Seven

INCOMMENSURABILITY AND DEMARCATION[1]

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1. Preface

If the term "relativism" is understood as relativists take it, everyone is a relativist. If, on the other hand, one understands "relativism" as absolutists do, no one really *could* consistently be a relativist, despite what they might think.

Stefan Amsterdamski is plainly not an absolutist. As for me, I prefer not to think about myself in such categories; but if I had to make a choice, I would prefer setting myself a bit closer to the absolutists than to the relativists. As I hope to show, however, much of this positioning of persons and philosophies is foolish. It misses much that is important in philosophical discussion and focuses attention in directions that lead to dead ends.

Thus, at the very outset of my discussion, I would like to applaud Amsterdamski's general conclusion, expressed as follows in his "Between Relativism and Absolutism: The Popperian Ideal of Knowledge":

> . . . though we are not and cannot be autonomous knowing subjects, it is also a mistake to think that we cannot at least to some degree . . . overcome our extra-logical determinations in any sphere of intellectual activity or in social life.[2]

Amsterdamski rightly deplores, especially in connection with the debate between Karl Popper and the "sociologists of knowledge," positions that are too one-sided. I second that pronouncement in the strongest possible terms. What this means, though, both for the Popper/Kuhn debate in particular and the absolutist/relativist debate in general, is what we may disagree about. As will shortly be made apparent, while Amsterdamski declares himself to be a relativist who has learned from Popper, I might be regarded (and as Amsterdamski eloquently argues, Popper might best be regarded this way too, Popper's own declarations to the contrary notwithstanding) as an absolutist who has learned from Paul Feyerabend and Thomas Kuhn.

2. An Introductory Parable

The members of the Crow tribe of North America, before they were either wiped out, or placed in what were plainly concentration camps, or assimilated, had an intriguing custom.[3] It involved the procedure to be followed by members of a war party, in the event that one of the warriors was killed during an expedition. The custom went like this: at the earliest opportunity after the death of the warrior, the war party was to ride back toward its home village. But it was not to enter the village. Instead, the war party was to camp well outside the village, but plainly within sight. There they were to make camp for ten full days before entering the village. This would effectively notify the villagers that a warrior had been killed in action and would allow the villagers to make preparations for the proper ceremonies.

What is important here is this: for these members of the Crow tribe, the issue concerning what the warriors were to do under such circumstances was plainly an important *moral* issue. If the war party were to violate the rule about camping outside the village and instead ride immediately into town, a grave wrong would thereby be committed. It would be wrong because it would render a deep insult both to the dead warrior (or, at least, to the memory of the dead warrior) and to the warrior's living relatives. Correspondingly, it was clear to the Crow what the morally *right* course of action must be in such circumstances: the war party must stay camped outside the village for ten days. What was right, and what was wrong, was quite clear.

The Apache tribe had no such custom. For the Apache, the thing to do if a warrior was lost was to ride immediately back to the village, bringing the dead warrior's body, if possible, and to inform the rest of the tribe what had happened. For the Apache, the idea that the war party should camp outside the village for ten days would seem absolutely ludicrous. Such a course of action would plainly show contempt for the dead warrior and for the warrior's family. Obviously, at least according to the Apache, war parties had a clear moral obligation to inform the tribe immediately in such circumstances. What was right, and what was wrong, was every bit as clear to the Apache as to the Crow. But the substantive answers that the Apache gave were precisely opposite to those given by Crow tribe members.

As should be apparent by now, this story is an example of the kind told most often by relativists, especially in support of the thesis that right and wrong are *culturally* determined. Here we have a good example of customs so divergent as to be actually contradictory. To help make the point sink in, note that it is not just that the Crow tribe members merely *think* that riding into town immediately would be wrong, or that the Apache merely *think* the opposite. For, first of all, what could it possibly mean to say that something *other* than what they think could be the *correct* answer to the question at issue? And secondly, perhaps more importantly, as we, who were raised, I suspect, neither in the Crow nor the Apache culture, reflect on this issue, what *we* should do seems fairly plain. If we were riding with a Crow war party, we should stay camped outside the village for ten days. If we were riding with an Apache war party, we should ride back into town immediately. When in Rome, it appears, we should do as the Romans do.

What could be a clearer and more decisive justification of this particular version of ethical relativism?

Let us reflect, however, upon what the absolutist would say about this issue. And let us take as our model of the *kind* of thing that absolutists tend to say the following statement by Karl Popper (as quoted by Amsterdamski):

> I am not a relativist. I do believe in absolute or objective truth . . . (although I am of course not an absolutist in the sense of thinking that I, or anybody else, has the truth in his pocket).[4]

How might this be applied in our story about the Crow and the Apache?

The absolutist would be inclined to think that this story offers good evidence that *absolutism*, not relativism, is correct. The story shows that right and wrong do *not* vary from culture to culture, although more would need to be said to make a decisive case for this conclusion. The point is, though, that the absolutist would take this story to *confirm,* in at least a small way, the absolutist's own conclusion. How is this possible?

The absolutist would simply argue, I think, that these two tribes do *not* differ as regards truly moral principles. Indeed, we see in our own reaction to this story that not only is there no disagreement between the Crow and the Apache, but there is also no disagreement between those two groups and us. We *all* agree about what is right and wrong in the case of what war parties should and shouldn't do when a warrior is lost. Now: what is it, specifically, we agree about, according to the absolutist, in the teeth of all the *dis*agreement plainly found in our story of the Crow and the Apache?

Well, we all agree that we should act in such a way as to avoid insult to the memory of the dead warrior and to the family of the warrior. To act in a way that denigrates the status or the memory of the warrior is wrong. That the Crow believe in this moral principle explains their commitment to the course of action they are inclined to take; that the Apache believe in this principle explains *their* commitment to a contrary course of action; and that *we* believe in it explains why we are prepared to adopt a "when in Rome" strategy. The only thing that differs between the Crow and the Apache is the answer to *this* question: What *constitutes* an insult to the memory of the dead warrior? This question, the absolutist is likely to argue, is not itself a moral issue, but is rather an empirical question about the practices of the two tribes.

The absolutist does not believe that just *any* agreement between the two tribes is sufficient to show *moral* agreement. For consider that each tribe is likely to subscribe to the view that "one should do what is right." If not analytic, this judgment comes pretty close. Certainly, agreement on principles that are as abstract as *this* does nothing to support the thesis that the members of the two tribes share any important moral beliefs and would not help the absolutist in arguing against relativism. The claim of the absolutist about the Crow and the

Apache, rather, is a substantive claim. The absolutist claims that the disagreement between the two tribes is not about morality as such, but rather about a symbolic issue: What constitutes an insult? The latter kind of question is plainly conventional—bearing a strong resemblance to questions like "What utterance shall be taken to indicate the presence of a rabbit?"—and not substantial, like the question "What do rabbits eat?"

Who is right in this argument between relativists and absolutists? I am inclined to say that this story should make us very cautious about taking sides in this apparent debate. For whether the two sides are even arguing about the same thing is not at all clear. If the relativist contention is expressed as "what is right and what is wrong varies from culture to culture," then whether we are to assent to this proposition will depend entirely upon what we think can be properly substituted into this expression in place of the words "what is right and what is wrong." Is it "What *specific actions* are morally right when a warrior is lost"? Then the relativist seems to have the right answer. But perhaps it is "What *general principles* are the morally right ones when a warrior is lost"? If that is the case, then the absolutist may appear to come out right. Everything depends upon what level of analysis one deems appropriate for the application of specifically *moral* categories.

In the last analysis, relativists define themselves in terms of the fact that statements of fact and of value always have a context and are senseless without at least tacit reference to that context. Thus everyone who agrees with this thesis —that fact and value claims make no sense absent a context—is thought, by the relativist, to be a relativist. Absolutists, according to the relativist, must be people who deny this basic fact. Since the fact in question is nearly undeniable, relativists can only conclude that *everyone* is really a relativist, despite claims to the contrary. Thus, in particular, Amsterdamski's conclusion about the later work of Karl Popper.

But people who think of themselves as absolutists do *not* characteristically define themselves in this way. Absolutists are not only willing, but *eager* to acknowledge the importance of context in the making and understanding of fact and value claims. In our story about the Crow and the Apache, the absolutist would urge that we have a solid indication of a moral claim that both tribes agree to, namely, that dead warriors should be given respect and not insulted. How this works out in practice, though, depends upon context. In particular, it depends upon what constitutes insult in the two tribes. Similarly, absolutists might argue that the principle that innocent people should not be hurt for any reason at all might come close to an absolute value. But how this works out in practice will depend, at least, upon who is to be regarded as innocent and which reasons are to be regarded as providing adequate justification for inflicting harm. These issues may be settled differently in different contexts, and *some* of the differences, at least, may be due to factors that are not themselves specifically moral.

In the absolutist's view, the relativist's claim itself is clearly an attempted statement of objective fact. Thus, according to the absolutist, in order to make any claim at all, even the relativist must tacitly be a closet absolutist. The perennial question facing relativists is whether the fact that *they* stress most often in arguments with absolutists—that statements of fact and value are senseless without reference to the context in which they are uttered—is itself senseless without reference to context. Pronouncements of the relativist position often sound for all the world like pronouncements of *absolute* fact, rather than relative fact. It is possible consistently to maintain the view that "there are no objective truths and this isn't one either," but I must confess I just don't understand what kind of claim this is. Some would maintain that the function of fact and value claims, by their nature, is entirely rhetorical and that their implicit objective is to move people. But for the sake of which values, and in the face of what kind of reality, may such rhetorical efforts be made? Somewhere in this rhetorical haze must exist something upon which effort may be based—some context that is not itself mere rhetoric. I have no doubt that description—and especially explanation—of the context will necessarily be informed and motivated by the needs and interests of the describer or explainer. But this does not mean that the descriptions and explanations are sheer arbitrary acts of will, either individual or social. An environment, not entirely social (and certainly not entirely dependent upon need and interest), supports all fact and value claims. Indeed, such claims make no sense at all without tacit reference to that environment.

In order to make any claims at all, whether of value or of fact, we must presuppose that such claims are at least in some measure independent of the will of whoever makes the claim. They may not ever be *entirely* independent of the subject, and I would argue that they never *can* be.[5] But neither can they be entirely determined by the subject, whether the subject is conceived individually or as socially constituted. The extreme view that fact and value claims really *are* solely dependent upon the will and whim of the individual subject (or of society) leaves those subjects (or societies) floating in a vacuum. Since no one really believes that subjects and societies can exist in a vacuum, it follows, according to the absolutist, that no one is really a relativist, in spite of what some people might think. To be a relativist, on this absolutist understanding, is to be committed to incoherence.

Hence my initial claim: If the term "relativism" is understood in the way relativists take it, everyone is a relativist. If, on the other hand, one understands "relativism" in the way absolutists do, no one really *could* consistently be a relativist, despite what they might think.

Another misconception is to imagine that absolutists have some particular set of fact and value claims in mind when asserting objective truths. Absolutists include among their number the most uncommitted and uncertain people in the world. I don't need to know *what* the truth is in order to be an absolutist; all I need to believe is that my claims are about *something*, and that these claims are not mere expressions of will in a vacuum. Thus it may not be true in all cases that we should not insult dead warriors or hurt innocent people, but it will always be true that we are trying to determine something when we agonize over what we should do, morally speaking, and this "something" is patently *not* merely what our society or our subjective whims tell us to do. If what we *should* do were merely equivalent to what private inclination or social dictate suggested, there would be no moral agony. All we would have to do would be to consult our inclinations or take a poll.

The same story applies to cases where we are trying to figure out what is true and what is false. In all cases of fact and value claims, the absolutist is committed *not* necessarily to the claim that what is right or true is known or even knowable. The absolutist claim is only that what is right or true is *not* merely a function of private or social whim. Popper's claim, quoted by Amsterdamski, is worth repeating here:

> I do believe in absolute or objective truth in Tarski's sense (although I am of course not an absolutist in the sense of thinking that I, or anybody else, has the truth in his pocket).

So much for the very general discussion about absolutism versus relativism. I am inclined to urge that the argument between absolutists and relativists should simply be abandoned. For consider this proposition: "I believe that the relativist position, which contends that not only *explanatory* but also *descriptive* propositions and beliefs can only be understood when placed in cultural and historical context, is absolutely and universally true." We might think that affirming this proposition commits us to a contradiction, since being relativized in the indicated way to cultural and historical context might seem to conflict with being absolutely and universally true. But the quoted proposition is extremely tricky. It does *not* claim that *all* that is at stake in explanatory and descriptive propositions and beliefs is the relativizing context. It merely says that such propositions and beliefs require, if they are to be understood, being placed in that context. This is perfectly compatible with their being absolutely and universally true (at least after they have been clarified by *reference* to their context). A parallel to this in a less confusing setting is the proposition "I believe that the special theory of relativity, which says that statements about motion can be understood only when placed within a frame of reference, is absolutely and universally true." In the case of *both* of these quoted propositions, the belief in the absolute and universal truth of the theory being discussed is *not* necessarily undermined by the content of the theory. In the case of special relativity theory, this is because the theory does not apply at all to propositions of the quoted kind. In the case of relativism taken more generally, this is because relativism need not be committed to cultural and historical context as the *only* factor determining the

truth of explanations and descriptions (although of course it may, in which case it is in logical trouble).[6]

To see how this applies in the particular case of the debate between Popper and his critics, we need to get down to more specific issues.

3. Kuhn versus Popper

A major source of debate in philosophy of science over the past thirty years and more is the contention of some that competing scientific theories are typically incommensurable. Below, I will examine in some detail what this means, but on the face of it the incommensurability thesis suggests that, in spite of contrary appearances, we have no real way rigorously to test competing scientific theories against one another to see which one is right. Thomas Kuhn advanced this thesis in *The Structure of Scientific Revolutions*,[7] and that book, perhaps somewhat oddly, has continued to be at the center of the controversy over the years.

Although Kuhn's early support of the incommensurability thesis consisted primarily in giving historical examples of such things as the different ways in which a single term was used by competing theories, it became apparent in later work that he regarded such theories, especially in times of "crisis," to be incommensurable in principle.[8]

Kuhn seems to have based this view, in turn, on arguments of the sort marshaled long ago by W. V. O. Quine in support of his doctrine of the "indeterminacy of radical translation" (occasionally referred to hereafter as "Quine's RI thesis").[9] Incommensurability being thus defended, Kuhn deployed it to argue—against Popper—that reliance on rigorous experiment cannot be the factor that distinguishes scientific enterprises from non-scientific ones (like philosophy). Distinguishing factors like these—factors which make the difference between non-science and science—are called "criteria of demarcation." Kuhn argued that such criteria of demarcation between science and non-science cannot rely on comparison of theories in the light of experiment, since mere "comparability" is too vague, and commensurability—that is, rigorous test—is not available.

But despite the tone of much of the Kuhn/Popper literature, Kuhn was *not* led by all of this to abandon the quest for a demarcation criterion. No matter what others who are often associated with Kuhn may have been inclined to do, Kuhn himself tried to identify a *new* criterion of demarcation, and he found it in what he called "normal science." Science seems to have some mechanism—"normal science"—for ending squabbles between competing theories, while non-science does not.

This new demarcation criterion placed Kuhn in apparent conflict with Popper's view, according to which science can be distinguished from non-science by its use of a logically special sort of empirical test. As is well known, Popper—as against Kuhn—had said that when a scientist constructs an experiment with the intention of testing a theory, reliance is placed upon the logical mechanism of *modus tollens* to indicate when the theory is wrong. Popper went so far as to suggest that, in subjecting theories to rigorous attempts at disconfirmation, scientists are trying to find out whether they can refute their own theories.[10] Popper's reliance on *modus tollens* suggests that his demarcation criterion involves both the use of empirical test and logic. This is summed up in his term "falsifiability" (see *The Logic of Scientific Discovery*, especially ch. 4).

I should emphasize now that I do not mean to discuss all of the areas in which Kuhn and Popper disagree. I only note below (section 7) that I think Kuhn comes out on top in many of these peripheral areas. What I wish to concentrate on here is the demarcation issue, which I take to be central to the work of both philosophers. I argue that this conflict between Popper and Kuhn may in fact be only apparent.

In particular, I argue that if science has a mechanism for allowing "normal science," then Popper's work can be regarded as a quest for an understanding of that mechanism. Popper himself came to the conclusion that the mechanism is a science-wide (physics-wide, for example) commitment to a particular use of logic in empirical tests. This may not be correct, but Kuhn and his followers have never persuasively refuted the conclusion. A Popperian could very well take in stride Kuhn's historical work, and many of his stronger criticisms of early Popperianism, and make use of them in showing more precisely how the Popperian model works. Imre Lakatos, in fact, took some large first steps in this general direction.[11]

4. Indeterminacy of Radical Translation and Ontological Relativity

As I have suggested, Kuhn's argument for the incommensurability of two competing scientific theories appears to rely on considerations of the sort marshaled by Quine on behalf of what seems at first glance to be a more far-reaching thesis. Quine argues that any two languages are indeterminate with respect to one another in certain interesting ways. This is called the indeterminacy of radical translation, or "Quine's RI thesis."

Quine seems first to have shown that in making a translation of a foreigner's language, no one translation-scheme is "absolutely correct." There will surely be incorrect schemes, but several (in principle, an infinite number of) translation-schemes are likely to fit the data we have to work with. Further, some of these may lead to incompatible results in specific translations.[12]

For example, in trying to translate a foreign language without the aid of a manual, we may have chosen a particular foreign utterance as serving the same role in the foreign language as does "is the same as" in English. But another translation may take that utterance to be playing the role that "belongs with" plays in English. The situations in which the utterance is observed to be deployed may, after all, not give us any clear reason to prefer one translation over another.

We have no reason, Quine argues, to presuppose that one translation could be proven correct and the other incorrect, so long as each translation-scheme provides compensating adjustments to its translation of other relevant utterances in such a way as to frustrate our attempts at testing the two translations.[13]

Yet "is the same as" is patently different from "belongs with." So in such a case we would be forever uncertain as to what a foreigner using the expression "really" meant, in so far as no data—no part of the observed use of language in the foreign community—gives us decisive grounds for preferring one translation-scheme over the other. It's not that there is any serious problem with making good hypotheses—rather, no decisive proofs are available.

What the RI thesis does—at this first stage—is this: it shows us that the issue as to what a speaker of a foreign language means or intends (the fully defended RI thesis applies even to what the speaker refers to) in the use of short segments of his or her language is not resolved once and for all when we have developed a comprehensive translation-scheme for the language.

Let us call this "interlinguistic indeterminacy." The proviso about "short segments" is important, for technical reasons, in Quine's exposition of the RI thesis. That indeterminacy affects whole sets of translational or "analytic" hypotheses, however, is also Quine's doctrine. The difference is this: for sets of analytic hypotheses, we have indeterminacy of correct choice from among competing sets. For short segments, taken by themselves, we have an indeterminacy of their role in the language *vis-à-vis* other segments. Formulation of any consistent set of analytic hypotheses clears up the indeterminacy of short segments but leaves the indeterminacy of analytic hypotheses to be dealt with.

But the RI thesis does more than highlight only the important issue of interlinguistic indeterminancy, for the issue can be brought "closer to home." The inscrutability of meaning and reference over translation affects the foreigner in the learning of his or her own language, in having had no more data than the translator as regards what the individual native instructors mean or refer to in their use of the language. Indeed, in a sense he or she has less to use, since the adult translator possesses a personal background language and a mature perspective on language use, both of which may give hints at how the language is to be used, whereas a child learning the language does not have these hints. (This last point seems to have been the main issue in the controversy between Quine and Noam Chomsky over language learning.[14]) Thus, an intralinguistic indeterminacy is to be dealt with.

The interlinguistic indeterminacy is further complicated by what Quine calls "ontological relativity."[15] Discussions of what a term or phrase means, or refers to, make sense only in so far as they make use of a "background" language; that is, a language not subject, for the moment, to our worries about meaning and reference. For example, we can imagine a German child coming to use the word "Hase" correctly—that is, learning to use the word to refer to rabbits. In the same spirit, we can imagine an American child learning to use the word "rabbit" in referring to rabbits. But in both cases, our ability to imagine these things relies upon our understanding of the background language we use to describe the situations in question, a language that is not, for the moment, suspect; namely, the language occurring on either side of the terms enclosed in quotation marks. Specifically, our talk about the reference of "Hase" and "rabbit" takes the following form:

"Hase" refers to rabbits;
and "rabbit" refers to rabbits.

When we speak this way, we have to know what the (unquoted) term "rabbits" refers to in order to understand the reference of the quoted terms. Further, the obvious alternative explanations of the reference of these terms (for example, "'rabbit' refers to furry little animals"; or "'rabbit' refers to those things" [while pointing]) are subject to similar relativistic remarks, since the meaning or reference of these explanations is assumed uncritically. To the extent that it makes sense to ask ourselves what our own expressions mean or refer to, then, there is a kind of intrasubjective indeterminacy.

One point in my exposition-in-stages of the RI thesis and ontological relativity may need further clarification. What ontological relativity gives us is not another form of indeterminacy, but rather a relativization of determinacy. Determinacy is relativized, not narrowly to the object language, but to some other language—the metalanguage. Ontological relativity says that no matter how clear we are about the reference or meaning of some term or short expression (and the RI thesis tells us that the more "radical" the situation, the less determinate we can be), the determinations are never absolute. We "determine" meaning or reference only with respect to a background language.

Quine's work on the RI thesis and ontological relativity is important because it tells us some surprising things about language. Quine does not deny that we communicate; he just says that we cannot be determinate in our decisions as to what expressions mean or refer to taken one by one. He says, for example, that ". . . a word adequate to acknowledging red episodes could be drawn from any of various referential roles. . . ." The adequacy of accomplishing such acknowledgment is not denied. So it is with communication generally.[16]

But since we do nevertheless communicate, in spite of the fact that we cannot be fully determinate about the meaning or reference of single expressions, we are driven to this key conclusion: *Determinacy with respect to reference or meaning is just not necessary for communication.* This conclusion I take to be the most important implication of Quine's work in this area.

5. Reference of Terms and Truth of Scientific Theories

The problems discussed above, dealing with various kinds and degrees of indeterminacy of reference or meaning with regard to short expressions, has led Quine to the conclusion that there is nothing for translation to be true to.

We certainly have limits to our translational activity, such that some possible translations will be fairly universally regarded as unacceptable. But such limits are determined by translational "maxims," which are adopted without benefit of empirical content; that is, the maxims must be interpreted as conventions for translation, rather than as empirical hypotheses. An example of such a maxim is: "That translation is better which attributes fewer absurdities to the commonly held views of the language community." Plainly this is a convention rather than an empirical hypothesis: we have no access, independently of translations of native language, to the views of the community; thus we have no way of knowing, independently of translation, whether the community believes absurdities or not.

So Quine argues that we should renounce our attempts to figure out what terms "really" refer to, since otherwise we set ourselves to meaningless tasks. There is no truth of the matter. For the interlinguistic case, RI means we have no determinacy whatsoever apart from "conventional" maxims. For the intrasubjective case, we have only relative determinacy, due to ontological relativity. For the intralinguistic but intersubjective case, we seem to find elements of both RI and ontological relativity at work. In no case do we have an absolutely determinate answer to questions of meaning and reference. Ultimately, there is nothing to determine (part of Quine's argument is that the case is worse for meanings than for referents).

The question that immediately comes to mind is whether the same is true of theory in general, since translational hypotheses, when taken in sets which provide comprehensive guidance in translating any sentence or expression of the object language into an expression of the background language, form one kind of theory. The resemblance between ontological relativity and Tarski's truth-relativity is enticing and may make us suppose that Quine would believe on its grounds that there is nothing for physical theory—or any theory at all—ultimately to be true of.[17] This, it seems would be a mistake.

Quine himself appears to have gotten progressively clearer on this latest point. As Michael Gardner has shown,[18] the doctrine of RI has become progressively more precise—and more convincing—in its evolution from *Word and Object*, through "Ontological Relativity" and "On the Reasons for Indeterminacy of Translation." As Gardner observes, the doctrine seems originally to have arisen out of Quine's "Two Dogmas of Empiricism."[19] The doctrine of ontological relativity, while fairly clear at the outset, has been clarified a bit through its career in "Ontological Relativity," through the "Replies" in *Words and Objections*,[20] and through a note or two in "On the

Reasons for Indeterminacy of Translation." Quine's position at this stage clearly seems to be that conclusions drawn about the truth of theories in general are not in nearly so bad a predicament as are conclusions about the truth of translational theories.

In particular, Quine is not inclined to think of theory construction in general as being as conventional as is construction of translational theory. For him, the decisions that guide our choice of natural theories (from among those that adequately cover our experimental data) are guided by methodological rules and principles that are—much more than are the conventional maxims guiding translational decisions—themselves subject, to some extent, to empirical review.[21] I will not embark here on a critique of Quine's view, since it is peripheral to the discussion in this chapter. I note only that Quine's position has become relatively clear on the issue.

What I am particularly interested in is the use, in particular, of Quine's RI thesis in the work of Thomas Kuhn. It is to this matter that I finally return.

6. Kuhn on Incommensurability

Supposedly for reasons like those outlined in connection with Quine's thesis of the indeterminacy of radical translation, Kuhn contends that competing scientific theories are actually incommensurable.

Kuhn is not, of course, alone in advocating the incommensurability thesis. I have chosen to discuss Kuhn rather than Paul Feyerabend, for example, because of Kuhn's earlier prominence in the particular debates addressed here.[22]

Intranslatability and incommensurability are not always problems for science, according to Kuhn. His general view of science leads him to contend that scientists are normally involved with elaboration of a given theory—with widening its scope—rather than with attempting to refute the theory via experimentation. (Kuhn uses the term "paradigm" rather than "theory" in the book, although he "loses control" of "paradigm" by using it in too wide a variety of different ways.[23] His later work reflects, among other things, his attempts to differentiate between these various usages and is notable for his abandonment of "paradigm" in favor of a set of more explicit terms, like "theory" and "exemplar," to sort out the several meanings of the overworked term.[24]) The value of this "normal" science is to be found in its ability to push a theory to its explanatory limits: it gives a theory the leeway it needs to demonstrate how much it can do. Indeed, normal science (Kuhn refers to normal scientific activity as "puzzle-solving") can be regarded as valuable in that it gives meaning and power to a theory. The interpretation of a theory is not just taken for granted by scientists doing normal science; it evolves through such work. I'm not sure that Kuhn makes this point explicitly anywhere, but it is certainly in the spirit of *The Structure of Scientific Revolutions*.

Without normal science, we would not be sure about what a theory could not do. Furthermore, we would not gain the sophisticated interpretations characteristic of, for example, recent physical theory. The first point means that without normal science, our criticisms and "refutations" of theory would stand a greater risk of being inappropriate. The second point suggests that without normal science our progress would be slower, due to the decrease in power associated with a refutation of an unsophisticated theory as opposed to a sophisticated one.

But, according to Kuhn, it is in times of "crisis" in science—non-normal situations—that problems of intranslatability crop up. Different comprehensive theories of some broad domain represent, for Kuhn, different Gestalts. Different theories differ not only in how they explain the workings within their domain, but in what they take the domain to be.

Take, for example, the crisis in physics that led, ultimately, to a replacement of Newtonian dynamics with relativistic dynamics. It might be thought (indeed, it usually is) that relativistic dynamics is merely an ingenious extension or adjustment of Newtonian dynamics to a broadened domain within the world. It might be thought that such an extension or adjustment affects only a limited portion of the old theory, and that after the adjustment, the two theories still have a good deal in common. For example, we might say that Newtonian dynamics is a limiting case of relativistic dynamics; that it can be derived—as a limiting case—from relativistic dynamics. All of this might seem quite reasonable. Kuhn argues, however, that we would be wrong on every count.

Since this view of Kuhn's is central to his whole philosophy of science, and since most of what follows will be dealing with it, it is useful to quote Kuhn at some length:

> Can Newtonian dynamics really be derived from relativistic dynamics? What would such a derivation look like? Imagine a set of statements, E_1, E_2, . . . , E_n, which together embody the laws of relativity theory. These statements contain variables and parameters representing spatial position, time, rest mass, etc. From them, together with the apparatus of logic and mathematics, is deducible a whole set of further statements including some that can be checked by observation. To prove the adequacy of Newtonian dynamics as a special case, we must add to the E_i's additional statements, like (v/c)2<<1, restricting the range of the parameters and variables. This enlarged set of statements is then manipulated to yield a new set, N_1, N_2, . . . , N_m, which is identical in form with Newton's laws of motion, the law of gravity, and so on. Apparently Newtonian dynamics has been derived from Einsteinian, subject to a few limiting conditions.
>
> Yet the derivation is spurious, at least to this point. Though the N_i's are a special case of the laws of relativistic mechanics, they are not Newton's Laws. Or at least they are not unless those laws are reinterpreted

in a way that would have been impossible until after Einstein's work. The variables and parameters that in the Einsteinian E_i's represented spatial position, time, mass, etc., still occur in the N_i's; and they there still represent Einsteinian space, time, and mass. But the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.) Unless we change the definitions of the variables in the N_i's, the statements we have derived are not Newtonian. If we do change them, we cannot properly be said to have derived Newton's Laws, at least not in any sense of "derive" now generally recognized. . . . [T]he argument has . . . not done what it purported to do. It has not, that is, shown Newton's Laws to be a limiting case of Einstein's. For in the passage to the limit it is not only the forms of the laws that have changed. Simultaneously we have had to alter the fundamental structural elements of which the universe to which they apply is composed.[25]

There is thus a difference in the reference of the term "mass," for example, when it is used in Newtonian dynamics, and when it is used in relativistic dynamics. Other terms vary similarly, and each of them is accompanied by associated adjustments in meaning supposed by Kuhn to disrupt communication between theoretical opponents. The references differ, and, as we seem to have shown above, reference problems are inscrutable across translations. This, then, is how Quine's work is supposed to support Kuhn's incommensurability thesis.[26] (Quine's own view is that "[t]ranslatability is a flimsy notion, unfit to bear the weight of the theories of cultural incommensurability that Donald Davidson effectively and justly criticizes.[27])

The reader might feel a bit uncomfortable about this conclusion in light of what was said above. In section 4, we saw that Quine's point was that determinacy of reference or meaning is not necessary for communication, not that communication was somehow impossible.

Far from offering support to Kuhn's incommensurability thesis, Quine's work offers us an interesting way around it. To see this, consider what incommensurability comes to once we take Quine's RI thesis to heart. What is the relation of two competing theories that offer different interpretations of key terms like "mass"?

For one thing, it cannot be the case that any major misunderstanding exists between the proponents of the two theories. Even revolutionaries have, overwhelmingly, been trained in the "established" traditions they are attempting to modify or overthrow. Indeed, where a revolution involves change in the meanings of terms like "mass," the proposed new terminology is often at the center of the conflict, and the terminological debate is usually fairly clear to both

sides. Davidson has expressed the general point beautifully: "[Benjamin Lee] Whorf, wanting to demonstrate that Hopi incorporates a metaphysics so alien to ours that Hopi and English cannot, as he puts it, 'be calibrated,' uses English to convey the contents of sample Hopi sentences. Kuhn is brilliant at saying what things were like before the revolution using, what else?—our post-revolutionary idiom. Quine gives us a feel for the 'pre-individuative phase in the evolution of our conceptual scheme,' while Bergson tells us where we can go to get a view of a mountain undistorted by one or another provincial perspective."[28]

The problem in scientific disputes is, typically, one of deciding whether to *adopt* the new terminology. Surely the sense in which it is true that "alloys were compounds before Dalton, mixtures after" is a verbal sense.[29] Does Kuhn wish to argue that either John Dalton or his opponents—or both—were ignorant of the verbal character of this part of their dispute? Surely he does not. The issue was which usage would be more profitable in some systematic sense. Or perhaps, seeing that the problem was "merely" verbal, one side or the other simply dismissed counter-arguments by calling them "radical" (or "reactionary"). Whichever way, the decision does not seem to be involved with incommensurability, unless it is the "incommensurability" of the prospective profitableness of the two alternatives. Perhaps a better word would be "uncertainty."

Another possible interpretation can be placed upon Kuhn's use of "incommensurability." Different theories often make use of different instruments in making measurements of characteristics of the world essential to them. Where the same term is used in two theories to refer to different characteristics and when two conflicting theorists make measurements of the properties they each refer to by the common term, they are likely to come up with different values. Even where they refer to the same characteristic, the use of different instruments may result in different values.

We have here two problems. The first is the verbal one about the reference of a common term, which we have already dealt with. The second problem has to do with preferred instruments. Let us isolate this second problem by dealing only with the case where different instruments are used to measure the same property. In what sense now would the values be incommensurable? It is hard to say.

For one thing, two opposing scientists could trade instruments and the poles of the conflict would be reversed, nothing in the conflict therefore depending on the different biases of the two theorists. A conflict might still arise as to which measurement was right, but this would be a rather silly conflict. We might as well say that the scientists were both right. Finally, and most likely, they might argue as to which measurement was more valuable, say, in handling a crisis situation. The establishment scientist might argue that the use of the revolutionary's instrument, and the theory which brought it into being, were all fine and good for the short term "solution" of the crisis problem, but it would fail in the long run. The revolutionary would mutter something about the other theorist's being a reactionary, and the debate would go without resolution.

This is what Kuhn seems to be emphasizing in talking about incommensurability: the indecisiveness of such debates. The only thing not measurable is the value of the two theories in the long-run scientific enterprise. Again, "uncertainty" seems like a much more appropriate word.

If this is what Kuhn means, I think he is right, especially in the clarified version of his views to be found in "Reflections on My Critics." In spite of what Popper may say, "refutations" are not as clear as all that. What is one scientist's refutation is another's anomaly.

But if this is what Kuhn means, then there is a different emphasis to be placed on the idea that two theories are related to one another in the way that different Gestalts are related. The difference in emphasis seems to be this: Whereas in *The Structure of Scientific Revolutions* the "Aha!" of the scientist who finally becomes radicalized is like the "Aha!" of a religious convert, in Kuhn's later work it is more like that of a person who finally sees the practicality of an alternative strategy in, say, a chess game. Surely the two "Aha!'s" are very different, and surely the later version makes theory choice considerably more rational, and thus potentially more congenial to people like Popper. In particular, I would argue that this more modest understanding of "incommensurability," based on a correct understanding of the import of Quine's RI thesis, leaves the door open to a renewed effort to establish a rational criterion of demarcation between science and non-science.

7. Kuhn on Normal Science as Demarcation Criterion

Where Kuhn and Popper seem really to disagree on fundamentals is in regard to the value of normal science. Since Kuhn is very much aware of the uncertainty associated with experimental tests of theories—that is, with the Duhem-Quine thesis, which says that any theory can be hung on to, come what may, so long as adjustments are made in the "background" theory that was presupposed in performing the experiment in such a way as to account for whatever comes—he does not feel that he can use criteria such as Popper's to distinguish science from non-science. Popper had said that science could be recognized by its emphasis on empirical refutability in the formulation of its theories.[30] Since refutations don't come so easy, Kuhn feels he must search elsewhere for a demarcation criterion. He finds it in normal science.

There are paradigm conflicts in all fields, Kuhn observes, but these can't amount to revolutions where there is no establishment to revolt against. Furthermore, the absence of such an establishment seems to make progress impossible. I have already discussed the values of normal science above: normal science, Kuhn argues persuasively, is good.

Normal science is the result of the presence in the scientific enterprise of some mechanism that provides for relatively universal acceptance, in due time, of some paradigm. This mechanism and its result—normal science—sets physics apart from such fields as, for example, philosophy, and makes physics a science. Other "sciences," like biology, sociology, and the like, may possess such a mechanism to a greater or a lesser degree. Insofar as they possess the mechanism, they are more or less deserving of the honorific name "science."

But doesn't the presence of the kind of mechanism discussed above indicate, in the case of physics, for example, the adherence of physicists in general to something very like a rather broad super-paradigm? Even in times of revolution, the "mechanism" that makes physics a science is at work to ensure that disagreements among physicists eventually *end,* specifically in a conclusion about which paradigm to use in future work. How else are we to understand talk of the presence of a mechanism in an enterprise, except in terms of something like a super-paradigm?[31] If I am right, couldn't the work of philosophers like Popper be regarded as attempts to characterize this very broad paradigm . . . to get at the distinguishing features of science . . . to clear up the demarcation issue?

Now, it may be that Popper is wrong in his characterization of this mechanism, but Kuhn often seems to be suggesting he is wrong even to look for it. This is hard to understand. Whatever the mechanism may be, it seems reasonable to look for it among the standards used for testing hypotheses—not the concrete standards, for they change, as Kuhn emphasizes. But it does not seem strange to search among logical standards, as Popper does.

To settle on a certain standard of testing—a logical standard—has some obvious limitations: (1) the Duhem-Quine thesis shows that tests are not decisive as to where, as Imre Lakatos puts it, the "arrow of modus tollens" gets directed;[32] (2) thus it is not necessary to abandon the theory that was thought to be undergoing the test, simply because of negative results; (3) where the arrow is taken to point will reflect the commitment of the experimenter (or interpreter of the experiment) to the practical value of one or another "research programme"—another Lakatosianism—and there seem to be no rigid standards for such commitment; (4) thus Popper seems not to have been clear enough about the "indeterminacy of *ad-hoc*ness," which is a major point in Kuhn's criticism of his work. According to Lakatos, there are several different Poppers who collaborated on *The Logic of Scientific Discovery*, so this is not surprising;[33] and finally, (5) although Lakatos shifts the ground to a consideration of degenerating as opposed to progressive research programs, he sees that there is a kind of "indeterminacy of degeneracy" at the time of a crisis, thus scoring another point in behalf of Kuhn.

But these several indeterminacies are not unique to science. To speak rather vaguely myself, they are elements of non-science congenitally present in science. They exist alongside the logic of testing within the enterprise of science. And the logic of tests doesn't seem like a bad candidate at all for a criterion of demarcation.

To be sure, Kuhn doesn't ignore the quest for the mechanism responsible for normal science. One thing he suggests is that the rigid educational process in the sciences (in physics, in particular), in which conformity is emphasized, is largely responsible for the existence of normal science. Surely this is the case. Only those students who accept the criterion of testing, among other things, will receive the degree necessary to becoming a working scientist. But let us look at those "other things."

We must abstract from the logic of tests, since that is Popper's criterion, with which Kuhn disagrees. We are left with dogma. Kuhn occasionally appears to favor dogma as the key element in making scientific education truly scientific. Now, there is nothing inherently wrong with dogma. Many of the advantages of normal science noted above are closely related to adherence to dogma. But the teaching of dogma—and the refusal of a degree to those who do not learn and accept that dogma—are not unique features of science. Other examples of such a policy can be found among the theological schools and, it seems, among certain schools of political thought. So the teaching of a rigid dogma does not seem adequate, by itself, as a demarcation criterion. But when we add to the insistence upon acceptance of dogma the logic of tests, which serves to some extent to control that dogma, we have a rather respectable demarcation criterion (or set of criteria).

8. Conclusion

The work of Erwin Schrödinger and Louis de Broglie in twentieth-century physics is different enough to warrant characterizing the two men as proponents of competing paradigms (or research programs). Still, the work of each man has modified and shaped the growth of physics. Contemporary quantum mechanics makes heavy use of the work of each of them.

This kind of dynamic is characteristic of the growth—or movement, anyway, depending on which author one reads—of science, as Kuhn, Lakatos, and Popper have all shown. But sometimes we can find that same phenomenon in non-scientific fields.

In particular, Kuhn may be regarded as having provided the impetus for progression in the Popperian program by introducing to the dialogue between Popperians and non-Popperians a variety of considerations that Popper had ignored or underemphasized. This progress was provided by Lakatos and by Kuhn's criticism of Lakatos.

Further, if read through broadly Popperian spectacles, Kuhn might be regarded as having provided, to some extent, the Popperian solutions to some of the problems he (Kuhn) raised. What we are left with is a view of science that is broadly fallibilist, and within which Popper's logic of tests plays a critical role.

These are Popper's contributions. We do not, however, always see in science the emphasis Popper placed on actively trying to refute theory. This is often an inadvisable tactic, and we will see it most often only during explicitly "revolutionary" periods, when it will be deployed in critique of the positions of one's opponents. Against the early Popper, we acknowledge the value of normal science; that is, the positive value of a strong "conservative" scientific community in which revolutions may find their footing, and in which theories may be given the leeway to fulfill whatever explanatory potential they may have. This is Kuhn's contribution. In times of crisis, we see competing research programs, the characteristic details of which have been sketched in largely by Lakatos. That is his important role, so long as Kuhn is there to remind us of the importance of Lakatos's acknowledgment that competing programs can be evaluated as degenerating or progressive only in retrospect.

The whole enterprise is uncertain, and thus fallible. Where alleged refutations are rejected by proponents of a theory, they may not however be dismissed out of hand. Adjustments must be made somewhere to accommodate the demands of the logic of tests. In such adjustments can be found the complex potential for growth in science, a potential that provides no guarantees but which offers humankind its best shot at better understanding the world.

Insofar as all of this is generated—and accepted—as a means of increasing our understanding of a world which is, in the end, not fully dependent upon us, however, it comes considerably closer to expressing a moderated absolutism than a softened relativism, thus somewhat closer to Popper than to Kuhn.

Notes

1. An earlier version of this chapter was read and discussed at a conference on "Criticism and Defense of Rationality in Contemporary Philosophy" held in Radziejowice, Poland, under the auspices of the Institute of Philosophy and Sociology of the Polish Academy of Sciences. I am grateful to the other participants in that meeting —especially to Stefan Amsterdamski, Barbara Tuchańska, and Wolfgang Welsch—for their warmly critical response. They helped considerably in directing my attention to portions of my paper which were confusing. I am also grateful to George Romanos for his helpful comments on an ancestor of this paper many years ago. Subsequent descendants were discussed at a symposium on "Philosophy and Natural Science," Rochester Institute of Technology, November 1988, at State University of New York College at Geneseo, February 1979, and at a Symposium on Methodology in the Social Sciences, University of Delaware, November 1977. I owe much to the participants in these affairs for helping me to make my point more clearly. Finally, I am grateful for the support of Fulbright Scholar Award #95-65079, of a Rotary International Grant, of the Rochester Institute of Technology, and of the Graduate School for Social Research at the Polish Academy of Sciences during the preparation of the final draft.

2. Stefan Amsterdamski, "Between Relativism and Absolutism: The Popperian Ideal of Knowledge," in Stefan Amsterdamski, ed., *The Significance of Popper's Thought*,

Poznań Studies in the Philosophy of Science and the Humanities, vol. 49 (Amsterdam and Atlanta, Ga.: Rodopi, 1996), pp. 59-71.

3. For further discussion of this charming example, see Patrick Grim, *Ethical Relativism in the Context of the Social Sciences*, unpublished Ph.D. dissertation, Boston University, 1976.

4. See Amsterdamski, "Between Relativism and Absolutism." Karl R. Popper, "Normal Science and Its Dangers," in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, England: Cambridge University Press, 1970), p. 56.

5. See especially John T. Sanders, "Affordances: An Ecological Approach to First Philosophy," in Honi Haber and Gail Weiss, eds., *Perspectives on Embodiment: The Intersection of Nature and Culture* (New York: Routledge, 1997), and Sanders, "Merleau-Ponty, Reality, and Berkeley's God," in Lawrence Hass and Dorothea Olkowski, eds., *Rereading Merleau-Ponty: Essays Beyond the Continental-Analytic Divide* (Atlantic Highlands, N.J.: Humanities Press, 1997). See also Sanders, "Merleau-Ponty, Gibson, and the Materiality of Meaning," *Man and World* (July 1993), and Sanders, "Merleau-Ponty on Meaning, Materiality, and Structure," *The Journal of the British Society for Phenomenology* (January 1994).

6. I am especially grateful to Barbara Tuchańska and Wolfgang Welsch for fruitful discussion of this issue.

7. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

8. Thomas S. Kuhn, "Reflections on My Critics," in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge*, pp. 231-278.

9. Willard V. O. Quine, *Word and Object* (Cambridge, Mass.: MIT Press, 1960); see also Quine, *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969), pp. 26-68, and Quine, "On the Reasons for Indeterminacy of Translation," *Journal of Philosophy*, 67 (1970).

10. Karl R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson & Co., 1959).

11. Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge*, pp. 91-196.

12. Quine, *Word and Object*, esp. pp. 68-76.

13. Quine, *Ontological Relativity*, p. 33.

14. See, *e.g.*, *Synthèse*, 19:1/2 (December 1968), pp. 53-68, 274-283.

15. Quine, *Ontological Relativity*.

16. See, *e.g.*, W. V. O. Quine, *The Roots of Reference* (LaSalle, Ill.: Open Court, 1973), p. 83. See also Quine, "Communication," in his *Quiddities: An Intermittently Philosophical Dictionary* (Cambridge, England: Harvard University Press, 1987), pp. 27-29.

17. Alfred Tarski, "The Semantic Conception of Truth," in *Philosophy and Phenomenological Research*, 4 (1943), pp. 341-375. *Cf.* Quine, *Ontological Relativity*, pp. 67-68.

18. Michael R. Gardner, "Apparent Conflicts Between Quine's Indeterminacy Thesis and His Philosophy of Science," *British Journal for the Philosophy of Science*, 24:4 (December 1973), pp. 381-393.

19. W. V. O. Quine, "Two Dogmas of Empiricism," in his *From a Logical Point of View* (New York: Harper & Row, 1953), pp. 20-46.

20. Quine's *Words and Objections* was a re-publication of the *Synthèse* issue cited earlier.

21. See, *e.g.*, Quine, *Word and Object*, pp. 75-76.

22. See Paul K. Feyerabend, "Explanation, Reduction, and Empiricism," in *Minnesota Studies in the Philosophy of Science*, University of Minnesota Press, vol. 3 (1962), pp. 28-97. See also Feyerabend, "Consolations for the Specialist," in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge*, pp. 197-230. For an interesting discussion of incommensurability and incomparability, especially in their historical roles, see P. T. Sagal, "Incommensurability Then and Now," *Zeitschrift für allgemeine Wissenschaftstheorie*, 3:2 (1972).

23. Margaret Masterman, "The Nature of a Paradigm," in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge*, pp. 59-89.

24. Thomas Kuhn, "Logic of Discovery or Psychology of Research?" and "Reflections on my Critics," in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge*.

25. Kuhn, *The Structure of Scientific Revolutions*, pp. 101-102.

26. For Kuhn's most explicit reference to Quine's RI thesis in his early work, see Thomas Kuhn, "Reflections on My Critics," in *The Structure of Scientific Revolutions*, p. 268.

27. See W. V. O. Quine, "On the Very Idea of a Third Dogma," in his *Theories and Things* (Cambridge, Mass.: Harvard University Press, 1981). See also Donald Davidson, "On the Very Idea of a Conceptual Scheme," reprinted in his *Truth and Interpretation* (Oxford: Clarendon Press, 1984). Among the theories criticized by Davidson was a rather uncharitable interpretation of Kuhn's theory.

28. Davidson, "On the Very Idea of a Conceptual Scheme," pp. 184, 196-197. In addition, Davidson observes that meaningful disagreement is possible only if there exists some substantial foundation of *agreement* among interlocutors (pp. 196-197).

29. Kuhn, "Reflections on My Critics," p. 269.

30. Karl Popper, *The Logic of Scientific Discovery*. For Popper's response to Kuhn's views, see Popper, "Normal Science and Its Dangers," in Lakatos and Musgrave, eds., *Criticism and the Growth of Knowledge*, pp. 51-58.

31. The same line of reasoning has been developed by Amsterdamski. See Stefan Amsterdamski, *Between History and Method: Disputes about the Rationality of Science* (Dordrecht: Kluwer Academic Publishers, 1992).

32. Lakatos, "Falsification and the Methodology of Scientific Research Programmes."

33. See Imre Lakatos, "Criticism and the Methodology of Scientific Research Programmes," *Meeting of the Aristotelian Society* (28 October 1968), pp. 149-186.