectical exploration.

Analytic and Nonanalytic Statements

The "kinetic energy definition." As a step toward clarification of the analytic-synthetic distinction, I should like to contrast a paradigm case of analyticity—'All bachelors are unmarried'—with an example which superficially resembles it: the statement that kinetic energy is equal to one half the product of mass and velocity squared, ' $e = \frac{1}{2}mv^2$.' I think that if we can see the respect in which these two examples differ, we will have made important progress toward such a clarification.

Let us take the second statement first, ' $e = \frac{1}{2}mv^2$ '; this is the sort of statement that before relativistic physics one might well have called a "definition of 'kinetic energy.'" Yet, its history is unusual. Certainly, before Einstein, any physicist might have said, "' $e = \frac{1}{2}mv^2$;' that is just the definition of 'kinetic energy.'" There is no more to it than that. The expression 'kinetic energy' is, as it were, a sort of abbreviation for the longer expression 'one-half the mass times the velocity squared.'

If this were true, then the expression 'kinetic energy' would, of course, be in principle dispensable. One could simply use ' $\frac{1}{2}mv^2$ ' wherever one had used 'kinetic energy.'

In the early years of the twentieth century, however, Albert Einstein developed a theory, a physical theory—but of an unusual sort. It is unusual because it contains words of a rather high degree of vagueness, at

least in terms of what we usually suppose the laws of physics to be like. All this notwithstanding, the theory is, as we all well know, a precise and useful theory.

What I have in mind is Einstein's principle that all physical laws must be Lorentz-invariant. This is a rather vague principle, since it involves the general notion of a physical law. Yet in spite of its vagueness, or perhaps because of its vagueness, scientists have found it an extremely useful leading principle. Of course, Einstein contributed more than a leading principle. He actually proceeded to find Lorentz-invariant laws of nature; and the search for a Lorentz-invariant law of gravitation, in particular, produced the general theory of relativity.⁶

But it would be a mistake to think of the special theory of relativity as the sum of the special laws that Einstein produced. The general principle that all physical laws are Lorentz-invariant is certainly a legitimate part of the special theory of relativity, notwithstanding the fact that it is stated in what some purists might call "the metalanguage." And it is no good to say that 'a physical law' means 'any true physical statement': for so interpreted Einstein's principle would be empty. Any equation whatsoever can be made Lorentz-invariant by writing it in terms of suitable magnitudes. The principle that the laws of nature must be Lorentz-invariant is without content unless we suppose that the magnitudes—to be contained in laws of nature must be in some sense real magnitudes—e.g., electricity, gravitation, magnetism—and that the equations expressing the laws must have certain characteristics of simplicity and plausibility. In practice, Einstein's principle is quite precise, in the only sense relevant to physical inquiry, notwithstanding the fact that it contains a vague term. The point is that the vagueness of the term 'physical law' does not affect the applications which the physicist makes of the principle. In practice, the physicist has no difficulty in recognizing laws or putative laws: any "reasonable" equation proposed by a physicist in his right mind constitutes at least a putative law. Thus, the Einstein principle, although it might bother those logicians who are worried, and rightly worried, about the right distinction between a natural law and any true statement whatsoever, is one whose role in physical inquiry is clear-cut. It means simply that those equations considered by physicists as expressing possible laws of nature must, if they are to remain candidates for that role in the age of relativity,

⁶ Of course, the general theory of relativity itself replaces the requirement of Lorentz-invariance with the requirement of covariance.

be Lorentz-invariant. Of course, the principle does not play only the purely negative role of ruling out what might otherwise be admissible scientific theories: the fact that laws of nature must be Lorentz-invariant has often been a valuable clue to fundamental new discoveries. The Einstein gravitation theory has already been mentioned; another famous example is Dirac's "hole" theory, which led to the discovery of the positron.

Returning now to our account of the history of the "energy definition": the principle just described led Einstein to change a great many physical laws. Some of the older laws, of course, survived: the Maxwell equations, for instance, turned out to be Lorentz-invariant as they stood. Some of the principles that Einstein revised would ordinarily be regarded as being of an empirical nature. The statements 'Moving clocks slow down' and 'One cannot exceed the velocity of light' are certainly statements which we should regard as synthetic. The interesting thing is that Einstein was to revise, and in an *exactly similar fashion*, principles that had traditionally been regarded as definitional in character. In particular, Einstein, as we all know, changed the definition of 'kinetic energy.' That is to say, he replaced the law ' $e = \frac{1}{2}mv^2$ ' by a more complicated law. If we expand the Einstein definition of energy as a power series, the first two terms are ' $e = mc^2 + \frac{1}{2}mv^2 + \dots$ '. We might, of course, reply that classically speaking ' $\frac{1}{2}mv^2$ ' defines not 'energy' in general (e.g., 'potential energy') but only 'kinetic energy'; we might try to say that the energy that a body has because of its rest mass (this is represented by the term ' mc^2 ') should not be counted as part of its kinetic energy, as Einstein does. The point is that even the magnitude in the theory of relativity that corresponds to the classical kinetic energy of a particle, that is, its total kinetic energy minus the energy due to its rest mass, is not equal to $\frac{1}{2}mv^2$ except as a first approximation. If you take the total relativistic kinetic energy of a particle and subtract the energy due to its rest mass, you will obtain not only the leading term ' $\frac{1}{2}mv^2$ ' but also terms in ' mv^4 ,' etc.

It would clearly be a distortion of the situation to say that 'kinetic energy = $\frac{1}{2}mv^2$ ' was a definition, and that Einstein merely changed the definition. The paradigm that this account suggests is somewhat as follows: 'kinetic energy,' before Einstein, was *arbitrarily* used to stand for ' $\frac{1}{2}mv^2$.' After Einstein, 'kinetic energy' was *arbitrarily* used to stand for ' $m + \frac{1}{2}mv^2 + \frac{3}{8}mv^4 + \dots$ '.⁷ This account is, of course, incorrect.

⁷ This formula assumes that the unit of time is chosen so that the speed of light = 1.

What is striking is this: whatever the status of the "energy definition" may have been before Einstein, in revising it, Einstein treated it as just another natural law. There was a whole set of pre-existing physical and mechanical laws which had to be tested for compatibility with the new body of theory. Some stood the test unchanged—others only with some revision. Among the equations that had to be revised (and formal considerations indicated a rather natural way of making the revision, one which was, moreover, borne out richly by experiments) was the equation ' $e = \frac{1}{2}mv^2$ '.

The moral of all this is not difficult to find. The "energy definition" may have had a special status when it came into the body of accepted physical theory, although this is a question for the historian of science to answer. It may even, let us suppose, have originally been accepted on the basis of explicit stipulation to the effect that the phrase 'kinetic energy' was to be used in the sense of ' $\frac{1}{2}mv^2$.' Indeed, there was some discussion between Newton and Leibniz on the question whether the term 'energy' should be applied to what we now do call 'energy' or what we call 'momentum.' Suppose, however, that a congress of scientists had been convened in, say, 1780 and had settled this controversy by legislating that the term 'kinetic energy' was to be used for $\frac{1}{2}mv^2$ and not for mv . Would this have made the principle ' $e = \frac{1}{2}mv^2$ ' analytic? It would be true by stipulation, wouldn't it? It would be true by stipulation, yes, *but only in a context which is defined by the fact that the only alternative principle is ' $e = mv$ '.*

Quine has suggested that the distinction between truths by stipulation and truths by experiment is one which can be drawn only at the moving frontier of science. Conventionality is not "a lingering trait" of the statements introduced as truths by stipulation. The principle ' $e = \frac{1}{2}mv^2$ ' may have been introduced, at least in our fable, by stipulation; the Newtonian law of gravity may have been introduced on the basis of induction from the behavior of the known satellite systems and the solar system (as Newton claimed); but in subsequent developments these two famous formulas were to figure on a par. Both were used in innumerable physical experiments until they were challenged by Einstein, without ever being regarded as themselves subject to test in the particular experiment. If a physicist makes a calculation and gets an empirically wrong answer, he does not suspect that the mathematical principles used in the calculation may have been wrong (assuming that those principles are themselves

theorems of mathematics) nor does he suspect that the law 'f = ma' may be wrong. Similarly, he did not frequently suspect before Einstein that the law 'e = ½mv²' might be wrong or that the Newtonian gravitational law might be wrong (Newton himself did, however, suspect the latter). These statements, then, have a kind of preferred status. They can be overthrown, but not by an isolated experiment. They can be overthrown only if someone incorporates principles incompatible with those statements in a successful conceptual system.

Principles of geometry. An analogy may be drawn with the case of geometry. No experiments—no experiments with light rays or tape measures or with anything else—could have overthrown the laws of Euclidean geometry before someone had worked out non-Euclidean geometry. That is to say, it is inconceivable that a scientist living in the time of Hume might⁸ have come to the conclusion that the laws of Euclidean geometry are false: "I do not know what geometrical laws are true, but I know the laws of Euclidean geometry are false." Principles as central to the conceptual system of science as laws of geometry are simply not abandoned in the face of experiment *alone*. They are abandoned because a rival *theory* is available.

On the other hand, before the development of non-Euclidean geometry by Riemann and Lobachevski, the best philosophic minds regarded the principles of geometry as virtually analytic. The human mind could not conceive their falsity. Hume would certainly not have been impressed by the claim that 'straight line' means 'path of a light ray,' and that the meeting of two light rays mutually perpendicular to a third light ray could show, if it ever occurred, that Euclidean geometry is false. It would have been self-evident to Hume that such an experimental situation, if it ever occurred, would be correctly explained by supposing that the light rays traveled in a curved path in Euclidean space, and not by supposing that the light rays traveled in two straight lines which were indeed mutually perpendicular to a third straight line but which nevertheless met. Hume, had he employed the vocabulary of contemporary analytic philosophy, might even had said that this follows from the "logic" of the words 'straight line.' It is a "criterion," to use another popular word, for lines being straight that if two of them are perpendicular to a third the two do

⁸ This is not a historical remark. I mean that no scientist *ought* to have come to this conclusion at that time, no matter what experimental evidence might have been presented.

not meet. It may be another criterion that light travels in *approximately* straight lines; but only where this criterion does not conflict with the deeply seated meaning of the words 'straight line.' In short, the meaning of the words 'straight line' is such that light rays may sometimes be said not to travel in straight lines; but straight lines cannot be said to behave in such a way as to form a triangle the sum of whose angles is more than 180°. If he had used the jargon of another fashionable contemporary school of philosophy, Hume might have said that straight lines are "theoretical constructs." And that light ray paths constitute a "partial interpretation" of geometrical theory but one that is only admissible on condition that it does not render false any of the "meaning postulates" of the geometrical theory.

Of course Hume did not employ this jargon. But he employed what was for him an equivalent jargon: the jargon of conceiving, visualizing, mental imagery. One cannot form any image of straight lines that do not conform to the laws of Euclidean geometry. This, of course, was to be true because any image of lines not conforming to the axioms of Euclidean geometry is an image which is not *properly* called an image of *straight* lines at all. Hume did not put it that way, however. Rather he explained the alleged "impossibility of imagining" straight lines not conformant to the laws of Euclidean geometry in terms of a theory of relations between our ideas.

Was Hume wrong? Reichenbach⁹ suggested that 'straight line,' properly analyzed, means 'path of a light ray'; and with this "analysis" accepted, it is clear that the principles of geometry always are and always were synthetic. They are and always were subject to experiment. Hume simply overlooked something which could *in principle*¹⁰ have been seen

⁹ Reichenbach actually claimed that there were various possible alternative "coordinative definitions" of 'straight line.' However he contended that this one (and the ones physically equivalent to it) "have the advantage of logical simplicity and require the least change in the results of science." Moreover: "The sciences have implicitly employed such a coordinative definition all the time, though not always consciously"—i.e., it renders the customary meaning of the term 'straight line.' *Space and Time* (New York: Dover Publications, 1956), p. 19.

¹⁰ Reichenbach does not assert that the Greeks could (as a matter of psychological or historical possibility) have understood the "true" character of geometric statements prior to the invention of non-Euclidean geometry: in fact, he denies this. But there is nothing in Reichenbach's analysis in Ch. I of *Space and Time* which *logically* presupposes a knowledge of non-Euclidean geometry. Thus, if Reichenbach is right, then the Greeks could *in principle* have "realized" (a) that the question whether Euclidean geometry is correct for physical space presupposes the choice of a "coordinative defini-

even by the ancient Greeks. I think Reichenbach is almost totally wrong. If the paradigm for an analytic sentence is 'All bachelors are unmarried'—and it is—then it is of course absurd to say that the principles of geometry are analytic. Indeed, we cannot any longer say that the principles of Euclidean geometry are analytic; because analytic sentences are true, and we no longer say that the principles of Euclidean geometry are true. But I want to suggest that before the work of nineteenth-century mathematicians, the principles of Euclidean geometry were as close to analytic as any nonanalytic statement ever gets. That is to say, they had the following status: no experiment that one could describe could possibly overthrow them, by itself.¹¹ Just plain experimental results, without any new theory to integrate them, would not have been accepted as sufficient grounds for rejecting Euclidean geometry by any rational scientist.¹² After the development of non-Euclidean geometry, the position was rather different, as physicists soon realized: give us a rival conceptual system, and some reason for accepting it, and we will consider abandoning the laws of Euclidean geometry.

When I say that the laws of Euclidean geometry were, before the development of non-Euclidean geometry, as analytic as any nonanalytic statements ever get, I mean to group them, in this respect, with many other principles: the law ' $f = ma$ ' (force equals mass times acceleration), the principle that the world did not come into existence five minutes ago, the principle that one cannot know certain kinds of facts, e.g., facts about objects at a distance from one, unless one has or has had evidence. These principles play several different roles; but in one respect they are alike. They share the characteristic that no isolated experiment (I cannot think of a better phrase than 'isolated experiment' to contrast with 'rival theory') can overthrow them. On the other hand, most of these principles can be overthrown if there is good reason for overthrowing them, and such good reason would have to consist in the presentation of a whole rival theory embodying the denials of these principles, plus evidence of the success of such a rival theory. Any principle in our knowledge can be revised for

tion," and (b) that once the customary definition has been chosen, the question is an "empirical" one.

¹¹ As Mill very clearly states; see *System of Logic*, Ch. V, Secs. 4, 5, 6. As Mill foresaw, "There is probably no one proposition enunciated in this work for which a more unfavorable opinion is to be expected" (than, that is, his denial of the a priori character of geometrical propositions, notwithstanding the "inconceivability" of their negations).

¹² This is not a historical remark.

theoretical reasons; although many principles resist refutation by isolated experimentation. There are indeed some principles (some philosophers of science call them "low-level generalizations") which can be overthrown by isolated experiments, provided the experiments are repeated often enough and produce substantially the same results. But there are many, many principles—we might broadly classify them as "framework principles"—which have the characteristic of being so central that they are employed as auxiliaries to make predictions in an overwhelming number of experiments, without themselves being jeopardized by any possible experimental results. This is the classical role of the laws of logic; but it is equally the role of certain physical principles, e.g., ' $f = ma$,' and the principles we have been discussing: the laws of Euclidean geometry, and the law ' $e = \frac{1}{2}mv^2$,' at the time when those laws were still accepted.

I said that any principle in our knowledge can be revised for *theoretical* reasons. But this is not strictly correct. Any principle in our knowledge can be revised or abandoned for theoretical reasons unless it is *really* an analytic principle in the trivial sense in which 'All bachelors are unmarried' is an analytic principle. There are indeed analytic statements in science; and these are immune from revision, except the trivial kind of revision which arises from unintended and unexplained historical changes in the use of language. The point of the preceding discussion is that many principles which have been mistaken for analytic ones have actually a somewhat different role. There is all the difference in the world between a principle that can never be given up by a rational scientist and a principle which cannot be given up by rational scientists merely because of experiments, no matter how numerous or how consistent.

To summarize this discussion of geometry: I think that Hume was perfectly right in assigning to the principles of geometry the same status that he assigned to the principles of arithmetic. I think that in his time the principles of geometry *had* the same status as the principles of arithmetic. It is not that there is something—"an operational definition" of 'straight line'—which Hume failed to apprehend. The idea that, had he been aware of the "operational definition of straight line" on the one hand and of the "reduction of mathematics to logic" on the other hand, Hume would have seen that geometry is not really so much like arithmetic after all, that geometry is synthetic and arithmetic analytic, seems a crude error. The principle that light travels in straight lines is not a definition of 'straight line': as such, it is hopeless since it contains the geometrical term 'travels.'

The same objection arises if we say a "straight line is defined as the path of a light ray." In this case the definition of 'straight line' uses the topological term 'path.' The principle that light travels in a straight line is simply a law of optics, nothing more or less serious than that. What is often called "interpreting mathematical geometry" is more aptly described as testing the conjunction of geometric theory and optical theory. The implicit standpoint of Hume was that if the conjunction should lead to false predictions, then the optical theories would have to be revised; the geometric theory was analytic. The Reichenbachian criticism is that the geometry was synthetic and the optical theory was analytic. Both were wrong. We test the conjunction of geometry and optics indeed, and if we get into trouble, then we can alter either the geometry or the optics, depending on the nature of the trouble. Before Einstein, geometrical principles had exactly the same status as analytic principles, or rather, they had exactly the same status as all the principles that philosophers mistakenly cite as analytic. After Einstein, especially after the general theory of relativity, they have exactly the same status as cosmological laws: this is because general relativity establishes a complex interdependence between the cosmology and the geometry of our universe.

Thus, we should not say that 'straight line' has changed its meaning: that Hume was talking about one thing and that Einstein was talking about a different thing when the term 'straight line' was employed. Rather, we should say that Hume (and Euclid) had certain beliefs about straight lines—not just about mental images of straight lines, but about straight lines in the space in which we live and move and have our being—which were, in fact, unknown to them, false. But we can say all this, and also say that the principles of geometry had, at the time Hume was writing, the same status as the laws of mathematics.

Law-cluster concepts. At this point, a case has been developed for the view that statements expressing the laws of mathematics and geometry and our earlier example ' $e = \frac{1}{2}mv^2$ ' are not analytic, if by 'an analytic statement' one means a statement that a rational scientist can never give up. It remains to show that 'All bachelors are unmarried' is an analytic statement in that sense. This is not a trivial undertaking: for the "shocking" part of Quine's thesis is that there are no analytic statements in this sense—that all of the statements in our conceptual system have the character that I have attributed variously to the laws of logic, the laws of the older geometry at the time when they were accepted, and certain physi-

cal principles. But before considering this question, there are certain possible objections against the account just given which must be faced. The objections I have in mind are two. (1) It may be argued, especially in connection with logical principles, that revision of these principles merely amounts to a change in the meaning of the constituent words. Thus, logical principles are not *really* given up; one merely changes one's language. (2) It may be held that the case of the principle ' $e = \frac{1}{2}mv^2$ ' merely shows that we were able to "change our definition of 'kinetic energy,'" and *not* that a principle which was at one time definitional or stipulative could be later abandoned for reasons not substantially different from the reasons given for abandoning certain principles which philosophers would classify as synthetic.

The first objection I have discussed elsewhere.¹³ The main point to be made is this: the logical words 'or,' 'and,' 'not' have a certain core meaning which is easily specifiable and which is *independent* of the principle of the excluded middle. Thus, in a certain sense the meaning does *not* change if we go over to three-valued logic or to intuitionist logic. Of course, if by saying that a change in the accepted logical principles is tantamount to a change in the meaning of the logical connective, what one has in mind is the fact that changing the accepted logical principles will affect the global use of the logical connectives, then the thesis is tautological and hardly arguable. But if the claim is that a change in the accepted logical principles would amount *merely* to redefining the logical connectives, then, in the case of intuitionist logic, this is demonstrably false. What is involved is the acceptance of a whole new network of inferences with profound systematic consequences; and it is a philosophical sin to say, even indirectly by one's choice of terminology, that this amounts to no more than stipulating new definitions for the logical connectives. A change in terminology never makes it impossible to draw inferences that could be validly drawn before; or, if it does, it is only because certain words are missing, which can easily be supplied. But the adoption of intuitionist logic as opposed to "classical" logic amounts to systematically forswearing certain classically valid inferences. Some of these inferences can be brought in again by redefinition. But others, inferences involving certain kinds of nonconstructive mathematical entities, are really forsworn in any form. To assimilate the change from one system of logic to another to the change that would be made if we were to use

¹³ "Three-Valued Logic," *Philosophical Studies*, 8:73-80 (1957).

the noise 'bachelor' to stand for 'unmarried woman' instead of 'unmarried man' is assimilating a mountain to a molehill. There is a use of the term 'meaning' according to which any change in important beliefs may be said to change the "meaning" of some of the constituent concepts. Only in this fuzzy sense may it be said that to change our accepted logical principles would be to change the "meaning" of the logical connectives. And the claim that to change our logical system would be merely to change the meaning of the logical connectives is just false. With respect to the second objection, there are some similar remarks to be made. Once again, to speak of Einstein's contribution as a "redefinition" of 'kinetic energy' is to assimilate what actually happened to a wholly false model.

Leibniz worried about the fact that statements containing a proper name as subject term seem never to be analytic. This seemed to be absurd, so he concluded that *all* such statements must be analytic—that is, that they must all follow from the nature of what they speak about. Mill took the different tack of denying that proper names connote; but this leaves it puzzling that they mean anything at all. Similarly, philosophers have wondered whether any statement containing the subject term 'man' is really analytic. Is it analytic that all men are rational? (We are no longer so happy with the Aristotelian idea that a necessary truth can have exceptions.) Is it analytic that all men are featherless? Aristotle thought not, thus displaying a commendable willingness to include our feathered friends, the Martians (if they exist), under the name 'man.' Suppose one makes a list of the attributes P_1, P_2, \dots that go to make up a normal man. One can raise successively the questions "Could there be a man without P_1 ?" "Could there be a man without P_2 ?" and so on. The answer in each case might be "Yes," and yet it seems absurd that the word 'man' has no meaning at all. In order to resolve this sort of difficulty, philosophers have introduced the idea of what may be called a *cluster concept*. (Wittgenstein uses instead of the metaphor of a "cluster," the metaphor of a rope with a great many strands, no one of which runs the length of the rope.) That is, we say that the meaning in such a case is given by a cluster of properties. To abandon a large number of these properties, or what is tantamount to the same thing, to radically change the extension of the term 'man,' would be felt as an arbitrary change in its meaning. On the other hand, if most of the properties in the cluster are present in any single case, then under suitable circumstances we should be inclined to say that what we had to deal with was a man.

In analogy with the notion of a cluster concept, I should like to introduce the notion of a *law-cluster concept*. Law-cluster concepts are constituted not by a bundle of properties as are the typical general names like 'man' and 'crow,' but by a cluster of laws which, as it were, determine the identity of the concept. The concept 'energy' is an excellent example of a law-cluster concept. It enters into a great many laws. It plays a great many roles, and these laws and inference roles constitute its meaning collectively, not individually. I want to suggest that most of the terms in highly developed science are law-cluster concepts, and that one should always be suspicious of the claim that a principle whose subject term is a law-cluster concept is analytic. The reason that it is difficult to have an analytic relationship between law-cluster concepts is that such a relationship would be one more law. But, in general, any one law can be abandoned without destroying the identity of the law-cluster concept involved, just as a man can be irrational from birth, or can have a growth of feathers all over his body, without ceasing to be a man.

Applying this to our example—'kinetic energy' = 'kinetic' + 'energy'—the kinetic energy of a particle is literally the energy due to its motion. The extension of the term 'kinetic energy' has not changed. If it had, the extension of the term 'energy' would have to have changed.¹⁴ But the extension of the term 'energy' has not changed. The forms of energy and their behavior are the same as they always were, and they are what physicists talked about before and after Einstein. On the other hand, I want to suggest that the term 'energy' is not one of which it is *happy* to ask, What is its intension? The term 'intension' suggests the idea of a single defining character or a single defining law, and this is not the model on which concepts like energy are to be construed. In the case of a law-cluster term such as 'energy,' any one law, even a law that was felt to be definitional or stipulative in character, can be abandoned, and we feel that the identity of the concept has, in a certain respect, remained.¹⁵ Thus, the conclusions of the present section still stand: A principle in-

¹⁴ Kinetic energy is only one of several kinds of energy, and can be transformed into other kinds (and vice versa). Thus an adequate physical theory cannot change the meaning of the term "kinetic energy" without changing the meaning of the term "energy," without giving up the idea that "kinetic energy" is literally a kind of energy.

¹⁵ Even the conservation law has sometimes been considered to be in doubt (in the development of quantum mechanics)! Yet it was the desire to preserve this law which led to the changes we have been discussing. In one context the law of the conservation of energy can thus serve to "identify" energy, whereas in another it can be the Hamiltonian equations of particular systems that do this.

volving the term 'energy,' a principle which was regarded as definitional, or as analytic, if you please, has been abandoned. And its abandonment cannot be explained away as mere "redefinition" or as change in the meaning of 'kinetic energy,' although one might say that the change in the status of the principle has brought about a change in the meaning of the term 'kinetic energy' in one rather fuzzy sense of 'meaning.'¹⁶ It is important to see that the principle ' $e = \frac{1}{2}mv^2$ ' might have been mistaken to have exactly the same nature as 'All bachelors are unmarried.' But 'All bachelors are unmarried' cannot be rejected unless we change the meaning of the word 'bachelor' and not even then unless we change it so radically as to change the extension of the term 'bachelor.' In the case of the terms 'energy' and 'kinetic energy,' we want to say, or at any rate I want to say, that the meaning has not changed enough to affect "what we are talking about"; yet a principle superficially very much like 'All bachelors are unmarried' has been abandoned. What makes the resemblance only superficial is the fact that if we are asked what the meaning of the term 'bachelor' is, we can *only* say that 'bachelor' means 'unmarried man,' whereas if we are asked for the meaning of the term 'energy,' we can do much more than give a definition. We can in fact show the way in which the use of the term 'energy' facilitates an enormous number of scientific explanations, and how it enters into an enormous bundle of laws.

The statement ' $e = \frac{1}{2}mv^2$ ' is the sort of statement in physical theory that is currently called a "definition." That is to say, it can be taken as a definition, and many good authors did take it as a definition. Analyticity is often defined as "truth by definition," yet we have just seen that ' $e = \frac{1}{2}mv^2$ ' is not and was not analytic, if by an analytic statement one means a statement that no one can reject without forfeiting his claim to reasonableness.

At this point one may feel tempted to agree with Quine. If even "definitions" turn out to be revisable in principle—and not in the trivial sense that arbitrary revision of our use of noises is always possible—then one might feel inclined to say that there is no statement which a rational man must hold immune from revision. I shall proceed to argue that this is wrong, but those who agree with me that this is wrong have often overlooked the fact that Quine can be wrong in his most "shocking" thesis

¹⁶ The "fuzziness" is evidenced by the fact that although one can say that 'kinetic energy' has a new meaning, one cannot say that 'kinetic' has a new meaning, or that 'energy' has a new meaning, or that 'kinetic energy' is an idiom.

and still right about very important and very pervasive epistemological issues. To give a single example, I agree with Quine that in that context of argument which is defined by questions of necessity, factuality, of linguistic or nonlinguistic character, there is no significant distinction to be drawn between, say, the principle of the excluded middle and the principle that $f = ma$; and this is not to say that the law ' $f = ma$ ' is analytic. (Of course we can imagine a physics based on $f = m^2a$, if we retain the identity of gravitational and inertial mass!) Nor is it to say that the laws of logic are "synthetic," if the paradigm for a synthetic sentence is "There is a book on this table." But still there are truths that it could never be rational to give up, and 'All bachelors are unmarried' is one of them. This thesis will be elaborated in the following section.

The Rationale of the Analytic-Synthetic Distinction

The problem of justification. Let us consider first the question How could one draw the analytic-synthetic distinction as a formal distinction in connection with at least some hypothetical formalized languages? If the inventor of a formalized language singles out from all his postulates and rules a certain subset (e.g., 'L-Postulates,' "Meaning Postulates," and "logical axioms") and says that the designated statements, statements in the subset, are not to be given up, then these statements may be reasonably called "analytic" in that language. In the context of formal reconstruction, then, this is the first model of analyticity that comes to mind. We draw an analytic-synthetic distinction formally only in connection with formalized languages whose inventors list some statements and rules as "Meaning Postulates." That is, it is stipulated that to qualify as correctly using the language one must accept *those* statements and rules. There is nothing mysterious about this. A formal language has, after all, an inventor, and like any human being, he can give commands. Among the commands he can issue are ones to the effect that "If you want to speak my language, then do thus and so." If his commands have an escape clause, if he says, "Accept these statements unless you get into trouble, and then make such-and such revisions," then his language is hardly one with respect to which we can draw a formal analytic-synthetic distinction. But if he says that certain statements are "to be accepted no matter what," then those statements in that language are true by stipulation, true by *his* stipulation, and that is all we mean when we say that they are "analytic" (in this model).

Hempel has proposed an answer to this sort of move. His answer is this: if by an analytic statement one means one which is not to be given up, then in science there are no such statements. Of course, an individual might invent a language and rule that in that language certain statements are not to be given up; but this is of no philosophic interest whatsoever, unless the language constructed by this individual can plausibly be regarded as reconstructing some feature which actually exists in ordinary unreconstructed scientific activity.

This brings us to our second question: If an artificial language in which a formalized analytic-synthetic distinction can be drawn is one in which there are rules of the form "Do not give up S under any circumstances," then what justification could there be for adopting such a language?

Certain philosophers have seen that the notion of a rule, in the sense of an *explicit* rule or explicit stipulation, is sufficiently clear to be worked with (Quine does not at all deny this), and they propose to define analytic statements as statements which are true by stipulation. Against this, there is Quine's remark that in the history of science a statement is often "true by stipulation" at one moment, but later plays a role which is in no way different from the role played by statements which enter the body of accepted truths through more direct experimental inquiry. Stipulation, Quine says, is a trait of historical events, not a "lingering trait" of the statements involved.

Philosophers who regard "true by stipulation" as explicating analyticity, and who take "true by stipulation" in its literal sense, that is to say, who mean by "stipulations" explicit stipulations, miss several points. In the first place, analytic statements in a natural language are not usually true by stipulation in anything but a metaphorical sense. "True by stipulation" is the nature of analytic statements only in the model. And even if we confine ourselves to the model and ignore the existence of natural languages, there is still the question What is the point of the model? But this is the question: Why should we hold certain truths immune from revision?

Suppose we can show that if we were to adopt an "official formalized language," it would be perfectly rational to incorporate into its construction certain conventions of the type described? Then I think we would have resolved the problem raised by Quine. Quine does not deny that some people may in fact hold some statements immune from revision; what he denies is that science does this, and his denial is not merely a

descriptive denial: he doesn't think that science ought to do this. Thus the problem *really* raised by Quine is this: Once we have managed to make our own Quine's insight into the monolithic character of our conceptual system, how can we see why there should be any exceptions to this monolithic character? If science is characterized by interdependence of its principles and by the fact that "revision may strike anywhere," then why should any principles be held immune from revision? The question at the moment is not What is the nature of the analytic-synthetic distinction? but rather Why ought there to be an analytic-synthetic distinction?

Rationale. The reply that I have to offer to the question of the rationale of the analytic-synthetic distinction, and of strict synonymy within a language, is this: First of all, the answer to the question Why should we have analytic statements (or strict synonymies¹⁷) in our language? is, in essence, Why not? or more precisely, It can't hurt. And, second, the answer to the derivative question How do you know it can't hurt? is I use what I know. But it is obvious that both of these answers will need a little elaborating.

The first answer should, I think, be clear. There are obvious advantages to having strict synonyms in a language. Most important, there is the advantage of *brevity*. Also, there is the question of *intelligibility*. If some of the statements in a language are immune from revision and if some of the rules of a language are immune from revision, then linguistic usage with respect to the language as a whole is to a certain extent frozen. Now, whatever disadvantages this freezing may have, there is one respect in which a frozen language is very attractive. Different speakers of the same language can to a large extent understand each other better because they can predict in advance at least some of the uses of the other speaker.

Thus, I think we can see that if we are constructing a language, then there are some prima-facie advantages to having "fixed points" in that language. Hence the only real question is Why *not* have them? Quine, I believe, thinks that there is a reason why we should not have them. No matter what advantages in intelligibility and uniformity of usage might accrue, Quine is convinced that it would block the scientific enterprise to declare *any* statement immune from revision. And it may seem that I have provided Quine with more than sufficient ammunition. For in-

¹⁷The close connection between synonymy and analyticity is pointed out by Quine in "Two Dogmas."

stance, someone might have proposed, "Let's make the statement 'kinetic energy = $\frac{1}{2}mv^2$ ' analytic. It will help to stabilize scientific usage." And accepting this proposal, which might have seemed innocuous enough, would not have been very happy. On my own account, we would have been mistaken had we decided to hold the statement 'kinetic energy = $\frac{1}{2}mv^2$ ' immune from revision. How can we be sure that we will not be similarly mistaken if we decide to hold any statement immune from revision?

In terms of the conceptual machinery developed above, the reason that we can safely decide to hold 'All bachelors are unmarried' immune from revision, while we could not have safely decided to hold 'kinetic energy = $\frac{1}{2}mv^2$ ' immune from revision, is that 'energy' is a law-cluster term, and 'bachelor' is not. This is not to say that there are no laws underlying our use of the term 'bachelor'; there are laws underlying our use of any words whatsoever. But it is to say that there are no exceptionless laws of the form 'All bachelors are . . .' except 'All bachelors are unmarried,' 'All bachelors are male,' and consequences thereof. Thus, preserving the interchangeability of 'bachelor' and 'unmarried man' in all extensional contexts can never conflict with our desire to retain some other natural law of the form 'all bachelors are . . .'

This cannot happen because bachelors are a kind of synthetic "class." They are not a "natural kind" in Mill's sense. They are rather grouped together by ignoring all aspects except a single legal one. One is simply not going to find any laws, except complex statistical laws depending on sociological conditions, about such a class. Thus, it cannot "hurt" if we decide always to preserve the law 'All bachelors are unmarried.' And that it cannot hurt is all the justification we need; the positive advantages are obvious.

As remarked, there may be statistical laws, dependent on sociological conditions, concerning bachelors. But these cannot be incompatible with 'All bachelors are unmarried men.' For the truth of a statistical law, unlike that of a deterministic law, is not affected by slight modifications in the extension of a concept. The law '99 per cent of all A's are B's,' if true, remains true if we change the extension of the concept A by including a few more objects or excluding a few objects. Thus, making slight changes in the extension of the term 'bachelor' would not affect any statistical law about bachelors; but by exactly the same token, neither would refusing to make such changes. And if the statistical law held true only provided

we were willing to make a large change in the extension or putative extension of the term 'bachelor,' then we would certainly reject the statistical law.

Let us consider one objection. I have maintained that there are no exceptionless laws containing the term 'bachelor.' But this statement is surely a guess on my part. Let us suppose that my "guess" is wrong, and that there are exceptionless laws about bachelors. Let us suppose for instance that all bachelors share a special kind of neurosis universal among bachelors and unique to bachelors. Not to be too farfetched, let us call it "sexual frustration." Then the statement 'All bachelors suffer from sexual frustration, and only bachelors suffer from sexual frustration' would express a genuine law. This law could still not provide us with a criterion for distinguishing bachelors from nonbachelors, unless we were good at detecting this particular species of neurosis. It is alleged that some primitive peoples can in fact do this by smell; but let us make a somewhat more plausible assumption, in terms of contemporary mores. Let us suppose that we all mastered some form of super psychoanalysis; and let us suppose that we all became so "insightful" that we should be able to tell in a moment's conversation whether someone suffered from the neurosis of "sexual frustration" or not. Then this law would indeed constitute a criterion for bachelorhood, and a far more convenient criterion than the usual one. For one cannot employ the usual criterion without asking a man a somewhat personal question concerning his legal status; whereas, in our hypothetical situation, one would be able to determine by a quick examination of the man's conversation whether he was a bachelor or not, no matter what one conversed about. Under such circumstances, possession of the neurosis might well become the dominant criterion governing the use of the word. Then what should we say, if it turned out that a few people had the neurosis without being bachelors? Our previous stipulation that 'bachelor' is to be synonymous with 'unmarried man' might well appear inconvenient!

The point of this fable is as follows: Even if we grant that 'bachelor' is not now a law-cluster term, how can we be sure that it will never become such a term? This leads to my second answer, and to a further remark, "I use what I know." It is logically possible that all bachelors should have a certain neurosis and that nobody else should have it; it is even possible that we should be able to detect this neurosis at sight. But, of course, there is no such neurosis. This I know in the way that I know most nega-

tive propositions. It is not that I have a criterion for as yet undiscovered neuroses, but simply that I have no good reason to suppose that there might be such a neurosis. And in many cases of this kind, lack of any good reason for supposing existence is itself the very best reason for supposing nonexistence.

In short, I regard my "guess" that there are no exceptionless laws about bachelors as more than a guess. I think that in a reasonable sense we may say that this is something that we *know*. I shall not press this point. But *bachelor* is not now a law-cluster concept; I think we can say that, although it is *logically* possible that it might become a law-cluster concept, in fact it will not.

Let us summarize the position at this point: I have suggested that the statement 'All bachelors are unmarried' is a statement which we might render true by stipulation, in a hypothetical formalized language. I have argued that this stipulation is convenient, both because it provides us with one more "fixed point" to help stabilize the use of our hypothetical language, and because it provides us with an expression which can be used instead of the somewhat cumbersome expression 'male adult human being who has never in his life been married'; and I have argued that we need not be afraid to accept these advantages, and to make these stipulations, because it can do no harm. It can do no harm because *bachelor* is not a law-cluster concept. Also it is not independently "defined" by standard examples, which might only contingently be unmarried men. I have admitted that my knowledge (or "state of pretty-sureness") that 'bachelor' will not become a law-cluster term is based upon what we might call, in a very broad sense, empirical argumentation. That *there are no exceptionless laws containing the term 'bachelor'* is empirical in the sense of being a fact about the world; although it is not empirical in the sense of being subject to confrontation with isolated experiments. More precisely: it occupies the anomalous position of being falsifiable by isolated experiments (since isolated experiments could verify an empirical generalization which would constitute a "law about all bachelors"); but it could not be verified by isolated experiments. One cannot examine a random sample of laws, and verify that they are all not-about-bachelors. But the statement is empirical, at least in the first sense, and it is "synthetic" to the extent that it is revisable in principle. So my position is this: a "synthetic" statement, a statement which could be revised in principle, may serve as a warrant for the decision that another statement should not be revised,

no matter what. One may safely hold certain statements immune from revision; but *this* statement is itself subject to certain risks.

But there is no real paradox here at all. To say that an intention is to do something permanently is not the same as saying that the intention is permanent. To marry a woman is to legally declare an intention to remain wedded to her for life; although the bride and groom know perfectly well that there exists such an institution as divorce, and that they may avail themselves of it. The existence of divorce does not change the fact that the legal and declared intention of the persons getting married is to be wedded for life. And this is the further remark that I wish to make in connection with my second answer. It is perfectly rational to make stipulations to the effect that certain statements are never to be given up, and those stipulations remain stipulations to that effect, notwithstanding the fact that under certain circumstances the stipulations *themselves* might be given up.

All of this may sound like a bit of sophistry, if one forgets that we are still in the context of formalized languages. Thus, if one has in mind "implicit stipulations" and natural language, one might feel tempted to say: "What is the difference between having a stipulation to the effect that every statement can be revised, and having a stipulation to the effect that certain statements are never to be revised, if the latter stipulations are themselves always subject to revision?" But in connection with formalized languages, there is all the difference in the world. The rule "Let every statement be subject to revision" is not sufficiently precise to be a formal rule. It would have to be supplemented by further rules determining what revisions to make, and in what order. And there is all the difference in the world between making a decision in accordance with a pre-established plan, and making the decision by "getting together" and doing whatever seems most cogent in the light of the circumstances at the moment and the standards or codes we see accepted at the moment. The first case would arise in connection with a language in which Quine's ideas concerning priorities and centralities had been formalized—a language in which any statement may be given up and in which there are rules telling one which statements to give up first and under what circumstances. Such a language could in principle be constructed. But compare the case of a scientist who is in difficulties, and who resolves his difficulties by using a predetermined rule, with the following case: we imagine that we have a formalized language in which 'All bachelors are unmarried' is a "meaning

postulate.” We further imagine, as in our “fable,” that all bachelors suffer from a neurosis and that only bachelors suffer from that particular neurosis. Also we suppose that the neurosis is detectable at sight and that it is used as the dominant criterion. Then it is discovered that one person or a very few people have the neurosis although they are married. The question might then arise as to which would be more convenient: to preserve ‘All bachelors are unmarried’ or to get together and modify the rules of the language. Contrast the procedure which would be employed if the latter alternative were the one adopted, with the procedure of settling the question in accordance with a predetermined plan. There would be, let us say, a convention at which some would argue that it is better to preserve the rules that were agreed upon for the language, and to give up the psychological law that had been thought to hold without exception; there might be others who would argue that the new use of the term ‘bachelor’ was so standard that it would be simpler to grace the new use with the hallmark of legality and to change the rules of the language. In short, the question would be settled by informal argument.

Thus, at the level of formalized languages, there is a difference, and a rather radical difference between these different systems: a formal language which can be described as having rules to the effect that every statement may be revised, and a formal language having rules to the effect that certain statements are never to be revised—notwithstanding the fact that, even if one employs a formal language of the second kind, one retains the option of later altering or abandoning it. And even if one uses a system of the first kind, a “holistic” system of the sort Quine seems to envisage, there is still the possibility that one might find it desirable to revise the rules determining the nature and order of revisions, when they are to be made—the centralities and priorities of this system. And the same difference mirrors itself in the difference between those questions which one settles in accordance with the antecedently established rules and those questions which one settles by informal argument when they arise.

In short: if we think in terms of people using formalized languages, then we have to distinguish between the things that are done inside the language in accordance with whatever rules and regulations may have been previously decided upon and published, and the informal argumentation and discussion that takes place outside of the language, and which perhaps leads to a decision, in its turn to be duly formalized, to alter the

language. This distinction is not the same as the analytic-synthetic distinction, but it is deeply relevant to it. If we use the model of people employing formalized languages, then we have to imagine those people as deciding upon and declaring certain rules. And it is perfectly rational in human life to make a rule that something is always to be done; and the rule is no less a rule that something is always to be done on account of the fact that the rule itself may someday be abandoned.

There are a host of examples: for instance, it is a rule of etiquette that one is not to address a person to whom one has never been introduced by his first name (with a few exceptions). The rule may someday be changed. But that does not change the fact that the present rule is to the effect that this is to be done under *all* circumstances. In the same way, a rational man may perfectly well adopt a rule that certain statements are never to be given up: he does not forfeit his right to be called reasonable on account of what he does, and he can give plenty of good reasons in support of his action.

The Analytic-Synthetic Distinction in Natural Language

The formal language model. The foregoing discussion is characterized by an air of fictionality. But this does not obliterate its relevance to Quine’s difficulties. Quine does not deny that there may be some statements which some individuals will never give up. His real contention is that there are no statements which science holds immune from revision. And this is not a descriptive judgment; judgments by philosophers containing the word ‘science’ almost never are. What Quine really means is that he cannot see why science ought to hold any statements immune from revision. And this is the sort of difficulty that one may well resolve by telling an appropriate fable.

Still we are left with the problem of drawing an analytic-synthetic distinction in natural language; and this is a difficult problem. Part of the answer is clear. We commonly use formalized objects to serve as models for unformalized objects. We talk about a game whose rules have never been written down in terms of a model of a game whose rules have been agreed upon and codified, and we talk about natural languages in terms of models of formal languages; and, if a formal language means a “language whose rules are written down,” then we have been doing this for a long time, and not just since the invention of symbolic logic. The concept of a rule of language is commonly used by linguists in describing even

the unwritten languages of primitive peoples, just as the concept of a rule of social behavior is used by anthropologists. Such reference is sometimes heavily disguised by current jargon, but is nevertheless present. For instance, if a linguist says: "The pluralizing morphophoneme —s has the zero allophone after the morpheme *sheep*," what he is saying is that it is a rule of English that the plural of 'sheep' is 'sheep' and not 'sheeps.' And his way of saying this is not so cumbersome either: he would not really write the sentence I just quoted, but would embed the information it contains in an extremely compact morphophoneme table.

Thus I think that we may say that the concept *rule of language*, as applied to natural language, is an "almost full-grown" theoretical concept. Linguists, sent out to describe a jungle language, describe the language on the model of a formal language. The elements of the *model* are the expressions and rules of a formal language, that is, a language whose rules are explicitly written down. The corresponding elements in the real world are the expressions of a natural language and certain of the dispositions of the users of that language. The model is not only a useful descriptive device, but has genuinely explanatory power. The distinction, at present very loosely specified, between a rule of language and a *mere habit* of the speakers of the language is an essential one. Speakers of English (except very small speakers of English) rarely use the word 'sheeps.' Speakers of English rarely use the word 'otiose.' But someone who uses the word 'sheeps' is said to be speaking incorrectly; whereas someone who uses the word 'otiose' is only using a rare word. That we behave differently in the two cases is explained, and it is a genuine explanation, by saying that it is a rule of English that one is to use 'sheep' as the plural of 'sheep,' and it is not a rule of English that one is not to use the word 'otiose'; it is just that most people do not know *what* the rule for using the word 'otiose' is at all, and hence do not employ it.

But all this will not suffice. True, we have a model of natural language according to which a natural language has "rules," and a model with some explanatory and predictive value, but what we badly need to know are the respects in which the model is exact, and the respects in which the model is misleading. For example, in many circumstances it is extremely convenient to talk about electron currents on the model of water flowing through a pipe; but physical scientists know very well in which respects this model holds exactly and in which respects it is extremely misleading. The same can hardly be said in the case just described—the case wherein

we employ a formal language as a model for a natural language. The difficulty I have in mind is not the difficulty of determining what the rules of a natural language are. The art of describing a natural language in terms of this kind of model is one that is relatively well developed; and linguists are aware that the correspondence between this kind of model and a given natural language is not unique: there are alternative "equally valid descriptions." The dispositions of speakers of a natural language are *not* rules of a formal language, the latter are only used to represent them in a certain technique of representation; and the difficulty lies in being sure that other elements of the model, e.g., the sharp analytic-synthetic distinction, correspond to anything at all in reality.

To give only one example: I argued above, and it was a central part of the argument, that there is a clear-cut difference between solving a problem by relying on a pre-established rule, and solving it by methods construed on the spot. But one might wonder whether the distinction is so sharp if the pre-established rule is only an *implicit* rule to begin with. It is clear that there is a difference between stipulations allowing for revisions and stipulations prohibiting revisions, but themselves always subject to informal revision. But is it so clear that there is such a distinction if the stipulations are themselves informal and "implicit"? In view of this difficulty, and other related difficulties, it seems to me that we must look at natural language directly, and try to draw the analytic-synthetic distinction without relying on the formal language model, if we are to be sure that it exists at all.

The nature of the distinction in natural language. The statements which satisfy the criteria presented below are a *fundamental subset* of the totality of analytic statements in the natural language. They are the so-called "analytic definitions," e.g., 'Someone is a bachelor if and only if he is an unmarried man.' Other statements may be classified as "analytic," although they do *not* satisfy the criteria, because they are consequences of statements which *do* satisfy the criteria. The older philosophers recognized a related though different distinction by referring to "intuitive" and "demonstrative" truths. The distinction had a point: there is a difference, even in our formal model, between those statements whose truth follows from *direct stipulation* and statements whose truth follows from the fact that they are *consequences* of statements true by direct stipulation. The latter statements involve not only arbitrary stipulation but also logic.

Nevertheless, the term 'intuitive' has bad connotations. And because of these bad connotations, philosophers have been led not to reformulate the distinction between intuitive and demonstrative truths but to abandon it. So today the fashion is to lump together the analytic statements which would traditionally have been classified as intuitive with all their consequences, and to use the word 'analytic' for the whole class. *The criteria to be presented do not, however, apply equally well to the whole class, or even to all the "intuitive" analytic truths, but to a fundamental subset. This fundamental subset is, roughly speaking, the set of analytic definitions; or less roughly, it is the set of analytic definitions which are also "intuitive" and not "demonstrative."*

In short, I shall present criteria which are intended to show what is unique or different about certain analytic statements. Such criteria do not constitute a definition but one might obtain a definition, of a rough and ready sort, from them: an analytic statement is a statement which satisfies the criteria to be presented, or a consequence of such statements, or a statement which comes pretty close to satisfying the criteria, or a consequence of such statements. The last clause in this "definition" is designed to allow for the fact that there are some "borderline" cases of analyticity, e.g., 'Red is a color.' However, it is not a very important point that the analytic-synthetic distinction is afflicted with "borderline fuzziness." The trouble with the analytic-synthetic distinction construed as a dichotomy is far more radical than mere "borderline fuzziness." Yet, there are borderline cases; and the reason for their existence is that the analytic-synthetic distinction is tied to a certain model of natural language and correspondence between the model and the natural language is not unique. To say that it is not unique is not, however, to say that it is arbitrary. Some statements in natural language really are analytic; others may be construed as analytic; still others really are synthetic; others may be construed as synthetic; still other statements belong to still other categories or may be construed as belonging to still other categories.

The following are the criteria in question:

- (1) The statement has the form: "Something (Someone) is an A if and only if it (he, she) is a B," where A is a single word.¹⁸

¹⁸ The requirement that A be a single word reflects the principle that the meaning of a whole utterance is a function of the meanings of the individual words and grammatical forms that make it up. This requirement should actually be more complicated

- (2) The statement holds without exception, and provides us with a criterion for something's being the sort of thing to which the term A applies.
- (3) The criterion is the only one that is generally accepted and employed in connection with the term.
- (4) The term A is not a "law-cluster" word.

Criteria (1) by itself is surely insufficient to separate analytic definitions from natural laws in all cases. Thus let us examine criteria (2), (3), and (4). A statement of the form "Something is an A if and only if it is a B" provides a criterion for something's being a thing to which the term A applies if people can and do determine whether or not something is an A by first finding out whether or not it is a B. For instance, the only generally accepted method for determining whether or not someone is a bachelor, other than putting the question itself, is to find out whether or not the person is married and whether or not he is an adult male. There are of course independent tests for both marital status (consult suitable records) and masculinity.

One objection must be faced at the outset: it might be argued that these criteria are circular in a vicious way, since knowing that the two statements, (a) "Someone is a bachelor if and only if he is an unmarried man," and (b) "Someone is a bachelor if and only if he is an unwed man," provide the same criterion for the application of the term "bachelor" is the same thing as knowing that "unmarried" and "unwed" are synonyms. For the present purposes, however, identity of criteria can be construed behavioristically: criteria (say, X and Y) correspond to the same way of ascertaining that a term A applies if subjects who are instructed to use criterion X do the same thing¹⁹ as subjects who are instructed to use criterion Y. Thus, if I were instructed to ascertain whether or not Jones is unmarried, I would probably go up to Jones and ask "Are you married?"—

to take care of words which consist of more than one morpheme and of idioms, but these complications will not be considered here. We can now give another reason why 'Kinetic energy = $\frac{1}{2}mv^2$ ' was never an analytic statement: its truth did not follow from the meanings of the words 'kinetic' and 'energy.' On the other hand, it would be absurd to maintain that, during its tenure of office, it was an "empirical statement" in the usual sense (subject to experimental test, etc.).

¹⁹ The use of the expression "do the same things" here will undoubtedly raise questions in the minds of certain readers. It should be noted that what is meant is not total identity of behavior (whatever that might be) but the absence of relevant and statistically significant regularities running through the behavior of the one group of subjects and not of the other. Separation of "relevant" from "irrelevant" regularities does not seem difficult in practice, however difficult it might be to "mechanize" our "institutions" in these matters.

and answer “No” to the original question if Jones’ answer was “Yes,” and vice versa. On any such occasion, I could truthfully say that I “would have done the same thing” if I had been instructed to ascertain whether Jones was “unwed” instead of whether Jones was “unmarried.” Thus, in my idiolect,²⁰ “being an unmarried man” and “being an unwed man” are not two criteria for someone’s being a bachelor, but one.

But let us consider a somewhat different type of objection. On what basis are we to rule out the statement “Someone is a bachelor if and only if he is either an unmarried man or a unicorn” as nonanalytic?²¹ Here three grounds are relevant: (a) the statement is a linguistically “odd”²² one, and is not clearly true; (b) the statement would not be generally accepted; (c) people do not ascertain that someone is a bachelor by first finding out that he is either an unmarried man or a unicorn. To take these in turn: (a) The English “or” and “if and only if” are not synonymous with the truth functions “ \vee ” and “ \equiv ” of formal logic. Thus it is not even clear that the quoted statement is an intelligible English statement, let alone true. (b) Even if we grant truth, it would not be generally accepted. Many persons would reject it, and others, who might not actually reject it, might decline to accept it (e.g., they might query its intelligibility or express puzzlement). (c) People (other than formal logicians) would certainly deny that they ascertain that someone is a bachelor by first finding out that he is *either* unmarried or a unicorn. In fine, the quoted statement does not provide a criterion for someone’s being a bachelor, in the sense in which ‘criterion’ is being used here; and it is not a generally accepted criterion for someone’s being a bachelor.

Since a good deal of the present discussion depends upon the way in which the word ‘criterion’ is being used, I should like to emphasize two points. Although sufficient conditions, necessary conditions, etc., are sometimes called “criteria” (e.g., the above “criteria” for analyticity), the

²⁰ An “idiolect” is the speech of a single speaker.

²¹ The difficulty here is that the class of bachelors = the sum of the class of bachelors and the class of unicorns (the latter being the null class). What has to be shown is that the so-called “intensional” difference between the two terms ‘unmarried man’ and ‘unmarried man or unicorn’ is reflected by our criteria, at least in connection with the definition of ‘bachelor.’

²² The quoted sentence is even *ungrammatical*, using the term in the sense of Noam Chomsky’s *Syntactic Structures* (The Hague: Mouton and Co., 1957); for its transformational history involves the ungrammatical sentence “Someone is a unicorn.” To change the example: “Someone is a bachelor if and only if he is either an unmarried man or eleven feet tall” is grammatical, but pretty clearly *false*, given the counterfactual force of the ordinary “if and only if.”

sense of ‘criterion’ in which an analytic definition provides a criterion for something’s being the sort of thing to which a term applies is a very strong one: (a) the “criteria” I am speaking of are necessary *and* sufficient conditions for something’s being an A; and (b) by means of them people *can and do determine* that something is an A. For instance, there are various things that we might call *indications* of bachelorhood: being young, high spirited, living alone. Using these, one can often *tell* that someone is a bachelor without falling back on the criterion; but the only *criterion* (satisfying (a) and (b)) by means of which one can *determine* that someone is a bachelor is the one which is provided by the analytic definition.

Returning now to our main concern, what is the relevance of the four criteria for analyticity? Someone imbued with the view that an analytic statement is *simply* one which is true by the rules of the language, i.e., one who insists on stating the distinction in terms of a model, instead of discussing the relevance of the model to that vast disorderly mass of human behavior that makes up a natural language, may be wholly dissatisfied with what has been said. I can imagine someone objecting: “What you are saying is that the difference between an analytic principle and a natural law consists in the accidental fact that no laws happen to be known containing the subject term of the analytic principle.” That is almost what I am saying. But the emphasis is wrong; and in any case the thing is not so implausible once one has grasped the *rationale* of analyticity.

In the first place it is not just that there do not *happen* to be any *known* principles concerning bachelors other than the principle that someone is a bachelor if and only if he is an unmarried man: it is reasonable to suppose that there do not exist any exceptionless (as opposed to statistical) scientific laws to be discovered about bachelors.²³ And even if there were an exceptionless law about bachelors, it is extremely unlikely that it

²³ It has occurred to me that someone might argue that “all bachelors have mass” is an example of an exceptionless “law about bachelors.” Even if this were granted, the objection is not serious. In the first place, in deciding whether or not a word is a “law-cluster” word, what we have to consider are not all the laws (including the unknown ones) containing the word, but only those statements which are accepted as laws and which contain the word. It does not even matter if some of these are false: if a word appears in a large number of statements (of sufficient importance, interconnectedness, and systematic import) which are accepted as laws, then in the language of that time it is a “law-cluster” word. And second, if a statement would be accepted as true, but is regarded as so unimportant that it is not stated as a law in a single scientific paper or text, then it can certainly be disregarded in determining whether or not a word is a “law-cluster” word.

would have the form "Someone is a bachelor if and only if . . ."—i.e., that it would provide a *criterion* for someone's being a bachelor.

But still we have to face the questions (1) Why is the exceptionless principle that provides the criterion governing a *one-criterion* concept analytic? (2) What happens if, contrary to our well-founded beliefs and expectations, a large number of exceptionless laws of high systematic import containing the subject term are someday discovered? The second question has already been discussed. If 'bachelor' ever becomes a "law-cluster" word, then we shall simply have to admit that the linguistic character of the word has changed. The word 'atom' is an example of a word which was once a "one-criterion" word and which has become a "law-cluster" word (so that the sentence 'Atoms are indivisible,' which was once used to make an analytic statement, would today express a false proposition).

But to consider the first question: Why is a statement which satisfies the criteria analytic? Well, in the first place, *such a statement is certainly not a synthetic statement in the usual sense*; it cannot be confuted by isolated experiments, or, what amounts to the same thing, it cannot be verified by "induction" in the sense of induction by simple enumeration. To verify or confute a statement of the form 'Something is an A if and only if it is a B' in this way requires that we have *independent* criteria for being an A and for being a B. Moreover, since the subject concept is not a law-cluster concept, the statement has little or no systematic import. In short, there could hardly be *theoretical* grounds for accepting or rejecting it. It is for these reasons that such statements might plausibly be regarded as constituting the arbitrary fixed points in our natural language.

There they are, the analytic statements: unverifiable in any practical sense, unrefutable in any practical sense, yet we do seem to have them. This must always seem a mystery to one who does not realize the significance of the fact that in any rational way of life there must be certain arbitrary elements. They are "true by virtue of the rules of the language"; they are "true by stipulation"; they are "true by implicit convention." Yet all these expressions are after all nothing but metaphors: true statements, but couched in metaphor nonetheless. What is the reality behind the metaphor? The reality is that they are true because they are accepted as true, and because this acceptance is quite arbitrary in the sense that the acceptance of the statements has no systematic consequences beyond those

described in the previous section, e.g., that of allowing us to use pairs of expression interchangeably.

Finally, the question as to whether it is *rational* to accept as true statements satisfying the four criteria is easily answered in the affirmative. This is the question as to whether all these statements may reasonably be taken as true in a "sensible" rational reconstruction of our actual language. To discuss this point in detail would involve repeating the argument of the preceding section, since this is just the problem which was treated in that section.

Does the fact that everyone accepts a statement make it rational to go on believing it? The answer is that it *does*, if it can be *shown* that it would be reasonable to render the statement immune from revision by stipulation, *if* we were to formalize our language.

In short, analytic statements are statements which we all accept and for which we do not give reasons. This is what we mean when we say that they are true by "implicit convention." The problem is then to distinguish them from other statements that we accept, and do not give reasons for, in particular from the statements that we *unreasonably* accept. To resolve this difficulty, we have to point out some of the crucial distinguishing features of analytic statements (e.g., the fact that the subject concept is not a law-cluster concept), and we have to connect these features with what, in the preceding section, was called the "rationale" of the analytic-synthetic distinction. Having done this, we can see that the acceptance of analytic statements is *rational*, even though there are no reasons (in the sense of "evidence") in connection with them.