APPENDICES

Appendix A:

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FEIGL & THE DEVELOPMENT OF ANALYTIC PHILOSOPHY AT THE UNIVERSITY OF MINNESOTA[1]

by
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ABSTRACT. This paper describes the development of analytic philosophy at the University of Minnesota from the time Herbert Feigl arrived there in 1940 to the late fifties when the author was there as a graduate student. The author relates how, after Feigl arrived, Wilfred Sellars soon joined the department and how the two men contributed to making the university an important center for teaching, research, and publication in analytic philosophy. He also describes how, with the assistance of gifted younger colleagues, they developed the journal, Philosophical Studies, how Feigl created and managed the Minnesota Center for Philosophy of Science, and what distinctive individual contributions the two men made to analytic philosophy.

Feigl came to Minnesota in 1940.[2] He had immigrated to this country from Austria ten years earlier, spending the intervening years mainly at the University of Iowa. Originally a member of the Vienna Circle, he was, as he once said, the first missionary for logical positivism.[3] In 1941 he did not actually consider himself a positivist, however; by that time he had moved on to the successor position he called logical empiricism.[4]

I shall describe the kind of analytic philosophy that Feigl endorsed in 1941, but before doing so I want say something about the philosophical scene that Feigl encountered when he came here. I cannot say a lot about that scene because it was before my time and I do not have access to the pertinent records. Yet I do remember "Perry" (George Perrigo) Conger, who was here before Feigl and still chairman of the department in 1950 when I visited the university as a prospective student; and I heard many stories during my undergraduate years about Aulbury Castell, another important department member, who preceded Feigl and left the university before I arrived. (Castell was one of the creators of the Humanities Program, which offered a series of great books courses greatly valued by undergraduates of my generation.) Both Conger and Castell published a fair amount– the library of my university[5] has six books written or edited by Conger and five written or edited by Castell—and it is clear that neither man was an analytic philosopher. Conger, who was also a Presbyterian minister, was an integrator of knowledge; some of the titles of his books are "Synoptic Naturalism," "The Ideologies of Religion," "New Views of Evolution" and "Theories of Macrocosms and Microcosms in the History of Philosophy." Castell, as I know from reading his *Modern Introduction to Philosophy* in my sophomore year, was a more secular thinker than Conger, but as we can infer from the claims of Augustine Castle, the dialectically astute but anti-behaviorist opponent of B. F. Skinner's protagonist in *Walden Two*, he was very critical of Feigl's logical empiricism. Skinner, who taught in the Psychology Department here before going to Harvard, was a regular participant in discussions Castell regularly held in his apartment near Dinkytown,[6] and he regularly disagreed with the position Castell took in those discussions.[7]

The Second World War broke out for the United States shortly after Feigl's arrival here, and in consequence of it the philosophy department was apparently a fairly quiet and uneventful place until the war ended and "the boys," the students, came back. But things became quite lively thereafter. Wilfrid Sellars arrived in 1946, and it couldn't have been long until John Hospers and May Brodbeck were also there, for their names, along with that of Paul Meehl, accompanied those of Feigl and Sellars on early issues of Philosophical Studies (the five of them may, in fact, have created the journal together). Michael Scriven, Burnham Terrill, and Francis Raab (all analytic philosophers) were here in the early fifties when I was an undergraduate; and so were Paul Homer, a Kierkegaard scholar and philosopher of religion, and Mary Shaw, a historian of philosophy. Burnham Terrill, who taught the first course in philosophy I took here, had Hector-Neri Castañeda as his TA. Castañeda, I might add, was an enthusiastic discussion leader but sometimes a very perplexing one. Once he had our class seriously baffled by some extended remarks about "trasses" in the snow: the trasses turned out to be footprints. This reference to Castañeda reminds me of the recently circulated photo of the department taken in Westbrook Hall in the mid 50's, a photo in which Hector appeared. The photo is noteworthy in showing that the department was ahead of its time in gender matters, for two of the six senior professors, one-third, were women. A person in the photo whom I have not yet mentioned was Alan Donagan; he joined the department in about 1956 and remained here for, I think, four years. He eventually became chairman of the department when Wilfrid Sellars left.

"Philosophical analysis" seems to have been the favorite adjective of the philosophy department in the early fifties. Not only did Feigl and Sellars entitle their anthology Readings in Philosophical Analysis, but Hospers entitled his textbook, the first edition of which was published in 1953, Introduction to Philosophical Analysis; and Wilfrid Sellars referred to his graduate seminar as "the analysis seminar" (I think its official name was "Seminar in Philosophical Analysis"). What did they mean by "philosophical analysis"? Probably not the same thing, though they might not have realized it the early fifties. Feigl and Sellars summarized their view in their preface to *Readings in Philosophical Analysis*. It is surprisingly inclusive:

> The conception of philosophical analysis underlying our selections springs from two major traditions in recent thought, the Cambridge movement, deriving from Moore and Russell, and the Logical Positivism of the Vienna Circle (Wittgenstein, Schlick, Carnap) together with the Scientific Empiricism of the Berlin group (led by Reichenbach). These, together with related developments in America stemming from Realism and Pragmatism and the relatively independent contributions of the Polish logicians, have increasingly merged to create an approach to philosophical problems which we frankly consider a decisive turn in the history of philosophy (p. vi).

If you read Feigl's "Logical Empiricism," you will see that the empiricism he espoused was in the forefront of empiricist thinking at the time and closely related to orthodox philosophical thought in English-speaking countries today. The philosophical tradition he represented centered its chief inquiries, he said, around two humble questions, "What do you mean?" and "How do you know?" A fundamental tool, for him, in the proper pursuit of the first question was a confirmation criterion of factual meaning, according to which no sentence is factually meaningful if it is not in principle capable of being confirmed or disconfirmed–

that is, as Feigl also put it, of being tested at least indirectly and incompletely (p. 10). Although no one, at least to my knowledge, espouses this kind of meaning criterion today–the last to do so, perhaps, was Carnap, who published his final paper on the subject in the first volume of *Minnesota Studies in the Philosophy of Science*[8]–most main-line analytic philosophers nevertheless share the spirit of Feigl's attitude to factual assertions that do not satisfy such a criterion. They don't pay any attention to them. To say this is not to imply or suggest that some of the objects or entities main-line philosophers recognize today would be enthusiastically embraced by Feigl: possible worlds are perhaps a case in point. But those who speak of such things with a good conscience–David Lewis is the obvious example here–always emphasize the rational basis they have for postulating them.

In fairness to Feigl I should mention the word of warning he appended to his discussion of the criterion of factual meaning in "Logical Empiricism." His words were:

> The danger of a fallaciously reductive use of the meaning-criterion is great, especially in the hands of young iconoclasts. It is only too tempting to push a very difficult problem aside and, by stigmatizing it as meaningless, to discourage further investigation. If, for example, some of the extremely tough-minded psychologists relegate questions such as those concerning the instincts, the unconscious, or the relative roles of constitution and environment to the limbo of metaphysics, then they cut away with Ockham's razor far into the flesh of knowledge instead of merely shaving away the metaphysical whiskers. No meaningful problem is in principle insoluble, but there is no doubt that the human race will leave a great many problems unsolved (p. 13).

Feigl's great friend Carnap reinforced these fair-minded words for me some years after I had first read them, when I attended a seminar of his at UCLA in the late fifties. Before a particular meeting got under way, a friend and I were chattering about metaphysics and, no doubt hoping to impress Carnap with our commitment to the tough-minded ideology he was noted for espousing, expressed our utter derision for some claim by Heidegger. Carnap's response was immediate: "Tolerance, boys, tolerance." It stopped us in our tracks.

The other humble question basic to Feigl's logical empiricism, "How do you know?", was associated with a distinction between a priori and a posteriori methods of obtaining knowledge or justifiable opinion. In opposition to Quine ("Two Dogmas" was published in 1951) and also to Tarski (who had expressed his opinion to Feigl orally), Feigl thought that an analytic/synthetic distinction was fundamental to critical analysis and that the existence of synthetic a priori knowledge should be firmly opposed by every empiricist. Officially, he restricted synthetic justification to just three sources, observation, memory, and inductive inference; and officially he claimed that a reasonable clarification of the terms "reality" and "justification" would suffice to avoid skeptical problems about the reality of the external world, the existence of other minds, and the justification of induction. Unofficially, as one can tell from a careful reading of "Logical Empiricism" and related essays, he actually found both observation and nondeductive forms of inference troubling in various respects, and his concern with problems related to them and to what he called semantic realism resulted in vigorous and productive discussions among the staff of and visitors to his Center for Philosophy of Science in the late fifties. The precise way an analytic-synthetic distinction should be understood or drawn was also a significant source of concern to him, and it too resulted in fruitful Center discussions. I recently reread Grover Maxwell's very brief paper, "The Necessary and the Contingent," which was originally written as a memo for such a discussion, and I was surprised and delighted by its shrewdness and profundity. It develops Feigl's objections to Quine's criticism in "Two Dogmas" and introduces novel ideas that deserve elaboration.[9]

According to Francis Raab, who was a student here before the war and on the Philosophy faculty during the fifties, Feigl was an aggressive defender of logical empiricism in his early years here. But if he was once an aggressive man, he certainly mellowed a lot by the time I knew him in the late fifties. Because he felt significant uncertainty about many philosophical issues, having (as he told me) view A on Monday, Wednesday, and Friday and view B on Tuesday, Thursday, and Saturday (maybe he didn't have any view on Sunday), he was eager to discuss central issues with other philosophers, and, unlike many important figures, he was genuinely interested in hearing what other philosophers had to say--even when their methods or views were significantly different from his. Seeing his modus operandi at the Center, I used to think that the success of the Center as a source of fruitful philosophical activity was owing in large part to his ability to stimulate discussion, to draw people out, and also to put up with the difficult egos and occasional abrasive behavior of some of his colleagues and Center visitors.

One serious source of philosophical uncertainty for Feigl was the status of what he called "existential hypotheses"–assertions about entities not directly observable. Although in "Logical Empiricism" he said that the seductive tendency to adopt a phenomenalist rather than a realist interpretation of the external world can be avoided by due attention to the actual meaning of "reality" (he says the term designates what is located in space-time and is in the chain of causal relations--p. 16), he acknowledges that the matter is much more complicated in his long paper of 1950, "Existential Hypotheses: Realistic Versus Phenomenalistic Interpretations."[10] After surveying nine different "points of view" regarding hypotheses that concern entities that cannot be directly observed, Feigl plumps for what he calls semantic realism on the ground that it does not possess the weaknesses he sees in the other points of view and that it fits in nicely with the anti-metaphysical, naturalistic idea (which empiricists should accept) that human beings are "severely limited in their direct awareness of (or immediate acquaintance with) the universe in which they are embedded and of which they form a natural part" (p. 59). Hempel and others attacked his positive case for semantic realism,[11] and Feigl himself was not entirely satisfied by it. Grover Maxwell, when he became a member of the Center in, I believe, 1957, became concerned with the matter and improved Feigl's case on this subject too–first in a memo,[12] whose title I have never forgotten, "Yes, Virginia, There are White Molecules," and then in his well-known paper, "The Ontological Status of Theoretical Entities."[13]

Feigl was ahead of his time in defending semantic realism, for phenomenalism was very much alive in 1950 and J.J.C. Smart's *Philosophy and Scientific Realism* did not appear until 1963. (It was even worth my while to attack philosophical behaviorism and to argue for an incompatibility between physical science and common-sense colors in a book I published in 1967.)[14] Another area in which Minnesota philosophy was ahead of its time was that of the mind-body problem. Feigl and Sellars both wrote important essays on the subject in the early fifties, Feigl focusing in 1950 on what he called "raw feels" and Sellars focusing in 1952 on what he regarded as mental states proper, these being intentional states such as belief, doubt, desire, choice, expectation, and fear (the list was his). The views expressed in these essays have a remarkably contemporary air; they are not all that different from views being expressed today.

Feigl's magnum opus on the mind-body problem was published in 1958,[15] but his earlier essay, which was called "The Mind-Body Problem in the Development of Logical Empiricism,"[16] contained the basic idea that he worked out in the longer paper. Although he embedded this basic idea in a very complicated dis-

cussion, the idea itself can be stated very simply. He put it this way in the longer paper: "[the] private states known by direct acquaintance and referred to by phenomenal (subjective) terms can be described in a public (at least physical$_1$) language and may thus be empirically identifiable with the referents of certain neurophysical terms" (p. 448). He also formulated it thus: "The 'mental' states or events (in the sense of raw feels) are the referents (denotata) of both the phenomenal terms of the language of introspection, as well as of certain terms of the neurophysiological language" (p. 46).

Philosophers who objected to Feigl's view shortly after his long essay was published were not generally bothered by the idea that an afterimage may be a physical occurrence in the brain; they were bothered by the idea that the qualities of the occurrence that we are or can be introspectively aware of in having the afterimage could be physical qualities.[17] Feigl himself seemed to have no doubt about the peculiarity of such qualities, for he said that "the central puzzle of the mind-body problem is the logical nature of the correlation laws connecting raw feel qualities [my italics] with neurophysiological processes."[18] Yet to my knowledge Feigl never responded specifically to the question he did acknowledge, "Aren't raw feel qualities–the qualities you concede that a subject may be 'immediately acquainted with'–different from the distinctively physical qualities of a neurophysical process?"[19]

A currently trendy strategy for coping with the problem Feigl failed to resolve is to argue that a sensory experience or raw feel is a representational state, a neural state that represents something as having phenomenal properties but does not possess those properties itself. William C. Lycan defended this strategy in a paper I heard him read last month at Amherst College--and also in a book he recently published but I have not yet read.[20] Speaking of an afterimage, he said that the occurrence of such a thing is the occurrence of a representational state, a state representing, say, a greenish-yellow disk but not itself exemplifying greenish-yellow or circular qualia. The disk thus represented does not belong to the actual world any more than the object of Meinong's idea of a golden mountain belongs to the actual world. It is what the afterimage is of, not what the afterimage is or is like. The afterimage does possess properties by virtue of which it represents a disk; it contains the disk's "mode of presentation." But it does not thereby possess phenomenal attributes, attributes that are incompatible with its character as a neurophysiological process.

I am afraid I am not convinced by Lycan's strategy for solving what I call Feigl's problem. If sensory states–Feigl's "raw feels"–are representational states, they represent what they represent by means of the features that are apparent to us when we attend to them. To think of a golden mountain, we do not have to form an image of a golden mountain; but if we do have an image of something golden–a "golden" afterimage–we are aware of something in some sense golden. In what sense golden? Not golden in the sense in which a gold watch may be perceived to be golden. But "golden" is the best word to use in describing the image, if one speaks English; and it is, I believe, reasonable to say that the attribute of an image that makes us want to describe it by "golden" is very similar to, if it is not the same as, a salient attribute of the visual experience we have when we see something golden in good light. In any case, if no phenomenal feature of the sort I describe by speaking of something "in some sense golden" actually existed in subjective experience, Feigl's mind-body problem would never have arisen and no one would have worried about it. But the problem has arisen, and thousands of people have worried about it.

The 1952 paper by Sellars on the mind-body problem, to which I referred when I introduced Feigl's early paper, was called "A Semantical Solution of the Mind-Body Problem."[21] Taking intentionality to be the mark of the mental, Sellars was concerned with the relation of such states as believing, doubting, and fearing to the body. Unlike Lycan and others–perhaps unlike the majority of current thinkers on the consciousness-physical problem–Sellars did not regard sensory experiences, afterimages, and feelings as having genuine intentionality. It is not as if he didn't consider the matter carefully. When he introduced his mind-body problem, he remarked that "instances of red or sweet or C# or adjacency in a visual field are not as such about anything, nor do they as such refer to anything, though, of course, they are referred to physical objects in what Professor [H.H.] Price has called 'perceptual consciousness' (p. 46)." The differences between Sellars and Lycan et al may be largely terminological, for the relation Sellars called "is referred to" may not differ significantly from what these more recent philosophers have in mind by "represents." But however that may be, the intentional character of believing, doubting, and choosing seems (at least to me) clearly different from the of-ness character of a pain, an after-image, or a sensory experience.

The reason why I wanted to attention to this early paper by Sellars–to this paper written in the early days of analytic philosophy at Minnesota–is that it is the first paper I know of by a philosopher in which a functionalist view of the mental is clearly and explicitly developed. It therefore, in my opinion, has considerable historical importance. I won't attempt to summarize Sellars's argument in the paper, but I do want to identify certain basic ideas. Perhaps his most basic idea–a highly general and abstract one but, I think, a true one nevertheless–was that (as he put it) "an adequate basis for the definition of all mentalistic terms [of the sort he has in mind] can be found in [the expression] 'act of thought' [or simply 'thought,' the abbreviation he uses] and 'about' together with expressions of a non-mentalistic kind. Thus [he added] 'x is a thought about O' would be a basic sentence of the mentalistic language." A thought, as he understood it, is essentially the same as what Jerry Fodor calls "a sentence in the language of thought,"[22] and Sellars's idea was that "believing," "choosing," and so forth can be defined by reference to "thinks" (in the relevant sense of "thinks"). Sellars's other basic idea was that an act of thought is "an event that is about something." He notes that one might be tempted to suppose that his *definiens* should read "a mental event which is about something," but if, as he pro- poses, "mind" and "mental" (in his sense) are to be defined in terms of thought and not vice-versa, the qualification is misplaced.

The third basic idea in Sellars's analysis was that an event (it amounts to a tokening) is about something just in case it "means something," and an event means something (in the relevant sense) just in case it plays an appropriate role in an event-system of the subject (here I am using my own words) that is relevantly similar to the speech-system, the spoken or otherwise inscribed language system, of a normal person. Thus, to take one of Sellars's examples, an utterance u of Schmidt's means that it is raining just in case u plays a role in the (conceptual) "economy" of Schmidt that is relevantly similar in kind to that played by an utterance of "It is raining" in my linguistic "economy." (Sellars thought that meaning-statements have a token-reflexive character that I won't go into here.)

Sellars's final idea, and his solution to his mind-body problem, was that a bodily event or state, by virtue of playing an appropriate role in system-entry, system-exit, and inferential transitions, could be considered a mental event or state. Since mental phenomena (in his sense) are all ultimately analyzable by ref-

erence to mental sentences, all mental phenomena (in that sense) may be physical–ultimately, physical role-players. This is the key idea of a physicalist functionalism of the mental, and Sellars, to my knowledge, was the first philosopher to espouse it.

I have devoted most of my attention to Feigl and Sellars, because they were the major figures in the development of analytic philosophy in this university. They shared a common purpose in philosophy, Sellars said later in some "Autobiographical Reflections"; it was "to formulate a scientifically oriented naturalistic realism which would 'save the appearances'."[23] Not all the colleagues who soon joined them here were equally committed to a philosophy of this kind, however. Though an analytic philosopher (at least of sorts), Paul Holmer was interested in Kierkegaard, the philosophy of religion, and even Ernst Cassirer's Philosophy of Symbolic Forms (not congenial subjects to Feigl and Sellars); and John Hospers, a specialist in esthetics, also had a much more humanistic orientation in philosophy. May Brodbeck, trained at Iowa by Gustav Bergmann, taught logic and the philosophy of science, and collaborated with Feigl on a companion to the reader Feigl did with Sellars; they called it Readings in the Philosophy of Science. By the time that Michael Scriven, Francis Raab, Burnham Terrill, and Alan Donagan joined the department in the mid-1950's, analytic philosophy at Minnesota had become quite varied. When I wrote my Ph.D. preliminary exams in 1959, I had a strong feeling that there was no longer a departmental consensus on how philosophy should be done, and my written prelims were gently criticized for being excessively noncommittal on philosophical method.

In spite of their common purpose in philosophy, Feigl and Sellars did not agree on everything philosophical. As Sellars re- marked in his "Autobiographical Reflections," although he and Feigl "hit it off immediately" when he joined Feigl at Iowa, the seriousness with which he took such ideas as causal necessity, synthetic a priori knowledge, intentionality, ethical intuitionism, the problem of universals, and so on and so forth, must have jarred Feigl's empiricist sensibilities. "Even when I made it clear," Sellars said, "that my aim was to map these structures into a naturalistic, even a materialistic, metaphysics, he felt, as many have, that I was going around Robin Hood's barn."[25]

When Wittgenstein's Philosophical Investigations came out in 1953, Sellars read it right away (as he did almost everything in philosophy), and got interested. In AY 1956-57 he gave a year's seminar on Wittgenstein, starting with the Tractatus (which he thought was a masterpiece) and then going on to the Investigations. I attended that seminar; it was my first seminar here, and the most exciting seminar I ever attended. But others here had been reading Wittgenstein, too: Francis Raab became something of a convert, and Scriven and Donagan, who had Oxford D.Phil.'s or B.Phil.'s, were also influenced by it, though they were not true believers. Hospers and Holmer both left the department at about that time; Mary Shaw retired; and three younger men were appointed: Reginald Allen, the classicist; Newton Garver, another person strongly influenced by the late Wittgenstein; and Gene Mason, someone whose views are familiar to all of you. May Brodbeck remained a Bergmanian logical atomist; she thought Wittgenstein's new testament was a mistake, and she was not happy about the attention it was getting.

Although the department had become more Wittgensteinian–and therefore less focused on science and logic–in the later 50's, the Center continued to be an active enclave of scientifically oriented philosophy. I know this because I eventually became Feigl's student (I had been away from the department for a year, at UCLA) and was appointed to a research assistantship in the Center. The mid to late 50's was a particularly exciting time at the Center. Arthur Pap, Hilary Put-

nam, Adolf Grünbaum, Russell Hanson, Paul Feyerabend, and Wesley Salmon all came for lengthy stays, and Grover Maxwell became an active member of the Center Staff. In spite of the philosophical diversity, all analytic in one way or another, that existed here in the late 50's, people seemed to speak freely with one another, and there was a lot of exciting discussion.

I want to conclude these remarks by calling attention to one very exceptional trait that Feigl and Sellars shared and that helped create the philosophical activity that I have been talking about. This trait was an ability to have philosophically worthwhile discussions with people who didn't share their views. Feigl was particularly good at this. In conversations with Wittgenstein, he once told Paul Feyerabend, Wittgenstein would often start out with a long outburst of criticism against Carnap; and then, having unburdened himself, he would contribute a lot to illuminating and constructive argument.[26] Feigl told me of similar incidents with Karl Popper. I can also recall hearing tapes of Center discussions in which Feigl would patiently carry on, putting questions to irascible or egoistic visitors, finding common ground and moving things along. He showed the same forbearance with me, when as a thesis-writing graduate student I attacked empiricist ideas dear to his heart with Wittgensteinian objections relating to private languages. Sellars was also good at talking with unbelievers. He unwittingly illustrated this himself in his "Autobiographical Reflections", when he spoke of the weekly discussions he had at the University of Iowa, when the department was still, in his words, "minute, and highly involuted" (possibly four people; there were only three when Feigl left): "Ideas of amazing diversity were defended and attacked," Sellars said, "with passion and intensity. It was not easy to find common ground, yet 'for the sake of discussion' [his words] we stretched our imaginations."[27] People who are willing to stretch their imaginations this way are good philosophical companions; and analytic philosophy got started here by people of this kind. I was fortunate to have been their student.

NOTES

1. This was read at a Philosophy Alumni Colloquium at the University of Minnesota on May 1, 1998.

2. Paul Meehl, Regents Professor of Psychology at the University of Minnesota, Twin Cities, and a close friend of Feigl, claimed to be a student in a philosophy course Feigl gave here in 1940. Paul Feyerabend, in his biographical sketch of Herbert Feigl, suggested that Feigl arrived in 1941. See Feyerabend (1966), p. 7.

3. His first missionary endeavor took place in Germany, he told Paul Feyerabend, at the Bauhaus in Dessau. Otto Neurath, who thought that the philosophy of the Vienna Circle and the radicalism of the Bauhaus had much to offer each other, arranged the visit. See ibid, p. 8.

4. He used this epithet in the programmatic article, "Logical Empiricism," which he first published in Runes (1943) and subsequently used the introduction to the influential anthology that he and Sellars issued in 1949. See Feigl and Sellars (1949), pp. 2–28. I have attempted to bring Feigl's brand of empiricism up-to-date in a new book, *An Empiricist Theory of Knowledge*, available on-line at my UMass.edu website and at www.hist-analytic.org. A paperback version can be obtained at a modest price from Amazon.com.

5. The University of Massachusetts Amherst.

6. Dinkytown was and is a small commercial center near the Minneapolis campus of the university.

7. I learned of these discussions from Mary J. Shaw, one of my undergraduate teachers who taught here when Castell did.

8. See Carnap (1956), pp. 38–76. When David Kaplan found a counterexample to the liberal criterion of factual meaning that Carnap developed in this article, Carnap gave up on the project of finding such a criterion, and no one, to my knowledge, attempted to formulate another one. See Kaplan (1971).

9. See Maxwell (1962b).

10. See Feigl (1950).

11. See C. G. Hempel (1950) and Feigl (1950b).

12. It was customary at the Center for staff and visitors to prepare short papers ("memos") that the Center secretary would type up and distribute in mimeographed form to potential participants at Center discussions. These memos provided a useful record of Center discussions and also contributed to keeping the discussion focused on specific issues. The memos often formed the basis for the papers later published in *Minnesota Studies in the Philosophy of Science*.

13. Maxwell (1962a).

14. Aune (1967).

15. His magnum opus was, of course, Feigl (1958).

16. See Feigl (1950c).

17. I objected to Feigl's view in substantially this way in Aune (1966).

18. Feigl (1958), p. 416.

19. Ibid., p. 46.

20. Lycan (1996).

21. The paper was published in 1953. See Sellars (1953).

22. Fodor acknowledges in Fodor (1983), p. 325 (fn 14) that Sellars was there first. He adds that "Sellars's work [in this area] seems remarkably prescient in the light of (what I take to be) the methodological presuppositions of contemporary cognitive psychology." Considering that one member of the "Minneapolis Pentagon" with whom Sellars discussed philosophical subjects during his earlier years at Minnesota (namely, Paul Meehl) was a distinguished psychologist, one should not find this fact surprising.

23. Sellars (1975), p. 289.

24. It was published by Appleton-Century-Crofts in 1953.

25. Ibid., p. 290.

26. Feyerabend (1966), p. 7.

27. Sellars (1975), p. 291.

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Appendix B

Sellars' letter to me of January 3, 1958.

Sellars letter was written by hand, but I give it in printed form because his handwriting may be difficult for many people to read.

January 13, 1958

Dear Bruce,

I was very pleased to get your letter and to learn how things are going with you and your wife at UCLA. How nice it must be—to put first things first—to philosophize in sight of "the Palos Verdes hills extending right out into the ocean…pastel houses and brilliant gardens…the hard, bright sun light through a misty atmosphere…." You can imagine how things are in Minneapolis and even the suburbs.

I had no idea that UCLA was such a thriving center for mathematical logic. I am quite sure that you are wise to grasp the nettle and get the feel of it so thoroughly that you will never be troubled by the anxieties which ignorance of the subject has generated in so many philosophers. On the other hand, of course, it isn't philosophy—and doesn't pretend to be.

I am interested in what you say about Wisdom. I have always thought that he was very good—often brilliant—but I have been a bit disappointed in his published work of recent years (--though I have heard that new things are on the way).

Eleven teaching assistants! Plus three research fellows! We shall simply have to get to work here for more money or die. We will probably have one or two places open for next year and die a thousand deaths in choosing between the candidates. How sorry I was to see you and Keith Lehrer go! The Wittgenstein seminar was one of the high points of my teaching career; I enjoyed every moment of it. I hope you have sweated your way though the later sections of the *Investigations*; perhaps even started on the *Mathematics* sequel.

I agree with your estimate of both Mates and Cavell. I shall look forward to seeing their papers in print.

By the way, I met Lehrer at the Boston Meetings. He seems to be enjoying himself at Brown. The meetings went well and I had a good hammer and tongs battle with Albritton at the Symposium. He certainly didn't convince me that I was wrong. But I can't argue that here.

My warmest best wishes to you both.

Wilfrid Sellars

Some comments. In regard to the Palos Verdes hills, I was then living in Redondo Beach, which in those days was essentially a fishing and retirement village lacking the tall condos that abound there today. The house, the first floor of which I was renting that year, was s just a block from the coast highway that went past a single row of private houses fronting the beach below. From my kitchen window I could see the ocean with, on clear days, Catalina Island straight ahead and the Palos Verdes hills extending out to my left. At that time the Palos Verdes hills were just beginning to be built up. The scene was extravagantly beautiful then. There was no smog at all. The town library of Redondo Beach was a lovely little building adjoining the beach. It was there that I first saw John Passmore's history, *A Hundred Years of Philosophy*. It captured my attention at once, and I started reading it right away.

Sellars' second paragraph shows something about his attitude to formal logic, which he learned from C. H. Langford at Michigan. Langford was the joint author (with C.I. Lewis) of *Symbolic Logic, one* of the first major texts on alethic modal logic. Sellars' third paragraph shows his generally respectful attitude to other philosophers, on which I comment in the text. His fourth paragraph contrasts the number of assistantships available at UMN and UCLA at the time I was there. I comment on this contrast in the text. My reference to Cavell and Mates was to their well-known symposium on ordinary language and philosophy. Mates' paper was "On the Verification of Statement about Ordinary Language," and Cavell's was "Must We Mean What We Say?" I had attended their symposium that year at the APA meetings at Stanford.