

Lewis and Cavell on Ordinary Language and Academic Philosophy*

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... sed tandem aliquando humanum genus, refrenata sectarum libidine (quam inanis novandi gloria stimulat), constitutis certis dogmatibus, inoffenso pede non in Philosophia minus quam in Mathesi ad ulteriora progrediatur.

— Leibniz

In many respects it is natural to view David Lewis as a successor to Carnap. The most striking feature of Lewis’s metaphysics, for example, namely what he calls “extreme modal realism” (which is in fact an extreme modal nominalism), amounts to a reduction of all objects to a common basis. It first appears, moreover, with the function of getting that reduction past the seemingly insuperable obstacle of dispositional predicates, i.e., of counterfactuals. In a similar vein, Lewis’s mereological interpretation both of set theory and of the universal–particular relation are the reflexes of Carnap’s attempt to carry out this reduction using the smallest possible number of ascension forms (subject to Goodman’s suggestion that we try mereology rather than set theory).

Another, perhaps subtler, point on which Lewis agrees with Carnap is on the possibility of an institution whose purpose is to seek truth. According to Carnap, “the goal of science consists in this: to find and to order the true statements about the objects of knowledge” (Carnap, 1974, §179,

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p. 252). Lewis agrees, and adds that, for the most part, every department of a university in good order — a “lucky” university — would be in that sense scientific. He also adds, however, that, generally speaking, the members of a department will not agree on what is true. Hence, in most departments, the truth will be sought via dispute:

Not perhaps [in] the department of frenchified literary theory, where skepticism runs rampant and the pursuit of truth is reckoned passé. Not perhaps [in] the mathematics department, where they are in confident agreement about what’s true and how to tell, and they disagree only about what’s fruitful and interesting. But in most departments, as in philosophy, (1) the advancement of knowledge is the agreed aim; but (2) there are prolonged disputes over what’s true. (Lewis, 2000a, 5)

Note that the department of philosophy is explicitly included.

There is and has been some controversy as to whether actual philosophy departments make progress toward truth. Indeed, there is controversy in this respect even about physical science. When Lewis himself was hired by the Princeton philosophy department, in 1970, one of its (decidedly unfrenchified) members was the man who had written this:

These last paragraphs ... show that a sort of progress will inevitably characterize the scientific enterprise so long as such an enterprise survives. In science there need not be progress of another sort. We may, to be more precise, have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth. (Kuhn, 1996, 170)

Kuhn reaches this conclusion, moreover, by examining the way scientific disputes are forced to end. A science has “matured,” according to him, only when its practitioners end their prolonged interschool debates and settle into the consensus brought about by a common paradigm.

Lewis never, to my knowledge, refers to Kuhn in his writings. In the case of philosophy, however, he faces an attack from closer quarters. For the disputes which Lewis everywhere describes as characterizing philosophy concern, in Bargle’s phrase, “debates over ontic parsimony” (Lewis and Lewis,

1983, p. 9),¹ or, in Quine’s phrase, about “what to admit to the universe of values of our variables of quantification” (Quine, 1960, 243). But these are questions of the type Carnap would later call “external,” and about which, early and late, he always maintained two things. First, that they are not questions at all in the “strict logical sense,” in which “the posing of a question consists in this, that a statement is given and the task is posed, to establish either this statement itself or its negation as true” (Carnap, 1974, §180, p. 254). Second, that, therefore, although answers to such questions may legitimately be used to express an “emotional and practical attitude” (*gefühl- und willensmäßige Einstellung*), we fall into delusion as soon as we treat them as true or false, and hence as proper subjects for *dispute*:

The metaphysician believes himself to move within the realm in which true and false are in question [*in dem es um wahr und falsch geht*]. In actuality, however, he has said nothing, but only brought something to expression, like an artist. We cannot conclude that the metaphysician finds himself in this delusion simply from the fact that he adopts speech as medium of expression and declarative sentences [*Aussagesätze*] as form of expression; for the lyric poet does the same, without thereby lying under this delusion. But the metaphysician produces arguments for his propositions [*Sätze*]; he demands agreement with their content; he polemicizes against the metaphysicians of other movements, in that he seeks to refute their propositions in his treatise. (Carnap, 1931, p. 240)

Carnap thus repeats, in a more radical register, Kant’s diagnosis of metaphysics: it contains antinomies, that is, contradictions in the law, that is, disputes in which the recognized procedures of argument do not produce agreement, because its practitioners are subject to an illusion of meaning something when they do not.

Lewis, of course, disagrees. But his most emphatic statement of that comes in the midst of *agreeing* with Carnap about the premise that there are, in general, no procedures for reaching agreement in ontological disputes:

If you say flatly that there is no god, and I say that there are countless gods but none of them are our worldmates, then it may be that neither of us is making any mistake of method. We may

¹David Lewis co-authored this paper with Stephanie Lewis. I assume, based on the final footnote, that both authors stand behind all of it.

each be bringing our opinions to equilibrium in the most careful possible way, taking account of all the arguments, distinctions, and counterexamples. But one of us, at least, is making a mistake of fact. Which one is wrong depends on what there is. (Lewis, 1983a, p. xi)

So for Lewis to maintain his position he must claim that it is rational to hold beliefs, and rational to *dispute*, about matters in which dispute is not a path to agreement.

Hence the connection to Cavell. Part III of *The Claim of Reason* begins with, and largely centers around, Cavell's criticism of "two assumptions, one about the nature of rationality and one about the nature of moral argument":

The first is the assumption that the rationality of an argument depends upon its leading from premises all parties accept, in steps all can follow, to an agreement upon a conclusion which all must accept. The second assumption is that the goal of a moral argument is agreement upon some conclusion, in particular, a conclusion concerning what ought to be done. (1979, 254)

The topic is moral, rather than ontological, argument. But the two cases are closely related. At the time of the *Aufbau*, Carnap still regarded moral questions as in a sense empirical and therefore scientific, but he soon adopted the view that they, too, are pseudoquestions.² This is the view, later adopted by Ayer and, following Ayer, by Stevenson, that Cavell confronts first of all: the view that disagreements in science are "disagreements in belief," whereas disagreements in ethics are "disagreements in attitude" (Cavell, 1979, p. 259, citing Stevenson, 1944, p. 7). The position is supported, Cavell says, by the fact that moral arguments are "always, and dishearteningly" liable to end in a mere "stalemate," in which "the questions which prompted the argument [are] either left without answer or with incompatible answers which any further argument would seem helpless to resolve" (Cavell, 1979, 247).

If it is natural to regard Lewis as a successor to Carnap, however, it is not so natural to regard him as an alternative to Cavell. Explicit engagement between the two, as far as I know, comes down to a single passage in Lewis's *Convention*:

²See (1974, §152, pp. 203–4) and cf. (1931, 237).

Here is a vindication of sorts for Stanley Cavell's doctrine that a native speaker has no need of evidence to justify what he says about what he would say. . . . [But] not because his knowledge of what we would say is not real knowledge, as Cavell seems to think when he says, "the native speaker can rely on his own nose; if not, there would be nothing to count." For the man who says what we would say is *not* just speaking for himself.³

Whatever Cavell means in the quoted passage, Lewis's response can hardly be adequate: if there is one point that Cavell makes clearly in the essay in question, it is that the native speaker does not speak for himself:⁴ "The primary fact of natural language is that it is something spoken, spoken together. Talking together is acting together" (Cavell, 1976b, 33). There are also a few places where Lewis apparently aims at ordinary language philosophy in general. Most importantly:

It might be otherwise if, as some philosophers seem to think, we had a sharp line between "linguistic intuition," which must be taken as unchallengeable evidence, and philosophical theory, which must at all costs fit this evidence. . . . But, whatever may be said for foundationalism in other subjects, this foundationalist theory of philosophical knowledge seems ill-founded in the extreme. Our "intuitions" are simply opinions; our philosophical theories are the same. (Lewis, 1983a, x)

The target of these remarks is not Cavell or Austin or Wittgenstein, however — not under any reasonable interpretation. We can no more read Lewis's relation to these thinkers out of them than we could read his relation to Derrida out of his dismissive description of "frenchified literary theory."

³Lewis, 1969, 62. The quote from Cavell is from Cavell, 1976b, 4.

⁴Or herself. But Lewis's (and Cavell's and Kuhn's) use of "man" and of masculine pronouns can't simply be corrected after the fact, as we might update the spelling and punctuation in an edition of Locke. These forms have always been double edged in English — ever since it lost a complete system of grammatical gender, at least. But, in any case, we are talking about pieces published in the late 1960's. Could it really be that nothing yet felt at all strange about this? How should we hear the following: "if we were, say, making statements . . . about the housewife's understanding of political slogans" (Cavell, 1976b, 5)? Are or are not the words "the housewife's understanding of political slogans" spoken in Cavell's own voice? Should they be corrected? How? (Certainly not by changing "housewife" to "stay at home dad.") If some of our currency bears the faces of slave owners, then we will pay with (and for) that.

Lewis's explicit statements are no help, then, and we may guess that he didn't intend them to be. He is in general very selective in the views he chooses to criticize or to compare with his own. The same can be said about Cavell, of course, who never, as far as I know, takes any notice of Lewis's work.⁵ Being thus left in the lurch by the principals, we will need to reconstruct a relationship between them from what they each say about topics of common interest.

1 Context

The principle of ordinary language philosophy — the one thing that distinguishes it from a vaguer philosophical appeal to common sense — is the principle of ordinary *context*: that “the profoundest as well as the most superficial questions can be understood only when they have been placed in their natural environments” (Cavell, 1976b, 41). This principle — that what is said, and can *possibly* be said, by means of given words depends on the context in which they are to be said — is also central to Lewis's thought. I have pointed out above that so-called modal realism first appears in Lewis as providing, in the terms of the *Aufbau*, a system-form which will allow the reduction of counterfactuals to indicatives. Equally important, however, is the way this reduction allows Lewis to deal with the dependence of the truth of a counterfactual on the context in which it is asserted.

The reason, first of all, that counterfactual consequence can't be truth-functional, is that a counterfactual conditional is normally asserted only in contexts in which both the antecedent and the consequent are false.⁶ If the counterfactual utterance can be used to assert anything at all, then, the context in which it is asserted must function to determine certain *other* contexts at which the antecedent and the consequent are to be evaluated: namely, certain contexts in which (a) the antecedent (or some appropriate counterpart to it) is true, but in which (b) actual background conditions and general principles more or less continue to hold. To assert the counterfactual

⁵I once asked Cavell in person what he thought of Lewis, and in reply he said something about time constraints which prevent us from reading everything.

⁶Since counterfactuals, like other statements, can be written or printed or recorded or otherwise stored for retrieval, and can also be automatically produced, there may not be just one context in which a given counterfactual is asserted, and it may not be easy to say in which context(s) it is asserted. Both Lewis and Cavell sometimes note the inscribability of language, and Cavell (following Thoreau) sometimes focuses on it as a theme, but even so it does not loom for them as it does for Derrida.

is to claim that (c) (some appropriate counterpart to) the consequent is true in all those contexts.

It is difficult to specify what kinds of background conditions and general principles are involved in (b), or to explain precisely the ranking implied by “more or less”: Carnap–Hempel style worries about the non-truth-functional nature of counterfactual implication lead directly to Quine–Goodman style worries about the relevant conditions and lawlike connection that license counterfactual inference. In addition to these questions, however (about which Lewis has much to say), there are also questions about the way specific contexts of assertion determine specific contexts of evaluation: in particular, about how the context of assertion serves to adjust the “more or less” parameter in (b). The combination of (a) and (c) means that a counterfactual is normally threatened on two sides by that adjustment: too far towards “more,” and there may be no contexts left in which (an appropriate counterpart to) the antecedent is true; too far towards “less,” and new contexts become available in which (an appropriate counterpart to) the consequent is false.

Though these are both equally threats to the assertibility of the counterfactual, Lewis, in his initial discussion of this issue, describes them quite differently, namely as the threat, on the one hand, of vacuous truth, and, on the other hand, of falsehood. It would be worth trying to understand better what motivates that distinction (and to ask whether Cavell recognizes any analog to it), but I will not pursue that here. The main point is this: if there are contexts in which the counterfactual is to be asserted and others in which it is not, then the context of assertion must, generally speaking, serve to determine how the “more or less” is to be adjusted, or, as Lewis puts it, how “strictly” the conditional is to be taken (where the “less” side is *stricter* — greater risk, so to speak, of falsehood).

Lewis first notes that the relevant features of the context of assertion can’t be ones that remain fixed as long as, for example, the same speaker continues to speak in the same place at (roughly) the same time. A statement such as “If Otto had come, it would have been a lively party; but if both Otto and Anna had come it would have been a dreary party,” might be non-vacuously true. But the non-vacuous truth of the first conjunct requires that (an appropriate counterpart to) “The party is lively” be true in all relevant contexts in which (an appropriate counterpart to) “Otto comes” is true. If we keep the criteria of relevance (and the counterpart relation) constant, then, and assume that no party can be both lively and dreary, the

second conjunct will come out false. Accordingly, Lewis concludes that, if we say that counterfactuals are “vague strict conditionals” whose strictness, and therefore truth or falsehood, depends on the context of assertion, we must mean that “the vagueness is resolved — the strictness is fixed — by very local context: the antecedent itself” (Lewis, 1973, 13). “That is not altogether wrong,” he continues,

but it is defeatist. It consigns to the wastebasket of contextually resolved vagueness something much more amenable to systematic analysis than most of the rest of the mess in the wastebasket. (Ibid.)

He then proceeds to introduce his own view, according to which a counterfactual is a “variable strict conditional,” one which “is as strict, within limits, as it must be to escape vacuity, and no stricter” (Ibid.).

Later, however, this systematic analysis expands to include the whole wastebasket and more. The rule that strictness changes to prevent vacuity is of the general type that Lewis later calls a “rule of accommodation,” namely, a rule according to which “conversational score [tends] to evolve in such a way as is required in order to make whatever occurs count as correct play” (Lewis, 1983d, 240). The index that sets the strictness of counterfactual conditionals is only one component among countless others that make up the “conversational score,” and non-vacuity of counterfactuals is only one among the many criteria which determine whether conversational “play” is correct. Among other things, the analysis now covers vagueness in general — the entire “wastebasket” of cases in which the strictness of some semantic standard or other must vary with circumstances. *Must* vary: in what sense of “must”? Lewis is explicit:

If Fred is a borderline case of baldness, the sentence “Fred is bald” may have no determinate truth value. Whether it is true or not depends on where you draw the line. . . . We cannot pick a delineation once and for all (not if we are interested in ordinary language). (Ibid., 244)

Needless to say, we are all interested in ordinary language. We all have an interest in it, that is, are all parties to the convention that constitutes it. Lewis is saying that we cannot avoid vagueness on pain of breaking that convention, that is, failing to speak at all: we must mean what we say. At

this point, then, Lewis declares himself to be a kind of ordinary language philosopher.

True, at this very point he also invokes Austin, and in a way which emphasizes the differences between them. “Austin’s ‘France is hexagonal,’” he writes, “is a good example of a sentence which is true enough for many contexts, but not true enough for many others” (Ibid., 245), where “true enough” has already been explained as meaning: “true under a large enough part of the range of delineations of its vagueness” (Ibid., 244). Whereas what Austin wrote about the example was this:

Suppose that we confront ‘France is hexagonal’ with the facts, in this case, I suppose, with France, is it true or false? Well, if you like, up to a point; of course I can see what you mean by saying that it is true for certain intents and purposes.... But then someone says: ‘But is it true or is it false?’ ... How can one answer this question, whether it is true or false that France is hexagonal? It is just rough, and that is the right and final answer to the question of the relation of ‘France is hexagonal’ to France. It is a rough description; it is not a true or a false one. (Austin, 1975, 143)

So Austin and Lewis agree that, first, if someone asks whether France is hexagonal, the right answer, in some contexts, is “yes,” and, in others, “no”; second, that if they ask “Is ‘France is hexagonal’ true?”, the right answer is that it is true enough for certain purposes but not enough for others; third, that if they continue: “But is it true or false?”, the right answer is that it is neither. Lewis and Austin disagree, not over whether this last answer is right, but over whether it is, as Austin puts it, right and *final*. If our interlocutor continues to press: but must not every meaningful declarative sentence express a proposition which is either true or false?, then Austin will reply that, no, that is not what we say, whereas Lewis shifts to a new context — call it a semanticist’s context — in which he can say that “France is hexagonal” expresses different propositions depending on a certain index of strictness, the value of which depends on the context of assertion. Hence its lack of a determinate truth value is no more (or less) mysterious, betrays no greater (or smaller) breach between ordinary language and formal semantics, than does the lack of determinate truth value due to an explicit indexical like “now” or “this.” Statements such as “‘France is hexagonal’ is either true or

false (one or the other, not both or neither),” which in the old context were false, are true in the new context, the semanticist’s context.⁷

The divergence between Lewis and Austin is clear enough, then. When we turn to Cavell and to Cavell’s Wittgenstein, however, the lines become less clear. The major difference that Cavell identifies between Austin and Wittgenstein is that

for Wittgenstein, while not for Austin, the title of his philosophy might equally well be the philosophy of metaphysical language. He has as fully worked out a theory of how language becomes metaphysical as he does of how language becomes ordinary, that is, of what is acquired in acquiring language. (Cavell, 1994, 6–7)

Austin has no explanation beyond the wile of the metaphysician as to how an ordinary question like “But how do you know?”, in just a few steps, sprouts into a threat to our knowledge of an external world. But Wittgenstein, according to Cavell, has two theories, each as fully worked out as the other: a theory as to what is acquired in learning to use such an ordinary question, and a theory as to why just that acquisition leaves us inclined to make the question “metaphysical” — where, for Cavell, “metaphysics” is always traced back to Descartes and no further, and to Descartes read in a particular way, as beginning with and responding to a threat of skepticism.

The second theory, in other words, is to explain how the same constraints of ordinary language at work in ordinary conversation — we *must* say, *must* mean, *must* respond, *must* admit — dictate, once a special context has been established, an inevitable march to the metaphysician’s skeptical conclusion:

The philosopher’s conclusion seems . . . to be right, and indeed to be deeper than our everyday, average ideas. . . . The convincingness of the conclusion [depends] upon its proceeding, or seeming to proceed, in just the ordinary way any ordinary person must

⁷It may be confusing here that I am discussing two different instances of dependence on context together. The semanticist’s context is a special context in which it can be truly asserted that (a) “‘France is hexagonal’ must always be either true or false.” In explanation of this, the semanticist then introduces the question of the context in which it is to be asserted that (b) “France is hexagonal.” In Austin’s context, in which (a) cannot be truly asserted, the context of assertion of (b) may be important for various reasons, but not, obviously, because it establishes a determinate truth value for (b): (b) is just rough, and that is the right and final answer.

(grammatically) proceed to establish a claim to know of something's existence. But the methods any competent speaker and actor would use to establish ordinary claims seem, in the hands of the philosopher, to establish the inferiority or weakness of those very ordinary claims themselves. (Cavell, 1979, 165)

And precisely such a theory is also found in Lewis. An ordinary "might" sentence, "It might be that ϕ ," is true (this is what we will say in the semanticist's context) if ϕ evaluates to true at some world within a certain range, where the boundary of that range moves in response to a rule of accommodation. The apparent success of the skeptical argument, Lewis explains, is due to the force of that ordinary rule:

The commonsensical epistemologist says: "I *know* the cat is in the carton — there he is before my eyes — I just *can't* be wrong about that!" The skeptic replies: "You might be the victim of a deceiving demon." Thereby he brings into consideration possibilities hitherto ignored, else what he says would be false. The boundary shifts outward so that what he says is true. Once the boundary is shifted, the commonsensical epistemologist must concede defeat. (Lewis, 1983d, 247)

If, moreover, the skeptic's conclusion not only seems to be correct in a special context, but, as Cavell puts it, "seems . . . to be deeper than our everyday, average ideas," that, too, is thanks to the operation of ordinary language constraints. There is, Lewis explains, an asymmetry to certain rules of accommodation. In the case of "France is hexagonal," for example, it is easier to raise the standards of precision, moving to a context where the sentence becomes false, than it is to lower them, moving to a context where the sentence becomes true. If the standards are lowered then "what is said, although true enough under the lowered standards, may still seem imperfectly acceptable" (Ibid., 245). On the other hand:

Raising of standards . . . manages to seem commendable even when we know it interferes with our conversational purpose. Because of this asymmetry, a player of language games who is so inclined may get away with it if he tries to raise the standards of precision as high as possible — so high, perhaps, that no material object whatever is hexagonal. (Ibid.)

The same thing holds in the case of the skeptic’s “might” claim, “You might be wrong”:

We get the impression that the sceptic . . . has the last word. Again this is because the rule of accommodation is not fully reversible. . . . Because of this asymmetry, we may think that what is true with respect to the outward-shifted boundary must be somehow more true than what is true with respect to the original boundary. (Ibid., 247)

So Lewis and Cavell are fully in agreement on this point. The skeptical argument works by shifting us to a special context — Cavell calls it “the philosopher’s context” — in which the rules of ordinary language themselves dictate the skeptical conclusion, and dictate, moreover, that the skeptical conclusion will *seem* more true, deeper, than our average, everyday knowledge claims, even in cases where doubt can be cast only by bringing in a deceiving demon.⁸

So Cavell and Lewis agree that any competent speaker must feel the force of the skeptical argument, but also that the success of that argument is only apparent (the skeptic *seems* to have the last word). They also agree broadly as to how we end up in that context to begin with, although in this case other differences between them tend to obscure the point — stylistic differences, one might say, but then keeping in mind that both Lewis and Cavell think of such stylistic differences as philosophically important. Cavell, in a characteristically portentous way, says that this context “has been forced upon us” (Cavell, 1979, 139) thanks to “a natural experience of a creature complicated or burdened enough to possess language at all” (Ibid., 140). It requires careful attention — attention of a sort that Lewis’s smooth prose scarcely invites — to notice that “a player of language games who is so inclined” says the same thing. But we all, creatures complicated or burdened enough to possess a language, are players of language games (these are not games we *choose* to play), and as for “so inclined”: in such close proximity to “language games,” it can only be read as a translation of Wittgenstein’s phrase *geneigt zu sagen*, behind which we may also hear the Kantian term

⁸I might note that Cavell and Lewis share, along with everything else, a common sloppy reading of the *First Meditation* (perhaps the one that was taught for the general exam at Harvard in that era). Descartes actually goes to great lengths to avoid any such procedure as they attribute to him, which can be seen, among other things, from the fact that he does *not* use the malignant genius as a reason for doubt.

Neigung, “inclination.” What Cavell portentously announces, Lewis hides under almost impenetrable irony (namely, by underplaying, making it easy to forget, the difference between a game to which technical rationality can be applied in a cunning way and a “game” which constitutes the very nature of finite rationality).

Now, the semanticist’s context bears at a least a passing resemblance to the philosopher’s context. If the latter is a context in which “But you might be wrong” seems deeper or more accurate than our everyday “I couldn’t be wrong about *that*,” the former is a context in which “But it must be either true or false” seems deeper or more accurate than Austin’s everyday “It is just rough.” The semanticist, like the skeptic, must have achieved this context by relying on rules of accommodation — that is, by using ordinary language rules themselves to force a context in which ordinary conversational purpose is thwarted. Lewis makes such moves over and over, but here at least is one key example: “If Fred is a borderline case of baldness, the sentence ‘Fred is bald’ may have no determinate truth value. Whether it is true depends on where you draw the line” (Lewis, 1983d, 244). The first sentence says, with Austin, that the law of excluded middle does not apply to “Fred is bald”: it is neither true nor false, but just rough. The second sentence, however, presupposes that the law of excluded middle applies to all sentences, and straightaway a context is established in which that holds — namely, a context in which the following can be truly asserted: “‘Fred is bald’ is always either true or false, but which depends on the context in which it is asserted.”

Aside from the *similarity* between the philosopher’s context and the semanticist’s context, however, there is also the following relationship between the two: it is only in the semanticist’s context that Lewis can explain how the skeptic’s *Neigung* motivates her to establish the philosopher’s context, and how the resulting argument is neither fully correct nor fully incorrect — not incorrect play, but nevertheless, so to speak, unsporting.⁹ But then if Cavell also differs from Austin in making just such a diagnosis, we should expect to find him, too, establishing something like a semanticist’s context. Which he does, for example, here:

It will help to ask: Can a child attach a label to a thing? . . .

Mightn’t we wish to say *either* Yes or No? Is it a matter of

⁹With respect to the differences between Austin, on the one hand, and Cavell and Lewis (and Rawls), on the other, it may be worth noting that, for “unsporting,” we could substitute “not cricket,” but that there is no analogous expression “not baseball.”

deciding which to say? What is it a decision about? Should we say, “Yes and No”? But what makes us want to say this? (1979, 174)

What would Austin say about this question? Presumably that we would answer “yes” for certain purposes and “no” for others, and that if our interlocutor presses: “But is the answer ‘yes’ or ‘no’?”, the right and final answer is: “Yes, sort of.” In continuing past that, Cavell is entering the same type of context that Lewis does. Once in that context, however, do they not say entirely different things? Different in some ways, perhaps, but we should not forget the similarity that led us here in the first place, namely that both use that context to give essentially the same diagnosis of the skeptic’s argument (a diagnosis not available to Austin).

2 The ends of language games

In a sense it should not be surprising that rules are asymmetrical in a way that causes irreversibility. In a competitive game such as baseball or chess or tic-tac-toe, after all, irreversibility — the absence of correct moves leading back to a previous state — is built in for good reasons: among other things, it helps ensure that games will, at least typically, be finite. Correct play always, or for the most part, leads forwards towards the point where the game will be over. But then, why do we want to ensure *that*? Games in general do not need such a guarantee: in non-competitive games such as Dungeons and Dragons or Minecraft, there may be a typical (not irreversible) progression towards a more advanced state of play, but there are no rules that will ever cut it off. The point of a competitive game, however, is for someone to win, in some typical amount of time which may vary from game to game but which had better at least be finite. Irreversibility in such games is therefore by definition an irreversibility of *progress*: progress toward the end of the game, in both a chronological and a practical sense.

The rules of competitive games conform to this requirement of irreversible progress towards the end to different extents and in different ways. In Major League Baseball, for example, the rules as such do not absolutely guarantee that the game will ever end, although, given certain background assumptions, the rule that each team must be able to field nine players would eventually make that happen. When the rules of a game *do* supply an absolute guar-

antee, they do so usually by including the possibility of a draw.¹⁰ A draw is a kind of failure of the game and hence of its rules: competition is designed to determine a winner. But the players do at least part without any disagreement as to who has won. Indeed, one might distinguish between the spectator's interest in competition (to see who wins) and the player's interest (to see whether she can win), and say that the players' end, though not the spectators', is as well accomplished in a draw as in a win for either side. This would help explain why the rules of Major League Baseball, in which the spectators' interest so completely dominates, aim to avoid ties at all cost. Be that as it may: given that a game has got to end sometime, the failure that consists in ending regularly without a winner is preferable to the more serious failure that would consist in ending irregularly, with each player perhaps claiming to have won.

The above considerations are enough to explain why regular irreversibility serves the practical end of a competitive game. What, however, is the practical end of ordinary argument? In this case agreement as to who (if anyone) is the "winner" will not normally be, in itself, desirable — not, at least, in the case that *es um wahr und falsch geht*, i.e. the "lucky" case where participants and spectators are mostly interested in the advance toward truth. What we are hoping for, it would seem, is agreement *about the topic under discussion*, e.g. about whether there is a bittern at the bottom of the garden, or whether heat is caloric fluid, or whether there are such things as numbers, or whether I may break my promise. "Without the hope of agreement," Cavell says, "argument would be pointless" (1979: 254). Or, as Lewis puts it: "each of two debaters tries to get his opponent to grant him — to join with him in presupposing — parts of his case" (1983d: 239): I don't want my opponent to agree that I am winning, but rather to agree with *me*. And yet, as we noted to begin with, both Lewis and Cavell deny that such agreement is the end — either in a practical or, typically, in a chronological sense — of moral or ontological argument. Towards what end, then, and *to* what end, are the

¹⁰A tic-tac-toe game must end after at most nine moves, of course; if at that time neither player has three marks in a row, the game is drawn. Infinite chess games were formerly possible in international competition if neither player ever claimed a draw, but are now ruled out by FIDE rule 9.6(b), which states that the game is drawn if "any consecutive series of 75 moves have been completed by each player without the movement of any pawn and without any capture" (unless the last move was a checkmate) (<http://www.fide.com/fide/handbook.html?id=171&view=article>) — a rule, note, that relies for its efficacy on the fact that capture and pawn movement are irreversible.

rules irreversibly moving us?

To understand the situation better, it may help to note how such arguments can fail when, and in fact because, they *do* end with a winner: that is, with an outcome in which one party must grant the whole case to the other (that is: would be mistaken, irrational or incompetent, not to grant it). Consider, for example, an argument Lewis imagines in (1983d), which can easily be put in the form of one of Cavell's sample moral arguments from *The Claim of Reason*:

A (an elected official): You see, I must either destroy the evidence or else claim that I did it to stop Communism. What else can I do?

B (rudely): There is one other possibility — you can put the public interest first for once!

A (mistakenly): I can't do that.¹¹

Lewis analyzes this conversation as analogous to what both he and Cavell think of as a Cartesian skeptical argument.¹² A's initial statement is true, just as the Meditator is initially correct to say: "I am seated by the fire in my nightgown, etc. How could I be wrong about that?" The initial contexts are such that "An evil demon might be deceiving me" and "I can put the public interest first (at the expense of my political career)," are, respectively, false. But when the skeptic and the "rude" critic, respectively, assert these possibilities, a rule of accommodation ensures that the context changes so as to make their assertions true. An asymmetry in the rules of accommodation then prevents the context from shifting back, so that if the Meditator now says (without further justification) "But that couldn't be!", or the official now says (without further justification) "I can't do that," these responses are simply mistaken. The truth conditions for "It couldn't be that ϕ " or "I can't do ϕ " have changed. Therefore, the rude critic, like the skeptic, wins.

The parallel with the skeptical argument, however, and the negative description of both participants ("mistaken," "rude"), suggest that this is not

¹¹In Lewis's own presentation: "Suppose I am talking with some elected official... He says: 'You see, I must either destroy the evidence or else claim that I did it to stop Communism. What else can I do?' I rudely reply: 'There is one other possibility — you can put the public interest first for once!' ... If he protests 'I can't do that,' he is mistaken" (p. 247).

¹²I repeat that this involves poor reading of Descartes.

an example of a *good* moral argument, one that achieves the end of such arguments. Lewis explains the problem:

We get the impression that the sceptic, or the rude critic of the elected official, has the last word. Again this is because the rule of accommodation is not fully reversible. . . . I see no reason to respect this impression. Let us hope, by all means, that the advance toward truth is irreversible. That is no reason to think that just any change that resists reversal is an advance toward truth. (1983d: 247)

The problem is not that the conclusion is false — presumably Lewis agrees that the official ought to put the public interest first,¹³ and he also agrees that the skeptic gives a valid reason for doubt: I *might* be thoroughly deceived, in ever so many ways; at countless possible worlds, a duplicate of myself is thoroughly deceived.¹⁴ The critic is *not* mistaken in the final context, any more than the official was mistaken in the original one. The problem is that the method, the rule, by which the conversation proceeds irreversibly from first word to last, is not a rule of advance toward truth. There is a final answer, to which both parties ought to agree, and it is a right answer (is a true statement, given the new context), but its rightness is in no way a consequence of its finality. The initial answer was just as right as the final one.

But then, if getting one's opponent to give away the *whole* case is not the goal, and in fact may represent a *failure* of the argument even when it happens, what rational point could there be in getting *pieces* of it? With respect to ontological arguments, at least, Lewis gives an answer, in the Introduction to his *Philosophical Papers*. “The reader in search of knock-down arguments in favor of my theories will go away disappointed” (Lewis, 1983a, x). So much we have already come to expect: after all is said and done, Lewis and his opponents may still differ. But then he adds something else: “Whether or not it would be nice to knock disagreeing philosophers down by sheer force of argument” — and then, in a footnote: “It would not be nice, of course” — “it cannot be done” (Ibid.).¹⁵ The effect of this sly footnote, with its sudden reminder that what is “nice” for me may not be

¹³See his remarks on the additional reasons “decent men” have for respecting the “convention of truthfulness” in a language (1983b: 31).

¹⁴See (Lewis, 1986, 116).

¹⁵The footnote continues: “Robert Nozick has drawn attention to our strange way

“nice,” per se, is to put philosophical argument into a moral and political context, and to remind us that the *argumentum ad baculum* lies always at hand. We will return to that point. Meanwhile, Lewis continues:

Philosophical theories are never refuted conclusively. (Or hardly ever. Gödel and Gettier may have done it.) The theory survives its refutation — at a price. Argle has said what we accomplish in philosophical argument: we measure the price. Perhaps that is something we can settle more or less conclusively. (Ibid.)

The reference is to the concluding part of “Holes”:

Bargle: I, for one, have more trust in common opinions than I do in any philosophical reasoning whatever. In so far as you disagree with them, you must pay a great price in the plausibility of your theories.

Argle: Agreed. We have been measuring that price. I have shown that it is not so great as you thought. . . .

Bargle: The price is still too high.

Argle: *We agree in principle*; we’re only haggling.

Bargle: We do. And the same is true of our other debates over ontic parsimony. Indeed, this argument has served us as an illustration — novel, simple, and self-contained — of the nature of our customary disputes. (Lewis and Lewis, 1983, 8–9; my emphasis)

So this is an example of a successful conversation. The parties do not come to agree with one another about the point at issue. But they do advance towards the truth about something, namely, about the true prices of their respective positions. This advance in accuracy of measurement depends, moreover, on the procedure of *argument*: the apparent price of Argle’s view will increase insofar as she grants Bargle — joins with him in presupposing — parts of his case, and/or gives away parts of the contrary case, and will decrease when

of talking about philosophical argument as if its goal were to subjugate the minds of our esteemed colleagues. and to escape their efforts to do likewise unto us.” The cited discussion in Nozick (Nozick, 1981, pp. 4–5) occurs in Nozick as part of an attempt to call into question the value of arguments in philosophy. Here, in other words, as in the dispute with Carnap, Lewis is defending the practice of philosophical argument against a general attack.

the same thing happens in reverse. Moreover, the parties do end by acknowledging an agreement: not an agreement forced a posteriori by the argument, but rather an agreement pre-existing from the first (“in principle”), which made the argument possible, and which the argument brings to light because actuality implies possibility. It is — and for once this can be said with perfect literalness and without sinister metaphysical implications — an agreement about *values*: an agreement, that is, about what kind of features in a thing (for example, in a presupposition) are responsible for its price. The purpose of argument in this case, in other words, is self-revelation. What Argle and Bargle each learn is what they were already prepared to demand from and grant to each other. Irreversibility, then, is here a consequence of what Lewis elsewhere calls the Rule of Attention: what is not ignored at all is never properly ignored (1999a: 434). I try to get the other to agree with me in order to learn, about myself, what it is I am prepared to request agreement with, and what I learn should not, and normally cannot, be unlearned. Irreversibility is in the service, not of progress towards the end, but of progress away from the beginning: away from the initial state of forgetfulness.

If such measurement is the proper end of a debate about ontic parsimony, then, switching back to the case of moral argument, we may expect that Lewis is substantially in agreement with Cavell when the latter says, about “questioning a claim to moral rightness” in the course of moral argument, that it “takes the form of asking . . . ‘Have you really considered what you’re saying?’, ‘Do you know what this means?’” (Cavell, 1979, 268), and that

assessing the claim is . . . to determine *what* your position is, and to challenge the position itself, to question whether the position you *take* is adequate to the claim you have entered. (Ibid.)

and that

The point of the assessment is not to determine *whether* it is adequate, . . . the point is to determine *what* position you are taking, that is to say, *what position you are taking responsibility for* — and whether it is one I can respect. (Ibid.; Cavell’s emphasis throughout)

If a moral argument results in a more accurate measurement of the price of our respective positions, in other words, it also reveals a prior agreement on values: it shows the opponents can not only take responsibility for (pay the

price for) their own positions but also respect (so to speak: accept payment for) one another's. This practical end is achieved, not in particular at the chronological end of the argument, but rather *at every point*, as long as each party continues to find the other's moves respectable. The continued commerce enriches both sides, insofar as the parties, by means of it, come to own (to possess justifiably) some part of their positions which they otherwise would not, each by paying prices acceptable to the other.

A failure of the parties to agree, at the chronological end of the argument, on the moral issue at hand is not, then, actually analogous to a stalemate (a type of draw), because the progress of the argument towards a hoped for agreement on that issue was all along in the service of revealing a pre-existent agreement on values. Something like a draw — that is, a regular failure that at least avoids a more serious, irregular one — will occur only if the argument instead reveals that there is no such prior agreement. “The outcome of the argument will affect whether the parties concerned are to continue to live in the same moral world” (Cavell, 1979, 295–6). For example, consider the following:

A: I've decided against offering him the job.

B: But he's counting on it. You most explicitly promised it to him. . . .

[one of four possible “continuations of this opening”:]

A: I know, but it has suddenly become *very* inconvenient to have him around, and there is someone else really better qualified anyway.

B: If you do this to him, I'll never speak to you again.

A: Don't make such an issue of it. I'll see that he gets a job, and I'll give him some money to see him through.

B: Goodbye. (Cavell, 1979, 266)

Assuming that the argument ends here, *something* has been revealed. It has now become clear that the parties cannot respect one another, because they cannot agree on the “price” of breaking this promise. Commerce, at least on this point, has come to a halt. Still, the failure is regular. Neither party is “mistaken” — nor, I think, could either well be described as “rude.” Each has competently taken responsibility for a position. The type of irregular

failure thereby avoided is the one evident in Lewis's case, where the rudeness of the critic, combined with the official's mistake, prevent the parties from determining *whether* they are still in the same world, i.e. whether or not they can agree on a price.

The rudeness, moreover, is responsible for the mistake. The official is not ignorant of the rules. Contrast, for example, the hypothetical example of someone who fails to understand the effect of making a promise. "I promise . . ." is a performative utterance, and Lewis gives a general account of performative utterances. As an example, he first discusses a sentence of the form "I hereby name this ship *n*." According to Austin, such a sentence has no truth value (Austin, 1975, 5). But Lewis considers this view unattractive, insofar as "we wish to respect the seeming parallelism in form between a performative like 'I hereby name this ship the *Generalissimo Stalin*' and such non-performative statements as 'Fred thereby named that ship the *President Nixon*'" (Lewis, 1983d, 247–8).¹⁶ Instead, Lewis suggests that the performative utterance "is true, on a given occasion of its utterance, if and only if the speaker brings it about, by means of that very utterance, that the indicated ship begins to bear the name 'Generalissimo Stalin'" (Ibid.). The *effect* of the performative is due to a rule of accommodation, which Lewis states ("roughly") as:

If at time *t* something is said that requires for its truth that ship *s* bear name *n*; and if *s* does not bear *n* just before *t*; and if the form and circumstances of what is said satisfy certain conditions of felicity; then *s* begins at *t* to bear *n*. (Ibid.)

Again, the rule brings about a change in context, and a change which is not easily reversed: if the ship's former name were, say, the *Prince Potemkin of Tauris*, someone who now refers to her by that name would simply be mistaken. Some performatives, moreover, can change more than just the truth or falsehood of future utterances:

"With this ring I thee wed" is verified by its felicitous use, since the marriage relation is a component of conversational score governed by a rule of accommodation. Is marriage then a *linguistic*

¹⁶The *Generalissimo Stalin* example is from (Austin, 1979, 239–40); in (1975, 23) it is *Mr. Stalin*. In both cases the contrasting legitimate name is the *Queen Elizabeth* (1979, 235; 1975, 5). *President Nixon* is perhaps aimed at Quine; cf. (Urmson et al., 1969, p. 90).

phenomenon? Of course not, but that was not implied. The lesson of performatives, on any theory, is that use of language blends into other social practices. We should not assume that a change of conversational score has its impact only within, or by way of, the realm of language. (Ibid.)

Clearly “I promise . . .,” as a performative, is, at least in this respect, of the same kind as “I wed” (indeed, marrying is, in at least some traditions, taken to be a special instance of promising). So what kind of mistake is made by someone who, when reminded of a promise, says calmly, “Oh, but I’ve decided I’d rather not”? Imagine this as a *mistake* (and not, rather, as a piece of rudeness). Then it is a mistake, not about the rules of promising merely, but about the relationship between speech and life, about the reach of “conversational score.” As Cavell puts it:

To suppose that there is (could be) a special rule to the effect that you may not break a promise because you would rather not keep it is like supposing that there is a special rule of chess to the effect that you cannot remove pieces to their former position whenever you would prefer to have them there. But not to know that is not to lack a knowledge of chess; it is to lack initiation into an entire form of life, to lack the whole concept of a (competitive) game. You can and do, of course, (sometimes) break a promise. But breaking a promise is not annihilating it; . . . moving a piece back is not the same as taking back a move. . . . Each move changes the situation in which the following move is to be made. (Cavell, 1979, 307–8)

That such mistakes are possible, just as someone may possibly fail to see the pattern we do in the series of even numbers, or in the set of things we call “chairs,” or in our procedures for measuring piles of wood, is important both for Cavell (as the allusion to Wittgenstein should make clear) and for Lewis (for whom the existence of a convention at one world supervenes on, among other things, the outcome of its failure to hold at other, nearby worlds). But the importance and interest of such possibilities stems from the fact that they remain more or less completely unactualized. No one in *our* society (as opposed to: in some “tribe”) ever makes this kind of mistake about promises or about games, except perhaps very young children, if indeed their behavior is aptly so described.

In particular, Lewis's elected official has obviously not made such a mistake. The official knows that the critic has made a valid move, and knows, too (though perhaps without understanding why), that "I can't do that" is not a valid reply. But what option has the critic's move left open? The old context, in which "I can't do that" expressed something true, was not arbitrary: it was suited to the cares and commitments of the elected official, of which the rude critic is well aware. The language game which the critic exploits, with its underlying conventions of trust and truthfulness, was only one of those commitments, and the critic's valid move has done nothing to change the others. Under pressure, the juncture between linguistic and non-linguistic commitments breaks, and the official is left with a choice between saying something true but insincere ("Yes, of course, that is my duty") or something sincerely felt but false ("I can't do that"). Lewis is correct, in other words, to draw an analogy between this moral case and the epistemic case of the skeptic: the skeptic's interlocutor faces just this choice between a reply that is true but not sincerely believable ("Yes, of course, I don't really know") and one that is believable but false ("There can be no doubt of that"). The type of rudeness involved here, in other words, is *inconsiderateness*. The critic fails to take the official's cares and commitment into account in deciding which move to make. If we assume that the conversation ends chronologically at this point, then its practical end has thereby been frustrated. The official's "I can't do that," because it is simply *mistaken*, does nothing to take responsibility for any position; nor, therefore, does it reveal whether the official's position is one that the critic or anyone else can respect.

Perhaps this conclusion is jarring. Isn't the critic the good guy in this conversation? The elected official *ought* to put the public interest first, after all! But even leaving aside the question of whether there are really good guys and bad guys in this world, or whether, if there are, morality has anything to say to them as such — even leaving that aside, it is important to remember that rudeness can be justified or (not quite the same thing) deserved. Moral conversation is itself a topic suitable for moral conversation: rudeness may be a price one has to pay. Lewis's repeated application of "rude" to the critic, but not at all to the skeptic, is his characteristically laconic way of marking what Cavell discusses at great length, namely the *disanalogy* between the moral and epistemic cases. At least: neither Cavell nor Lewis is much impressed by his claim that the, so to speak, rudeness of Descartes's argument is justified because it is the only path to certain knowledge. The critic's

move in frustrating the end of moral conversation, on the other hand, may ultimately be in the service of moral action, or of some other end common to the official and the critic. We could say more, and imagine better what might be the official's reaction in the long term, if we knew how the critic might excuse her rudeness.

The very possibility of rudeness, on the other hand, and of the mistake-ness which can be forced by rudeness, is itself a price that has been payed. The rules of moral (and of ontological) argument might, like the rules of competitive games, guarantee a regular chronological end to every such argument, and in that case rudeness of this kind would be impossible. Austin can seem rude, and this is perhaps the effect, or perhaps the cause, of taking philosophical argument to have the nature of such a game. Whole philosophical cultures can seem infused with rudeness, with like causes and/or effects. Lewis and Cavell, however, have in common an understanding of the ends of philosophical (moral and/or ontological) argument according to which it is an activity in which interlocutors can make progress only by respecting one another's cares and commitments. The rules governing the activity cannot force this respect, and cannot make this respect deserved. In its absence, a forced regular chronological end would serve no purpose; in its presence, there is no need to force one. Hence the rules are such that a rude interlocutor can bring about an irregular conclusion.

3 Truth in the Academy

Another point that might be added to our list of similarities between Lewis and Carnap would be their willingness to speak on behalf of a school of philosophy, and in response to someone outside that school. Carnap's "Principle of Tolerance," in its original formulation, concerns just such a situation:

In logic, there are no morals. Everyone may construct [*aufbauen*] his logic, i.e. his language-form, as he wishes. Only, if he wishes to debate with us, he must clearly state how he intends to do it, give syntactical determinations instead of philosophical arguments. (Carnap, 1934, §17, p. 45)¹⁷

Tolerance, apparently, runs only so far: use whatever language you want, but, if you want to talk to *us* — i.e., to the members of "our 'Vienna Cir-

¹⁷The translation (Carnap, 1959, p. 52) reads "if he wishes to discuss it," but the original is *wenn er mit uns diskutieren will*.

cle’” (ibid., iv) — you must learn to speak as we do. “Logical syntax is to supply a structure of concepts [*Begriffsgebäude*], a language, with the help of which the results of logical analysis can be exactly formulated” (Ibid.). Here, in other words, Carnap adopts the suggestion which he elsewhere attributes to Neurath: recondition others to replace their speech dispositions with ours. If some cannot be reconditioned in this way (reconditioning, after all, “succeeds in some cases, with some animals and humans, and then in other cases does not”), we simply rule them out of “the circle of those by whom intersubjective science is constructed and applied” (Carnap, 1932, 222). Quine makes a related proposal, though now, of course, no longer with the idea that ontological theses are to be replaced with syntactical ones, and no longer with the idea that we may freely formulate translation rules which thus become analytic:

Futile caviling over ontic implications gives way to an invitation to reformulate one’s point in canonical notation. We cannot paraphrase our opponent’s sentences into canonical notation for him and convict him of the consequences, for there is no synonymy; rather we must ask him what canonical sentences he is prepared to offer, consonantly with his own inadequately expressed purposes. If he declines to play the game, the argument terminates. (Quine, 1960, 242–3)

Lewis, taking on Richard Routley/Sylvan’s “noneism,” finds himself actually in this situation: “Routley sees himself as defying an established orthodoxy; and I am prepared to appoint myself spokesman for the orthodoxy he defies. Or at least for those among the orthodox, if any, who will accept me as their spokesman” (Lewis, 1999b, 154). Again the problem is that Routley has not expressed himself as we would: “His own words do not answer the question what *we* ought to say in reporting his position” (Ibid., 156). But Lewis’s response is subtly different.

Why this fuss over translation? Routley writes in English, after all. Is he not the final authority on his own position? Should we not translate him homophonically? No. He is the final authority on *his* position, but not on *ours*. Therefore he does not have the final word either on how his position should be expressed in our language, or on how ours should be expressed in his. Nor do we. There is no authoritative final word; we can only seek

the translation that makes him make sense to us, and us to him.
(Ibid., 156–7)

Our school lacks authority over him. He can be expected, therefore, to continue speaking as he does. If we want a debate, the burden is on us to become translators.

That Lewis should in this way give up on the hope of getting our opponent to speak our language is surprising, if we keep in mind his general explanation for the existence of linguistic conventions: namely, that, among other things, each speaker prefers that everyone abide by the convention. It is perhaps even more surprising if we recall that Lewis takes philosophy to be, like science, an institution that aims at the advance of truth. For the situation he describes and accepts with respect to Routley is the one Kuhn finds characteristic of pre-paradigm, “immature” science: a period in which “evidence of progress, except within schools, is very hard to find” (Kuhn, 1996, 163). Kuhn blames this lack of general progress on a feature famous from his account of scientific revolution, namely on *incommensurability*:

What differentiated these various schools was not one or another failure of method — they were all “scientific” — but what we shall come to call their incommensurable ways of seeing the world and of practicing science in it. (4)

Here incommensurability is characterized as a difference in ways of “seeing.” But Kuhn also describes it, especially in the later “Postscript” to (1996), as breakdown in communication due to linguistic difference: proponents of incommensurable views “cannot . . . resort to a neutral language which both use in the same way and which is adequate to the statement of both their theories” (201). The only hope, then, for is for the two sides to “recognize each other as members of different language communities and then become translators” (202). In a footnote Kuhn adds that “the already classic source for most of the relevant aspects of translation” is (Quine, 1960). We can expect to find *progress* in science, according to Kuhn, only once these disputes between schools come to an end, namely with the acceptance of a common paradigm:

Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That

commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and continuation of a particular research tradition. (11)

If Lewis expects progress in philosophy, should he not, as Carnap and Quine advise, try to establish for it a common paradigm — that is, a common language?

But how do paradigms avoid dispute, according to Kuhn? One might imagine a set of rules for science which were not arbitrary conventions, but rather were established for a definite practical purpose: their practical end would be, at least in part, to settle scientific arguments in a rational way and in finite time. To determine correct rules of this kind is the task Popper assigned to the discipline of scientific methodology or *Logik der Forschung*, and the particular rules he determined indeed resemble the rules for competitive games. As Popper himself puts it: “We treat methodological rules as conventions [*Festsetzungen*]. One might call them the rules [*Spielregeln*] of the game, ‘empirical science’” (Popper, 2002 §11, p. 25). But Kuhn claims that the actual rules by which science normally proceeds are entirely unlike this. Scientists do not “normally aim to invent new theories, and they are often intolerant of those invented by others” (24). In a mature science, he says, there is rather, normally, an “apparent consensus” (11) as to what theory is true. In reality this theory is always underspecified (remains to be “articulated”), but what is real, not merely apparent, is that those who share a paradigm “are committed to the same rules and standards for scientific practice” (ibid.). These rules are not constant, as if designed in advance for some purpose; they contain “an apparently arbitrary element, compounded of personal and historical accident” (4). And the apparent consensus at any given (normal) time is the *effect* of these arbitrary conventions, which are or amount to the arbitrary conventions characteristic of different linguistic communities. The arbitrary rules serve specify, to within some small range of ambiguity, a particular theory — the “paradigm theory” — which is to account for all results. “The range of anticipated, and thus of assimilable, results is always small compared with the range that imagination can conceive. And the project whose outcome does not fall in that narrower range is usually just a research failure, one which reflects not on nature but on the scientist” (35). The rules produce agreement directly, not via a rational exchange between two parties. They are there not to decide between outcomes (a win for A versus a win for B), but rather to ensure that, with sufficient

skill, the one acceptable outcome can be achieved. In other words: they are unlike the rules of a competitive game and are instead, as Kuhn says, like the rules of a puzzle.

The solution of a puzzle does reveal a certain truth, namely about the skill of the solver. This presumably is part of the interest of the puzzle, at least to the one who solves it — and, in general, puzzle-solving does not excite the interest of spectators.¹⁸ If we thought, however, that the purpose of science was to advance our knowledge *of nature* — which, whatever it means exactly, surely rules out as a proper topic the skill of this or that individual scientist — then the kind of rules and standards Kuhn depicts as characteristic of normal science are unsuitable for producing that result. It is obvious, then, on Kuhn’s description, that science can’t *normally* be expected to reveal new truths: it is not characterized by rules suitable to that purpose. And as for the “extraordinary” periods in which an old paradigm breaks down and is eventually replaced by a new one, those periods are characterized by the return of incommensurability, and hence by the impossibility of any kind of general progress at all. In such a situation, scientists addicted to puzzle-solving will ultimately look for knock-down arguments, as Kuhn explains in his key comparison between scientific “revolutions” and the real, political kind. As a political revolution proceeds,

society is divided into competing camps or parties, one seeking to defend the old institutional constellation, the others seeking to institute some new one. And, once that polarization has occurred, *political recourse fails*. Because they differ about the institutional matrix within which political change is to be achieved and evaluated, because they acknowledge no supra-institutional framework for the adjudication of revolutionary difference, the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force. . . .

The remainder of this essay aims to demonstrate that the historical study of paradigm changes reveals very similar characteristics in the evolution of the sciences. (Kuhn, 1996, 93–4)

¹⁸If we tend, as Kuhn does, to speak of people as *addicted* to solving puzzles, that is perhaps because the frequency of attempted solution (e.g., of the daily crossword) goes beyond what could rationally be accounted for by that interest. Then the reason we do not tend to speak of addiction to competitive games (of skill) might be that the standing challenge of autonomous others provides, even in the absence of a wager or of paying spectators, an inexhaustible motive for submitting once again to the test.

But that would not be nice, of course.

Lewis may or may not agree with Kuhn's account of the "mature" sciences. His story about the "prolonged disputes over what's true" in every department is relevant only to the extent that he believes those science departments to be "lucky."¹⁹ He does offer, in *On the Plurality of Worlds*, a kind of defense of inductive rationality, but it is rather weak and Kuhnian, if not Humean: we call it "inductive reason," "as we are right to do, because that is indeed the name we have given it" (Lewis, 1986, 117): that is, it is reasonable because *this* is what we call "reasonable." Cavell, who shows more interest in issues of scientific methodology, is of course engaged in some way with Kuhn's views — though perhaps more via the untraceable medium of personal conversation than via any reading of *Structure*. It is not clear, however, whether or how far Cavell accepts Kuhn's view of science. The following passage, especially, suggests substantial agreement:

But are we any longer quite so willing to take that Aristotelian who refused to look through Galileo's telescope in order to "see" the valleys of the moon, as a comic and irrational figure? If *now* a man refused to accept the evidence of telescopes as telling us of the nature of the moon, he would either be (we would either regard him as) irrational or else incompetent *in science*. That man is no scientist, given the procedures or canons of science which now constitute that institution. Or again: what he says will not count for or against any proposition of science. Once these procedures and canons are established, then agreement is reached in familiar ways; but that simply means: agreement (or absence of disagreement) about what constitutes science, scientific procedure, and scientific evidence, is what permits particular disagreements to be resolved in certain ways. Being a scientist just is having a commitment to, and being competent at, these modes of resolution. (Cavell, 1979, 261)

But his discussion over the following few pages serves to complicate the pic-

¹⁹Popper implies that such departments have had especially *bad* luck, thanks to "the sudden need for huge numbers of trained technologists," which is "a consequence of the modern armaments race, perhaps" (Popper, 1974, 1146). It may be a sign of wisdom in our leaders that they did not see fit to train up battalions of philosophers for the struggle (although some have argued that Kuhn was trained up by James Bryant Conant for just such a purpose). Cf. (Hume, 1868, 396).

ture somewhat. In particular, he eventually asks whether these considerations imply that science is not “fully rational,” and replies: “What I am urging is that we not assume we know what makes it rational; that we take the question of its rationality or irrationality to be as problematical as the question of the rationality or irrationality of morality” (Ibid., 263). This is followed by a disclaimer, “But I know too little of the history and actual prosecution of science to have much confidence in any discussion I could offer of it,” and then, in parentheses, an ambiguous reference to Kuhn: “Now, of course, there is T. S. Kuhn’s *The Structure of Scientific Revolutions*, and the discussions it may figure in” (Ibid., 264).

Sticking to the philosophy department, however, we can say, first, that Lewis thinks that it remains, in Kuhn’s terms, “immature,” and that, second, he (unlike Carnap or Quine) proposes no attempt to mature it. May we hope, then, that our actual philosophy departments are sufficiently lucky as to advance towards truth?

We have seen that Lewis, like Cavell, thinks ordinary arguments may aim at such an advance: an advance towards a more accurate measurement of prices. But, precisely because they are, in the relevant sense, *ordinary*, their rules are not the rules of any special institution. In a related case, indeed, Cavell argues that *promising* is not correctly called an “institution” at all, not at least as Rawls defines the term:

Now by an institution I shall understand a public system of rules which defines offices and positions with their rights and duties, powers and immunities, and the like. These rules specify certain forms of action as permissible, others as forbidden; and they provide for certain penalties and defenses, and so on, when violations occur. As examples of institutions, or more generally social practices, we may think of games and rituals, trials and parliaments, markets and systems of property. (Rawls, 1971, 55)²⁰

In response Cavell points out that the “office” of promisor is unlike offices properly so called: “there is no special procedure for entering it (e.g., no oaths!), no established routes for being selected or training yourself, etc.” (Cavell, 1979, 297) and that the defenses which may be competently entered

²⁰Cavell actually addresses directly the very similar definition which Rawls gives, in “Two Concepts of Rules,” of the term “practice” (Rawls, 1955, p. 3 n. 1). But it quickly emerges in Cavell’s own discussion that he considers the term in this sense to be interchangeable with “institution,” and Rawls, based on this later definition, clearly agrees.

for failing to keep a promise “are just the defenses we learn in learning to defend any of our conduct which comes to grief” (Ibid., 296). The same points apply equally well to the “office” of participant in an ordinary moral argument, *or* to that of participant in an argument about ontic parsimony. The same applies, moreover, to the office occupied by Socrates: his claim that the god (Delphic Apollo) has appointed him official gadfly of the city is of the same kind as Thoreau’s list of public offices, beginning with “for many years I was self-appointed inspector of snow-storms and rain-storms, and did my duty faithfully” (Thoreau, 1992, IB.7), and concluding

In short, I went on thus for a long time (I may say it without boasting), faithfully minding my business, till it became more and more evident that my townsmen would not after all admit me into the list of town officers, nor make my place a sinecure with a moderate allowance. (Ibid., IB.9)

It is a joke, in other words, although my no means *merely* a joke. It is a joke whose bite lies precisely in calling attention to the incongruity between the true situation in which we find ourselves and the purposes for which we have established public offices.²¹ To quote *Walden* once more: “If you are chosen town clerk, forsooth, you cannot go to Tierra del Fuego this summer: but you may go to the land of infernal fire nevertheless. The universe is wider than our views of it” (18.2). To justify the Academy, then, in both the original and the extended sense of the term, would be to somehow bridge that laughable incongruity. And the task is indeed laughable, from some point of view: as if we expect every hiring committee to reverse the verdict of Athens and grant Socrates a stipend rather than having him put to death.

It is difficult to determine Cavell’s opinion about this, in part because, when he uses the term “academic,” as he does with some frequency, he typically has in mind more the French *Académie des Beaux-Arts* than the Academy of Plato. When he says, for example, that “academic art is (with notable exceptions) bad art, whereas academic science is — just science” (Cavell, 1976a, xxvii), he is drawing the same contrast between science and art that Kuhn implies when he contrasts the solution of a jigsaw puzzle with the way “a child or a *contemporary* artist” (my emphasis) might use its pieces to “make a picture,” and adds: “The picture thus produced might be

²¹A similar joke might be told about the gap between ordinary bargaining procedures and Rawls’s “markets and systems of property.” Marx often indulges in such jokes.

far better, and would certainly be more original, than the one from which the puzzle had been made” (Kuhn, 1996, 38). The word “contemporary” is there because, as is clear from Kuhn’s brief discussion of the history of painting (ibid., 161), he believes that painting was, like science, a form of puzzle solving before the Impressionists rebelled against the Academy. Nevertheless, both Kuhn and Cavell connect the academic nature of (mature) science with a feature characteristic of Plato’s Academy, namely its withdrawal from the agora. Kuhn’s normal science, like puzzle-solving more generally, is of interest only to participants, not to spectators. Outside of these mature sciences, Kuhn points out, “there are no other professional communities in which individual creative work is so exclusively addressed to and evaluated by other members of the profession” (Kuhn, 1996, 164) — that is, by those who “by virtue of their shared training and experience, must be seen as the sole possessors of the rules of the game or of some equivalent basis for unequivocal judgments” (Ibid., 168). In the context cited above, Cavell makes the exact same point about the distinction between art and science:

It is tautological that art has, is made to have, an audience, however small or special. . . . It could be said of science, on the other hand, that it has no audience at all. No one can share its significance who does not produce work of the same kind. The standards of performance are institutionalized; it is not up to the individual listener to decide whether, when the work meets the canons of the institution, he will accept it — unless he undertakes to alter those canons themselves. (Cavell, 1976a, loc. cit.)

Cavell, then, regards the Academy generally speaking as both a good site for science and a bad site for art. As to the divergence between Socrates and Plato in its original form, however, Cavell reaches no definite decision: “Now, what is academic philosophy? It seems significant that this question has no obvious answer” (Ibid.). This is a surprising thing to say,²² but it should be less surprising coming from someone who, like Cavell, asserts that America is yet undiscovered. It could be taken to mean that, after all this time, it remains unclear what features are particular to academic philosophy, and hence it is impossible to say whether academic philosophy is like academic

²²Unless one is thinking of the way “Academic” might refer either to the Old Academy of Plato or to the New Academy of Arcesilaus. Hume chooses the latter when he speaks of “the sceptical or Academic philosophy.” As important as Hume’s skepticism is to Cavell, however, he never shows Hume’s interest in its ancient precedents.

art (bad) or like academic science (simply philosophy). But the statement could also be taken to mean, more pessimistically, that, after all this time, it is unclear whether or why we should expect anything worthy of the name “philosophy,” whether good *or* bad, to take place in the academy. The latter interpretation gains support from what Cavell says elsewhere: for example, about the activity of “looking for an explanation in a region in which you have no inclination to suppose it may lie” that “we might call such an activity ‘academic’” (Cavell, 1979, 21). If you are addicted to some type of puzzle, of course, you are “inclined to suppose,” or rather forced to presuppose, that the puzzle’s *solution* lies in a certain region. But you are neither inclined to suppose that solving the puzzle will produce anything useful, for example an explanation, nor inclined to examine your motives for nevertheless undertaking to solve it. One must be distracted from any question about what value the solution will have. If insulation from the demands of spectators is a prerequisite to such distraction, then the continuation of puzzle-solving as a field of purported study — as, that is, a purported search for, among other things, explanation — depends on the closed political structure of a mature scientific discipline, which forbids appeal to any paradigm-external standard. The quote suggests that Cavell takes this closed political structure and its consequences to be both characteristic of the academy in general and generally inimical to the kind of self-revelation which is philosophy’s only path towards truth.

Since Lewis agrees with Cavell about the kind of truth philosophical argument can reveal, he must, if he has a more benign view of academic philosophy, disagree both with Kuhn and with Cavell about how that institution might be organized. We have not established that Lewis *does* have such a benign view. But he does at least have the requisite different understanding of academic philosophy as an institution. For Kuhn’s mature science to take hold in a field, recall, there must be an end to competing schools, each with its own view as to which facts are significant, which problems are important, and which methods are legitimate. This “is usually caused by the triumph of one of the pre-paradigm schools” (Kuhn, 1996, p. 17). But Lewis’s main answer to the title question of (2000a), “Why ignore the advantage of being right?”, is that rival “schools of thought,” within a lucky field, are parties to an “tacit treaty,” the purpose of which is precisely to prevent any such triumph.

To clarify the reasons for this, he imagines first a case both simplified and ironically described. The simplifications are, first, that there are exactly two

schools — materialists and dualists — and, second, “that all concerned think the errors of their opponents matter more than the errors of their misguided allies” (Lewis, 2000a, 198). Lewis, himself a materialist (about the actual world), describes the situation as follows:

In my own opinion as a materialist, the best thing for the advancement of knowledge would be the universal acceptance of the true philosophy: materialism. Or near-universal, anyway. . . . Worst would be the universal, or near-universal, acceptance of dualist error. Second best would be a mixture, as at present. A treaty requiring us all to ignore the advantage of being right when we make appointments will raise the probability of that second-best outcome and lower the probability both of the best and of the worst. (Ibid.)

He then goes on to point out that, although the dualists will rate the three outcomes in the opposite order, they may well find a common interest with the materialists in setting up a treaty that establishes what both agree to be the second best outcome, thus preventing either’s worst fears from coming about. These simplifications, moreover, are easily seen to be inessential: if anything the case for the tacit treaty is stronger, from everyone’s point of view, if we are all involved in multiple disagreements and also cannot always count on our supposed allies.

The irony in the description, however, is this. Lewis describes dualists and materialists, himself included, as hoping that the institution of academic philosophy will reveal truth by choosing the correct side of the argument, much as we may hope academic physics will do in the case of string theory (and much as an inquisitor might hope that society will protect true religion by suppressing heresy: see (Lewis, 2000b)). The dispute between materialists and dualists, however, is precisely a dispute about “ontic parsimony,” and we know that Lewis’s real hope for such arguments is quite different: he hopes they will reveal the truth about *prices*, and hence about our antecedent agreement on values. Why would Argle, asked to vote on hiring Bargle, ignore the “disadvantage” that, in her opinion, he is wrong? Isn’t it because there is no such disadvantage, because, rather, Argle could not purchase her view without Bargle as a counterparty? Or, to put it differently: whether or not it would be nice to use the *argumentum ad baculum* and win our arguments by literally knocking our opponents down — it would not be nice, of course, but, in any case, Lewis assures us, *it can’t be done* (for the

most part: perhaps Gödel or Gettier could have done it). But we have an inclination, a *Neigung*, to try, and the rules of ordinary argument will not prevent such rudeness. In short: on the surface, the treaty is necessary to prevent the bad outcome that the opposing school wins; but, in truth, it is necessary to prevent the worse outcome that I and my supposed allies are the victors. The *Neigung* or, say, *conatus* to *win*, which has no place in philosophical argument, is characteristic of schools of thought, with their offices and positions, their presupposed limits on what can be said and done (consistent with continuing one's career), their formal and informal rules of successorship. Only the humiliating need to treat with other schools can restrain this *libido sectarum*.

If Lewis and his followers have been more comfortable in academic philosophy than have Cavell and his — and have they not? — then I suggest that this is why. Cavell, perhaps, dreams, with Descartes and with Thoreau, that philosophy might be secure in some snowed-in, stove-heated room. But Lewis agrees with Leibniz that no such protection could be sufficient. We can expect help in these matters only from our enemies. Cavell sees Kuhnian maturity as a threat to the existence of philosophy; Lewis sees Kuhnian immaturity as the only hope of its survival.

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