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Line Drawing

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Abstract: I argue that without the analytic/synthetic distinction there can be no One True Theory of The World. In constructing and mastering a language, we begin with cases we consider clear, then extend, correct, and refine them to achieve a system of thought in reflective equilibrium. In so doing, we are sensitive to the interests, objectives, methods, and standards of the system they figure in. To achieve reflective equilibrium, tradeoffs have to be made. A variety of tradeoffs may be equally good on balance. Pluralism results. For systems of thought that draw their lines in different places are equally tenable. The sentences they contain are all true.

We can understand each other with out agreeing. Indeed, if we did not understand each other, we could not disagree. If I do not understand the contention that chitin is a polysacchride comprising chains of N-acetyl-D-glucosamine, I can neither agree nor disagree with it. This is obvious. A natural conclusion to draw is that understanding one another's utterances consists in attaching the same meanings to them, agreeing consists in assigning the same truth values to sentences we attach the same meanings to, and disagreeing consists in assigning different truth values to those sentences. If so, the meaning of a term is determinate, independently of any statements of fact it figures in. Interpretation consists in mapping antecedently fixed meanings onto the world. A sentence is true then just in case it is true under the mapping that preserves its meaning.

If we accept this picture, it is perhaps reasonable to claim that there is exactly one comprehensive true theory. Then there is exactly one correct and complete mapping of meanings onto the world. All truths derive from that mapping. To arrive at The One True Theory of The World, conjoin all the individual truths and eliminate redundancy. If there is more than one way to do so, the different 'theories' are mere notational variants of one another.

Despite its seeming naturalness, the account just sketched is untenable. Although the

truth value of a (nonvacuous) sentence depends both on meaning and fact, there is no way to distinguish the contributions made by meaning from the contributions made by facts. There are, moreover, multiple mappings that preserve whatever structural relations among terms we like. These are familiar themes in the work of Quine, Goodman, and others.[1] I don't want to rehearse the arguments for them here. Rather, I want to consider how they bear on the conviction that truth must be univocal. I will suggest that once we give them up, there can be no One True Theory of the World. There will be many true theories of many sorts of things. There will even be multiple comprehensive theories, if we are clever enough to devise them. But because such theories are reflective of diverse and sometimes divergent interests, choices, and ends, there is no hope of conjoining them to form The One True Theory of the World.

The analytic/synthetic distinction encourages the view that each term has its own necessary and sufficient conditions of application. Mastery of a term then involves knowing those conditions. But if we free ourselves from the grip of this picture and attend to our own way with words, we find that we typically know far less. I unhesitatingly apply the term 'tree' to the leafy, green thing brushing against my window. But I can neither define 'tree' nor recognize necessary and sufficient conditions for its application. This is not surprising, given the way we learn language. Mastering the word 'tree' involves coming to recognize clear instances and clear counter-instances. If something strikes me as sufficiently like clear instances of the term, I call it a tree. If it strikes me as sufficiently like clear counter-instances, I withhold the term. My efforts are subject to peer review. If I apply the term where others think I should not, they are apt to correct my usage, disagree with my claim, or be bewildered by my utterance. Because idiosyncratic usage thwarts communication, there is pressure to conform. So my uses of 'tree' are apt to converge on community standards.

But community standards fall far short of necessary and sufficient conditions. There is a range -- perhaps a vast range -- of cases that fall between the clear instances and the clear counter-instances. Even a community that accepts bivalence, and so holds that

(x) (x is a tree) v (x is not a tree),

does not specify where to draw the line. Intermediate cases must be decided one way or the other. But standard usage does not dictate a unique solution. Once we move beyond the clear cases and the clear foils, the weight of precedent is relatively weak.

It might seem that this indecisiveness is due to the fact that 'tree' is a term of ordinary language, and is learned by ostension. Ordinary language is something of a lingua franca. It cannot be too demanding if it is to serve as a vehicle of communication between disparate points of view. Moreover, ostensive learning is bound to be gappy. Obviously, we cannot preclude the possibility of undecided cases in languages of this sort. But whatever the utility of such a medium of communication in daily life, one might think, it is not suited to serious theorizing. So perhaps the indecisiveness of ordinary language does not preclude the possibility of One True Theory of the World.

The difficulty is that we acquire technical terms in much the way we acquire nontechnical ones. Granted, ostension plays a smaller role. Our teachers do not literally point to the referents of 'germ' or 'neurotic' or 'empiricist'. Still, such terms are introduced contextually. We are presented with contexts in which they clearly apply and contexts in which they clearly fail to apply. And we are expected to extrapolate from these. Thus, our teachers tell us that Locke and Hume are empiricists, Descartes and Hegel are not. They identify a few quintessential empiricist commitments and concerns. Then they expect us to classify other thinkers as empiricists or not, and to refine, extend, and emend the characterization of empiricism we were given. Here too, there is apt to be a range of undecided cases. Perhaps nothing in standard scholarly usage determines whether we should

call Nelson Goodman an empiricist. Like classical empiricists he thinks that evidence bears on theory assessment and rejects the a priori. Unlike classical empiricists, he denies that there is a given element to experience that is uninformed by conceptualization.

Where should we draw the line? Does Goodman belong to the extension of 'empiricist' or not? If it really is an undecided case, there is no fact of the matter. Still, we're apt to think, such lines ought not be drawn arbitrarily. But why not? If there currently is no fact of the matter, why can't we settle the question as we please? To be sure, any line we decide to draw will resolve the indeterminacy that is currently troubling us. But if we resolve it by drawing an arbitrary line, we think, we might draw it in the wrong place.

A critical question is: What makes a place the wrong place? A realist might reply that the predicate 'empiricist' has a determinate extension, independent of any lines we may draw. Goodman either belongs to it or he does not. If we draw an arbitrary line to settle the status of undecided cases, we will fix *an* extension. But it might not be the extension of 'empiricist' or any other genuine class. Its membership may be a hodgepodge. To confuse a hodgepodge with the class of empiricists would be a mistake. On this reading, the problem of how to classify Goodman is just epistemological. What vexes us is not indeterminacy but ignorance.

By approaching the problem through language learning, I may have encouraged such a construal. Given the way we acquire terminology, it is hardly surprising that sometimes we don't know whether a particular term applies. But ignorance is not the same as and is not obviously indicative of indeterminacy. Given the way I learned American history, it is not surprising that I do not know the full sequence of American presidents. Still, there is no indeterminacy about who was president of the United States in 1847. There is a determinate fact -- one that I do not know. Likewise, one might think, there is a determinate fact about whether Goodman is an empiricist. Owing to our inadequate education, we don't know that

fact either. Granted, the epistemological predicament is collective, not individual. And it is not so easily remedied. Perhaps we'll never know. Still there are plenty of determinate facts we'll never know -- exactly who painted the caves at Lescaux, for example. Collective ignorance, like individual ignorance, is compatible with determinacy.

If indecision is just an outgrowth of educational inadequacies, the epistemological construal looks plausible. If there are necessary and sufficient conditions for inclusion in the class of empiricists, then our inability to classify Goodman results from our not knowing what those conditions are or not being able to tell whether they apply. The problem is that there are no necessary and sufficient conditions. There are characteristic empiricist doctrines, approaches, interests, and methods. When a thinker's work manifests all or most of them, we unhesitatingly classify him as an empiricist. When a thinker's work manifests few of them, we typically deny him the title. But when neither condition obtains, we are in a quandary. Language is no more and no less than a public human practice. The only facts that bear on the interpretation of our terms are facts about the ways members of a linguistic community are disposed to use those terms. If we are undecided about whether to apply or withhold the term 'empiricist' or if knowledgeable speakers are about evenly divided in their verdicts, the matter is indeterminate. Without the analytic/synthetic distinction, there is no fixed criterion independent of what speakers say and do that all and only empiricists satisfy. Hence there is no decision procedure for telling who is and who is not an empiricist.

Even so, our reluctance to settle such matters arbitrarily is not misguided. It reflects the realization that decisions have consequences. Suppose, for example, that we stipulate that Goodman is to be included in the extension of 'empiricist'. Our stipulation will do more than settle Goodman's status. It will shift the precedent class against which further cases are to be decided. Philosophers such as Hilary Putnam, whom we might initially have been disinclined to consider empiricists, now look like plausible candidates. Even though they

have little in common with Berkeley and Hume, their views have a lot in common with Goodman's. The stipulation effects a reweighting of grounds as well. To count Goodman an empiricist is to concede that empiricists need not maintain that the mind is mostly passive in the reception of its sensations. What was previously considered a central tenet of the doctrine is now consigned to a more marginal role.

These considerations might seem to show that my test case is poorly chosen -- that Goodman ought definitely not to be classified as an empiricist. But the decision to exclude him is fraught with equally worrisome consequences. If we decide that Goodman is not an empiricist, how does that bear on our classification of Quine and Carnap? Should we exclude them as well? If Goodman's rejection of the Given is sufficient to rule him out, should we also exclude all philosophers of science who insist that observation is theory laden? I do not intend to answer these questions. Nor will I attempt to decide whether Goodman belongs in the empiricist camp. My point in belaboring the issue is not to argue for a particular answer, but to highlight the sort of issue it is. The more we investigate it, the less confident we become that there is a determinate, independent fact of the matter. As our inquiries proceed, we come to appreciate the role of interests, precedents, priorities, and objectives in deciding matters of this sort. We realize how much remains to be done after the facts are in.

I don't know whether Goodman should be called an empiricist. Moreover, I suspect that my indecisiveness would not be alleviated by additional information. Deepening our understanding of Goodman's philosophy and the empiricist tradition would no doubt be intellectually rewarding. But at the end of the day, I suspect that we would find ourselves facing the same predicament. We would have a more refined understanding of the similarities and differences between Goodman's theory and traditional empiricism. Our problem then as now would be how to weigh them. If the similarities outweigh the

differences, Goodman should be classed among the empiricists. If the differences outweigh the similarities, he should not.

The problem is to find a scale on which to do the weighing. Facts don't determine what significance we should attach to them. So scholars who agree that Goodman rejects the Given and agree that a commitment to the Given plays a central role in traditional empiricism, might still disagree about how repudiation of the Given affects Goodman's empiricist credentials. If so, they disagree not about what the facts are, but about how much they matter.

We cannot settle on a suitable scale until we decide what we want our scheme of classification for -- what cognitive and/or practical projects we want it to figure in, what ends we want it to promote. Moreover, the decision must be made holistically. We need to attend not just to how our decision bears on what we say about Goodman, but also on what it enables us to say, and precludes us from saying about other figures, doctrines, traditions, and the like. If, for example, we decide to exclude Goodman because the phenomenalism developed in *The Structure of Appearance* cannot serve as a basis for physicalism, we are forced to concede that the extension of 'empiricist' is empty. For despite grandiose promises and protestations to the contrary, no one else has been able to contrive a phenomenalist basis for physicalism either. Such a conclusion is not obviously misguided. For some purposes, it may be reasonable to construe empiricism as an ideal, long espoused but never realized. For other purposes, though, we want to hold fast to the conviction that some philosophers are empiricists and others are not. In that case, we cannot take empiricism to require that all talk of physical objects reduce to or supervene on a phenomenalist base. We need another way to mark the distinction.

Reluctance to draw arbitrary lines reflects the recognition that classification is responsive to reasons. There are reasons to think that Goodman is an empiricist and reasons

to think that he is not. To classify Goodman correctly, we must give those reasons their due. But there is no saying what reasons are due independently of the values, priorities, and goals of the theory we seek to construct. They determine whether, for example, it is unobjectionable, regrettable, or intolerable to construe empiricism as an unrealized and potentially unrealizable ideal. And the verdict on that question in turn will affect the range of revisions we are willing to entertain to avoid that conclusion.

Eliminating vagueness is not so easy as we're apt to imagine. There are generally a range of options, each drawing lines in different places, each yielding different truths. Any resolution is apt to affect other commitments, occasioning reconsideration and even revision of previously accepted findings. The platypus provides a nice example. It seems to be an intermediate case between a mammal and a bird. But it is a longstanding principle of taxonomy that there can be no such intermediate cases. If we respect our intuitions, and classify the platypus as both a mammal and a bird, or as part mammal and part bird, we must abandon that principle, and devise new taxonomic standards. Clearly, that could be done. And the more seemingly intermediate cases we find, the greater our incentive will be to take that route. But to the extent that the hierarchical structure of contemporary taxonomy serves biology's purposes and advances the sort of understanding the science seeks, we have reason to prefer a different resolution. We might then declare the platypus a bird. In that case, we must revise our settled conviction that birds have feathers, not fur. Or we might declare it a mammal. If so, we must give up our conviction that mammals don't lay eggs. We can classify platypuses as birds, as mammals, as neither, or as both. Whatever we decide, the impact on zoology will be significant.

Classifying hitherto undecided cases thus often requires revisions of prior commitments. To choose among alternatives we need to assess the impact of those revisions. We cannot make such an assessment without understanding the system of thought they figure

in, the interests it is designed to serve, the ends it seeks to promote. Plainly, classifying a platypus as both a mammal and a bird has the effect of creating an intersection of two categories hitherto considered disjoint. But unless we appreciate the role taxonomic categories play in biology, and the sort of understanding biology seeks to supply, we cannot tell whether that is a good or a bad thing. On the other hand, once we appreciate such matters, the answer may be obvious.

Resolving indeterminacy often requires revising, not merely augmenting antecedent cognitive commitments. A proposed resolution is justified if the resulting cluster of commitments best serves our relevant cognitive (and perhaps practical) objectives. If this is so, then other modifications of schemes of classification are justified in the same way. Even if there is no relevant vagueness or indeterminacy, a scheme of classification may be ill suited to the discipline that seeks to deploy it. If so, it can be revised to better serve the discipline's objectives. Suppose, for example, we discover important physiological features, *F*, to be characteristic of a wide range of animals including orangutans, bats, lions, and whales, but excluding turtles, robins, lobsters, and trout. If we retain the initially tenable commonsense classification of whales as fish, our findings conform to the hypothesis

All mammals and some, but not all, fish display *F*,

a cumbersome, seemingly ad hoc generalization ill suited to science. But if we reclassify whales a mammals, our findings support

All and only mammals display *F*.

By streamlining our terminology, we accommodate our findings to tenable commitments about science.

A category scheme imposes an order on a domain, classifying some elements as alike, others as unlike. Its merits depend on its utility, an effective scheme being one whose organization of its realm suits our purposes. Rightness of categorization thus consists neither

in blind fidelity to tradition nor in accord with an antecedent metaphysical order, but in meshing with other tenable commitments to promote tenable ends. If our revised taxonomy proves more effective than its predecessors and competitors, if its adoption advances understanding, the suggested revision is justified.

Of course, goals are not sacrosanct either. We may discover that our objectives are jointly unsatisfiable or require revisions of beliefs, methods, or standards we consider beyond cavil. Despite our best efforts, we cannot simultaneously maximize precision and intersubjective accord. A science that seeks both will have to settle for a compromise. For the more exact our measurements, the less agreement there will be about readings.

I have argued elsewhere that a tenable system of thought is a network of cognitive commitments in reflective equilibrium.[2] To construct such a system, we begin with whatever commitments -- beliefs, standards, categories, methods, and goals -- we consider to be relevant and reasonable. These are our initially tenable commitments. They are apt to be incomplete, inchoate, mutually inconsistent, or otherwise at odds with one another. Although they are not acceptable as they stand, they give us a place to start. For they are our current best guesses about the subject at hand. To arrive at a tenable theory or system of thought, we need to revise, correct, and extend these commitments to bring them into reflective equilibrium. The standard of acceptability is high. The commitments that constitute a tenable system of thought must be reasonable in light of one another, and the system as a whole must be at least as reasonable as its competitors in light of the initially tenable commitments that tether it. This is not to say that the system that emerges must incorporate the commitments that comprise its tether. But if a system diverges from its tether, it should reveal why the initially tenable commitments seemed reasonable to the extent that they did. Thus, for example, although the theory of relativity does not deploy Newtonian concepts, it shows that they yield good approximations at short distances and

slow speeds. Hence it shows that those concepts were reasonable when the evidential base was restricted to information about what happens over short distances and at slow speeds.

A tenable system does not, of course, consist entirely of initially tenable commitments or their emendations. To achieve a mutually supportive system of thought, we typically have to incorporate factors we have no independent reason to hold. Thus, for example, considerations of symmetry might lead us to build a commitment to positrons into our physics, even though we have no independent evidence that positrons exist. The justification for a commitment to positrons then is that that commitment is integral to a tenable system of thought.

Reflective equilibrium results from a process of adjudication where competing commitments are modified to bring them into accord and novel hypotheses are introduced to forge connections among its various elements. Plainly, not just any revisions will yield a tenable system. For tenability requires that the system be one we can on reflection accept. In contriving a system, we are guided by precedents, priorities, and higher order commitments. But these are neither fine-grained nor well-ordered. Often they will fail to yield a unique solution to the problem we face.

Adjudication requires tradeoffs, and different tradeoffs may be equally reasonable. One system might sacrifice a measure of precision to achieve a higher degree of intersubjective accord. Another might demand greater precision and settle for less accord. One might classify platypuses as mammals; another might classify them as birds. Both would require revisions in our antecedent commitments about the animal kingdom, but the theories that reflected those revisions would diverge considerably. One might, for example, mesh with a system that tracks geneology, the other mesh with a system that advances the interests of morphology. Biology could decree that genetics trumps morphology (or vice versa), and mandate that all the life sciences deploy the same system of categories. If

agreement across subspecialties is sufficiently important, this is a reasonable line to take. But it is a line that need not be taken. For such a mandate has a price. The best taxonomic system for the biological sciences taken collectively may be significantly less than the best for some of them considered separately. The lines the shared taxonomy draws may be relatively ill-suited to the issues a subspecialty seeks to investigate. If genetics and morphology address different questions and reflect different interests, it would be neither surprising or distressing to find that divergent systems of categories best suit their purposes. Then the kinds recognized by genetics would diverge from the kinds recognized by morphology.

Pluralism results. For a variety of tradeoffs yield systems in reflective equilibrium. A black hole counts as a star under one system, as the residue of an extinguished star under another. Both classifications serve the interests of astronomy. But they give divergent answers to a wide range of questions, such as 'How many stars are in the constellation Cygnus?' and 'How dense can a star be?' If, as I have supposed, both systems are in reflective equilibrium, both are acceptable. Both, moreover, yield truths.

This might be doubted. The method of reflective equilibrium, as I've sketched it, is epistemologically motivated. If I'm right, it shows that we often have no reason to prefer one theory to another. Both are then equally acceptable. But this hardly shows that both are *true*. Truth concerns what is the case, not what we have reason to think is the case. If my theory is correct, why doesn't it show that we often cannot tell which tenable theory is true? In that case, there may well be exactly One True Theory of The World. But we will never be able to tell which theory it is. This is a familiar skeptical conclusion.

The foregoing objection ignores the points I made about vagueness at the beginning of the paper. If a term is vague, there are a number of divergent extensions, any one of which is qualified to serve as *its* extension. All the candidates include the items that clearly

instantiate the term and exclude all the items that clearly fail to instantiate it. They diverge only over undecided cases. Under any resolution to the problem of vagueness, the term will have a determinate extension. There will be truths and falsehoods about what the term applies to. If different resolutions are equally good -- equally serviceable, elegant, economical, and so on -- different distributions of truth and falsehood will be tenable. And if they are equally good, nothing favors one of them over the others. Nor is this only a consequence of vagueness. Whenever disparate tenable systems assign divergent interpretations to the same terms, we may expect it to occur. Under one tenable interpretation, 'Goodman is an empiricist' is true. Under another, it is false. Under one, 'All stars are visible' is true, under another, it is false. Truth then is relative to interpretation, and multiple, divergent interpretations may be equally good.

A problem remains. According to a system that counts black holes as stars, 'Some stars are invisible' is true. According to one that denies that black holes are stars, 'All stars are visible' is true. If both sentences are true, their conjunction is true. But

(A) (Some stars are invisible) & (All stars are visible)

looks like a contradiction. If it is, then every sentence is true. For every sentence follows from a contradiction. This is disastrous. It eliminates the distinction between truth and falsity. Nor is truth the only casualty. The distinctions between the tenable and the untenable, the justified and the unjustified, the rational and the irrational evaporate as well. For the immediate, obvious logical consequences of a truth we accept are tenable, justified, rational. Blatant contradictions that we recognize as such are untenable, unjustified, irrational. But if we countenance (A) and (A) is a contradiction, the same sentences are both immediate, obvious logical consequences of accepted truths and blatant contradictions that we recognize as such. They are then simultaneously tenable and untenable, justified and unjustified, rational and irrational. We have then no grounds for favoring some sentences,

theories, systems of thought, and repudiating others. For we have no cognitive norms to underwrite our conclusions.

Appearances can be deceiving, though. Although (A) looks self-contradictory, it is not. For 'star' is ambiguous, bearing one extension in the first conjunct and another (narrower) one in the second. Although imperspicuous, (A) is not only self-consistent, but true. Some of the items in the extension of the term 'star' that appears in the first conjunct (viz., black holes) are invisible. All of the items in the extension of the term 'star' that appears in the second conjunct are visible, since that extension contains no black holes. The existence of divergent tenable systems that employ common terminology gives rise to ambiguities, not contradictions. Interpreting a claim like 'All stars are visible' then involves knowing what system it is functioning in, and where that system draws its lines.

Even though I have the resources to avoid countenancing contradictions, there remains the worry that I can't block the conclusion that anything goes. I urged that tenability results from a process of adjudication of competing commitments. We preserve one commitment by suitably modifying others. If so, what is wrong with digging in our heels, holding fast to whatever benighted view is dearest to our hearts, and making the necessary adjustments elsewhere in the system to obtain equilibrium? Obviously, if we're sufficiently stubborn and clever, we could design a system in equilibrium around whatever we like. But not every equilibrium is reflective. To incorporate a claim like 'The Earth is flat' or 'π is a rational number' or 'The Holocaust never happened' would require massive, implausible revisions of other initially tenable commitments concerning geography, mathematics, and history, and the methods, norms, and standards appropriate to these fields. The resulting systems of thought would be far less tenable than their rivals. They would not then be systems we could on reflection endorse.

I have argued that the boundaries we draw are not dictated by nature or built into the

language we speak. They are influenced by precedents, interests, values, and objectives. These are multiple and can be variously realized. So there is no uniquely best place to draw our lines. Pluralism results. If any system of thought is tenable, several are apt to be. But since precedents, interests, values, and priorities provide constraints, it is not the case that all systems of thought are tenable, or all statements true. There are multiple tenable systems of thought, and multiple, divergent truths. But not every system of thought is tenable, nor is every statement true. If many theories, statements, and systems are right, many more remain wrong.

NOTES

[1]See W. V. Quine, 'Two Dogmas of Empiricism,' in *From a Logical Point of View* (New York: Harper Torchbooks, 1961), 20-46; Nelson Goodman, 'On Likeness of Meaning,' in *Problems and Projects* (indianapolis: Hackett, 1972), 221-230; Morton White, 'The Analytic and the Synthetic: An Untenable Dualism', in *Semantics and the Philosophy of Language, ed. Leonard Linsky (Urbana, Illinois: University of Illinois Press, 1972), 272-286.*

[2]*Catherine Z. Elgin,* Considered Judgment, Princeton: Princeton University Press, 1996.