

The First Dogma of Logical Negativism

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Whatever we may decide upon more mature reflection, it appears *prima facie* that arguments come in two varieties, at least: arguments may be salient for their persuasive capacity; they may on the other hand be useful in searching for truth, whatever that is. In this division, we do not have only the meaning of words to go by. To be sure, the word 'argument' is used at least in these two ways. It may be used to refer to an act of persuasion, or perhaps to failed mutual acts of persuasion (as in 'they had a terrible argument'). But it may just as well be used in the pursuit of truth (in 'science', to use a figure of speech) as in the example 'your argument is worthless because it is viciously circular'. Unhappily, an argument may be both of these at once, unpersuasive and fallacious, for example.

But word meaning is not at issue. The word 'argument' is also used in mathematics, in describing a function, and in Ptolemaic astronomy, but these uses do not enter discussions in argumentation theory. Argumentation theory is heir to a vibrant history, which used to be called 'rhetoric' which makes the first two uses of the word 'argument' dialectically opposed to one another. It is in this context of conflict that we need to look at their difference. The study of arguments is part of a long intellectual tradition. It goes back at least to the time before Plato was writing his dialogues. Therefore, it predates Aristotle's invention of logic. Plato portrays Sophists as claiming that persuasion is all there is to both kinds of arguments listed above. Socrates, in opposition, defends arguments as an aid in the search for truth. Plato portrays Socrates' speech in the Apology, for instance as unpersuasive. The question which always arises in the Apology concerns the intellectual imperialism of rhetoric, if we accept Plato's portrayal of them. Was Socrates right to seek to do more than to persuade? Perhaps he was wrong headed even to try. Perhaps he had a point, on the other hand. This is a celebrated issue.

Much depends in these dialogues on whether truth can be pursued, as Socrates claims, after allowing for all the claims made on behalf of persuasion. Socrates' view is clear: Even the rhetorician must accept the importance of the difference between what is true and what is false. For a rhetorician, like a General in an army, must plan a strategy of persuasion (or attack, in the General's case). And what good General plans a strategy based on what is merely persuasive prior to examining the alternatives? On the contrary, a good General devises a battle plan on the evening before

and invites criticism, so that any losses incurred are incurred on the drafting table rather than in the battlefield.

J. Agassi summarises this Socratic position by the motto 'to lose an argument is to win'. In the pursuit of truth this is quite right. In the special situation of the general devising strategy, or in the case of the Socratic philosopher inquiring into the nature of virtue, truth requires that we *want* to lose whenever possible, until we cannot lose any more. The ordinary interim goals of rhetoric are turned around in the special case of argumentation in the service of a search for the truth. If Socrates is right that truth is ever at issue over and above what we happen to be persuaded of, then the study of argument in aid of science must be different from the study of argumentation in law or in politics. There are, as it were, rhetorical devices and there is a scientific organon, and their goals are so opposed, at least in the interim, that their analysis must be different.

The logic of truth is therefore different from the logic of persuasion because their interim goals are different. But a Sophist, ancient or modern will not be persuaded by this because it is not clear that there is an acceptable logic of truth. In the aftermath of the supposed failure at all attempts to provide one, the argumentation theorist must withdraw from a Socratic conception of argument, in the service of truth, and study persuasive arguments exclusively. But is there an acceptable logic of truth, as Socrates had hoped?

Recognizing a place for scientific or rational discourse within rhetoric is not quite the view of Plato's Socrates, who argues strenuously for the suppression of the first in favour of the second, wherever possible (for example in politics). In this study, we need assume no such rationalist imperialism, to counter the rhetorical imperialism which Plato portrays among Sophists. We merely note that there is a method of the Socratic eristic which is uniquely suited to his kind search for truth. This kind of search can sometimes make sense. And this is in need of study.

In modern times, this Socratic eristic, or sceptical inquiry, is manifest in the doctrine of logical negativism, which is the object of this study. Logical negativism has arisen anew not from a critique of rhetoric, but in a different background of recent ideas concerning logic, though the new eristic retains the claim that it can assist us in our scientific pursuit of truth.

MODERN SOCRATIC THEORIES

The title of this paper suggests that logical negativism has at least two dogmas. One reason for neglecting to call this paper 'Two Dogmas of Logical Negativism' is that there may be a third. How many dogmas there are depends somewhat on how dogma is cut, which is a matter of convention.

Logical negativism is one way to describe the view espoused by W. V. Quine.¹ Many of Quine's distinctive theses are developed to counter certain theories of logical positivism. Because Quine shares something with his famous opponents his views are best studied *via* and *contra* logical positivism. It is in this context that I speak of two or possibly three dogmas of logical negativism. In this sense logical negativism is a philosophy of knowledge and of language at once, like the philosophy of Rudolf Carnap.

In fact, the expression 'logical negativism' may also be used to pick out a more particular doctrine of Quine's. This is the doctrine that we can learn from our experience only negatively.² This is a view concerning the nature of knowledge which may or may not be linked to a theory of language. In this claim Quine concurs with Duhem and Neurath regarding mathematical physics and with Popper regarding all but sensory knowledge. Quine's views on language and fact appear to have influenced D. Davidson. So my critique extends to the views espoused by the latter though this suggestion will not be explored to a review of all of Davidson's views.

The first of the two dogmas of logical negativism is that any recalcitrant phenomenon will overthrow all theoretical premises conjointly in any system of hypotheses. This is often called 'holism'. The second dogma is that in order to avoid metaphysics we must avow behaviourism. (A third dogma will only be mentioned. It is that one's knowledge has solely sensory input as its source which is all in the form of sentences. It is not being examined, though most of my research is more recently concentrated there.) It is the first of these or 'holism' that is being examined though the second dogma undermines itself in the logical negativist's scheme of things.

'Holism' is the doctrine that when several statements together face empirical refutation as a system, the empirical refutation does not pick out any one statement of the system as responsible: It may be regarded as the denial of the logical positivists' hope that each scientific statement be independently testable. When a system of statements is refuted all the statements fail jointly as a system. The system may be modified by changing one of several statements in it. But placing the blame on one statement rather than another leaves us with a *choice*. This choice allows one to defend one statement by modifying others as one wishes. Quine's locution that any statement may be defended 'come what may' is an apt and satisfying slogan for holism.

The connection of holism to language in Quine's particular form of holism distinguishes it from the logical negativism of Duhem or Popper. According to Quine, since any hypothesis out of a system may be defended come what may: there is no difference in principle between analytic and synthetic hypotheses. The two are distinguished merely by the fact that we are more inclined to modify a synthetic hypothesis under fire than one that we call 'analytic'. Moreover, there is nothing more to a statement being analytically true than our disposition to defend it under siege.

The claim that analytically true and synthetically true statements are such that a person is more likely to give up one of the latter than one of the former is something nobody denies. But the statement that the only difference between the two is our disposition to so respond to empirical difficulty is surprising. For this inference the reason that Quine provides is found first in his essay 'Truth By Convention'. In this essay, after suggesting that logic and mathematics are no more conventional than any body of empirical knowledge such as chemistry, he goes on to say 'Still there is the apparent contrast between logico-mathematical truths and others that the former are *a priori*, the latter *a posteriori*; the former have "the character of inward necessity" in Kant's phrase, the latter do not. Viewed behavioristically and without reference to a metaphysical system this contrast retains reality as a contrast between more and less firmly accepted statements; and it obtains antecedently to any *post facto* fashioning of conventions. There are statements which we choose to surrender last, if at all, in the course of revamping our sciences in the face of new discoveries; and among these are some which we will not surrender at all so basic are they to our whole conceptual scheme.'³

Quine clearly suggests that we should characterise the difference between statements labelled 'true by convention' and those 'true by fact' simply in terms of our disposition to respond to empirical difficulty. The reason for doing this is: *in order to avoid metaphysics*.

Some years later Quine reviewed *Meaning and Necessity*,⁴ in which Carnap distinguishes between two sorts of statements which employ existential quantifiers. In the first place there are statements like 'there are numbers' which Carnap regarded as external to a language of science and statements like 'there are more than twenty prime numbers' which are internal to the language. Carnap's now famous view of this difference was that external statements which are metaphysical dictate a style of speech without making a difference to the facts. Internal questions on the other hand make a difference and are scientifically significant.

Quine took issue with Carnap with success. There is no difference between internal and external existential statements as such because restrictions in the scope of quantifiers can be introduced either from without or from within. Variables which range over everything may still be qualified by devices like 'x: such that x is a number . . .' to say whatever Carnap could say in his language. Differences between languages with and without restrictions on quantifiers are merely lexicographic differences. Any question which is external can with a suitable broadening of the range of variables be made internal to a language. Quine concludes that metaphysical questions such as whether there are numbers at all are conventional only if it be allowed that all statements of physical theory also be classed with them as conventional. In the last analysis the reason for this conclusion is holism.

Since metaphysics is unavoidable according to the critique of Carnap it

might seem that there is no need to continue advocating behaviourism. Behaviourism was advocated originally in order to avoid metaphysics. But even if metaphysics or 'ontological commitment' as Quine calls it ('allow for a shudder between the word "ontological" and the word "commitment"') is known to be unavoidable, he continues to invoke behaviourism all the same. It seems that behaviourism is undermined, but invoked.

This is defensible. The first dogma of negativism allows that any doctrine may be defended come what may. One is therefore perfectly safe in defending behaviourism provided only that the first dogma is true. So we see thus that the defence of the second dogma turns upon the first.

We note, in confirmation, that Davidson does not accept the second dogma but only the first. Davidson's negativism may nevertheless be read as a variant of Quine's ('Without whom not').⁵ Quine suggests that no description of meanings can be made antecedently of fact. For if one could then there would be a distinction between 'true by virtue of meaning alone' which would be conventional and 'true post facto' which is not. This is contrary to holism. But in trying to apply Tarski's concept of truth to natural languages Davidson finds himself parting company with respect to the second dogma concerning behaviourism precisely in order to defend holism.

Tarski has provided a semantic theory of truth for a formalised language which is widely regarded as a model of logical analysis. Tarski begins by requiring that an adequate definition of truth would be one which captures the extension of the predicates 'true' and 'false'. The word 'true' is redundant when applied to a sentence *p* (to say that it is true is to say no more than to assert it). We can define truth if we can deduce from our definition of truth, for every class of equivalent sentences *p* in the formalised language (each equivalence class corresponds to what we may colloquially call a statement or a proposition), a sentence of the bi-conditional form: '*p*' is true if and only if *p*. Tarski actually provides such a theory which generates such bi-conditionals for the formalised language he constructs, thereby satisfying his requirement of an adequate theory and definition of the concept of truth. We note that Tarski requires that the bi-conditionals be derivable for each *class of equivalent sentences*. But equivalence presupposes synonymy from which analyticity and truth by convention follow trivially. (This is *contra* Quine.)

Davidson suggests a reconciliation. Meanings are not *antecedently* definable for reasons that Quine adduces. At the same time Tarskian truth stands: We must reinterpret Tarski's definition of truth itself as a definition of the equivalence of sentences based on a primitive concept of truth. Colloquially speaking, this is what a statement *means*. So instead of assuming for a Tarskian theory an antecedent semantic equivalence which is employed in the service of truth-definition, Davidson envisages a Tarskian definition which establishes conditions of equivalence for sentences by way of Tarskian bi-conditionals in terms of a primitive concept of truth.

We see that Davidson has abandoned the view that meaning is to be understood dispositionally and substituted for it the extraordinary doctrine for which he has become famous, namely that meaning is to be understood in terms of truth. A theory of meaning is only a certain kind of Tarskian theory of language according to this perspective. We note that Davidson and Quine differ as to the second dogma of logical negativism: Davidson rejects behaviourism. But if my reconstruction is right then he does so only in order to defend the first dogma of negativism, namely holism, conjointly with a Tarskian theory of truth.

SOME ODD CONSEQUENCES OF HOLISM

It is easy to get distracted by the dispute between Quine and Davidson: Is meaning proximal or distal? To take sides is already to adopt holism by default. The question is not, given holism, whether meanings are to be understood in terms of behaviour or truth. Is holism true in the first place?

(We had left modern Sophists after the introduction. But we may wish to revisit them here. If holism is true, then the resources left to the scientist are so meagre that we may as well become modern day Sophists. So the issue is utterly central to the contemporary defense of Socrates.)

Logical negativists do not give us even *prima facie* an adequate account of the phenomenon of meanings. To take up only the merest of difficulties consider for example how on Davidson's account we would learn about the world. Consider a child who does not know the truth about the solar system. When the child uses words like 'earth', 'planet' or 'sun' she could not possibly mean what we mean by these words. But if these words mean what the truth about the solar system determines we have a curious paradox: The child could not learn what we know because she can never acquire our concepts. To acquire our concepts she must first learn the truth which seems to be an impossible condition if the learning necessarily involves a use of language.

Readers of Davidson's 'On the Very Idea of a Conceptual Scheme' are often puzzled by this description. For in criticism of 'complete failure of translation' or 'complete incommensurability', Davidson's view seems moderate. Could Davidson really say anything which has these drastic consequences? If we read his more suggestive remarks in the second and shorter part of the essay, devoted to partial failure of translation, the truth, as it were, lets out.

'If we merely know that someone holds a certain sentence to be true, we know neither what he means by the sentence nor what belief his holding it true represents. His holding the sentence true is thus a vector of two forces: the problem of interpretation is to abstract from the evidence a workable theory of meaning and an acceptable theory of belief.'

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What, then, is the solution? 'Since knowledge of beliefs comes only with the ability to interpret words, the only possibility at the start is to assume a general agreement on beliefs. We get a first approximation to a finished theory by assigning to sentences of a speaker conditions of truth as they actually obtain (in our opinion) just when the speaker holds those sentences true.'

Later he tells us, 'We make maximum sense of the words and thoughts of others when we interpret in a way that optimizes agreement (this includes room . . . for explicable error, i.e. differences of opinion)'. Yet, when we have someone who appears to disagree with us, and whose language we wish to construe, what do we have to go by in deciding which is said? 'But when others think differently from us, no general principle, or appeal to evidence, can force us to decide that the difference lies in our beliefs rather than in our concepts'. Since there is no principle to tell us which it is, but we have the principle that to make the maximum sense of words we must interpret in a way that optimises agreement, it follows that every apparent disagreement between two people leaves them, as he says in his quip about Kuhn's description of incommensurability, merely words apart.

His conclusion is thus clear: 'Given the underlying methodology of interpretation, we could not be in a position to judge that others had concepts or beliefs radically different from our own.' In fact, though a concession was recorded earlier that in fact disagreements of a minor sort can be accommodated, no principle exists for its detection, and no situation is presented where any two can actually disagree about anything given Davidson's underlying methodology of interpretation. In denying the very idea of a conceptual scheme Davidson denies the very existence of different points of view, or opinions. And if he allows that sometimes opinions may differ, and that speakers may in fact disagree, then whatever principle we invent to make this reveal itself in his methodology of interpretation thereby resuscitates conceptual schemes in all their glory.

As Frege notes, our language is an epistemic tool. To define meanings in terms of what is the case yields the consequence that those not blessed with omniscience know not whereof they speak, and so cannot learn verbally. Could they do so their own basis for language which is other than truth has a better claim on semantics than Davidson's. To make the same point in another way, no two people could disagree significantly because each of them simply misunderstands the other. Understanding and disagreement pull in opposite directions. We shall see that the same point emerges in another way for Quine.

Quine's behaviourism does not fare much better. There are two forms of behaviourism. The ontic behaviourist denies that there is anything other than behaviour to which to refer; the linguistic behaviourist does not deny that there may be something colloquially called 'mental activity', but urges us to abandon such locutions. Quine treats the mind in much the same way as Victorians treated limbs when they decreed that though pianos may have

legs they had better be covered and left unnoticed. In a similar vein Quine would that we left meanings unnoticed.

Adopting behaviourism we find that the only points of contact between any two speakers are their behavioral dispositions to respond to stimuli. Two statements embodied in two different languages are effectively synonymous or adequately intertranslatable and interchangeable just so long as they are indistinguishable on overt criteria of behavioural assent or dissent in public situations. Since holism leaves us considerable choice in each language to defend any hypotheses, each language or conceptual scheme is underdetermined by the facts. Radical, or behaviouristic, translations are many and all equally adequate so long as they are adequate empirically. These are well known consequences of Quine's celebrated study, and they lead to other consequences such as the inscrutability of reference and ontological relativity. These are often adduced as deep discoveries where they may be construed less kindly as surface signs of a deeper inadequacy.

In translating from another's language into one's own one can but use one's own criteria for adequacy. So if it appears that another's statements are self-contradictory then we retranslate them to our tongue as consistent statements so that we can understand them. Similarly if another's utterance is a palpable falsehood so that it contradicts one of our pet beliefs we interpret it in the home tongue so as to make it congenial to the home philosophy. We get the odd consequence, all over again, that to understand is to be unable to disagree.

Extraordinary conclusions follow from this. We recall that on Davidson's scheme it is also the case that we cannot disagree with someone that we understand. So either Quine does not understand Davidson and *vice versa*; or they agree unbeknownst to us and to each other. Another astonishing consequence is this. One can never contradict oneself: no sooner than one finds that one has inadvertently done so one can retranslate back into one's language in such a way as to render everything true, perhaps even trivially true. One knows this even before one finds that one has made a mistake. No one need ever worry about contradicting oneself. It is so easy to remedy, come what may. Had Russell only known this when he struggled for twelve years with the antinomies of the theory of classes we might well have been spared many of his thoughts on mathematics. He could have found solace in holism, which is a soothing philosophy as philosophies go.

THE LOGIC OF NET DIFFERENCE

Holism is a thesis that has the consequence that all statements are empirical whether these are customarily thought to be analytic or synthetic. Another is that metaphysics cannot be distinguished from physics by the

testability of the latter because physics and metaphysics face the empirical music as an ensemble. Philosophy is not therefore prior to science but works alongside science. Quine is obliged to adopt a view which he calls 'epistemology naturalised'. According to this view epistemology merely describes what we do in science. We cannot say on transcendental grounds that one resolution of an empirical refutation is superior to another. If we have grounds, be they called 'transcendental', they face the tribunal of experience with the rest of science.

If we adopt a naturalised epistemology we can no more recommend holism as a method than an earth-centred astronomy. We can merely report whether it is accepted or not. As scientists we are left with the question 'Are we disposed to accept it or we are not?' The result is an empirical matter, not one for prior philosophic decision.

We may recast Quine's holism thus: if our ideas form a system of belief, and if this system faces empirical refutation, then we can defend any doctrine 'come what may'. It is often neglected that logical negativism is conditional in this way, depending upon a model situation of the science in which a system of coherent statements faces refutation. But do our ideas form coherent systems and do they face refutation as a whole? This is an empirical matter on which Quine's boat founders.

It is important to remind ourselves that holism does not leave itself the option of a transcendental analysis of method. All of Quine's dicta are immanent, as he is fond of saying. This immanence or naturalism is a consequence of holism which denies a distinction between empirical science and prior philosophy. So the empirical question about how statements confront one another in science is as scientific or empirical a matter as how planets relate.

But it is difficult to determine empirically whether we do hold some views dear to our heart, defending them at all cost, though we have the choice not to do so. It is quite possible that we are actually quite open minded and willing to jettison any statement when appropriate. Since actually we change only a small percentage of our views there will always be many which seem to be held 'come what may' because they are in fact not overturned. Just because some views are not overturned it does not follow that we are disposed not to overturn them. On the other hand it is equally possible that we think ourselves free to choose various options that we do not, where the truth is that we had no reasonable choice in the first place. How does one judge the amount of slack when a theoretical system faces a negative result?

There is a way. Let us grant Quine that logic in some form more or less like ours is not about to be jettisoned. Let us ignore the question of choice with respect to it, noting merely as naturalists of epistemology that logic is usually presumed to apply. If Quine's model of our knowledge as a coherent system of statements is correct then by virtue of logical analysis Quine's holism is true. Let us first concede that. But is there any model of

knowledge in which Quine's dicta do *not* apply? Is there such a model in which we accept classical logic but in which holism does not obtain? There is such a situation indeed with little or no logical slack to it. It seems a better empirical fit as well.

Consider a two stage model of science in which at the first stage scientific theory is highly contested and the disputes are lively. When scientific controversy disappears on an issue the second stage commences and one of the contesting doctrines which prevails is used for possible application and benefit. If we pay attention to the first stage of theoretical controversy in science, ignoring the applied and the technological end of science, we find a model there which is quite different from Quine's.

When two systems of thought compete they do not differ on every statement. Several systems of geology are quite indifferent to the truth of where I was born or what I wore to my graduation. Common to the two are also many if not all statements of physics, chemistry, logic, mathematics and biology. When two systems of thought compete a great many statements which are acceptable may be shared by them. We may construe each system as the sum total of all we believe, with the addition of this or that hypothesis, with suitable adjustments for consistency. There is then a net difference between the systems. The two theories together with the different ways of fitting each into the rest of our views constitute a net difference. A differential refutation or one which refutes one of two systems but not the other ends up by refuting the net difference. In this model the amount of slack in a system is negligible, and we can no longer defend the refuted net difference come what may.

Suppose we have two hypotheses T1 and T2. Let us imagine them to be two ways of construing some phenomena, let us say the particle and the wave theory of light. Consider a test result, let us say the two slit experiment. From each of the two theories, each with appropriate assumptions, we are able to deduce two different conclusions, and these contradict one another. From wave theory with its assumptions we conclude that an interference pattern will be seen. From the particle theory we are led to believe that no interference pattern will be seen. As it happens, an interference pattern is observed. How can we now defend a particle theory?

We note that the assumptions common to the two different systems are antecedently thought to be true: If we did not we certainly would not bother with the test case. The test case yields the result that the common assumptions {A} together with T1 (particle theory) yields the false conclusion – C, whereas the same set {A} with T2 yields the true conclusion C. Now it is up to us to decide which theory we will decide to reject. We have here two plausible hypotheses, each of which could well have antecedently been judged to be in error, and we can lay the blame on one of them (T1 as it turns out). Or we could lay the blame on an unknown statement in {A} which we have been assuming antecedently to be true.

Suppose we argue that one of the {A} is false. Let us suppose we

question the experimental arrangement, claiming that the wave pattern arises out of something not to do with light, but with the apparatus. But there is still the old view of the apparatus to compare with the new suggestion. So we have two theories of the apparatus, TA1 and TA2, for which we find a differential test (for instance by varying the apparatus to take account of TA1). TA1, which is invoked to explain away the experimental result (interference) is soon in trouble, and we have to find a new fault in the subset of {A} that the differential test between TA1 and TA2 utilised. This process must continue until a test case comes up in favour of the particle theory, because by default it remains accused.

The hypothesis that we can defend the particle theory, come what may, needs to be reconsidered. What comes may well lead us to abandon particle theory, if we cannot find a way to combat wave theory. The attempt to answer the difficulty concerning interferences takes on a peculiarly hazardous route in a situation of competition between them. On the wave side of the dispute there is a ready answer, on the other a potentially endless series of analyses which do not help until a differential test comes up in its favour. Moreover, we must not forget that our other beliefs also have their pragmatic uses. To question any of them in defence of particles behooves us to find alternatives of equal value. One may try to explain the interference pattern, for instance by reference to an optical illusion, but not without throwing our psychology and physiology of perception into disarray. We need to understand more than merely how we see interference patterns with the aid our psychology and physiology of perception. So to tamper with those theories leads to further differential tests which do no better than the first. If we question the mathematics, we will perhaps have to provide a better analysis of motion than the one available to us. If we question the details of the apparatus, we have to explain how a change in the arrangement still produces the same effect. At every turn there is another differential test between what the particle theorist once believed and the new found alternative explanation of the result.

It sometimes happens that a differential test of the sort noted between two contested hypotheses goes clearly against one, only to be later discovered to be an experimental error, or an error of another kind. But such a discovery is not the kind of choice that is envisaged in logical negativism. The discovery of the new 'correct' explanation of the phenomenon in question is itself capable of differential test. If it passes the test, then the alternative explanation clearly fails, and now the alternative has the possibly endless analyses to contend with in order to bolster light waves. Instead of Quine's optimistic slogan that we can defend any hypothesis 'come what may' it is tempting to adopt the pessimistic slogan in theoretically contested realms that nothing works. Every theory in any interesting field seems fraught with difficult questions for which good answers are wanting. The slack that Quine sees in diagnosing error arises only in the single theory model of science.

The difference between a one theory model and a two theory model of tests is interesting to compare in its generality: In a one theory model, the criterion of note for the revamped system of statements is logical consistency, or weak coherence. In the competition between theories the criterion of adequacy may be called 'strong coherence'. When two theories clash, and are tested differentially, the common hypotheses act like a bank of transcendental judgements. We may, of course modify some part of the transcendental bank to accommodate our predilections. But not without difficulty, for each such move creates a new transcendental bank which acts as a basis for judgement on the new issue in question.

Transcendental banks are transitory banks. In this they remain immanent to our whole scheme of thought. But they do impose a strong coherence on the competing hypotheses which constitute the net difference. When we appeal to any criterion of adequacy in a single theory model the criterion itself is liable to modification to accommodate a new phenomenon. There is no judgement which falls outside the set of statements which we may modify. The sole criterion for the new set is consistency and even this may be redefined in the new set. But when theories are in competition, every modification of any belief made in defence of a beleaguered statement is in turn beleaguered. If we modify an empirical hypothesis it is in turn to be judged on the basis of its coherence with other of our views. To modify one of those leaves us the task of reconciling that with the rest of our views. Every attempt to save the obvious culprit seems to leave a trail of evidence. Sometimes, as a historian, one finds only a long trail, without a satisfactory end, in which case the guilty party stands convicted as charged. At other times there is in fact a resolute end to a trail, in which case the theory caught red handed is saved from the firing squad at the very last hour. This matter is judged from the basis of what continues to remain from time to time outside the net difference of opinion.

The extremely esoteric nature of scientific inquiry is often a product of this kind of logical search to defend an alternative which is beleaguered. Like Socrates pursuing logical difficulties with his interlocutors in the early Platonic dialogues we too can pursue the difficulties of defending an apparently defeated alternative. The result is another eristic exercise. There are many reasons why we may embark on such an exercise when the prospects appear bleak. Personal pride in an invention, doctrinal objections to the alternative, conservatism, and many other factors intervene. But the single most important reason why scientists will defend an obvious culprit is that there are other empirical arguments, independent of the one being considered, which point the finger the other way. If wave theory has the two slit experiment on its side, particle theory adduces the fact that thin plates display an exertion of a measurable pressure upon them when bombarded by light. In the latter case it is the wave theory which is caught in the act, and the particle theory which enjoys the upper hand.

In any lively intellectual history proponents of each of the alternative

systems of thought can point to several difficulties which differentially trouble the others. As long as there are several alternatives it remains lively.

Quine's model does sometimes obtain. A popular holistic example in theoretical science is perhaps that of mechanics, though even this is doubtful. The best examples of Quinean models are outside of active research. There is a good reason for this. If there are no good alternatives there is only weak coherence in dealing with empirical refutations, so *ad hoc* modifications become acceptable. Science becomes rather dull except in application. But where there is controversy the net differences get modified and evolve, perhaps giving the false impression that whatever lies outside of the scope of the net differences is held to be true come what may.⁶

There are situations in which we can decide pretty quickly and accurately if a statement of theory is false. In these situations our decisions are not based on dispositions to assent but on the logic of net difference. So I conclude that Quine is empirically in error in his model of science. This suggests a possible modification of Quine's (and Davidson's) theory of language too.

Language has two functions upon which different philosophers might put different weight. On the one hand language is a system of communication as Wittgenstein stressed in later life. On the other it is a way of understanding the world as logical positivists and negativists remind us. It seems to me that logical negativists find understanding and communication (in disagreement) pulling against each other. They cannot do justice to both. We need language most for communication when our disagreements are most marked. Language gives us a means of discussing issues neutrally. It would otherwise be useless for science. It follows from this that language is not a deductive system, *contra* Quine, Davidson and Carnap. In a deductive system disagreement is difficult to interpret without incoherence. We may try to get around this by mapping language into a weak deductive system. This allows many undetermined statements to be discussed in the language. But there is always something which remains uninterpretable in any deductive system, however weak.

If we knew how we could define a number of artificial languages each of which is a weak deductive system and each of which differs from the other so that no one assumption is shared by all of them. Such a set would be the least we would need into which we could map an artificial language. To be realistic the set would have to cover a large part of the different theories we have entertained in our past.⁷ Such a model of language would not force us to choose between communication (to resolve disagreement) and understanding.

It is not that language is neutral. Bias is endemic to language and thought. There is no language such that it is neutral between any two perspectives. But given any two perspectives we can often find a neutral language to discuss the issue. It is all in the order of quantifiers.

NOTES

¹ Quine, W. V., 'Two Dogmas of Empiricism' in *From a Logical Point of View*, Cambridge: Harvard University Press, 1953.

² References to text are omitted here, but may be found in J. N. Hattiangadi, 'Basic Quine for Social Scientists', *Philosophy of the Social Sciences*, 1989. A bibliography of Quine's work until 1986 is to be found in *The Philosophy of W. V. Quine* ed. L. E. Hahn and P. A. Schilpp, *The Library of Living Philosophers*, Open Court Publishing Co., LaSalle, Ill. 1986.

³ W. V. Quine, 'Truth By Convention', in *Philosophical Essays for A. N. Whitehead*, ed. O. H. Lee New York: Longmans 1936 and reprinted in *Philosophy of Mathematics: Selected Readings*, ed. P. Benacerraf and H. Putnam Englewood Cliffs New Jersey: Prentice-Hall 1964.

⁴ R. Carnap, *Meaning and Necessity*, Chicago: University of Chicago Press 1947.

⁵ This motto is his dedication in the book. All the material quoted from Donald Davidson is from his paper, 'On the Very Idea of a Conceptual Scheme', which is reprinted in his collected *Inquiries Into Truth and Interpretation*, Oxford: Clarendon Press, 1984.

⁶ Examples from the history of science of cases which fit the model of net difference are found in J. N. Hattiangadi, 'A Methodology without Methodological Rules', in *Boston Studies in the Philosophy of Science*, Vol. 31, *Language, Logic and Method*, ed. R. S. Cohen and M. W. Wartofsky, Dordrecht, Holland: Reidel Publishing Co., 1983.

⁷ These thoughts are explored in *How Is Language Possible?* LaSalle Ill.: Open Court Publishing House, 1987.