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## EPISODES FROM A LIFE IN PHILOSOPHY: A MEMOIR Part One

When I began this memoir, I intended to describe some of the teachers I had and some of the philosophers I knew in the early years of my academic career. I formed this intention chiefly because I was instructed by an unusually large group of philosophers, many of whom were sufficiently notable to deserve mention in a respectable history of twentieth century philosophy, and because I met or got to know, in the early years of my academic career, many philosophers who were leading figures in their day. But as my writing progressed, I strayed from this intention, gradually introducing material about myself and my experiences in philosophy. This development, when I became fully conscious of it, made me uncomfortable. Having grown up in Minnesota in the kind of mainly Scandinavian-American community Garrison Keillor has made familiar,<sup>1</sup> I am temperamentally reluctant to write about myself: “Who does he think he is?” I can almost hear a Minnesotan say. Yet I have had some experiences that may interest others because of the circumstances that produced them—for instance, I was “present at the creation” of two well-known departments of philosophy—and other experiences provide the occasion for reflections that have some philosophical content. I have therefore repressed my Minnesota modesty and freely introduced remarks about myself. Readers who disapprove can always skip these remarks or simply stop reading. Unlike the bore at the dinner table, I do not have a captive audience.

To begin at the beginning, then, as Dickens said at the outset of *David Copperfield*, I was introduced to philosophy at the Minneapolis campus of the University of Minnesota where I was privileged to spend my late teens and early twenties pursuing B.A., M.A., and Ph.D. degrees. Although I had no idea of the reputation that the U of M philosophy department possessed when I entered the university (it was then called “the U of M,” or “the U” for short), it was my good fortune to have gone there. Because I had been something of a celebrity in high school athletics<sup>2</sup> and was known to be a superior student, representatives of some Ivy League schools had urged me to apply for scholarships with them, but for a number of reasons I decided to remain in Minneapolis, my hometown, and accept a small scholarship from the U of M. Like other big ten schools, the U of M had claims to being a great university, but owing mainly to the presence of Herbert Feigl, Wilfrid Sellars, and the others who helped found the journal *Philosophical Studies*, the philosophy department was particularly well known. Herbert

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<sup>1</sup> My surname “Aune” is in fact Norwegian. Norwegians pronounce it as “Ow-neh.” My paternal grandmother is responsible for the pronunciation our family uses now; she thought “Au-nee” sounded more American than the Norwegian pronunciation.

<sup>2</sup> In high school I was a state champion in swimming in both my junior and senior years, and I regularly set records in both the breaststroke (the original butterfly) and the individual medley. In both years I was among the country’s top four “prep school” swimmers in the breaststroke. I won a letter in swimming in my first year at the university (I was number one in the butterfly), but when I became a serious student of philosophy at the university, I dropped out of swimming and put competitive athletics firmly behind me.

Feigl was the principal figure.<sup>3</sup> Originally a member of the famed Vienna Circle and, as he put it, the “first missionary” for logical positivism, he came to the United States as a refugee from Nazi aggression. After a visiting appointment at Harvard, he took a position at the University of Iowa and then moved on to Minnesota in 1940. By the time I attended the U of M he had created the Minnesota Center for Philosophy of Science, the first such center to exist in the United States. In addition to having department members either belonging to or associating with Feigl’s Center, the philosophy department was itself a center of sorts for a style of philosophy its practitioners called “philosophical analysis.” Feigl and Sellars published a well-known and highly influential collection of documents under the title *Readings in Philosophical Analysis*; John Hospers, a younger colleague, produced a very successful textbook, *Introduction to Philosophical Analysis*; and Wilfrid Sellars gave a yearly seminar called “Seminar in Philosophical Analysis.” Not every member of the department actually subscribed to this style of philosophy, but those who did were sufficiently well known to give the department its distinctive reputation.

When I first came to the university I was interested in philosophy, which I had begun to read about while still in high school, but I had no intention of making a profession of teaching it. Instead, I thought of becoming a medical doctor or a lawyer. As time went in, though, my philosophical and literary interests began to assert themselves, and I eventually turned away from what I initially thought of as a more practical course of study. As a student in the college of Science, Literature, and the Arts, I was required to have both a major and a minor field of concentration, so I chose philosophy as my “major” and English literature as my “minor.” I also devoted much of my time to the Humanities program, a sequence of courses concerned with the “great books” of Western civilization. I continued with the same major and minor as a graduate student. Having spent a year away from Minnesota doing graduate work at UCLA, I had to devote much of an academic year to preparing for the preliminary exams that the English department required of Ph.D. candidates with a minor in English. I prepared for exams in 19<sup>th</sup> century literature, poetry, and literary criticism. I never regretted having to fulfill this “minor” requirement.

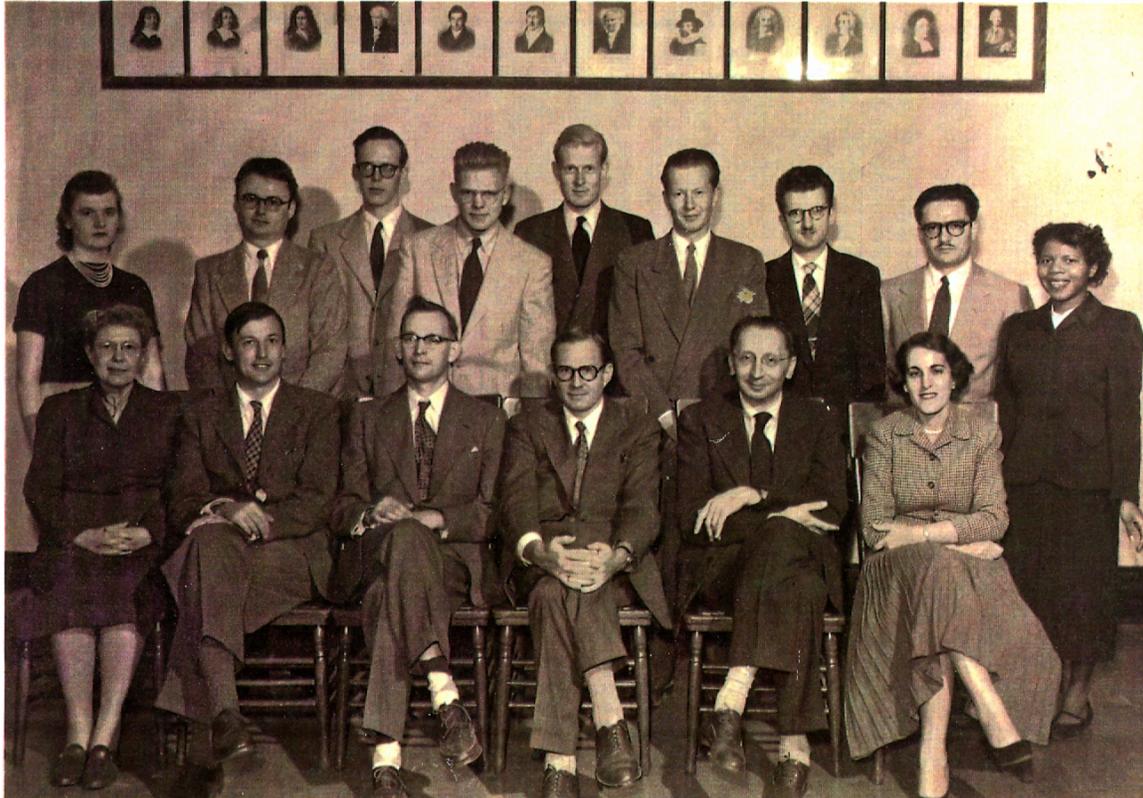
I can no longer remember how undergraduate philosophy majors were expected to choose their courses in philosophy, but knowing little about the perceived strength of the philosophy department, I concentrated on the history of the subject. My favorite teacher was Mary Shaw, a skeptically minded specialist on Hume (she published a book on him under her married name of Mary Kuipers)<sup>4</sup> who taught a history of philosophy sequence that met five days a week for a full academic year. Miss Shaw, as we called her then (she had been divorced at some point and the title “Doctor” was not customarily used for ordinary professors at Minnesota), was not a philosopher with an important reputation—in fact, owing to her slender publication record, she was never promoted to full professor (something that would be unheard of today)—but in memory she remains one of my most formative mentors and a striking example of how important to a

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<sup>3</sup> I discuss Feigl’s role in the origin of analytic philosophy at the University of Minnesota in an essay that I reproduce in Appendix A of this memoir.

<sup>4</sup> The book is *Studies in the Eighteenth Century Background of Hume’s Empiricism* (Minneapolis: University Of Minnesota Press, 1930). Reissued by Garland Publishing, 1983.

university some of its less distinguished members often are. (Mary Shaw was actually a favorite teacher of a great many of the undergraduates I knew at Minnesota.) Her history of philosophy sequence, covering the major philosophers of the ancient, medieval, and modern periods, provided the basic framework for my subsequent study of the history of philosophy. In later courses I simply studied these philosophers in greater detail. When I was her student (this was from 1952-57), Mary Shaw was a tough-minded woman in her sixties who differed greatly from any woman her age I had ever met. Wearing her gray hair short and in curls and dressed in a skirt, jacket, and stout sensible shoes, she first reminded me, a naïve young man who had no experience of female professors, as a



**University of Minnesota Department of Philosophy, ca. 1955**

Back row: unknown, Juarez Paz, Robert Binkley, Peter Warburton, Michael Scriven, Francis Raab, Burnam Terrrell, Hector-Neri Castañeda, unknown. Front row: Mary Shaw, Alan Donagan, John Hospers, Wilfrid Sellars, Herbert Feigl, May Brodbeck.

strangely dressed grandmother, but her piercing dark eyes, her irreverent attitude toward conventional verities, and her learned diction put her in a class all by herself. She was, by today's standards, remarkably formal with her students, addressing them as Mister X or Miss Y (she was the first person ever to call me Mister Aune), and also quite strict, once simply walking out of the class when, finding most of us unable to answer her questions on the day's lecture topic, she concluded we had not done our assigned reading. But she was really a kindly, supportive person, seriously interested in the teaching she was doing.

In her history of philosophy sequence she was mainly a lecturer, because those classes usually contained two hundred or more students, but even then she constantly asked questions and expected thoughtful responses. In advanced courses with as few as ten to fifteen students she proceeded almost entirely in a Socratic fashion by asking questions about the assigned text and then discussing the answers her students dared to give. The answers she wanted should ideally spring from close reading of the text, something we could accomplish, she insisted, only by creating an outline of the author's argument. "If you don't construct such an outline," she said, "you can't possibly understand what a philosopher is trying to say—and this, she added with a wry smile, is true even for me." In response to her urging, I developed the habit of outlining the important philosophy I read, and I held to it throughout my career. Mary Shaw didn't have a doctrinaire approach to philosophy; she didn't subscribe to some distinctive philosophical method, traditional or *avant garde*. An admirer of the French and Scottish Enlightenment, she simply discussed philosophical issues carefully and critically, and her teaching was always focused on the text at hand. Her method of teaching was in fact generally similar to the methods employed by the better teachers in the English department. The New Critics dominated that department, and they were officially dedicated to the close reading of literary texts.

As an undergraduate I took more philosophy courses from Mary Shaw than from any other teacher, and in my early years as a graduate student I became her assistant in the history of philosophy sequence, grading the mid-quarter and final exams she gave each student. (The academic year at the university was divided into three "quarters" rather than two semesters.) In the year before she retired, when Alan Donagan had become a member of the department, Mary joined Alan and several of us graduate students in drinking beer and eating hamburgers at a nearby pub (we called it a bar) on Friday evenings. We usually drove a car to get there, and one Friday in late spring my friend Bill Capitan gave us a ride in his Chevy convertible. The top was down and Mary and I sat with him in the front seat, she sitting in the middle with her gray curls streaming in the breeze and my arm around the top of her seat. As we passed a group of male undergraduates, one of them looked our way, grinned, and gave us the hand sign of success or "AOK!"—the one making a circle with thumb and middle finger. She was not exactly our "date" for the evening, but we were all enjoying ourselves nevertheless.

Although I am getting ahead of my story in saying this, I might add that Mary and I remained good friends until she died. After she retired from teaching at the University of Minnesota, she went to live in New York City, where, at Columbia, she had taken her PhD, but her vision became impaired several years later and she moved to an Iowa nursing home to be near a younger sister. Because her vision was diminished then, I bought her a subscription to the large-type edition of the New York Times. When she wrote to thank me for the subscription, she was very upset, saying that she had just been asked by the nursing home directors to leave the facility and find another place to live. She said she had no other place to go to. I am not sure what the problem was; she did not want to discuss it. Mary was too civilized to be a difficult person, but she was very secular—in fact, very suspicious of pious people. When I was still her student she told me with indignation that a religious nurse had been withholding

the morphine a doctor had prescribed for an older sister who was dying of cancer. The nurse wanted the sister to remain conscious so that she could settle accounts with her Maker before she died. It was hard for me not to suppose that the pious ladies of that rural Iowa town had been affronted by Mary's lack of religious reverence. I am not sure how long she stayed in that home; when I wrote her again, I was told that she was no longer there. Some time later (I can no longer remember how much later) I got word that she had died. More than twenty years after this I dedicated my book on metaphysics to her and Grover Maxwell. The publisher of the book was the U of M Press, and it seemed fitting to dedicate it to the memory of these two wonderful U of M people.

I am not used to writing about myself, at least in a personal way. But writing this document brings to mind facts about myself that I had never confronted before. One is the long-term effect of Mary's teaching and example on my subsequent life. Having grown up in a blue-collar family with, owing to my athletic success, only a slightly wider range of experience than that background would suggest, I had never been acquainted with a thoroughly secular intellectual and had only the foggiest idea of what the life of a professor was like. She was a living example of both, and my association with her changed me in a way that proved to be permanent. I didn't consider this before, because I more or less put the history of philosophy behind me as I developed into a self-conscious analytic philosopher and found new role models. But as I look backward now, I can see that no one else actually had a greater influence on my present life than she did: her example was fundamental. And she introduced me to two of the philosophical writers I admire above most others, Voltaire and Hume. When I read them, I always think of her.

Although most of my undergraduate curriculum in philosophy was devoted to the history of the subject and therefore was somewhat old-fashioned, I took introductory logic from Michael Scriven who, as a recent graduate of Oxford University where "ordinary language philosophy" was in vogue, approached the subject in a very up-to-date way. He concentrated on arguments taken from newspapers and magazines that, instead of being clearly deductive and having a structure that could readily be handled by the formalism of modern deductive logic, had the messy character of vernacular disputes, where interrogatives occur alongside declaratives, premises are often hard to distinguish from conclusions, and many transitions are tacit or explicit inferences to the best explanation. I have always considered Scriven as one of the brightest teachers I ever had, but his approach to elementary logic left me innocent of mathematical logic until I was a graduate student. All things considered, I am not sure whether that was a good or bad thing. I know Scriven made me more sensitive to the peculiarities of ordinary argumentative prose.

About halfway through my senior year I discovered that I had enough credits to graduate, so I became a graduate student in the spring of my fourth year. In those days at Minnesota the M.A. degree was taken very seriously in the philosophy department; it was regarded as a "trial run" for the Ph.D. Because I had studied painting and had close friends seriously involved in music and the design of furniture and textiles,<sup>5</sup> I was almost as interested in these other arts as I

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<sup>5</sup>One friend, Lothar Klein, who was like a brother to me until his death in 2004, was a composer who eventually became Professor of Composition at the University of Toronto and produced a very large number of

was in philosophy and literature. I therefore found the study of esthetics especially interesting. As an undergraduate I had taken a course in the subject from John Hospers, and his teaching made me think of writing my master's thesis on an issue in esthetics.

Still active in his 90's (he was born in 1918), Hospers was the first writer of books and learned papers I had come to know personally. Some of my friends had him as a teacher before I did, and I had seen his book, *An Introduction to Philosophical Analysis*, before I ever heard him speak. He was still (by my present standards) a young associate professor, and he was extremely popular with students. In most analytically-oriented philosophy departments esthetics is regarded as a subject of marginal importance, but at Minnesota, whose department was, as I said, officially devoted to philosophical analysis, Hospers' esthetics course required a large lecture hall: the one I took met in the Psychology building, which was not only suited to large classes but was reached by a path running, to our delight, through a clearly-labeled "poison plant" garden. Hospers was a witty, animated lecturer with (for us) an astonishing familiarity with the classics in music, painting, and European literature. His principal text for the course, his own *Meaning and Truth in the Arts*, led me to the topic of my master's thesis, "The Cognitive Content of Literary Art." This title sounds pretentious to me now, but it effectively recalls a preoccupation of logical positivists, whose activities still produced echoes in the philosophy building.

The preoccupation concerned the kind of meaning characteristically possessed by an unverifiable statement about a "world" that is avowedly fictitious. The logical empiricists at Minnesota no longer accepted a verification theory of cognitive meaning, but they were convinced that cognitively meaningful claims should be the sort of thing for which evidence is at least possible, and fictional statements seem to belong to a very different class. My own preoccupation was a bit different from the logical empiricists, however. John Wisdom, an English philosopher I had begun to read, said that the writers (of fiction) that mean most to us do not merely tell us of fairylands, and this remark was at the forefront of my thinking when I undertook my thesis. I wanted to pin down the distinctive kind of meaning (I called it the "sense") possessed by sentences, paragraphs, and texts of imaginative literature (particularly modernist literature) that is often difficult to grasp although its ingredient words and basic syntax is easy to understand." An example of the sort of issue I am attempting to describe is posed by the first line of T.S. Eliot's poem, "Morning at the Window": the words are simple, the basic syntax is prim and proper, but the sense of the line is decidedly obscure: "I am aware of the damp souls of housemaids sprouting despondently at area gates."<sup>6</sup> In opposition to the current dogma set forth in Wimsatt and Beardsley's "Intentional Fallacy," I took the line, later made respectable by Paul Grice, that what needs to be understood is the author's intention in producing the relevant words, what he or she was trying to accomplish by means of them.

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substantial orchestral pieces that were performed by major orchestras in the United States, Canada, and Europe. We talked about music off and on for more than fifty years, and we paid absolutely no attention to such things as jazz or rock and roll. For us, pop music was part of pop culture, something we viewed with indifference or contempt.

<sup>6</sup> The basic issue (or cluster of issues) to which I allude here is discussed at length by Arne Chen in *On What We Mean* (Lanham, MD: University Press of America, 2002).

Hospers left the university before I finished my master's thesis, but in retrospect I can see that he greatly influenced my mature manner of writing philosophy: I even wrote advanced semi-textbooks on philosophical subjects that bear a significant similarity to his semi-textbook, *Human Conduct*, which I used in classes at UMass in the 70's. One feature of Hospers' exposition, whether in lectures or in writing, is his careful, dispassionate consideration of alternative views. In this respect his approach to philosophical topics is akin to that of John Stuart Mill, a philosopher Hospers admired then and spoke of devoting a course to. I myself became a serious admirer of Mill only in the 80's, when I began to use his *On Liberty* as one of the set texts in my annual *Introduction to Philosophy* course. My interest in this work led me to explore Mill's life and his other philosophical writing. I now regard him as one of the three most important philosophers England has produced. The other two are Thomas Hobbes and Bertrand Russell. (I think of Hume as a Scot rather than an Englishman.)

Hospers did not merely introduce me and other students to philosophers and philosophical topics; he also introduced us to artists and works of art. One of his favorite composers at that time was Hector Berlioz. I can still remember him playing a recording of the *Symphonie Fantastique* at a small gathering of graduate students at his Minneapolis apartment. I don't think I had ever heard Berlioz's music before that evening, though I had no doubt heard his name from my composer friend, Lothar. I greatly enjoyed Berlioz's music on first hearing, and I still enjoy hearing it after all these years. I do own, but I have never finished reading, Jacques Barzun's wonderful two-volume work, *Berlioz and the Romantic Generation*, but the reading I did led me to Barzun's other books, and he has long been my favorite intellectual historian and one of my favorite writers of English prose. I would not be surprised if it was Hospers who first directed my attention to Barzun.

In addition to inspiring us by his erudition in philosophy and the arts, Hospers provoked us by some of the unusual topics he sometimes discussed. One that I still recall was his resentment at having to spend so much of his life sleeping. He may have had this resentment for only a short time, but he once expressed it when he was getting ready to swim at a pool where I happened to be working as a lifeguard. I remember him complaining that he could accomplish nearly twice as much in life if he didn't have to sleep, and he indicated that he was (at least then) trying to reduce his sleeping time by a factor of two: instead of sleeping eight hours, he was trying to make do with four. Even less might be possible, he said. I have no idea how long he pursued this goal and how successful he was in achieving it, but I had never met anyone who expressed a similar purpose. I thought it was a perfectly reasonable purpose to have. I myself had no interest in such a thing, because at the time I was getting less sleep than I would have liked to have. The idea of having such a purpose was nevertheless something I spent some time thinking about.

Although I had many discussions and even a short correspondence with Hospers, we never did discuss politics. Other students had intimated that he had conservative political ideas, but I was nevertheless quite surprised later on when I heard that he was a supporter of Ayn Rand's political philosophy (I had read her novels) and even more surprised when he became the first Presidential candidate of the Libertarian Party, the first substantial expositor of Libertarianism,

and the recipient of one electoral vote in the 1972 presidential election. I did read much of his book on Libertarianism,<sup>7</sup> and although my own political views were then (and are now) much more to the left than his, I thought his exposition admirably lucid and his reasoning carefully considered. I would not have expected anything else.

I was still mainly interested in history and esthetics in my second year of graduate study, but I nevertheless bit the bullet and signed up for Wilfrid Sellars' Analysis Seminar, which had the reputation of being very difficult and even terrifying for younger graduate students.<sup>8</sup> The seminar's reputation owed more to Sellars' manner of teaching than to the subjects he taught. This year the subject was Wittgenstein, the first quarter on the *Tractatus* and the second on the *Investigations*. Sellars' procedure was to have his students prepare reports on assigned topics and then cross-examine them on the material as if he were a prosecutor and they were reluctant witnesses--or, as we thought then, as if he were Socrates and we were dull enthusiasts like Euthyphro. He allowed us to start our reports--we had to have them prepared--but he would soon say "Would you unpack that?" and our responses would give rise to further questions. We were on the "hot seat," a chair with a special upholstered seat in, I seem to recall but I may be merely imagining this, faded red wool. The chair was right next to Sellars' chair, which was at the end of the long seminar table.

Often in responding to Sellars' queries we would be asked to "charge again" if our first charge deserved to be improved upon; and sometimes we would have to "beat the neighboring bushes" with his help, and consider matters related to the subjects we were officially discussing. We came to relish Sellars' pet sayings, and we often directed them to each other when he was not around, squinting our eyes and frowning in a way that mimicked what we took to be his characteristic expressions when he cross-examined us. Although we all greatly admired him, we referred to him among our selves as "Wilf," an irreverent nickname first used by my friend Bill Capitan. As if in punishment for this irreverence, Bill suffered in a later Sellars seminar on topics in the history of philosophy. Instead of taking this seminar for credit, Bill was merely "sitting in" as a visitor, but he had to give a report, anyway ("Everyone who attends gives a report," Sellars said). When the time came for his report, he did not prepare as carefully as he should have done, and in consequence of this he stumbled badly when he attempted to explain what Abélard, the subject of his report, was saying in a certain paragraph. Sellars demanded that he reread the paragraph and explain it again. After rereading the paragraph while Sellars and the rest of us patiently waited, Bill looked down at the paragraph as he attempted to explain it. Sellars promptly put his hand over the page. When Bill faltered, Sellars asked him to read the paragraph again. Again he stumbled; again the hand went over the page. Again he was asked to read it. This seemed to go on for a very long time. At the end of the day Sellars gave a qualified apology. "I was sorry to have to do that, Mr. Capitan, but some of you students were not preparing your reports carefully enough, and I wanted

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<sup>7</sup> John Hospers, *Libertarianism: A Political Philosophy Whose Times Has Come* (Santa Barbara, CA: Reason Press, 1971).

<sup>8</sup> I had taken Sellars's course on Kant as an undergraduate, but I found him and Kant difficult to follow. My response was probably typical of many students who attended the course. Although the class contained a roomful of students, I learned later that only three or four of them were taking the class for credit.

to establish a precedent. In this he succeeded. We were all terrified at the thought of Bill's ordeal.

When I studied with him, Sellars did not discuss his own philosophical writing in class, nor did he ask us to report on it. He sometimes discussed it outside of class with a select group of students, but for one reason or another I never attended these sessions. His practice was to work very carefully through some selected text by means of reports and cross-examination, and to put on library-reserve a lengthy list of pertinent books and articles. He expected us to "familiarize ourselves" with the items on the list, and some of us were very conscientious about doing so. He assigned no term papers in his courses, and he appeared unconcerned about completing the material (finishing the text) that we were discussing in class. At the end of a seminar he would often say, "I think we have gone through enough of this so that you can complete the rest on your own."<sup>9</sup> He may have been excessively optimistic about this when the text was Wittgenstein's *Tractatus* or *Philosophical Investigations*, but his courses helped us to "work through" (as he put it) difficult papers and books on our own. This is something I have always been able to do; I have never found it important to belong to a discussion group on a philosophical topic.

Although Sellars had the reputation, when I first took his Wittgenstein seminar, of being stern and demanding, he proved to be kind and supportive of his students, particularly his graduate students. In spite of his kindly attitude, he seemed formal and aloof to most of us; it occurred to me later that I had never seen him without a necktie or with his suit coat unbuttoned. He was basically a shy man who was generally reserved even with intimates. I became fairly close to him after I became a colleague of his, but he remained noticeably reserved with me too. In later years he told me that the Wittgenstein seminar was one of the best he ever taught—the best when he taught it, and the best for a long time afterward. Possibly the contrast with earlier seminars impressed him the most. The seminar I attended was also attended by Keith Lehrer, Herbert Heidelberger, Murray Kiteley, Daniel Merrill, William Capitan, Walter Anderson, Pat Crawford, Ved Sharma, and several other people who went on to be professors and achieved some distinction in their careers. Fred Dretske was also a graduate student at Minnesota then, but I do not recall him ever being in a Sellars seminar. Fred was a student of May Brodbeck, and I doubt if she would have recommend Sellars' courses to her students. Her intense partisanship in philosophy was my first experience of that phenomenon.

I can't resist saying something about some of the students in Sellars' seminar on Wittgenstein. In view of remarks Sellars made to me over the years, I think he was particularly pleased with the performance of Keith Lehrer and (I am glad to say) myself. Keith was only an undergraduate when he took the seminar, but he was probably the most voluble member of the class and seemed undaunted by Sellars' attempt at cross-examination. I can still remember one meeting in which Lehrer, to the astonishment of the rest of us, made it very difficult for Sellars to get a word in edgewise, as we used to say. Sellars was not really annoyed by this;

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<sup>9</sup> In a letter he wrote to me when I was away at UCLA, he asked me if I had followed up his seminar on Wittgenstein by "sweating my way through the later sections of the *Investigations* and perhaps even started on the *Mathematics* sequel." In this letter he spoke of the Wittgenstein seminar as "one of the high points of [his] teaching career." I include a copy of this letter below in Appendix B.

in spite of himself, he seemed delighted by Keith's fearless intelligence. I was not as voluble or as verbally articulate as Keith, but I had the virtue of being always very well prepared. I was in fact the first student to volunteer to give a report in this seminar. When Sellars asked for volunteers, the others were very slow to respond, and to avoid awkwardness I simply put my hand up and agreed to go first. Knowing Sellars' reputation as a relentless cross-examiner, I soon began to wonder what I had got myself into and, in self-defense, I proceeded to work especially hard on my report, which concerned two chapters of G.E. Moore's *Some Main Problems of Philosophy*. (Sellars assigned these chapters as a background for things Wittgenstein would say in the *Tractatus*.) I am not sure how many times I went over those chapters in preparation for my report, but I was determined to be so well prepared that I could handle any question on the chapters Sellars might ask me. As it happens, my preparation succeeded: I came through my report unscathed. I think this pleased Sellars as much as it pleased me.

Perhaps the most colorful student in the seminar was Ved Sharma, who went on to teach philosophy at San Jose State University in California. Ved was an immigrant from India and, to us, a very amusing and exotic person. Of course, we gave him the nickname, "Swami." He had grown up speaking English, so he spoke it very well. But, to our amusement, he had formed the habit of over-using several of Sellars' verbal mannerisms. One was the expression "you see," which Sellars almost always used when he explained something to us. Ved used it all the time, however. Once, on an evening at my apartment when several friends were drinking beer and reading some of T.S. Eliot's poetry, Ved began to recite Eliot's "*La Figlia che Piange*." In a strong sonorous voice, he proceeded as follows:

Stand on the highest pavement of the stair, you see—  
Lean on a garden urn, you see—  
Weave, weave the sunlight in your hair, you see—  
Clasp your flowers to you with a pained surprise, you see...

And so on. His performance was terribly amusing—not so much because he introduced the extra words, but because he was unwittingly giving us a parody of Sellars reciting a poem.

My real purpose in speaking of Ved Sharma was to include another anecdote that has remained alive in my memory and still influences my thinking. In about 1960 the annual meetings of the Western Division of the American Philosophical Association took place at the University of Wisconsin, Madison. Bill Capitan, Ved, and I attended the meetings and discovered the presence there of the Maharaja of Mysore, an amateur philosopher being led around in an unctuous way by the well-known editor, Paul Arthur Schilpp. The Maharaja was enormous: he must have weighed at least 350 pounds. As irreverent young men, we found him a very amusing figure. We laughed about him among ourselves, and Bill imagined him calling out to his wife, the "Maharina," at the end of the day, "I am hungry; bring me another bale of hay." Ved thought this was going too far and quickly objected, "You must not speak of the Maharaja that way. He is a *very* important man—why, he must have a hundred elephants." Ever since, Bill and I have found the very mention of "an important man" amusing, and on the rare occasions when we now see one another (we live in different parts of the country

now), we jokingly refer to one another as important men. To both of us, I think, and certainly to me, people who feel important or act as if they are important always seem a little ridiculous. Two family friends as well as my youngest daughter, because they remember the anecdote, occasionally add to my growing collection of miniature wooden, glass, and ceramic elephants: some day they hope I will have a hundred of them—so that I can justifiably claim to be “an important man.” I have only eight of them now; clearly, I have along way to go.

Another fellow student who requires a word is Misha Penn. He may not have attended Sellars’ Wittgenstein seminar (I have no actual memory of his being there), but I want to mention him for a reason that I think is important. It concerns religion. I am far from religious myself, but thanks to Misha I am now sensitive to one thing that many religious persons seem oblivious to, the unwanted effects of religious acts and symbols on people who do not share their religion. Although Misha was enrolled in a generally secular philosophy program, he had attended rabbinical school and was a particularly devout orthodox Jew. I was unaware of Misha’s religious sensitivity, but one of the other graduate students noticed that Misha had an aversion to viewing Christian symbols. When the rest of us learned this, we began to make the sign of the cross in his presence. Sometimes we would do this ostentatiously; sometimes we would be more guarded, drawing it on a sheet of paper, for instance, and gradually exposing it as we talked to him. We were not trying to be cruel, though we knew he did not enjoy the experience we were creating for him. He would say, “Come on, you guys, cut it out.” We would laugh, because we thought it absurd for a budding philosopher to be bothered by a mere symbol. I would not tease him this way now. I know that religious symbols really matter to people, those who are positively moved by perceiving them, and those who are negatively moved. And many people are negatively moved. What one person views with reverence, another may view with revulsion, even fear. The last time I saw Misha, which was just a year or two ago, he told me that seeing Christian symbols no longer bothers him. I am not sure that I believed him when he said this, but I know that his old attitude is much more common than people commonly suppose. For this reason, it is a poor idea to inflict your religion on another person. Including others in your family prayers without asking their permission, or declaiming prayers in the midst of secular activities, is just as objectionable as our mistreatment of Misha all those years ago. You shouldn’t do it, however pious you may be.

The aggressiveness of some of my fellow students of that time resulted in an incident that Sellars’ older students laugh about even today. In the seminar in which Sellars gave the special lesson to my friend Bill, the student who did not prepare carefully enough, I later gave a report on William of Occam’s views on thought and intentionality. At that time I was new to the philosophy of mind and unaware of the contemporary interest of Occam’s views on thinking. The same was true, of course, of the other students; Occam was not someone they admired. When I prepared my report, I became very interested in Occam’s views, and my interest became evident when, sitting on the hot seat, I gave my report. Some of the other students began to voice their criticisms in an energetic way, at which time Sellars surprised everyone by defending me. I can no longer remember his initial remarks, but he ended by saying, “This a very profound view—in many ways it is similar to mine.” No one was amused when he said this, but later on

we could quickly produce laughter among ourselves simply by saying "That is a very profound view." Everyone knew what conclusion the speaker expected the others to draw.

When I was an undergraduate or first-year graduate student, I heard Sellars read the lectures that went into his famous "Empiricism and the Philosophy of Mind." I enjoyed the experience but the subject matter was well beyond my competence. (In retrospect I know what he meant when, referring to the blue notebook in which he had written his lecture with a ballpoint Scripto pen, he spoke of reading from his own "blue book.") After taking his two-semester Seminar in Philosophical Analysis, I became very interested in his work and started reading his "EPM," which had just appeared in the first volume of Feigl's *Minnesota Studies in the Philosophy of Science*.<sup>10</sup> I was very excited by the essay, because it seemed to solve key problems that were raised in my mind by Wittgenstein's *Philosophical Investigations*, which I greatly admired. In fact, I had become more Wittgensteinian at that point than Sellars approved of. I know this because, as a result of Hospers' teaching, I had been reading John Wisdom's papers on other minds, which were strongly influenced by Wittgenstein's ideas, and when I spoke approvingly of Wisdom to Sellars, I remember him saying that reading Wisdom could be expected to "unhinge" a careful reader.<sup>11</sup> However Wittgensteinian I was at that time, I do not recall being greatly puzzled by the ideas in "EPM." I may have absorbed something of Sellars' way of thinking in the three seminars I took with him (one partly on Russell and Moore but mostly on the *Tractatus*, one entirely on the *Philosophical Investigations*, and one on topics in the history of philosophy), but I have never found his work as difficult as others have claimed it to be. Possibly this is because of the training in reading I got from Sellars and Mary Shaw.

In at least the last part of the year when I took Sellars' seminars, I was working on my M.A. thesis in esthetics, and I completed my thesis in time to graduate in the spring. Hospers had left the university at the end of the previous year, so I needed a new director. Officially, my thesis had two co-directors, Mary Shaw and Alan Donagan, but Donagan was hired to replace Hospers, and he did almost all the work. That work was largely minimal, because I did not need much direction: I had a clear problem; I thought I had a good solution; and my work with Hospers had given me a good sense of the relevant literature, which I continued to study as I wrote. Donagan, Shaw, and, I believe, some other examiner read the thesis after Donagan had provisionally gone over it with approval. The thesis passed, and I was ready to begin work for the Ph.D.

I wanted to remain at Minnesota for this remaining work because I had become interested in the philosophy of mind and wanted to pursue the subject with Sellars; but I needed a fellowship or assistantship, and Minnesota didn't

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<sup>10</sup> As the date on the inside cover of my copy of this volume indicates, I bought the volume in March of 1957, and that is when I began to read it. I believe that I discussed it with Sellars that spring, but it may have been at a later date, possibly some time when he was visiting Feigl's Center. I left Minnesota for California in August of that year.

<sup>11</sup> Sellars soon added, "But he has, of course, many good ideas" (or something to this effect). He seems to have made it a practice never to disparage the work of another philosopher. (See his comment on Wisdom in his letter to me at UCLA.) Like Feigl and Carnap, he always treated other philosophers with respect. He was very Kantian in this regard.

have one to give me.<sup>12</sup> Relying on the advice of Sellars and Donagan, I therefore applied to four other graduate schools: Michigan, Brown, Cornell, and UCLA. The first three schools were very strong at that time in the subjects I had been studying with Sellars, but UCLA had the advantage of having an able polymath, Abraham Kaplan, who was knowledgeable in esthetics. As it happened, UCLA was the first to reply with an offer, and, for the embarrassingly bad reason that I had just seen the movie version of *Oklahoma!* and was eager to see the western United States, I accepted their offer before I heard from the other schools. When I did hear from them, I got three further offers, but it was too late to do anything about them--and Sellars was not amused by this fact. It turned out that I made exactly the right choice.

When I arrived at UCLA in the fall of 1957, I discovered that Abraham Kaplan was away in Japan and would be there for the rest of the year. Evidently, he was studying mysticism, a subject in which I have never had the slightest interest. With no permanent faculty member teaching esthetics,<sup>13</sup> I looked about for substitute courses to take, and I hit upon logic. Apart from the elementary or "baby" logic I took at Minnesota with Scriven, I also had a graduate-level course in "symbolic" logic taught by May Brodbeck, a doctrinaire former student of Gustav Bergman, but I had a lot more to learn about the subject. Also, most of the alternative courses were on epistemology or the history of philosophy, which I had studied fairly extensively at Minnesota. Another reason for studying logic was that UCLA was particularly strong in that subject. Rudolf Carnap was still there, and Richard Montague, who had just recently been hired, was in effect advertised as a young genius who was collaborating on a revolutionary logic text with Donald Kalish, a more senior but youthfully-behaving department member. I took courses from all three of these men during the year. In the fall semester I also attended a seminar given by John Wisdom, who to my astonishment was a temporary visitor.

Kalish, who was often called "Don," and Montague, who was always called "Richard," were favorites of the graduate students and spent a lot of time with them. They were doing the work the students found exciting. I was particularly pleased by their way of teaching logic and evaluating their student's progress. At almost every meeting of their class they assigned homework problems, and a student's grade was based mainly upon his or her ability to solve difficult problems. Difficulty counted highly for them; speed at solving problems did not. (As someone educated mainly in literature and the arts, I was not very fast when I started taking their courses.) They also greatly valued argumentative rigor, al-

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<sup>12</sup> At that time the Minnesota philosophy department had just five assistantships for the support of its graduate students. Sellars comments on this unfortunate fact in his letter to me at UCLA. I include the letter in Appendix B.

<sup>13</sup> Kaplan's replacement for the year was William Barrett, the author of *Irrational Man*, a self-styled existentialist with strong interests in contemporary literature and political theory. At that time he was still, I think, on the editorial board of the *Partisan Review*, a role he described in his memoir, *The Truants*. I became his teaching assistant and attended his Introduction to Philosophy, which, like my own course in later years, featured Plato's *Republic*. Although I did not agree with his philosophical ideas, I enjoyed talking to him and hearing his lectures. He criticized my way of conducting the discussion sections for his course--he wanted me to do things "his way, not mine"; he wanted less logic--but I didn't object and we remained on good terms. He was a very interesting man to know and a first-rate writer. He had once been Carnap's student at Chicago where he got his Ph.D. at a very early age. Carnap attended the talk he gave at UCLA that year and became quite upset at what he heard. As I recall, Barrett was arguing that the world, or our experience of it, contains "contradictions." Carnap's movements were noticeably jerky as he raised his objections.

though for advanced students they required it only in connection with "non-trivial" problems. If a conclusion could be derived from certain premises by a series of inferences that a logically mature student could be expected to produce as a matter of course, they soon allowed us to omit the series and attach "by QL" to the conclusion: "by QL" meant "by quantifier logic." We were in real trouble if we used "by QL" when the derivation was not trivial. An incident they used to relate with amusement featured Alonzo Church, the editor, then, of the *Journal of Symbolic Logic*. Church was revered for his unerring logical judgment, but the speed of his inferring was supposed to be surprisingly slow and deliberate. As they described the incident, Church told a class that a particular conclusion was inferable from certain premises. One of the students asked if the inference were trivial. Church thought about it for a moment and then said, "I'll tell you next time." At the beginning of the next class he announced, "Yes, the inference is trivial." He then proceeded with the next topic on his agenda.<sup>14</sup>

Although Kalish favored argumentative rigor as strongly as Montague, Montague seemed to value precision a little more strongly than his colleague did. By "precision" here, I mean the quality of being accurate and exact. Kalish was sometimes willing to speak a bit loosely in giving us an intuitive idea of the strategy involved in a proof, but Montague always wanted to be exact. To this end he would often pause in a lecture and say, "Remark," after which he would write "Remark:" on the blackboard followed by a carefully worded sentence.

In my year at UCLA I took two courses from Montague and two from Kalish. From Montague I took set theory and the Foundations of Probability and Statistics, but the set theory consisted mostly of first-order logic, since Montague wanted to begin with the manuscript of his and Kalish's new logic text. From Kalish I also took two courses, one on the metatheory of first-order logic and one on semantics. All four courses were valuable, although in Montague's probability course I had to follow set theoretical proofs before I learned set theory (I boned upon the notation by studying the remarks Reichenbach said about it in his *Theory of Probability*, which was one of our texts in the course). Kalish's semantics course was particularly valuable for me. We discussed meaning and truth in a way Kalish considered nonformal, but he assigned important papers by Russell, Frege, Carnap, Quine, Goodman, and Tarski, and as homework commonly required us to formulate their central arguments in a more rigorous way. Kalish had a wonderful facility of using simple models to help us understand the basic structure of important theories, such as Tarski's theory (or definition) of truth for formal languages, and I have often tried to follow his example in later years when I taught or wrote about similar material<sup>15</sup>.

Although both Montague and Kalish advocated a formal approach to philosophy, Montague once saying to me, "Bruce, philosophy is just a branch of set theory," I did not climb on their bandwagon. Nevertheless, both permanently influenced my thinking in important ways. I still think of the probability calculus as providing the only inductive logic (or confirmation theory) that we need, and I am convinced that a satisfactory conception of analyticity can be developed only

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<sup>14</sup> In his "Retrospective Epilogue" to his *Studies in the Way of Words*, p. 325, H.P. Grice spoke of a similar incident involving the Cambridge mathematician, G.H. Hardy. According to Grice, it took Hardy a quarter of an hour to decide if the proposition in question were actually obvious.

<sup>15</sup> As I did in my book, *Metaphysics: The Elements* (Minneapolis: University of Minnesota Press, 1985).

in relation to a formal or "regimented" language.<sup>16</sup> But in opposition to them I thought that many of the problems that really interested me (the intentionality of thought, say, or the question whether knowledge needed a foundation of certainty) could be solved, or resolved, only by more informal methods. I wasn't sure what these less formal methods should be, exactly, but I thought Wittgenstein and the later Sellars offered tempting clues.

The seminars I took from Wisdom and Carnap were very different from the courses I took from Montague and Kalish. Neither seminar was very demanding; neither required much work from us. Wisdom's seminar was given in the fall. The subject was officially metaphysics, but Wisdom was not concerned with any particular metaphysical issue or problem: his concern was with ways philosophers are led into metaphysical perplexity and how they can escape from it. He seemed to think an escape is always possible if the right method is used; he called the method he recommended the case-by-case method. I will say something this method shortly, because I think it is often very useful. But I want to begin with some remarks about the man; I have never seen a philosopher like him.

When I think of Wisdom's manner of conducting a seminar meeting or giving a public lecture (I heard him give one such lecture), I am reminded of the words Norman Malcolm used in his description of the lectures Wittgenstein gave when Malcolm was a student at Cambridge:

His lectures were given without preparation and without notes. He told me that he had once tried to lecture from notes but was disgusted with the result; the thoughts that came out were 'stale', or, as he put it to another friend, the words looked like 'corpses' when he began to read them. In the method that he came to use the only preparation for the lecture was to spend a few minutes before the class met, recollecting the course the inquiry had taken in the previous meetings.<sup>17</sup>

I cannot claim to know that Wisdom's lectures were given without preparation, but they certainly appeared that way. It was as if he were trying to think things through in a spontaneous way. On the occasion of the public lecture I attended, I remember him leaning motionless with his forehead against the blackboard, and he remained that way for at least five minutes. Members of the audience had become very uncomfortable before he started talking again. He would often grasp his brow and sometimes he would hold his head with both hands--as if he were struggling to find words for elusive thoughts. He never apologized for, or showed any discomfort at, these moments of silence. The rules of his game seemed to be that stopping and thinking is always all right--far preferable to moving right along saying things that are only approximately right or apt. I got the impression that he would be very suspicious of a glib statement. A slow, stumbling discourse was the mark of serious thought.

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<sup>16</sup> I defend both of these convictions in my recent book, *An Empiricist Theory of Knowledge*, a version of which can be downloaded free at [www.hist-analytic.org](http://www.hist-analytic.org). A later version with an index is available at Amazon.com.

<sup>17</sup> Norman Malcolm, "Biographical Sketch," in *Ludwig Wittgenstein: A Memoir* (London, 1958). p. 24.

Speaking of the students attending Wittgenstein's class at Cambridge, Malcolm said, "Few of us could keep from acquiring imitations of his mannerisms, gestures, intonations, exclamations."<sup>18</sup> Although Wisdom was not a student of Wittgenstein, he taught at Cambridge when Wittgenstein was there, and it is clear that he was affected by Wittgenstein in just the way the students were. He was not just mimicking Wittgenstein in his classes and lectures at UCLA; he was doing his own thing under Wittgenstein's influence. I had no doubt about Wisdom's integrity; I found him admirable as a man and very interesting as a sensitive, cultivated intellectual. (It was he who by his writing first drew my attention to the paintings of Giorgio de Chirico.) But I was disappointed by the content of his lectures. I was somewhat interested in his case-by-case method, whose merits I now appreciate more than I did then, but I do not think he actually accomplished much with it.

Wisdom's case-by-case method was essentially an application of the point Wittgenstein emphasized in his famous paragraph about games:

Consider for example the proceedings that we call "games." I mean board games, card-games, ball games, Olympic games, and so on. What is common to them all? --Don't say "There *must* be something common, or they would not be called 'games'"--but *look and see* whether there is anything common to all. --For if you look at them you will not see something that is common to *all*, but similarities, relationships, and a whole series of them at that. To repeat: don't think, but look!<sup>19</sup>

Because Wisdom was convinced that our natural tendency to ignore the diversity commonly present in the instances falling under a problematic general term is a prime source of philosophical error and perplexity, he thought it vitally important to explore the diversity in a case-by-case manner. I can no longer recall problems that he disposed of convincingly by means of this method, but I can identify a host of current concerns that I now think can be illuminated by it.

Take the almost obsessive concern by epistemologists over the past forty years to find an acceptable definition of knowledge, or knowing-that. Is there really a good reason to suppose that an acceptable definition can be found? I was taught that what one properly defines are words, not things. Does the word "knows," or the predicate "knows that," possess an exact meaning that can actually be pinned down by a precise definition? I would say no, but many of the philosophers who have taken an active part in the hunt have been seeking something other than the meaning of a word anyway. Matthias Steup, the writer of a recent textbook in epistemology, is a representative example: He seeks an analysis of the "concept" of knowledge. Such a concept should not be confused, he said, with either words or ideas in the mind. The concept of knowledge is, he says, a "property"; it is "what people have in common when they know something."<sup>20</sup> But how does Steup know that such a property exists? How does he know that people who know something have a distinctive property in common? He doesn't say. Since he immediately turns to a discussion of "propositions" and

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<sup>18</sup> Malcolm, p. 26.

<sup>19</sup> *Philosophical Investigations*, §66.

<sup>20</sup> Matthias Steup, *An Introduction to Contemporary Epistemology* (Upper Saddle River, NJ, 1996), p. 21.

a criticism of philosophers who do not acknowledge their existence, he may believe that a common property of knowing should be acknowledged by anyone who rejects nominalism. The falsity of nominalism (if indeed it is false) may lend credibility to the existence of some properties or other, but it hardly supports the idea that there is a unique, special property of knowing.

When Wittgenstein objected to the assumption the games must have something in common, he was not objecting to the existence of properties; he was asking us to "look and see" whether anything is common *in the case of games*. The idea that properties (or "universals") exist but that a distinctive property does not belong to the members of every specifiable class is actually standard doctrine among property theorists. David Armstrong, in his two-volume defense of immanent realism, holds that only "absolutely determinate" universals exist. He holds this view, I suspect, because he realizes that only determinate universals are distinct attributes that are either possessed or not possessed by every subject. If *being a game* were a determinate property, every thing or activity would either be a game or not be a game; there would be no borderline, indeterminate cases. But there surely are such cases. The same may be true of knowing. The predicate "knows that" may be as vague as the term "game," and borderline cases are common with the application of vague expressions. They are easily recognized by means of Wisdom's case-by-case method.

To appreciate the utility of Wisdom's method, it is advisable to begin with a philosophically unproblematic example. Consider chairs. It is easy to identify obvious instances of chairs, the sort we see in kitchens, living rooms, and offices. It is also easy to identify objects that are clearly not chairs. A dining-room table is not a chair, and a floor lamp is not a chair either. Is a baby's highchair a chair? Is a highchair a high chair? Some would say, "Yes"; others would say, "No, it is furniture of a different but related kind." Is one person clearly right and the other clearly wrong in this dispute? Who is to say? Highchairs are built for a specific purpose, and they are clearly similar to but different from typical chairs. If there were a chair authority, we could have a clear ruling; but the question is unimportant and no ruling is forthcoming, though we could decide "for ourselves." Consider also the desk-chairs often found nowadays in classrooms. Unlike the student desks of my childhood, which were not physically attached to what we sat on (the seats were more like short folding benches than chairs), the desk-chairs I am talking about have a small, non-retractable, desk-like protuberance that is attached to what, lacking the desk part, would qualify as a chair. Is it actually a chair? There is no correct answer to this. The concept of chair is not that determinate. The same is true of the concept *stool*. By adding some parts to a stool and subtracting others, we can create a recognizable chair, but judicious additions and subtractions can result in an intermediate object that is not "correctly" classifiable either way.

Reflection on the vagueness of common classifications makes it obvious that we do not invariably recognize instances of a kind by applying a concept or criterion that specifies a feature, a "property," that is possessed by all and only those instances. In one way or another, usually, we learn to recognize typical positive instances, typical negative instances, and when we identify other instances, both positive and negative, we do so by means of their similarities to typical instances. Very often, we are simply uncertain where to draw the line, and sometimes we

choose (usually in connection with others) to include or exclude a borderline case, where the relevant similarities are tenuous or puzzling.

If, to return to the example of knowing, which Professor Steup confidently claims to be a common (and peculiar) common property, we consider particular cases where we would ordinarily agree that some person knows a particular thing, do we encounter any interesting diversity? I think we do. One kind of case requires the sort of rational certainty that Descartes attributed to *scientia*. In a recent letter to the *Scientific American*, a man calculated that to win the \$160-million with his lottery ticket, he would have to beat the winning odds of 1 to 120,526,770. In spite of these odds, he was willing to buy the ticket, and when he did we would not agree that if his friend Tom believes he will lose, Tom knows he will lose if that is what will happen. In spite of the very strong evidence Tom possesses, the possibility remains that the man will win--and this is enough to defeat his Tom's claim to know he will lose. In this case, actually knowing that the man will lose requires rational certainty. This sort of case is very different from that of my neighbor's child who certainly knows, as everyone would say, that she has a pet kitten: she plays with it every day. Yet her evidence does not amount to rational certainty, since she has not ruled out the possibility that her "kitten" is a cleverly designed robot. The evidence she has is consistent with her being mistaken.

A philosopher presented with these two examples might argue that we should distinguish certain knowledge from rough-and-ready knowledge or knowledge loosely so called. Such a distinction does accord with some familiar patterns of speech, as when a person asks another, "Do you know that *for certain?*", and it may be philosophically useful to draw it. The possibility of doing so increases the dubiousness of Steup's contention that there is a distinctive property of knowing that is common to all cases, and it is fully compatible not only with Wittgenstein's idea that the different cases are in various ways more or less similar, but with Quine's suggestion that we would do well to think of knowledge--that, ordinarily understood, as a matter of degree.<sup>21</sup> There is really no question that as commonly used the predicate "knows that" is vague. Even if knowing does not, as people ordinarily understand it, require belief (a patently vague notion),<sup>22</sup> it does require some kind of evidence, and in cases other than certain knowledge the rational support provided by the evidence is always more or less strong, with no precise point where the support becomes strong enough for knowing.<sup>23</sup>

These remarks about knowledge were prompted by my claim that John Wisdom's case-by-case method can illuminate a host of contemporary concerns. I won't try to list a number of these concerns, but the tendency of many philoso-

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<sup>21</sup> W.V. Quine, *Quiddities: An Intermittently Philosophical Dictionary* (Cambridge, MA: Harvard, 1987). pp. 108ff.

<sup>22</sup> David Lewis argues that knowledge does not require belief in "Elusive Knowledge," *Australasian Journal of Philosophy*, 74 (1996), 549-567. The predicate "believes" is patently vague because belief admits of degrees (one believes more or less firmly) and there is no precise point where believing begins and ends.

<sup>23</sup> In chapter one of *An Empiricist Theory of Knowledge* I defend a twofold conception of knowing that p, a strong conception that recalls Descartes' conception of "perfect" knowledge, and a weak conception that does not require rational certainty. This book came into existence as a continuation of its first chapter, on knowing, which arose from these remarks on Wisdom's case-by-case method. It is a very curious fact that what is probably my last substantive book in philosophy originated in remarks about a graduate seminar that I regarded, when I took it, as something of a waste of time.

phers in the past couple of decades to provide precise definitions for important "properties" such as intentionality, belief, purposive action, or composition (as in the striking case of Peter van Inwagen) should make the expression "host of" seem amply justified.<sup>24</sup>

Before climbing up on my soapbox, I was describing the seminars I took at UCLA in the academic year 1957-58. In the semester following Wisdom's seminar I took Carnap's seminar in logical theory. This seminar was no more demanding than Wisdom's, but it was considerably more technical. The subject was Carnap's version of the logic of relations (he followed pretty much the exposition in *Principia Mathematica* but he used his lambda operator in place of the symbolism of class abstraction) and its extension to a non-quantitative treatment of space-time topology. Except (as I recall) for one report by David Kaplan,<sup>25</sup> who appeared to be enrolled in the seminar although he was probably engaged in preparing exercises for the volume in which the seminar material was later published,<sup>26</sup> Carnap himself presented material in the seminar sessions. His procedure was to hand out mimeographed sheets containing the formulas he proceeded to discuss. He would read a formula, explain its meaning if its meaning were not obvious, sometimes indicate how it could be proved if it were a theorem, and then go on to the next formula. (In indicating how a theorem could be proved in the logic of relations, he liked to use arrow diagrams as heuristic aids. If a relation were transitive, say, it could be represented by a diagram in which an arrow would be drawn between points *a* and *c* if it connected points *a* and *b* and also points *b* and *c*.) Listening to him presenting such material was like reading a textbook. If he were a lesser person, the class might have seemed to be a waste of time; but I and the other students were so impressed by his intelligence, his learning, and his earnest, kindly personality that we felt fortunate to be in his presence. He was not teaching so much as presenting the results of his research. It was our job to understand him.<sup>27</sup>

In my experience philosophers who have achieved some distinction often possess large, unattractive egos, and it is not uncommon for them to speak ill of other philosophers, often equally distinguished, whom they consider rivals.<sup>28</sup> Carnap was not like this at all--at least in my experience. He was obviously self-confident, but he was not in the least vain, self-important, or disparaging of those who disagreed with him. On one occasion he gently admonished me and another

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<sup>24</sup> I am thinking of van Inwagen's attempt to specify "what composition is" in his book *Material Beings* (Ithaca, 1990).

<sup>25</sup> David Kaplan was, like me, a first year graduate student, but he had studied logic with Carnap and Kalish as an undergraduate and so knew far more logic than his coevals. No doubt, he had more talent for logic than the rest of us, too.

<sup>26</sup> *Introduction to Symbolic Logic and its Applications* (New York: Dover, 1958).

<sup>27</sup> For thoughtful, sensitive remarks about Carnap as a man and teacher, see Abraham Kaplan, "Rudolf Carnap," in Edward Shils, ed., *Remembering the University of Chicago: Teachers, Scientists, and Scholars* (Chicago: University of Chicago Press, 1991), pp. 32-41.

<sup>28</sup> Roderick Chisholm possessed this unattractive trait in a marked degree. Once at a dinner at Dartmouth College, after he had given a lecture, Susan Brison asked him a question about the Sellars-Chisholm correspondence on intentionality. Instead of answering her question, he observed that Sellars was "not a good philosopher." When he saw that I had overheard him, he said "Of course Bruce would not agree with me about this." I said, "No, I wouldn't," and let it go at that. I was tempted to add, "Sellars might think the same of you, but he would not be so *gauche* as to say such a thing in a setting like this." I probably didn't say this because I knew its first conjunct wasn't true. Sellars was very critical of Chisholm's philosophy, but he didn't hold the silly view that Chisholm wasn't a "good philosopher." What is a "good philosopher," anyway, and how many of them exist in American philosophy?

student when, no doubt hoping to impress him with our commitment to the tough-minded ideology he was noted for espousing, we expressed our utter contempt for some claim by Heidegger. His response was immediate: "Tolerance, boys, tolerance." It was clear that he didn't object to our being critical of Heidegger; he objected to our intolerant manner: We should treat others with respect even when we think they are wrong. He obviously felt we should be careful of tooting our own horn, too, for he was noticeably self-effacing in discussion. He often said such things as "We logical empiricists now think that ...," speaking as if he belonged to a team of investigators in which personal achievement is subordinate to a collective purpose of working out a mutually acceptable "scientific" philosophy. I have never felt that I belonged to an investigative team in philosophy, but in subsequent years it has always seemed to me that Carnap and his friends Herbert Feigl and Carl Hempel, who shared his kindness, tolerance, and lack of self-importance, were models of professorial behavior.<sup>29</sup>

Before moving on to the next important step in my philosophical life, I should say something about my fellow graduate students at UCLA. It was a very impressive group, I thought. As I noted in a letter I wrote to Sellars that year, the department had eleven teaching assistants and three research fellows. This is a much greater number than we had at Minnesota. The outstanding member of the group was no doubt David Kaplan, who was a very agreeable fellow student, amiable, helpful, and fun to be with. John Turk Saunders, Maurice Josephs, Hal Louter, Narayian Champawat, Stanley Soderstrom, Tom Anesee, and John Forthman also greatly impressed me with their good nature and philosophical ability. (I don't mean to disparage the others, but these are the people who come vividly to mind after fifty-two years.) Maurice Josephs, who became my closest friend at the time, was a pure delight. He had studied ballet before turning to logic, and I can vividly remember him telling Richard Montague that logic, rigorous as it was, was not nearly as rigorous as ballet. (As I mentioned earlier, Montague was constantly emphasizing the importance of rigor in philosophy.) Maurice was far and away the most flamboyant member of our group. He had a wonderful smile, long curly hair combed back on his head, and he moved as if he were still a ballet dancer. He was also highly mischievous; in fact, he was the one who, along with me, drew the gentle rebuke from Carnap. His interest was mainly logic; he had worked through Hilbert and Ackermann's formidable *Mathematical Logic* when he was an undergraduate at Brooklyn College, and the word "rigor" was never far from his lips. Alas, he died of cancer when he was still a graduate student. Richard Montague told me that he had transferred to UCal Berkeley a year or two after I left UCLA, but that he managed to marry and father a child before the disease succeeded in killing him.

John Turk Saunders also became a close friend, one with whom I had some helpful philosophical correspondence in the late sixties and early seventies. He was the author in the early seventies of several excellent articles and the joint author of good book on Wittgenstein's private language argument,<sup>30</sup> but like Maurice he was cut down long before his time, having contracted a deadly form

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<sup>29</sup> In saying this I don't mean to imply that other philosophers I knew were not also models of professional behavior. Wilfrid Sellars was less humble than the group I mention here, but he was equally admirable in his own way.

<sup>30</sup> John T. Saunders and Donald Henze, *The Private Language Problem: A Philosophical Dialogue* (New York, 1976).

of hepatitis on a visit to Mexico. I remember dining with him and his first wife at his house in the Hollywood hills; they served wild ducks, which his wife was urged not to over-cook, but they seemed virtually raw to all four of us (my wife Ilene was there too), so she had to cook them again while we drank more wine and waited. Unlike the others, Stanley Soderstrom seemed particularly adept at most subjects, logic as well as literature (I remember discussing *Daniel Deronda* with him, a book the others had never heard of), but I never heard of him again after I left UCLA. I expected him to have a very distinguished career, and I have often wondered what happened to him.

Toward the end of my year at UCLA I got a letter from Alan Donagan, who had become chairman of the philosophy department at Minnesota. He told me that the department could now give me an assistantship if I wished to return. Although I was happy at UCLA and pleased with what I was learning, I thought I would do better finishing my degree at Minnesota than I would at UCLA. Minnesota had a more visible philosophy program than it does now; I was eager to study with Sellars, who was still there; and UCLA's strength in philosophy was then mainly in logic, a subject on which I was not prepared to write a thesis. I had done well in logic courses at UCLA, but thanks mainly to Richard Montague's influence and example, I thought of logic as a branch of mathematics and believed that I had come to the subject too late to make it my philosophical specialty. I could realistically hope to teach it, as a high school math teacher might teach algebra and calculus, but I seriously doubted that I would be able to make a significant contribution to the subject. Another reason for returning to Minnesota was that I was married to a Minnesota Swede, and she was eager to go back home. I therefore accepted Donagan's offer and returned to Minnesota.

When I got back to Minnesota, I discovered that Sellars had resigned from the department and taken a position at Yale. That was very disappointing. I had corresponded with Sellars while I was at UCLA,<sup>31</sup> but he had little to say about personal things and did not say that he expected to leave the Minnesota department. Although I was sorry to hear that Sellars had left, I was still happy to be back at the Minnesota department. Alan Donagan was there and so were Feigl and my friend Grover Maxwell, who had become Feigl's associate at the Minnesota Center for Philosophy of Science.<sup>32</sup> Of course, there were other philosophers there, too, but Feigl and Donagan were the ones I wanted to work with. To my surprise and displeasure, I discovered that the department had developed a good deal of doctrinal dissention, although the conflicting parties seemed to be on good personal terms. Feigl and Maxwell were basically Carnapians; they were in broad agreement with Carnap's aims and outlook--and they were also tolerant of other ways of "doing" philosophy. Donagan had been educated at Oxford, and he was generally sympathetic with the Wittgenstein of the *Philosophical Investigations*, which was just beginning to be fashionable, but he had begun his book on R.G. Collingwood and also had more respect for formal logic and philosophy of

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<sup>31</sup> As I mentioned earlier. See Appendix B below.

<sup>32</sup> When Maxwell and I took courses together at UMN, he had already earned a Ph.D. in physical chemistry; in fact, I believe he came to UMN as an instructor in the Chemistry department. But he became interested in philosophy of science and started taking or sitting in on courses in philosophy as a means of schooling himself in the subject. By the time I returned from UCLA he was teaching in the philosophy department (I became his TA in a logic course) as well as having a position in Feigl's Center, and he eventually became a professor of philosophy without ever taking a degree in the subject.

science than most neo-Wittgensteinians. He too was a tolerant, agreeable teacher and colleague. May Brodbeck, my first logic teacher, was, as I mentioned, a follower of Gustav Bergmann; she was a true-believer who had resented Sellars' former control of the department and, according to what I was told, was a leader in persuading the dean of the college to appoint another department head in Sellars' place. Then there were three younger men who were, as far as I could tell, convinced neo-Wittgensteinians, and a couple more faculty members whose philosophical orientation was less obvious or less extreme. As a doctoral student who had to anticipate more than twenty hours of written preliminary exams, I was alarmed by this absence of agreement on how philosophy should be pursued. I didn't mind taking courses from teachers who favored different methods; I myself was still uncertain about how I wanted to proceed. But I was bothered by the disparaging attitude some of these people betrayed toward the views of others. How would they evaluate my answers to questions on the preliminary exams? As it turned out, in the telegram I got informing me that I had passed the prelims, the examining committee expressed their disappointment that I was not more forthcoming about the philosophical method I preferred.

In spite of the doctrinal conflicts in the department, I enjoyed being back. The high point of my first year was a seminar in the philosophy of mind conducted by Alan Donagan. One of the essays to which he devoted a great deal of attention was Norman Malcolm's "Knowledge of Other Minds," which had just appeared in the *Philosophical Review*. Malcolm argued that the traditional analogical argument for the existence of other minds was fundamentally defective and that the so-called problem of other minds didn't actually make sense. As I reflect on his argument now, I see that it presupposes a kind of verificationism that is patently objectionable, but this defect was not evident to me in the fall of 1958. I thought then that Malcolm's argument succeeded in exposing defects with the so-called problem of other minds that had hitherto not been appreciated. The seminar made me excited about the whole issue of mind and behavior, and it created a cluster of problems that I was eager to pursue. The problems remained with me until I began to write my doctoral thesis at the end of the following year.

The next semester I attended Herbert Feigl's seminar in "philosophical psychology." This was a very instructive seminar for me. Most of the students attending it were from psychology rather than philosophy. I had taken two courses in psychology as an undergraduate, one at an advanced level, but the discussion in Feigl's seminar made me realize how little I knew about the subject. The principal topic discussed in the seminar was Feigl's essay, "The 'Mental' and the 'Physical'," which had not yet appeared in print: we read it in mimeograph form. Feigl didn't try to teach us in the seminar, or convince us that he had succeeded in solving the mind-body problem; he conducted a discussion of the relevant issues, suggesting further reading but otherwise treating us as colleagues in an ongoing investigation. I learned a lot from the discussion, and I made a serious effort to improve my knowledge in both cognitive psychology and philosophy of science. I soon realized that Carnap's recent work on scientific concepts was crucially important for respectable work in the philosophy of mind. It exposed very significant gaps, I thought, in arguments by Malcolm and the later Wittgenstein.

In addition to the serious reading I was doing in philosophy and psychology this first year back at Minnesota, I was also spending a lot of time learning to

read German. Doctoral candidates in philosophy were expected to pass exams in two foreign languages, and I chose French and German. I had started French in high school and pursued it further as an undergraduate, taking advanced courses in French literature, but I had not yet taken German. I started it in the fall, but I was daunted by the homework the teacher required when I was so busy with other things. Instead of completing the course, I later enrolled in an off-campus class in German for Graduate Students given by a retired Professor of German. I later dubbed the content of the class "Pavlovian German." The teacher whose surname was "Carleton" had a well-organized, well-tested course, and his students had a good record of passing the German examination, which was given by the Department of German at the university. The purpose of the exam was to insure that a student could read pertinent publications in his or her subject "with the aid of a dictionary." The course moved swiftly through the language, and we worked night and day memorizing words. Meetings were daily and at night, when the teacher would often use a projector to flash words from his "minimum vocabulary" on a screen and expect the student whose name was selected at random to provide a prompt translation. If the student failed to provide the translation at once, he (no she's were in the class) would be scolded: "You are too stupid (too lazy or too irresponsible) to deserve a graduate degree." "Why don't you leave?" I stayed in the class, and I passed the exam the first time I took it. But taking Carleton's course was the wrong way to learn to read German. I started forgetting words within a month, and I have had to re-learn German in later years, doing so more than once. I worked on the language part-time when I was on leave at Oxford writing my first book; I worked on it again a number of years later when I was writing my book on Kant's theory of morals; and I worked on it several other times when I had to read documents prepared by German students or German philosophers. It was always hard getting it back; losing it again was always effortless.

My final year as a student was mainly devoted to preparing for the written preliminary examinations (the "prelims") in both philosophy and English. The prospect of taking these exams was daunting. I seem to recall having to take five exams in philosophy and three in English. When I finished the philosophy exams, which took more than fifteen hours, my right hand was weak and cramped. I worked hard preparing for both sets of exams; I read a great deal and took lots of notes. Written exams of this kind are no longer given in most graduate departments, but a lot can be said in favor of them. I and my friend Bill Capitan, who took the philosophy prelims at the same time I did, got hung up in preparing for questions on certain philosophers; I kept reading about Kant, whose views seemed elusive to me; Bill kept reading about Plato. Of course, we didn't just read about these philosophers; we read them, too. But were worried about what influential commentators said about them, and we probably devoted too much attention to this task. The first time I felt I really understood Kant was when I taught my first course about him. After teaching that course I adopted a practice I never abandoned: I stay away from all commentators until I have mastered the primary text and have developed my own interpretation of it. Philosophers write to be understood by their readers. To be sure, some are clumsy, confusing writers; Kant is an obvious example. But if an important philosopher (even Kant) is read with sufficient care, he or she is generally understandable

without the help of any commentator. Commentators, in my experience, are often poor readers and unimpressive philosophers; I am sometimes astonished at the interpretations they come up with. Any commentator deserves to be seriously questioned, and this cannot be responsibly done if one is not thoroughly familiar with the primary text.

I did take two significant courses in my last year of graduate study; one was Huntington Brown's course in Advanced Shakespeare, which was required for all persons with a graduate major or minor in English; the other was an interdisciplinary seminar that lasted the entire year. Brown's course was not philosophical, but it had a significant impact on my career in philosophy, nevertheless. The course had three parts. One was concerned with scholarly matters pertaining to the history of the Hamlet texts and early plays that may have influenced Shakespeare in writing *Hamlet*. Another was devoted to a discussion of *Measure for Measure*, *Antony and Cleopatra*, *King Lear*, *The Winter's Tale*, and *The Tempest*. The final part, which influenced my academic career, was designed to give us training in "writing for the learned journals, where space is at a premium." For this part, Brown required ten short papers, each on a topic he specified and each to contain no more than a specified number of words. One topic I recall was "Should the Fool in *King Lear* be Cast as a Man or a Boy? (500 words.) I quickly learned that to get an A on a paper from Brown you had to write a version with about twice as many words as the announced limit and then cut it down to the required length. Only in this way, I discovered, could you say everything in the required number of words that, in Brown's opinion, needed to be said. The task was to say it all without wasting a single word. Brown was an enormously learned man who expected a lot from us as graduate students (he didn't bother to translate the quotations from French and German that sometimes appeared in his exams), and he demanded "good carpentry" in our compositions. He worked harder than we did in the course (he returned our papers in just a few days); he was honorable and kind; and we all admired him. I still worry about "good carpentry" when I write anything at all.

The year-long (or three-quarter) seminar I took that year was given jointly by Alan Donagan and Martin Steinmann, Jr., the latter a professor of English. As I recall, the title of the course was "Studies in Aesthetics." Donagan and Steinmann were personal friends who convinced their respective departments to list the course as a joint offering of Philosophy and English. The course was a big success, in my opinion. There were, I think, just two students from philosophy and four or five from English. Some of the English students were extremely impressive; at least one of them became distinguished in his field and an editor of the journal *Victorian Studies*. The seminar was focused on literary criticism; we started with Aristotle's *Poetics*; we read classics such as Coleridge's *Biographia Literaria*; and we worked our way up to T.S. Eliot and critics such as Yvor Winters, whose polemical work, *In Defense of Reason*, was a current favorite of Alan Donagan's. I thought then and I think now that an interdisciplinary course of this kind can be an exceedingly valuable thing. It makes a student much less narrow-minded, academically; and if it is well conducted, it should convince the student of something every future professor should fully understand—namely, that competent people in other academic specialties are generally just as capable as competent people in one's own.

Although I ended up writing my dissertation under the direction of Herbert Feigl, I greatly admired Alan Donagan and was strongly influenced by him. When he taught the seminar with Martin Steinmann, he was working on his book about R. G. Collingwood, and in a paper he published and in conversation with my friend Bill Capitan and me, he made a very strong case for Collingwood's expression theory of art. T. S. Eliot was the dominating voice in literary criticism at Minnesota then—in fact, he gave a talk at the university when I was there<sup>33</sup>—and Collingwood's theory was designed to accommodate the modernist art of which Eliot's "Waste Land" was a striking instance. It also accounted for the importance of the expressive painting and music that we admired by such figures as Picasso and Stravinski. Alan Donagan was a cultivated man who accorded art and history the importance we thought they deserved, and he became something of a role model for Capitan and me. He and his elegant wife, Barbara, often invited groups of us graduate students to intimate parties at their apartment, and the two of them became, for Ilene and me, a model of how a faculty couple should behave. Alan and I remained good friends for the remainder of his life.

In spite of my admiration for Alan, we disagreed on about as many topics as we agreed on. Our disagreement had nothing to do with philosophical strategy or method; it was mainly the result of our differing temperaments, and it always remained amicable. On the whole, he was what Feigl called a tender-minded philosopher.<sup>34</sup> He was, I believe, a thoughtful, serious Christian, though he did not advertise this fact; and he was temperamentally inclined to accept doctrines that were implied by his religion or suggested by it. (I must admit that I was often repelled by such doctrines.) When I was his student, I strongly objected to the death penalty for so-called capital crimes. I can remember arguing with him about this and being astonished, almost flabbergasted, when he told me that if he murdered someone, he would *want* to be hanged for it. I thought then that his attitude was bizarre. I now think it was what I should have expected from him, a consequence of his thoughtfulness and intellectual integrity. Having reflected carefully on his religion, he was clearly committed to the idea that moral evil is not just something we should avoid or be deterred from performing but something we should *pay for* if we deliberately cause it: it requires atonement. For a thoughtful Christian, there is, of course, divine atonement, accomplished by the crucifixion of Jesus, the "Son" of God; but this cannot be expected to save evil doers without a significant contribution on their part. If we have done a great evil, our contribution involves an appropriate penalty on our part—a supreme penalty if the evil is sufficiently great. Utilitarians can make light of such a contribution, but a serious Christian cannot do so—or so I believe Alan believed.<sup>35</sup>

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<sup>33</sup> He gave his talk to a huge audience at Northrop Auditorium, expressing his utter astonishment that he or anyone would ever address such a large audience on the topic of poetry.

<sup>34</sup> Feigl introduced this terminology in his programmatic essay "Logical Empiricism," which he reprinted as the introductory paper in his and Sellars' anthology, *Readings in Philosophical Analysis*. (See footnote *x* above.)

<sup>35</sup> In his book, *The Theory of Morality* (Chicago, 1977), Donagan argued that traditional, Judeo-Christian morality is very different from the secular morality expounded by secular writers such as C. D. Broad, W.D. Ross, and the English Utilitarians. Donagan did not believe that the common morality he accepted is defensible only on a religious basis, but I argue in my book on Kant that the non-religious defense he had in mind does not succeed. See my *Kant's Theory of Morals* (Princeton, 1980), *p.*

Other points of disagreement proved to be very instructive for me. One concerned universals. When I was at UCLA, the philosophers there took the writings of W.V.O. Quine and Nelson Goodman very seriously, and I had spent many hours reflecting on the most influential papers of these men. The result was that when I returned to Minnesota I had little respect for realist theories of universals: I had become convinced that they were a part of Plato's beard that deserved to be cut away. To my surprise I discovered that Donagan was convinced that one such theory was true. I don't think he and I ever debated the issue, but I devoted a lot of thought to a paper on universals that I believe he wrote when I was working on my dissertation.<sup>36</sup> Unlike most believers in universals, Donagan did not insist (as some of my former colleagues have) that the reality of universals is "obvious"; he actually offered an argument for his belief. I no longer remember the details of his argument, but I do remember thinking that it was fundamentally an inference to the best explanation. At the time, I did not object to arguments of this kind,<sup>37</sup> but after thinking about Donagan's argument (which was quite complex) I gradually came to the conclusion that the explanatory account offered by his realist theory was actually a bad one, raising far more problems (about particulars, about exemplification, and about universals themselves) than it solved. I published my first reply to his argument years later in the *Canadian Journal of Philosophy*;<sup>38</sup> I improved my criticism more than a decade after that in my book on metaphysics;<sup>39</sup> and I believed I perfected it in a recent article published thirty years after my first attempt.<sup>40</sup> Donagan's paper therefore stimulated forty years of thinking on its subject.

When I was taking my seminar with Donagan and Steinmann, Feigl suggested that I apply to the National Science Foundation for one of the summer fellowships it was currently funding. I was not a specialist in the philosophy of science and I didn't think I had much of a chance to be chosen for such a fellowship, but I applied anyway, hoping to be lucky. To my surprise, I was one of the lucky ones; and the award meant that, for the first time since I entered graduate school, I could afford to spend the summer doing something other than manual labor.<sup>41</sup> My plan for the summer was to start work on my dissertation, and if I could pursue it full-time, I could expect to make real progress. As it happened, my wife, who taught fifth grade during the school year, got a job for the summer helping with the U.S. Census, so I was able to work at home without interruption. The quiet apartment and the excitement of working full-time on a fascinating philosophical topic (the mass of issues Feigl called the mind-body problem) had an astonishing result: I worked all day almost every day in a state of intense concen-

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<sup>36</sup> The paper was published in *The Monist*, vol. 47, No. 2 (1963) under the title "Universals and Metaphysical Realism." I seem to recall reading his paper in manuscript form before I left Minnesota in the fall of 1960. In a footnote he acknowledges a debt to Gustav Bergmann's *Meaning and Existence* (Madison, WI, 1969), and I recall thinking that he was probably initially attracted to a realist view by discussions with May Brodbeck, who was, as I said, a disciple of Bergmann's.

<sup>37</sup> In writing my recent book, *An Empiricist Theory of Knowledge*, I became convinced that inferences to the best explanation are objectionable if they cannot be reconstructed as applications of Bayes' theorem. I defend this conviction in chapter 6.

<sup>38</sup> "On Postulating Universals," *Canadian Journal of Philosophy*, III (1973), 285-294.

<sup>39</sup> *Metaphysics: The Elements* (University of Minnesota Press), 1985.

<sup>40</sup> The article is "Universals and Predication," in Richard M. Gale, ed., *The Blackwell Guide to Metaphysics* (Oxford, 2002), pp. 131-150. [Note added in 2011: I did not "perfect" my criticism in my book on metaphysics; I improved it further in *An Empiricist Theory of Knowledge*, chapter 4.]

<sup>41</sup> Of the jobs available to me at the time, manual labor paid the most.

tration, and I completed the first draft of my dissertation in less than two months.

Long hours and intense concentration could not have produced my dissertation without the feedback of my dissertation director, Herbert Feigl. He proved to be an ideal director for me. In some remarks I prepared many years later in memory of him, I said that I was almost unaware of the teaching he was doing when he supervised the writing of my thesis. In comparison with other directors I have known, Feigl was extremely permissive: for him, nothing I said or did was off-limits. Since I was still partly under the influence of the later Wittgenstein, he disagreed with much of what I was saying, but he didn't trample me: he let me go my own way. He guided me, all right, but his guidance was indirect, and it brought out the best in me. "Such and such a book (or article) has a bearing on this argument of yours; you ought to read it," he would say. And then, after I had read it, he would ask, "What do you think? Was the writer correct, or not? Tell me." In seeking my opinion as he did, he was rewarding me for my efforts, treating me as a serious philosopher and making me, in the process, more serious and more of a philosopher. This treatment continued, I might add, after I finished my degree and was out in the philosophical world. When I would see him on visits to Minneapolis, my home town, he would always ask me what I was working on—and really be interested in hearing what I had to say. He did the same with other visitors, and his practice in this regard partly accounts, I believe, for his success as a director of his Minnesota Center for Philosophy of Science. Instead of attacking a visitor's paper, trying to refute it as philosophers commonly do, his aim was to make sense of what the visitor is saying—to see if any light is being shed on serious problems. His approach was encouraging, not daunting; and people were stimulated by it. In my experience (and here I am getting ahead of my story) the Center was a gratifying place to visit largely because of his mediating presence.

When classes began in the fall of 1959, I was on campus with a dissertation that was basically completed. With just a little more revision it was ready to be defended in a final examination. I was eager to have my degree in hand, but I would lose my teaching assistantship if I graduated and had no prospect of any other job. I stalled until spring, polishing my thesis and teaching my discussion sections. Life was easy though a little dull. But Feigl soon came to the rescue. If I wanted to graduate, he said, he could offer me a research assistantship in his Center. This assistantship would require no teaching; the duties would consist in helping to edit manuscripts from a collection he and Grover Maxwell were publishing on current issues in the philosophy of science.<sup>42</sup> I eagerly accepted Feigl's offer; it proved to be the best choice I could have made at the time.

I defended my thesis in late winter before an unusual group of examiners appointed by the Dean of the Graduate School. (In those days students were not allowed even to express a preference about their examiners.) Those from philosophy included Herbert Feigl, Alan Donagan, and May Brodbeck. Since English was my official minor field of study, it also supplied an examiner, Samuel Monk, a distinguished 18th century scholar whom I knew of but had never actu-

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<sup>42</sup> The volume was composed of papers originally presented to a meeting of the American Association for the Advancement of Science; it was published as *Current Issues in the Philosophy of Science*, Herbert Feigl and Grover Maxwell, eds. (New York, 1961).

ally met. Because the subject of my dissertation was closely related to topics in psychology, the Dean also included Paul Meehl as an examiner from the Psychology Department. (Meehl was a distinguished psychologist with strong philosophical interests; he was one of the original editors of the journal, *Philosophical Studies*.)<sup>43</sup> Grover Maxwell sat in as a visitor. From my point of view, the exam went very well, and I enjoyed it. May Brodbeck, who could be quite fierce, disagreed with the line I took in my thesis, but I was not daunted by her aggressive questions. Meehl and Monk were civil and even somewhat congratulatory.

After receiving my degree in early spring, I became a research assistant in Feigl's Center. My editing duties were essentially copy-editing; I worked on a huge manuscript containing papers by such philosophers as N. R. ("Russ") Hanson, Paul Feyerabend, and Adolf Grünbaum. I can no longer remember which papers I actually edited myself; I know I worked on papers by Hanson and Feyerabend, which needed a lot of work, but I am foggy about the others. One thing I clearly do remember (it was nearly fifty years ago) was how much I learned about the philosophy of science as it was then practiced. I thought carefully about the content as well as the style of the essays I worked on, and I got to know some of the writers personally. Paul Feyerabend was actually in residence at the Center when I was there; and several other philosophers of science, including Sellars, came there for talks and conferences. With Feigl's kind encouragement I took an active part in reading and even preparing "memos," short papers on pertinent topics, which the Center secretary would type up and then copy for discussion by the Center staff. I had sufficient free time to produce several such memos, one on the problem of other minds, a later version of which appeared in the *Philosophical Review*. In my first year of teaching I actually published four papers, all of which grew out of discussions I had at the Center.

The ones who discussed my work were Feigl and Grover Maxwell. Feigl was mainly concerned to get clear about the structure of my arguments and the premises I relied on; he was generally supportive rather than judgmental even when it was clear that I was disputing something he accepted. Maxwell was quite different. He was characteristically taciturn, a man of few words, and when he disagreed, as he often did with me, he didn't dwell on details: he might say, "I think that's a bunch of crap" or something similar. Coming from another person, this kind of criticism would hardly be helpful; but I had a high opinion of Grover's judgment, and when he expressed disagreement with an argument or claim, I always thought it over and at some point tried again. Sometimes we had to agree to disagree, but often I eventually saw that he was right. Sometimes his very silence was instructive. When I said something stupid or *gauche*, he often acted as if he didn't even hear me. It was really a gentle way of correcting me.

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<sup>43</sup> Meehl was in fact a polymath who became something of a role model for me. Apart from his work in experimental psychology, he was active in many other fields. He carried on a practice in psychological counseling (Saul Bellow, as James Atlas discloses in his biography of Bellow, was a patient of Meehl's when Bellow had the breakdown that led to the writing of *Hertzog*); he wrote philosophical essays, was a regular participant in discussions at Feigl's Center, and, to Feigl's dismay (Feigl had once been his teacher), he spent a summer when I was there writing a book on theology. After that he became an expert in forensic psychology and joined the School of Law as an adjunct professor; in later years he was also on the faculty at the Medical School. He impressed me because he was an unassuming man who, without calling attention to himself, showed that one could master many different fields simply by doing the requisite work. He didn't seem to require applause for his attainments.

Although I had few discussions with Paul Feyerabend, I heard him read a number of papers at the center and heard him lecture several times. I thought he was something of a wild man, philosophically, but I was greatly stimulated by his frequent remarks about art—specifically, about Bertolt Brecht’s criticism of an Aristotelian conception of drama. By an Aristotelian conception of drama, I mean a conception of it as creating a dramatic illusion productive of emotions and ideas considered beneficial for members of an intended audience. Aristotle himself described the emotions produced by a successful tragedy as pity and terror, and he thought these emotions were produced by an illusion in which actors wearing wooden masks are seen as kings or heroes and an unadorned stage is seen as a setting for tragic actions. Since almost all drama and all novels are intended to produce some kind of dramatic illusion, almost all drama and every typical novel is broadly Aristotelian in conception. Feyerabend, rapidly limping around the lecture hall and leaning on a crutch,<sup>44</sup> would develop Brecht’s criticism that such drama is productive of an uncritical state of mind and that, to produce critical understanding, drama and imaginative literature generally should frequently shatter dramatic illusions and promote a critically attentive view of what is being seen or read. I didn’t realize it at the time, but Brecht’s criticism of the Aristotelian view is essentially the same as Plato’s basic criticism of mimetic art. Plato viewed dramatic illusion as a form of intellectual bewitchment that is seriously detrimental to the frame of mind of a philosopher king in his ideal Republic—and to a good man in the real world trying to live a good life. I developed this criticism in a recent essay on Plato’s criticism of mimetic art, not realizing until I wrote this paragraph that I was echoing ideas I first heard from Paul Feyerabend.<sup>45</sup>

Another frequent visitor to the Center when I was there was Norwood Russell (“Russ”) Hanson. Russ was a large, aggressive, adventuresome man with a wonderful sense of humor. A philosopher of science, he made his reputation from his criticism of the distinction between theory and observation, but his later work was less philosophical, I thought, and more closely related to the history of science. I think he was admired as much for his exuberant personality as for the work he was doing toward the end of his career. In the time I knew him he owned and flew two WWII military airplanes, a trainer of some kind (he told me it was a Corsair) and then a Grumman Bearcat fighter. Having run out of gasoline with an airfield in sight, he crashed the first plane, and the consequent repairs to his face permanently altered his expression, making him appear (at least to me) decidedly fierce. He had that expression on his face when he rode into the tiny town of Oberlin, Ohio for a philosophy conference a year or so later. He was teaching at the University of Indiana then, and he rode into Oberlin on a huge Harley-Dickenson motorcycle, which rattled the whole town. He said he just bought the motorcycle; no doubt it was previously owned, Sellars quipped, by an elderly lady who rode it only to church on Sundays. Not long after this Russ was killed in his Bearcat flying to Ithaca, NY, where he was scheduled to give a lecture. Evidently the skies were dark and cloudy when he left his home airport.

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<sup>44</sup> He was wounded while serving in the *Wehrmacht* in WWII and he remained crippled for the rest of his life. See his autobiography, *Killing Time* (Chicago, University of Chicago Press, 1995).

<sup>45</sup> “Plato’s Objections to Mimetic Art,” available on-line at <http://www.umass.edu/philosophy/faculty/faculty-pages/aune.htm>.

As one can infer from the volume I worked on,<sup>46</sup> the issues animating the Center when I was there concerned the reality of theoretical entities, the analytic-synthetic distinction, the relation between the mental and the physical, the status of introspective reports, and the theoretical importance of ordinary language. Feigl and Maxwell were main-line empiricists strongly influenced by Bertrand Russell and Rudolf Carnap; they were therefore sympathetic with the distinction between analytic and synthetic truth, which Quine was currently challenging, and they were eager to discuss ways of meeting Quine's criticism. Verificationism was still in the air, at least in spirit, and there were doubts to resolve about how we could meaningfully speak and claim to know about imperceptible entities. The relation between varieties of the mental and the physical as it is known scientifically was just as puzzling then as it is now; and the later Wittgenstein's claims about the impossibility of "private languages," the peculiarity of so-called introspective reports, and the methodological primacy of ordinary language (if I may put it this way) were debated by almost everyone. My dissertation was focused on these issues, and my preoccupation with them fueled the memos I began to write. At the time I thought I was, like Sellars, breaking away from the kind of empiricism Feigl and Maxwell defended, but I realize now that my agreement was far greater than my disagreement. Although I was more strongly influenced by Wittgenstein and Sellars than they were, I was still an empiricist. I thought an analytic/synthetic distinction that involved Carnap's notion of a meaning postulate was defensible, and I was comfortable with the inductive methods that Feigl, Sellars, and Maxwell accepted. I was dismissive of private languages then, but I did not think they were essential to an up-to-date empiricism.

At the beginning of my account of my philosophical education, I spoke of my primary education in philosophy; that took place when I was a student both at UCLA and Minnesota. My secondary education in the subject began at Feigl's Center, and it continued as I began my teaching career at Oberlin College in Ohio.

In the spring of 1960, when I began looking for a teaching position in philosophy, there were very few openings for young PhDs, and most of the ones that were available were not advertised or announced. Because possibilities in the Midwest seemed nonexistent, Bill Capitan and I drove to New York City, where the APA Eastern Division meetings were being held, to see if any jobs were advertised there. We ran into some old friends at those meetings, people who had spent some time at Minnesota but were now attending Brown and Princeton: Keith Lehrer and Herbert Heidelberger. They knew of jobs, but they wouldn't tell us where they were; they didn't want any more competition. There was a table or perhaps a room where we could leave our names, specialties, and our affiliations, but no jobs, as I recall, were actually listed there. I left the relevant information and went back to Minnesota feeling extremely discouraged. To my surprise, the information I left resulted in an inquiry. There was a position open at Washington State University in Pullman and I was asked to apply. I did so and in due course I was offered an appointment there.

Although I was unaware of it, Herbert Feigl, Wilfrid Sellars, Alan Donagan, and possibly even others were recommending me to departments that had or

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<sup>46</sup> See footnote #35.

might have openings for the following year. When I was on the point of accepting the job at Pullman, I got a letter from C.D. Rollins at Oberlin announcing an opening there and suggesting that I apply for it: Feigl, Sellars, and I think Donagan had all strongly recommended me to him. (The concept of a tenure-track position did not exist in those days; there were simply positions, temporary and "regular.") I quickly applied and, as requested, sent a copy of my thesis. Rollins read my thesis and also read the half-dozen or more other theses sent by my competitors. I am not sure if Paul Schmidt, Rollins' other senior colleague, also read all those theses (I doubt that he did), but Rollins liked mine best and invited me to the campus, where I was interviewed by all the members of the college's Faculty Council, as I think it was called—about ten people. They eventually recommended that I be given an appointment. Although I had my Ph.D. in hand, Dean Love (that was really his name) appointed me as an Instructor rather than an Assistant Professor; he said I looked too young to be a professor. I was twenty-six.

After I accepted the offer from Oberlin but before I began teaching there, I was invited to take part in the first Oberlin Philosophy Colloquium. Ronald Butler, who was on the staff of Oberlin at the time, organized the colloquium. I was invited to be a commentator on a paper by Kurt Baier, called "Itching and Scratching." Baier still held a position in New Zealand then, but he was soon appointed Chairman of the philosophy department at the University of Pittsburgh. When I arrived at the colloquium I was delighted to discover that Wilfrid Sellars was there. I had corresponded with Sellars since he left Minnesota and I had several discussions with him when he returned to the university to consult with students whose dissertations he was still advising<sup>47</sup> and to take part in discussions at Feigl's Center, but I was eager to talk to him again face to face. I cannot remember if we actually had much time for private discussion, for many other philosophers were there, some from as far away as Princeton. I was very excited by the colloquium; it was the first conference in which I had taken an active part. I drove back to Minnesota as if the road were high above the trees.

When I arrived at Oberlin to begin my teaching duties in the fall, I discovered that the department had appointed another instructor; he may have been an assistant professor, though he looked as young as I did: Irving Thalberg, Jr. Yes, Irving was the son of the famous director, but his name meant nothing to me at the time. Like me, Irving was married, and he, his wife Suzanne, my wife Ilene, and I were approximately the same age. The four of us also had similar interests, although Suzanne, unlike Ilene, was a philosophy Ph.D. As a replacement for Roger Buck, who had gone on to the University of Indiana, I was assigned to teach logic and also the history of philosophy and esthetics. I cannot remember what Irving was appointed to teach, but I can remember that he was as interested in philosophy of mind as I was. It was from him that I first learned about the philosophy of Donald Davidson, Irving's former teacher at Stanford. In spite of the similarity of our ages and interests, Irving and I never developed the camaraderie I enjoyed with fellow graduate students at Minnesota and UCLA. Irving and I were always on good terms, and we became even closer after I left Oberlin. I

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<sup>47</sup> One such student was Murray Kiteley, now Emeritus Professor at Smith College, who reported seeing Sellars, who was his thesis adviser, on such visits to the university. I had discussed my dissertation with Sellars on such visits; he was a major influence on the thinking that went into it.

wondered about this lack of camaraderie for some time, but I eventually realized that we were probably unconsciously competing for status in the department. Tenure was difficult to get then, and we may have felt it unlikely that both of us could receive it. I think Irving was more sensitive to this possibility than I was, but I can't deny that it may have affected me, too. As I said, we seemed to grow closer when I left the department.

Although I was always on very good terms with Paul Schmidt, whose sympathy with French Existentialism seemed to aggravate the Oxford-trained department chairman, C. D. Rollins, I was on even better terms with Rollins himself. In fact, Rollins (or Cal, as everyone called him) became one of the best friends I ever had. He was a wonderfully eccentric academic in the 60's British style. He grew up in Nebraska (his initials abbreviated "Calvin Dwight") and he went to Oxford as a Rhodes scholar. He spent some time at Cambridge, too, but he wrote a thesis under Gilbert Ryle's direction and in due course received the degree of D.Litt. He lived in England long enough to assume the cultivated but eccentric manners of an Oxbridge don. On one of my first days in Oberlin I accompanied him to the local hardware store, where he had ordered something for his house. I can no longer remember what the ordered object was, but when the storeowner brought it to him and announced the price, Cal promptly fell to the ground in apparent shock. (He had a very good sense of humor.) Cal was having some trouble writing philosophical papers at that time, but he greatly enjoyed writing comments on papers written by others. I profited greatly from the many pages of comments he wrote on my work. He was a very careful writer and a very close reader; I have never had comparable comments from anyone else.

Cal proved to be an outstanding departmental administrator, and he was helpful to me, and stayed helpful, for many years. He eventually married Helen Draper, one of his Oberlin students, after disguising her identity in an amusing way for most of an academic year. (He identified other, unlikely women as the object of his affections.) Helen, a bright, beautiful, and vivacious young woman, was nearly twenty years younger than he was, and he found this somewhat embarrassing after he married her. To compensate for her youth, he urged her to dress in a distinctly dowdy way, a fact that my wife's eighty-year-old grandmother, on meeting her, observed at once. "Why does she try to dress like an old woman?" she asked. Cal moved away from Oberlin several years later, but we remained friends for the rest of his life. Unfortunately, I learned of his death from an obituary in the Proceedings of the American Philosophical Association. He and Helen had been divorced for some years by then, and no one close to him thought of notifying me. As a mark of gratitude for some favor I had done, he had given me just a year or two before he died an album of LPs signed by Lily Krause, the soloist in performances of all of Mozart's piano concertos. (I had long been an admirer of her as an interpreter and performer of Mozart.) I now have the performances digitalized, so that I can play them on my iPod. They are still personal favorites.



**Oberlin Philosophy Department, 1960-62.** From left: C.D. Rollins, Paul Schmidt, Bruce Aune, Irving Thalberg, Jr.

My philosophical education at Oberlin was mainly in the field of logic. The logic course I was assigned to teach, Philosophy 30, was considered introductory, but I was warned that the students were eager to learn and that I should expect to cover far more ground than one would normally cover in an introductory course. I started out teaching the material that was then considered standard in an introductory course--the propositional calculus and the traditional doctrine of the syllogism--but the students learned so quickly that I immediately moved on to other things. I went through Lukasiewicz's formalization of the syllogism, discussed the differences between the Boolean and the so-called presuppositional interpretations of it (the latter was then defended by P.F. Strawson), and then developed the algebra of classes in an axiomatic form, showing how it could do the work of the syllogism and why its basic structure was the same as that of the propositional calculus. Since the students assimilated this material as quickly as I presented it, I developed the Montague-Kalish system of the predicate calculus

with identify and, over a period of several weeks, assigned the principal theorems as homework problems. I then discussed both Russell's and Frege's theories of descriptions. By this time I realized that almost half the class had taken or were taking Robert R. Stoll's course in the foundations of mathematics, which began with the algebra of classes and included a brief treatment of what he called symbolic logic.<sup>48</sup> Wanting to avoid further overlap with Stoll's mathematics course, I moved on a discussion of many-valued logics, developing the three-valued and five-valued systems worked out by Lukasiewicz and Tarski, and concluded the course with a treatment of propositional modal logic using natural deduction techniques (developed originally by Frederick Fitch) that complemented the Montague-Kalish system. I used the notation of Carnap's *Meaning and Necessity* and rewrote the list of theorems given in Lewis and Langford's *Symbolic Logic*. Kripke's first publication on modal logic appeared the year before, in 1959, but it was unknown to me in 1960, as it was to most teachers of logic outside of major universities.

It would not have been possible, obviously, to cover so much material in a single class if the students did not possess unusual mathematical ability. Not all of them were accomplished in the subject of mathematics, however; some of them, in fact, originally had a little trouble getting used to formal manipulations. After observing these latter students carefully, I realized that they were generally handicapped by a habit common to readers of nontechnical literature: they read from left to right and sometimes overlooked the fact, obvious to mathematics students, that an equivalence, a statement of the form  $\Phi \leftrightarrow \Psi$ , permits one to go from  $\Psi$  to  $\Phi$  (or to replace  $\Psi$  by  $\Phi$  in compound formulas) as well as from  $\Phi$  to  $\Psi$  (and to replace  $\Phi$  by  $\Psi$ ). To develop the mathematical intuitions of such students early in the course the next time I taught it, I resolved to let the mathematical students go on vacation for a week while I drilled the others on Boolean operations. I carried out the plan the following year and to my delight I discovered that it worked. Some of the nonmathematical students quickly moved to the top of the class.

In addition to my regular classes at Oberlin, I also conducted informal mini-seminars for advanced philosophy majors. It was in my first year there that Quine's *Word and Object* appeared, and I went through this carefully with one group of students. I believe it was in the following year that I discussed Arthur Pap's *Semantics and Necessary Truth* with a small group. Later that year I heard an announcement that Quine would be giving a lecture at nearby Western Reserve University, and I took two of my students with me to hear him. His lecture was on simplifying truth-functions, a topic important to students of electrical engineering, and he was visibly surprised to be interrogated in the question period by two of my students, Jeffrey Sicha and Roger Rosenkrantz,<sup>49</sup> on claims he had

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<sup>48</sup> In the following year Stoll published the material covered in his course in his book, *Sets, Logic and Axiomatic Theories* (San Francisco: Freeman, 1961).

<sup>49</sup> Sicha graduated from Oberlin *summa cum laude* in philosophy and mathematics and then went on to Oxford as a Rhodes Scholar, eventually earning the degree of D.Phil. Rosenkrantz graduated from Oberlin before I came there and was currently working with me as a graduate student (Oberlin gave an occasional M.A. in those days) after having spent a year in England working with A.N. Prior at Manchester University. He would later receive a Ph.D. in philosophy from Stanford University and a Ph.D. in mathematics from Dartmouth College. Both Sicha and Rosenkrantz are now highly accomplished philosophers.

made in *Word and Object* and claims pertinent to arguments advanced by Arthur Pap. As it happens, Quine had himself been an Oberlin undergraduate, and when I later introduced my students to him, he expressed some amusement by the interchange. It was the sort of thing he might have done when he was a student there.

In my second year at Oberlin I followed up my course on logic with a seminar on formal semantics, using Richard Martin's rather difficult text, *Truth and Denotation*. I learned a great deal about semantics and logic (particularly modal logic) from teaching these courses, and I also learned what I know (or did know) about set theory there, though I did not attempt to teach this subject. Near the beginning of my second year, Patrick Suppes' *Axiomatic Set Theory* appeared (I had heard about Suppes' work when I was at UCLA) and I set about working my way through it in my spare time, proving the theorems one by one. I did not actually complete the book; I did not work through the last four chapters on rational, real, and ordinal numbers; but I was very conscientious in proving the theorems that Suppes left to the reader in the first half of the book. (He proved the ones that his readers couldn't be expected to prove, such as the Schröder-Bernstein theorem.) Suppes's book was the first mathematical text that I attempted on my own, and I was pleased to learn that tenacious work can take one a long way in a technical subject without a living teacher supervising one's efforts. A good, well-written book is an adequate teacher of the material it contains.

Although my advanced teaching at Oberlin was in logic and, owing to the Quine tutorial, philosophy of language, the writing I did was mainly in the philosophy of mind and metaphysics. The first paper I published, "The Problem of Other Minds," was a revision of a memo I wrote when I was a research assistant in Feigl's Minnesota Center for Philosophy of Science. Neither Feigl nor Grover Maxwell were impressed by the argument of the paper, because it was strongly influenced by the neo-Wittgensteinian arguments Norman Malcolm presented in his paper "Knowledge of Other Minds" and in his review of Wittgenstein's *Philosophical Investigations*. The other papers in philosophy of mind grew out of my doctoral dissertation, which was strongly influenced by Wilfrid Sellars's "Empiricism and the Philosophy of Mind." Gilbert Ryle published one of these papers, "Feelings, Moods, and Introspection," in *Mind*. Ryle liked the paper (it was congenial to his thinking on its subject), and he wrote me a substantial letter when I submitted it to him. His letter was specifically concerned with the changes he wanted me to make, which were entirely stylistic. He wanted me to remove most of the footnotes, and he also wanted me delete most of the qualifications I included in parentheses. "Cut out everything you have in brackets," he said, "and then read it to one of your grumpiest friends. Whenever he scowls, delete the offending remarks." The words I have just quoted are not Ryle's actual words (unfortunately I have lost his wonderful cranky letter), but they give the spirit of his words. His letter was a good lesson for me. Trying to guard against objections, I had begun to make my language almost unreadable, loading it with qualifications. I had put aside what I had learned from Huntington Brown; Ryle's letter brought it back to me.

The other major papers I published in my Oberlin years were on the freedom/determinism issue. The first was a response to an argument Richard Taylor

offered in support of fatalism. Like “The Problem of Other Minds,” it appeared, to my delight, in the *Philosophical Review*. It and its longer sequel, “Abilities, Modalities, and Free Will,” resulted from the modal logic I was teaching in my logic class. Taylor, I argued, was making the modal error everyone in philosophy is aware of today; but at that time, thanks partly to the influence of the later Wittgenstein, Anglophone philosophers were generally very ignorant of modal logic. The *Philosophical Review* rejected the longer sequel, however; the editors told me they thought the reasoning was sound, but they doubted that many of their readers would “fight their way to the end.” Wilfrid Sellars liked it very much, though. We discussed it in a correspondence we had on the topic of free will.<sup>50</sup> Sellars agreed that the modal errors I pointed out were in fact errors, but he thought a treatment of the subject more sympathetic to writers like Taylor should formalize the key argument by introducing a dyadic modality,  $N(q/p)$ , representing a relative rather than an absolute sense of necessity. Sellars made a substantial paper out of the remarks he put in his letters to me; he called it “Fatalism and Determinism,” publishing it in a collection edited by Keith Lehrer.<sup>51</sup> Our correspondence on this topic was the beginning of a much longer correspondence we continued to have for nearly twenty years on the logic of practical reasoning. Sellars had started working on this topic at the University of Minnesota with his students Robert Binkley and Hector-Neri Castañeda. I criticized the views of all three in the last chapter of my book, *Reason and Action*; my final statement on the topic, which was still critical of them, was published in 1986: I called the paper “The Logic of Practical Reasoning.”<sup>52</sup>

The two years I spent at Oberlin proved to be the highlight of my teaching career. Just about every student I had even in beginning survey courses was eager to learn, and I would never again have so many capable advanced students. My colleagues in philosophy were all congenial and supportive, and I took real pleasure in working so closely with people in other academic departments, who would occasionally discuss developments in their subjects and advise me on books I ought to read.<sup>53</sup> Why, then, did I ever leave? It was the lure of joining a university community and teaching students at the graduate level.

In the winter of my second year at Oberlin I received a letter from Charles A. Peake, Vice Chancellor of Academic Affairs at the University of Pittsburgh, offering me an assistant professorship in the department of philosophy there, which he was engaged in transforming into a world-class center of philosophical teaching and research. Adolf Grünbaum, whom I knew as a visitor to Feigl’s Center, was already appointed as a Mellon Professor, and Nicholas Rescher and Kurt Baier had been appointed as well, Baier to be the new department chairman. I knew only some of Rescher’s papers on logic, which I greatly respected (I had no idea what a polymath he was), and I had served as commentator on a paper by Baier at the first Oberlin Colloquium.<sup>54</sup> The prospect of joining a graduate

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<sup>50</sup> The correspondence is available on-line at [www.ditext.com/sellars/csa.html](http://www.ditext.com/sellars/csa.html).

<sup>51</sup> *Freedom and Determinism*, ed. Keith Lehrer (New York: Random House, 1966), pp. 141-174.

<sup>52</sup> A revised version of this essay is available on-line at the UMass Philosophy website and at [www.hist-analytic.org](http://www.hist-analytic.org).

<sup>53</sup> Edgar Harden of the English department became a life-long friend. He moved on to teach at Simon Fraser University in British Columbia, where he became a distinguished Thackeray scholar, but we saw each other at intervals until his death in 2010.

<sup>54</sup> Baier’s paper was entitled “Itching and Scratching.” I don’t think the paper was ever published.

department with colleagues like these was impossible to pass up, so I gladly left Oberlin behind. I was the fourth appointment to the new Pitt department, but Peake assured me that additional appointments would soon be made.

When I arrived at Pitt the following September, Kurt Baier was still in New Zealand; he was committed to staying there until January. No further appointments had yet been made, but Brian Ellis and George Schlesinger were visiting from Australia, and my former colleague from Oberlin, C.D. Rollins, was also visiting for the year. Grünbaum and Rescher spent much of their time by themselves (Rescher was constantly writing and Grünbaum was equally busy and also physically isolated from the other members of the philosophy department),<sup>55</sup> but the younger group had a good time together, talking and arguing about philosophy. (I did play a lot of chess with Brian Ellis, though.) In the absence of Baier, Oliver Reiser, a very senior professor from the old department, was acting chairman. Reiser was the author of many books, a sort of *éminence grise* before the new people arrived, but he was an a “integrator” of knowledge, a speculative philosopher of an older school rather than an analytic philosopher, and the new arrivals had little in common with him. Like several other members of the previous *régime*, he played almost no role in the life of the new department. That life was centered on philosophical activity—writing, discussion, and a certain amount of teaching—and it was open to a steady stream of philosophical visitors.

In addition to the lively faculty and visitors, the department had already assembled a group of very able students. Two graduate students of that year’s group (two of the ones I got to know) are leading philosophers today: Brian Skyrms and Ernest Sosa. In January I wrote a paper on a priori knowledge, which *Journal of Philosophy* very promptly published,<sup>56</sup> and Skyrms and Sosa quickly wrote a criticism of it, which they later published and to which I published a reply. I also had some interesting exchanges with a very able undergraduate, Richard Grandy, who is also having a distinguished career in philosophy. In class Richard usually sat close to a young woman who seemed to occupy his attention (their eyes were often just inches apart, like the eyes of a male and a female figure in a sculpture by Gustav Vigeland),<sup>57</sup> but his arguments with me left no doubt that he knew exactly what I was saying. In fact I still have details of two arguments I had with him. He expressed one of them in a term paper, which I criticized then and he elaborated upon in a further paper that he published at least thirty years later; he sent the latter to me with a note saying, “Here’s my response to the argument you used in 1963 when you criticized my paper on colors.”<sup>58</sup> I commented on his other argument in an entry in the diary I kept at the time. He would be amused by what I said (I ran across the entry on the very day I wrote these words): “Yesterday I chatted with a student, Richard Grandy. He was defending an out and out, crassly materialistic theory of mind. He tried to argue that he couldn’t understand what a sense-impression is. In arguing with him, I recalled how similar my views are to the ones Feigl defended

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<sup>55</sup> Grünbaum, as a Mellon Professor and Director of his own Center for Philosophy of Science, had a suite of rooms higher up in the Cathedral of Learning.

<sup>56</sup> “Is There an Analytic A Priori?” *Journal of Philosophy*, LX (1963), 281-291. Note the amazing fact (by today’s standards) that JP published this paper in the year that I submitted it.

<sup>57</sup> The sculpture is in Frogner Park, outside of Oslo, Norway.

<sup>58</sup> These are not his exact words, but they convey the general idea of what he said.

when I was his student." Needless to say, I was greatly impressed by this bright and fearless undergraduate.

My recollection of the actual teaching I did in my first year at Pitt is now fairly dim, but I have a very vivid recollection of an afternoon I spent at the height of the Cuban missile crisis. Possibly because my teaching commitments at Pitt were less strenuous than at Oberlin, I had more time to myself, and as a restorative interlude in my usual at home activities of reading and writing, I had begun to sculpt an idealized human head from several blocks of modeling clay. I had never done a sculpture before, and I found the task highly interesting. On the tensest day of the crisis, I spent the late afternoon in a lecture hall in the Cathedral of Learning listening to a visiting speaker and scrutinizing the ears of the man sitting in front of me. I was paying particular attention to the way his ears were attached to his head. Their attachment interested me because I was having some trouble making the ears on my sculpture look the way they should look. But my fixation on the man's ears had another purpose, to help me avoid thinking of a map I had recently seen with the city of Pittsburgh circled in red, representing it as a prime site for a possible missile strike. I was not alone in fearing a possible attack. We didn't really know how the crisis would end. I thought I should be home with my family, but I felt duty bound to be there at the lecture. Everyone acted as if there was really nothing to fear, but we were occupied with keeping our attention on here-and-now things anyway. You can imagine the relief we felt when the president announced that the approaching Russian ships were turning around. It was a difficult afternoon.

Although I found the Pitt department a very exciting place that year, I was not happy with the city of Pittsburgh. It was already being called the Renaissance city, but the air still smelled strongly of steel mills, and soot was present everywhere. When I first looked for housing, I went out to Forest Hills, which I thought was a suburb, and found a reasonably attractive brick house on a shady street with a pleasant view of rolling country leading up to a high hill. After I rented the house and moved in with my wife and two-year-old daughter, the house seemed far less pleasant. On the other side of the scenic hill was a steel mill that was on strike when I rented the house. When I moved in, the mill was back in action and huge orange clouds rose above the hill and moved our way. Because of the trees, hills, and atmosphere, the light became dim early in the day, and we needed to turn on the lights in the house long before evening. The house lacked a room I could use as a study, but I whitewashed the walls of an old coal bin in the basement and made a room large enough to enclose a picnic table that I could use as desk. I worked at that table in the fall and winter, but one day in early spring I found our cat in that room with a serious wound on its side and gouts of blood covering the manuscript I was working on. It turned out that our cat had got into a fight with a neighborhood tomcat and was knocked through the basement window above my desk, being seriously cut as the glass shattered from the impact.

In the spring of that year Frank Wadsworth, Dean of Humanities at Pitt, called me into his office. After a pleasant discussion ranging over a variety of topics, he made the suggestion that I apply for a Guggenheim fellowship. It was something I ought to do, he said, and he added that I could expect to be well supported by senior members of the department as well as by other influential

philosophers who recommended that Pitt should hire me. I was surprised by his suggestion, but having been successful in my application for a National Science Foundation Summer Fellowship when I was a student, I was persuaded to apply. Even though getting the fellowship seemed to me something of a long shot (at the time I did not know how much of a long shot getting a Guggenheim fellowship actually was), I thought it would be highly desirable to spend a year at Oxford University, meeting the leading English figures in the philosophy of mind and language and attempting to complete the book I had been working on since coming to Pitt. Wadsworth said that if I received the fellowship, he could award me some funds for traveling on the continent and meeting some of the leading figures there. To my surprise and delight I received the fellowship, and I soon began making plans for putting my furniture in storage and arranging for my family's year abroad.

As we usually did in those years, we spent the summer (in this case the early summer) in Minneapolis, staying at the house of my mother-in-law, Iva Carlson. While we were there, we bought a SAAB 96 automobile, which we would take possession of in Denmark rather than Sweden to avoid a stiff Swedish tax. We also arranged to meet Brian Ellis later in the summer at Cambridge, UK; he would be visiting the university there with his wife and mother. While in Minneapolis, I had a number of discussions with my old friend Misha Penn, and I also made regular visits to Feigl's Center for Philosophy of Science. The Center was generally a quiet place in the summer, but Feigl and Maxwell were often there nevertheless, and they were always eager to talk. If they were there when I appeared, we would adjourn to the conference room for a serious discussion. This was something Feigl was fond of doing. I was still thinking hard about the book I was writing, but I doubt that I actually put many words to paper that summer. Life seemed to be moving too fast for that kind of activity.

Irene, I, and our two-year-old daughter, Alison, left Minneapolis for New York in early July, taking our first airplane flight. We flew on a Douglas DC 10; I can still vividly remember the plane. After a night in New York, where, according to my wife's travel diary, we paid \$17 for an acceptable hotel room, we boarded a German ship, the T.S. Bremen, and sailed, as people commonly did at that time, for Europe. The cruise was pleasant and uneventful; four days later we arrived in Southampton and put our feet firmly on English soil. After sending most of our baggage to Oxford for the rest of the summer, we visited London and then made our way to Cambridge, where Brian Ellis, his wife, and mother were staying. They had rented a very large Victorian house, which was surrounded by the most elaborate private garden I had yet seen. Having worked at a garden center one summer when I was a graduate student, I had developed a serious interest in gardening, so I greatly enjoyed exploring that Cambridge garden. At the time I never dreamed that fifty years later I would have become an accomplished gardener myself and have a garden just about as elaborate as that one.

Brian and I had conversations with just two of the Cambridge philosophers of science, Mary Hesse and Gerd Buchdahl, but the discussions were largely social occasions rather than serious philosophical talks. After just a few days touring the Cambridge environs, I and my little family said goodbye to Brian and his family, and we proceeded with a continental tour, first going north to Denmark (where

we picked up our blue SAAB), then to Sweden and Norway (my paternal grandparents grew up in Norway, and Ilene's ancestors on both sides came from Sweden) and finally going south to Germany, France, and Italy. This was our first trip to Europe, and we were eager to explore it in our shining new SAAB.

Jeff Sicha, one of my former Oberlin students, had gone to Oxford a year earlier as a Rhodes Scholar, and having found his room at Corpus Christi College a cold place the previous winter, he offered to find us a house to rent if he could stay with us as a boarder. We gladly agreed, so after our European tour we drove to Woodstock, a village close to Oxford, and took possession of the house we had rented. Jeff was touring Greece when we arrived; he appeared in Woodstock shortly before the fall (or Michaelmas) term began at Oxford. The house we rented was located opposite the main gate to the Blenheim Palace. In the following spring, when I was writing the last chapter of the book I would publish under the name of "Knowledge, Mind, and Nature," I worked in a second-floor bedroom and looked out a window from which I could see the road leading up to the palace and, in the distance, the palace itself. The view was inspiring. Browning was not exaggerating when he said, "Oh, to be in England now that April's there." The entire spring was a pure delight. In the front of our house a huge horse chestnut tree was in bloom, and daffodils were everywhere.

While I was still in Europe I began to feel guilty about touring rather than writing, and I actually started working on a paper when we were staying in a student hotel in Stockholm. Ilene had taken Alison to a little circus that was in progress nearby, and I began to write as if I were addicted to that activity. On reflection I think I was addicted, odd as that sounds. If you write nearly every day, as I had been doing for most of the past three years, you get so attuned to the activity that you don't want to do anything else. I managed to refrain from further writing until I got to Woodstock, but when we got there I could refrain no longer. I started to write as if my life depended upon it. Ilene was eager to explore Oxford and visit the colleges, but I wanted to put off this visit until later. I succeeded in doing so, but in the mean time my behavior was the source of some embarrassment. The occasion for this was a dinner party given by Jean Austin, the widow of J.L. Austin, who had died three years before. It was a very small party: aside from Ilene and me, the other guests were Peter Strawson and his wife, Grace. Strawson, knowing that we had been in Oxford only a short time, asked me if we had had time to see the Oxford colleges. I said that we hadn't seen them yet, but that we had seen many of the colleges in Cambridge. To this Ilene added with a perverse smile, "And Bruce said, 'Once you have seen one English college, you have seen them all.'" Strawson, also smiling, just said "Indeed." I think I had said something like what Ilene said when she pressed me to spend some time touring Oxford. I didn't really believe what I said, but Ilene thought I deserved a little chastising nevertheless. I guess she was right.

In 1968 Andy Warhol made his famous prediction, "in the future everyone will be world famous for fifteen minutes." I arrived in Oxford five years before this, but I was treated as if I were already famous in the world of analytic philosophy. (The treatment lasted for more than fifteen minutes, but not nearly as long as a young man would wish.) Either before I left the United States or shortly after I arrived in Oxford (I can no longer remember which), I was invited to join the Common Room at Corpus Christi College, the college where my friend Jeff

Sicha was a student. He had probably spoken of me to William Kneale, his dissertation supervisor, but Kneale became, in any case, something of a sponsor of me. Kneale, joint author with his wife, Martha, of the remarkable treatise, *The Development of Logic*, was an extraordinarily learned man; I had never met anyone who knew so much about so many things. He seemed to know all the languages a philosopher should ideally know—Greek, Latin, French, German, Italian, and possibly even Spanish—and he was well at home with history, mathematics, physics, and even astronomy. (The first time I visited him at home, he had a telescope set up to capture a special view of Jupiter.) In spite of his extraordinary learning he was modest, kindly man who went out of his way to make his often-*gauche* American visitor feel confident and at ease. It was a great privilege to share his company.

Just a week or so after the Austin dinner party I was invited to give a paper at the Dons' Club, a group consisting of philosophy dons from the various Oxford colleges. It was quite a large group. Looking at all the faces before me, I was quickly convinced that Oxford contained more philosophers than any other university in the English-speaking world. (At the time about sixty philosophers were in residence there.) The paper I read to the group was the one I was writing in Stockholm. I eventually sent it to Max Black of Cornell University, who had solicited papers from younger American philosophers for a volume Allen and Unwin would publish as *Philosophy in America*.<sup>59</sup> Black chose fourteen papers from the ones submitted, and my paper, "On the Complexity of Avowals," was one of the papers he selected. The target of that paper was a group of arguments by Wittgenstein and Norman Malcolm that were widely discussed at the time. They concerned the supposed "criteria" for being in pain, for understanding talk about pain, and for using the word "pain" correctly in first- and other-person uses. In the course of developing my criticism of some of these arguments I drew a distinction between what a statement implies and what this or that person might imply in making that statement. This distinction was similar to one that Paul Grice was then making in his work on what he called conversational implicatures, and perhaps because of this, Grice was quite pleased by my talk. At any rate, after my talk or shortly after it, Grice invited me to take part in his "Saturday mornings," the discussions he held on Saturday mornings at Corpus Christi College.

The Saturday discussions that Grice led when I was there were a continuation of the Saturday morning discussions previously led by J.L. Austin. The meetings I attended generally had five or six discussants; I can no longer remember all the people who attended. R.M. Hare was nearly always there, but he never, as I recall, addressed a single word to me. He was not superior or rude; I think he was simply reticent or shy. I think J.O. Urmson sometimes attended; he was then a don at Corpus. And Robert Nozick, the other young American visitor, was always there. Nozick was younger than I; he had finished his Ph.D. earlier in the year at Princeton. His sponsor at Oxford, as it were, was Grice; he was attached to St. John's College as I was attached to Corpus.

I was greatly impressed by Grice's ideas, his intelligence, and his critical ability, but I gradually came to the conclusion that his way of doing philosophy

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<sup>59</sup> The book appeared in 1965. It was published in the United States by Cornell University press; my paper appeared on pp. 36-57.

was not mine. After a couple months, I gradually stopped attending the meetings. There were really two reasons for this. One was that Grice's procedure in the meetings left me seriously dissatisfied. We generally discussed recent journal articles (one was Rawls' "Justice as Fairness"), but the room lacked a blackboard and, instead of attempting to formulate clear and definite assertions about the arguments used, we discussed numerous examples in what seemed to me an indefinite and inconclusive way. We seemed, in fact, to make very generous use of the case-by-case method that John Wisdom employed in the seminar I described earlier. I found it dissatisfying. I had no justifiable philosophical objection to the procedure; I could not reasonably claim that it would not or could not bring solutions to significant problems or result in a greater understanding of significant issues. But I didn't find the procedure satisfying; I didn't enjoy it. The other reason was that I wanted to be working at my own task; I wanted to be writing. At that time of my philosophical life, I worked out my ideas on my typewriter, not in talk. Grice's rambling, leisurely, and seemingly inclusive discussions took too much time away from the work I wanted to be doing myself. Philosophy is a highly personal pursuit, at least for me, and admirable as I thought he was, Grice pursued philosophical issues in a way I simply did not find congenial.

Although I eventually dropped out of Grice's discussion group, I did attend the lectures on Logic and Conversation that he gave that year. I found them very interesting when I heard them, but I did not realize then that they would have the importance that they turned out to have. My impression at the time was that Grice was still in the process of organizing the many examples he had assembled and drawing conclusions from them. I don't remember his lectures well enough to estimate their relation to the revised William James lectures on Logic and Conversation that he eventually published in his book, *Studies in the Way of Words*,<sup>60</sup> but he dated the public lectures as 1987, which is twenty-three years later than the ones I heard. It is natural to suppose that the published lectures had undergone a good deal of development over that period of time.

In addition to my writing, which I began to pursue in a fairly single-minded way, I gave several talks at Oxford colleges and in addition to attending Grice's lectures I attended two seminars, or classes, that were given during that year. One class was by Gilbert Ryle; he called it "Early Edwardian Theories of Meaning." I liked Ryle; he had generously taken me to lunch shortly after I arrived, and I greatly enjoyed his personality: he had a great sense of humor, often acting as if he were just a rowing coach rather than a serious philosopher. I was eager to see how he would conduct his class. After a few sessions I stopped coming, though; I was not greatly interested in the theories he was discussing, and I felt it was more important for me to get on with my work. The other class, or seminar (I can't remember what it was called) was concerned with J.L. Austin's book, *How to Do Things With Words*. My memory of this class is now fairly dim, but it interested me greatly and I attended all its meetings. According to a diary I kept at the time, Richard Hare and J.O. Urmson ran the seminar, but they did not do most of the talking. Other philosophers, including Strawson and, I believe, Grice, also took part: they gave papers, and those in the audience asked questions and made remarks afterwards. The discussion was often quite lively. My impression at the time was that many of the people giving papers had long disagreed with

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<sup>60</sup> The publication date of *Studies in the Way of Words* is 1989.

Austin on various topics related to his speech acts theory, and this class gave them a venue for airing their disagreements. All the talks were interesting and well prepared; I felt fortunate to be there. The subject of speech acts had interested me since I first heard of it, and it interests me still: actual speech and thought is the basic reality when it comes to language, not “propositions”; and speech and thought is something done, or performed.<sup>61</sup> Statements, let alone propositions, are abstractions from this primary conceptual activity.

I mentioned earlier that Jeff Sicha, my former Oberlin student, was boarding with us that year. It turned out that he was a valuable resource for me. He was writing his dissertation on the philosophy of mathematics, but the approach he was taking was strongly influenced by the philosophy of Wilfrid Sellars, so it was easy for him to relate to the views I was expressing in the book I was writing, which were also strongly influenced by Sellars’ philosophy. (Jeff had in fact learned about Sellars from me when he was my student at Oberlin; and he met Sellars personally when Sellars came to Oberlin to examine him for his Honors project.) I therefore passed on to Jeff the first drafts of the chapters I was writing, and he would respond with comments, which would prompt more comments from me. Our discussion was fairly constant because he lived in the house and I was always eager to get a reaction to my latest work. As a result of this, I discussed philosophy more with Jeff that year than with anyone else. I found him to be a first-rate sounding board for the ideas I was developing.

Although I began to spend less time at the university and more time at home working on my manuscript, I continued to enjoy the hospitality of the Oxford philosophers. I was treated to memorable guest-night dinners at high table with Kneale, Strawson, Ayer, and Alasdair MacIntyre; and I lunched with L.J. Cohen at Queen’s College and, as I mentioned, with Ryle at Magdalen. I also lectured at Wadham College and enjoyed a guest-night dinner there with Martin Powell, a young mathematician who had become a personal friend. But these were (except for the lecture) mainly social events, not occasions for serious philosophical discussion. I did talk some philosophy with Ayer, with whom I had once dined at an American Philosophical Association meeting in company with Wilfrid Sellars; on that occasion I had become greatly embarrassed when I knocked over a fine bottle of wine. (I think Sellars, who then regarded me as his protégé, had introduced me to Ayer as a future visitor at Oxford.) I greatly admired Ayer as a philosophical writer, although I was then more sympathetic, as far as Oxford philosophers were concerned, with the work of Strawson. I regarded *Individuals* as the outstanding work of current Oxford philosophy, and I found Strawson, with his urbane manner and literary interests, to be a man I wished to emulate.

When I returned to Pitt in the fall of 1964, I discovered that the philosophy department had many new members. The principal additions were three transfers from the Yale philosophy department, Wilfrid Sellars, Nuel Belnap, and Jerome Schneewind, who would be followed a year later by another person from Yale, Alan Ross Anderson. Two younger philosophers, who would become close friends of mine, were also added: Richard Gale and John Robison. The location of the department had also changed. When I left for Oxford, the department was located on the ninth floor of the Cathedral of Learning; when I returned, it was

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<sup>61</sup> I discuss all this in chapter four of my new book, *An Empiricist Theory of Knowledge*.

located across the street on the third (or fourth) floor of the former Schenley Hotel. Although the Cathedral of Learning was, and is, an extraordinarily beautiful building, our new offices were much more luxurious: they were carpeted former hotel rooms in an elegant old hotel, and between some of them, as there was between my office and Nuel Belnap's office, there were semi-private bathrooms in which the office holders could even take a shower. The newer additions to the department and the departure of the remaining members of the "old" department resulted in a very lively group of philosophers. As before, the group contained a visiting fellow, this time Edmund Gettier. The younger group to which I belonged—the others were Gale, Robison, and Gettier—had a very lively time together. We regularly lunched together at a neighboring restaurant, The Clock, where we joked and argued. I remember it as a very pleasant time. My pleasure was enhanced, I suppose, by having far more pleasant lodgings than I had the year before. After just a little looking, I was able to rent a town-house apartment unit in a spanking new development on the outskirts of Pittsburgh. The development housed other couples of Ilene's and my age, and it had an attractive playground where children could play. A happier wife makes a happier husband. And I now had two children rather than one. Another daughter, Patty, was born in Oxford.

In addition to having new colleagues, the department also had a group of very able new students. Bas Van Fraassen, Paul Churchland, and J. Michael Dunn have emerged as the most accomplished of that group, but there were others who seemed equally, or almost equally, promising. Mike Dunn, Peter Woodruff, and, I believe, Bliss Cartwright had studied logic with me at Oberlin, and they followed me to Pitt only to discover that as a logic teacher I was superseded by Nuel Belnap and also, the following year, by Alan Ross Anderson, who left Yale as well. I was still very interested in logic, but I was not sufficiently accomplished at it to compete with the likes of Belnap and Anderson. I was mainly interested in philosophy of mind at this point, and I gave a graduate seminar on that subject my first year back. I used some of the material from the manuscript I had completed at Oxford, and I also used Stuart Hampshire's *Thought and Action*, which had appeared in 1959. I can no longer remember why I used Hampshire's book; I thought it was very badly written for a philosophy book: the paragraphs were far too long, and the argument seemed quite formless. In any case, after reading one chapter the students refused to go on with the book, so I had to switch to something else. I know I discussed some of Sellars' ideas as well as parts of my own new book. I must have discussed my chapter eight, "Thinking," because I can distinctly remember reading a very excellent term paper on it written by van Fraassen. Apart from Brian Skyrms, who was still in residence as a graduate student, Bas stood out from the others. I think it was the following year when Mike Dunn began to do his conspicuously admirable work in logic.

Although Sellars was on the Pitt faculty in the fall of 1964, I do not remember seeing him that trimester.<sup>62</sup> If he had been teaching then, I do not think I would have given the seminar in philosophy of mind: he would probably have done it himself. I remember him being there in the winter, because I recall our departmental discussion group meeting once at his apartment. It is odd that my

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<sup>62</sup> Pitt had a system of trimesters rather than semester or quarters. Their trimesters were as long as semesters, but they were called trimesters because the school year was divided into three of them.

memory should be so cloudy on this matter, but like Adolf Grünbaum, Sellars had an office apart from the rest of the department and we did not see him every day. When I wanted to talk to him, I would have to make an appointment, and sometimes our discussion would take place at lunch at his club (I think it was called the Pittsburgh Athletic Club), which had no connection with the University. Sellars was very conscientious in attending the departmental discussion group, which some of the other senior professors neglected. Looking back on those years, I am astonished at how much discussion took place in that department. I never experienced anything like it later on in my career.

I spoke earlier of belonging to a “younger” faculty group containing, in addition to me, John Robison, Richard Gale, and Ed Gettier, but Nuel Belnap and Jerry Schneewind were just couple years older than we were. They stood apart from our group only because they were occupied with different things. Belnap was working on his and Anderson’s System E of Entailment, which was foreign to our principal interests, and he was seriously occupied with his logic students (I have always been astonished by his success bringing his best students to the publishing stage so rapidly). Schneewind was occupied with the history of ethics and also with 19<sup>th</sup> century English literature, subjects that were foreign to most of the younger group. In 1970 he published a little but very erudite book called *Backgrounds of English Victorian Literature*, and he may have been doing some of the preliminary reading for this book in the year I am speaking of. As it happens, Victorian literature was one of my major non-philosophical interests at that time, and I do remember having conversations with him about it.

In spite of the congenial departmental activity during AY 64-65, I was very busy not just with my teaching but also with writing. Although I had a version of the book I had completed at Oxford, I wanted to get feedback on it from Sellars and other friends whose philosophical judgment I respected, and I got this feedback (and responded to it) during the year. I had correspondence with Norman Malcolm on certain matters, and I also exchanged letters with Roderick Chisholm on his most recent criteria for intentionality, a topic he continued to pursue for many years, possibly to the end of his life. I also was committed to writing articles, essays, and comments on a variety of other subjects. I wrote five articles for the *Encyclopedia of Philosophy*, of which “Thinking” and “Possibility” were the longest; I wrote a reply to Skyrms and Sosa, and a symposium comment on Reinhardt Grossman’s paper on intentionality. (When I read that comment, which was critical of ideas originally developed by Gustav Bergmann, Bergmann vigorously responded from the floor, saying that he had improved on those ideas in print. When I asked him where the improvements could be found, he replied, “Don’t ask me; I am not a peddler of my wares.” That was my first, and last, personal encounter with the formidable Bergmann, a man his students regarded as the world’s greatest philosopher.) All the writing I was doing this year took up great deal of time, and I often stayed late in my office typing away. Richard Gale, a wonderfully humorous person and a great colleague and friend, once remarked that when he passed my office at the end of the day and heard me typing inside, he always felt like turning around and going back to his office to do another page or two. Like me, he was obsessed with his writing.

Because writers these days nearly all use computers, it is worth recounting how younger writers had to proceed in the day of the typewriter. I used to do my

initial writing in a kind of longhand shorthand, a sequence of abbreviations that I simply fell into: I didn't consciously invent it. When I reached the end of a paper, or chapter, in this shorthand, I would then type up a clean copy, which I would read over and then revise. I would then type up a revision on a ditto master—that is, for those who do not remember these things, a white sheet attached to a carbon sheet in such a way that when words are typed in, a reverse carbon image of them is produced on the back of the white sheet. The latter would then be run through a machine, a “ditto machine,” that could make many copies of the original. I would usually make at least a dozen such copies to distribute to interested friends and colleagues, who, I hoped, would be willing to read them and send back critical comments. If they did so, or if, as always happened, I made changes to my own ditty copy, I would type it up again, making a revised ditto copy. This might happen several more times. Younger writers commonly did this because, unlike leading senior professors such as Adolf Grünbaum, Nick Rescher, or Wilfrid Sellars, they did not have private secretaries who would retype their papers for them. Younger writers could sometimes have their papers retyped by department secretaries, but those secretaries did not have the time to do this more than once. Sometimes secretary pools were available for further typing, but not always. Younger writers generally had to be their own typists, a very time-consuming job for those who wrote many papers or even occasional books. How different all this was from today's practice, when the earliest version of a document can be typed into a computer and then modified again and again without retyping the whole document. Today's younger writers have an enormously less laborious task when they produce an academic paper.

Since I am on the topic of how manuscripts used to be created, I might mention the succeeding stage of manuscript production. This was the stage created by the invention of the Xerox machine. When I became a Fellow at the Center for Advanced Study in the Behavioral Sciences, I was assigned a secretary to help me with my work, but the Center had a roomful of large Xerox machines, which any Fellow could use. Instead of having a secretary continue to type up revisions of my papers, I began to make my revisions on separate sheets of paper, which I would then (if necessary) cut out and paste onto the earlier version, sometimes adding extra pages in the process. I would then make a Xerox copy of the pages with the pasted-on revisions, and so have a clean copy in just a few minutes. In the group of Fellows to which I belonged, I was the only one to work this way. The result was that I had no work for my assigned secretary to do. She spent all her time with the work of other Fellows. By that time I was thus in little need of secretaries. When I got my first computer, a Kay-Pro, I had no need of secretaries at all. That was a permanent change in my writing life. It seemed almost miraculous.

An important change in my academic life occurred in 1964-65. I think it was in the spring semester that I was given tenure at Pitt and promoted to Associate Professor. My salary improved a little, but otherwise my life in the philosophy department was basically unchanged. When I was an assistant professor, I had very little say in departmental affairs. The department was very actively engaged in recruiting new faculty, but no one asked my opinion about what faculty to pursue, and no one asked my approval when choices were being considered. In fact, I usually had no knowledge of what appointments were under considera-

tion. One exception to this was a day in 1963 when Kurt Baier told me that Wilfrid Sellars would be visiting for four days and that the department was trying to hire him. By "the department" Baier really meant "the Executive Committee," for it was this committee, composed then of the three full professors, Baier, Grünbaum, and Rescher, that made the decisions. In due course Sellars was appointed, but I learned of this, as it happened, from a graduate student who worked for one of the full professors. John Robison called these department leaders "the big fellas" and he called the rest of us "the little fellas." The big fellas were very kind to the rest of us; I even felt a little pampered. But the big fellas were very authoritarian: they made all the important departmental decisions. When I was promoted to associate professor I thought I would be at last consulted on prospective departmental decisions, but I was mistaken. My academic life remained essentially the same. I was not actually unhappy about this, because I was not eager to spend time on administrative affairs. But I still was still a little unhappy about Pittsburgh, not finding it a pleasant place in which to live and raise a family.

Sometime during the year of my promotion I got a letter from Richard Brandt, then Chair of Philosophy at the University of Michigan, inviting me to teach a course in his department during the next summer session. He said that it was the practice of his department to bring in visitors for the summer session and that he was inviting Keith Donnellan of Cornell University to visit as well. He thought that the two of us would enjoy getting to know one another and also enjoy interacting with the regular Michigan philosophers who would be there that summer. I accepted with pleasure. I thought it would be interesting to visit Ann Arbor and good to be away from Pittsburgh during the hot months. When the spring trimester ended at Pitt, Ilene and I therefore sublet our condo apartment, packed up our car, and drove to Ann Arbor. Brandt had arranged an apartment rental for us, the top floor of a duplex on Olive Street close to the university campus, and we moved in right away. It was in that rental apartment with its screened in back porch that I completed my revision of the book I had been working on and sent it off to Random House. It was not until the fall, when I was visiting the University of Minnesota, that I got the good news that they were willing to publish it. But it took another year for me to revise the manuscript again, to correct the proofs, make the index, and submit the corrected proofs and index to the publisher. The occasion for the further revision was the complexity that had again crept into my language. Vere Chappell, who was then the philosophy editor for Random House, may have alerted me to this, but I had become convinced in any case that my style of writing had become excessively convoluted. The only way to put it right, I thought, was to go through the whole thing again, sentence by sentence, and simplify the language. This was a big job, because the book contained nine long chapters and was about 450 pages of small (12 point) type. Also, I had to retype the whole thing again. *Knowledge, Mind, and Nature* turned out to be the longest, most difficult book I would ever write.

The Michigan philosophers proved to be a congenial group, but I didn't actually see much of them that summer. I remember a very pleasant picnic where the department and visitors played softball before the meal was eaten, and where I later talked a little philosophy both with Bill Alston, who was still teaching there, and with Carl Ginet and his wife, Sally, whom I had come to know at

Oberlin philosophy colloquia. The philosopher I saw most of that summer was Keith Donnellan. As visitors and teachers in the same building, we naturally gravitated toward one another. Keith was still working out the ideas he would express in his well-known paper on definite descriptions,<sup>63</sup> and I can remember him trying out some of those ideas on me. At the time I thought he regarded his ideas as incompatible with Russell's well-known views on definite descriptions, but I now think that, whether Donnellan actually thought this or not, a definite incompatibility is very hard to sustain. Donnellan was certainly right in claiming that one can use a definite description to single out an object that does not satisfy that description (as Tom might use "the man over there drinking a Martini" to single out a man who is actually drinking water out of a Martini glass) but this claim is not incompatible with the contention, which I think Russell actually made, that the literal import of a sentence containing this description concerns a Martini drinker. The *speaker* may refer to a water drinker, but *the sentence the speaker utters* literally ascribes something to a (supposed) Martini drinker.

I can no longer remember if I told Wilfrid Sellars that I would be teaching at Michigan that summer, but he must have known that I would be there, because his father, who was then eighty-five, called me and invited me to lunch. Sellars *père* (as Wilfrid once described him) was a highly distinguished professor *emeritus* at Michigan who still lived in Ann Arbor, where Wilfrid grew up.<sup>64</sup> Having read Wilfrid's paper, "Physical Realism,"<sup>65</sup> in which he compares his philosophy with that of his father, I knew some basic facts about Roy Wood's career, and I was therefore very interested in meeting him—all the more so, because he was Wilfrid's father. But he did seem terribly old to me (I was then thirty-one), and when he began discussing his philosophical rivals (who flourished in the 1930's) and the pros and cons of their views, I thought he was immersed in an intellectual world located far in the past. (He spoke of Strong, Holt, and Santayana as if they were still engaged in philosophical debate.) He then told me a little story, which amused me then but does not amuse me now. He said that just a few years before he was planning a trip to the western United States, California or perhaps the state of Washington, and, in the interest of earning a little money to help defray the costs of the his trip, he wrote ahead to some philosophy departments, offering to give a lecture on a topic that he thought would interest them. To his utter surprise, he said, the people he wrote to seemed to have had no knowledge of him or his work, and his offers were uniformly refused. I now empathize with his situation, for I share it myself. The work that people remember me for (if they remember me at all) was published nearly thirty years ago. It seems "only yesterday" to me, but to a person in his or her early thirties, it is ancient history. A fact every philosopher must sooner or later accept is that our philosophical work is almost invariably ephemeral. We are, in effect, with very few exceptions, here today and gone tomorrow.

Sometime during the previous fall I had been invited to replace Grover Maxwell who was going on sabbatical at Minnesota; as his replacement I was appointed Visiting Professor of Philosophy at the university and Visiting Re-

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<sup>63</sup> Donnellan, Keith, "Reference and Definite Descriptions," *Philosophical Review*, 75 (1966), 281–304.

<sup>64</sup> See Andrew Chrucky's bibliography of R.W. Sellars' work; it contains 134 entries of which 14 are books. The Internet address of this bibliography is <http://www.ditext.com/rwsellars/bib-rws.html>.

<sup>65</sup> Reprinted in Sellars' *Philosophical Perspectives* (Springfield, IL: Charles Thomas, 1967), pp. 185–208.

search Professor at Feigl's Minnesota Center for the Philosophy of Science. (Temporary as it was, this was my first appointment as a full professor.) We therefore went directly to Minnesota from Michigan and took up residence in Grover's pleasant house in south Minneapolis. Great elms still tented the streets there, and when the leaves fell in late fall, they were still raked into rows along the curbs and burned. I can still remember the wonderful smell of the burning leaves; it was the last time I would experience that smell. Burning fall leaves was soon prohibited everywhere.

I taught just one course in the Minnesota philosophy department that quarter; it was on the philosophy of Wilfrid Sellars. (When I told Sellars this, he was delighted at the news.) The rest of the time I worked at Feigl's Center, having discussions and carrying on with my written work. Also I gave a series of five special lectures on my work in progress at the university, sponsored by the Center. Apart from my academic work, I spent a wonderful fall with my Minnesota family (my mother, oldest brother, and my oldest sister) doing something I had never done before or done since: learning to use a shotgun and hunting pheasants and ducks. Minnesota men, at least at that time, liked to do that sort of thing, and I, having spent some years in the solitary life of a writer, was eager to try it. I wouldn't do it again. Killing animals (even birds) for sport is not something I now approve of. I wouldn't want to see it made illegal (I am a serious gardener and therefore a foe of voles and deer), but it is not something I have any interest in doing myself.

Sometime that fall my old friend Murray Kiteley, who had been a fellow graduate student at Minnesota and was now a professor at Smith College, visited Minneapolis and invited me to a little party in the house he was temporarily renting. In the course of the evening he asked me about the Pitt philosophy department and how I liked living in Pittsburgh. I said I thought the department was absolutely first-rate but I admitted that I didn't like the city very much. From my remarks he got the impression that I was "moveable," as we used to say: I could be induced to leave the Pittsburgh department if I received a sufficiently attractive offer. Not long after this I got a letter from Clarence Shute, Head of the philosophy department at the University of Massachusetts, saying that they were looking for a new department head and asking if I was open to the possibility of taking on the job. After some thought I said yes, and in due course I visited the university to talk with the administration and members of the philosophy faculty.

Although the University of Massachusetts had a small, young, and generally undistinguished philosophy faculty, I had two basic reasons for considering a job there. One concerned the peculiarities of Amherst, the town where the university was located. I had a friend who once taught English at the university, and he had often praised Amherst as a very pleasant, highly civilized little town in which a person attracted to rural life would enjoy living. Murray Kiteley, in fact, assured me that the Pioneer valley, the larger area in which the university is located, is one of the "greatest places in the universe to live." Not only did I not enjoy living in Pittsburgh, but Pittsburgh was not, as I said before, the kind of city in which I wanted to raise my two daughters. Also, it was not a congenial place for my wife, Ilene. She had enjoyed living in Oberlin, finding many friends with compatible interests and values in the college community, but such people were

in short supply in Pittsburgh, at least in the neighborhoods where we lived. She had also enjoyed living in Woodstock, the village outside of Oxford, where she found highly congenial neighbors and friends. I myself regarded Woodstock as being, if I were English, an ideal place for me: a small, beautiful rural town with many highly educated, secular people and close to a university with all the amenities a good university possesses. That is the sort of place where I wanted to live and raise my children. Amherst, particularly after I saw it and learned about its outstanding schools and the kind of people living there, seemed to come close to the ideal I had. The university was not very distinguished, at least as yet, but it was developing at a remarkable pace. The total situation in Amherst was well worth considering.

The other reason I had for considering a job at UMass (the nickname the university now possesses) is that I would need a significant jump in pay if I were to be able to buy a satisfactory house for our immediate family. The Pitt philosophy department had so many people senior to me, I reasoned, that it would be many years before I could expect a promotion to full professor. By contrast, such a promotion would not be far in the future if I went to UMass. I didn't discuss these ideas with anyone at Pitt, and I may not have discussed them with the philosophy people I knew at Minnesota. But I did proceed to visit UMass, and I was very highly impressed both with the officials at the university and with the amenities of the town. I was particularly pleased by the New England rusticity I saw, or felt, almost everywhere. It was writ large in the small faculty club building, where I was put up during my visit. I occupied the only bedroom in this building, known as Stockbridge House, which had been built in 1731 and had been updated, apparently, only by the addition of electricity, running water, and a functioning toilet.

The president of the university was John Lederle, a vigorous man in his sixties who was assisted by Oswald Tippo, a very exceptional provost who piled his files on his office floor and seemed to know the intimate details of every department on the campus, and Moyer Hunsberger, a highly ambitious, hard-working Dean of Arts and Sciences. The university was growing at an astonishing rate, and Lederle, Tippo, and Hunsberger were assembling an impressive group of newer faculty. They were interested, above all, in recruiting a distinguished faculty, and they were interested in me as a potential Department Head, I was told, mainly because they were unable to recruit a sufficiently distinguished older person. This was a time when money was flowing and professorial positions were becoming available at a greater rate than professors were being produced. If an outstanding professor were offered a new job, his or her employer would commonly meet the offer, and thus not be tempted away. Moyer Hunsberger, the intense dean who recruited me, having failed to hire a senior professor with the right credentials, decided to seek a promising younger person who would grow into the job—who would become the kind of department head they were looking for. He thought I was the kind of younger person he was after, and he wasted no time pursuing me. After I visited the UMass campus, he flew to Minneapolis to make me an offer. I decided to accept. I told Kurt Baier, the department chairman at Pitt, what I proposed to do; I didn't attempt to solicit a counter offer.

As it happened, a suitable counter-offer was really out of the question because of a further offer I received from Ohio State University. Robert Turnbull, a

senior professor in the philosophy department there, was a friend and former student of Wilfrid Sellars, and when he heard (probably from Wilfrid) that I was contemplating a move to Massachusetts, he persuaded Everett Nelson, the philosophy chairman there, to offer me a full professorship right away. Nelson did so, and Hunsberger thereupon offered me a full professorship as well. Sellars, Baier, Adolf Grünbaum, and even Jerry Schneewind thought it would be a mistake for me to take the UMass job, and they urged me to find an alternative. There was no thought of offering me a full professorship at Pittsburgh; they had awarded me tenure and raised me to associate professor less than a year before, and they could not think of vaulting me ahead of people (such as Nuel Belnap and Jerry Schneewind) who were well ahead of me in the professorial pipeline. Besides, they knew that I wanted to leave the town of Pittsburgh. Schneewind suggested that if I were dead set on moving to a smaller, more rural town, a job could probably be arranged at North Carolina (the chairman there was David Faulk, a man I knew who was also a friend of his, Kurt's, and Sellars'), but I thought UMass was the place for me. So I accepted Hunsberger's offer. I became the youngest full professor and department head at the university. When I accepted the offer, I was still thirty-two.

Looking back on the decision I made so many years ago, I can see that it was professionally a mistake but right for the larger purposes of my life. As I write this memoir, UMass is still struggling for academic distinction and still opposed by stubborn political forces. The philosophy department, which I was instrumental in developing, has had its ups and downs and has achieved some distinction, but owing to personal rivalries and doctrinal dissent, it was never for me, after the first year or so, a very rewarding unit to be part of. Life in Amherst, on the other hand, has been everything I hoped it would be. My wife enjoyed living there, and my three daughters thrived: they had excellent teachers, enlightened friends, and grew up with the secular, humanistic values I wanted them to have. (Two became artists, one also a professor; and the other a biologist who is now a laboratory supervisor at a superior university.) But these are matters I will address in a later part of this memoir.

One thing I should say more about here is my attitude to the department I was prepared to leave. I said that I told Murray Kiteley that I thought it was absolutely first-rate. I did think this, and I knew I would probably never belong to another department of equal distinction. But my day-by-day association with the people on whom the reputation of the department depended was never very close, and belonging to a distinguished group has never been important to my self-esteem. I am by nature a loner, and the same was actually true of such people as Sellars, Adolf Grünbaum, or Nicholas Rescher. (I am not sure what was true of Kurt Baier: he was an extraordinarily fine person, considerate, warm-hearted, and kind, but he was remote in a way that I never fully fathomed.) These first three achievers spent most of their lives by themselves, working: you don't accomplish as much as they did (and do: Grünbaum and Rescher are still with us) by standing around having conversations. I was the same way: I spent most of my time alone, reading or writing. Since I was more strongly influenced by Sellars' work than by that of anyone else, I was naturally reluctant to move away from him; but he was usually busy, and talking to him personally was generally not more helpful than writing to him. In fact, writing to him was much

more helpful. As his surviving papers indicate, philosophical correspondence was an important part of his life. Because of this, I could exchange ideas with him whether we lived in the same town or not. And this is the way our relationship turned out. We had a long correspondence on practical reasoning that spanned nearly twenty years, and we had a number of other exchanges. Some of this material is now available on the Internet.<sup>66</sup>

There were of course other people at Pitt whom I would miss talking to, but I didn't feel dependent on their conversation. I had just spent several leaves away from the Pitt department—at Oxford, Michigan, and Minnesota—and I didn't actually miss anyone when I was gone. It would turn out that I would never again have the kind of philosophical conversations or friendships that I had at Pitt, but I did not know this then. And I didn't realize then how valuable a strongly supportive department actually is. I was really quite innocent of the academic world.

In view of the great interest in things Sellarsian among a growing group of today's philosophers, I want to say a little more about Sellars' fondness for philosophical correspondence. Unlike some leading philosophers, Sellars was not primarily interested in merely expounding his views to his students, colleagues, or intimates; he was interested in seeing how his ideas were received and, in the face of resistance or objections, using the occasion to "improve the formulation," as he often put it, and trying to reach general agreement. When I discussed philosophy with him in the early sixties and even later, I felt that I was engaging in a serious dialogue in which I could freely raise difficulties, ask for clarification, and move things along. There was never a significant problem of not understanding one another's meaning, because we could quickly clear up this kind of problem by further dialogue.<sup>67</sup> Sellars' practice of writing philosophical letters and responding to the answers they occasioned has long impressed me as an indispensable way of making progress in philosophy, and I, like him, have sometimes displayed a striking *naïveté* in carrying it on. Sellars was always convinced that if he succeeded in formulating his ideas in the right way, he could convince a fair-minded correspondent interesting in finding the truth. His occasional *naïveté* in correspondence, which nevertheless shows, I think, his high-minded philosophical integrity, is worth illustrating, because it tells us a lot about what the man was really like.

One time, in the 70's, I think, the philosophers of SUNY at Albany organized a mini conference in which Sellars and Chisholm would give papers on the subject of intentionality. The two of them, as I noted earlier,<sup>68</sup> had once had a correspondence on intentionality that became quite well known, and the Albany philosophers thought it would be interesting to see the two of them discuss the

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<sup>66</sup> See footnote 46.

<sup>67</sup> In recent years some of the newer, younger Sellarsians have complained that I didn't or do not now really understand Sellars on certain matters, or understand him very well. I find this somewhat astonishing, and I am quite sure that Sellars did not share this opinion. In fact, he expressed a contrary opinion quite clearly in a letter to me dated July 3, 1973, which is available on the internet. See [www.ditext.com/sellars/csa.html](http://www.ditext.com/sellars/csa.html), where he said, "You have long been one of the very few on whom I can count for understanding criticism. You are at home -- if not always comfortably -- in the dialectic, and can quickly spot questionable moves. This is why, in the few cases in which you have not convinced me, I keep on trying to convince you."

<sup>68</sup> See footnote 28 above.

topic again. Together with my colleague Herbert Heidelberger (who had once studied with Sellars at Minnesota) and several UMass graduate students, I traveled out to Albany to see the show. Sellars, hoping to engage Chisholm, began with long and complicated paper setting forth his most recent thinking on the subject. Chisholm, when it was his time to speak, gave a shorter paper containing a number of definitions that, taken together, were intended to provide a sufficient condition (or perhaps a necessary one—I can no longer remember which) for a proposition with intentional content. His paper contained no reference to Sellars, and in the question period members of the audience pretty much ignored Sellars as well, concentrating on finding counter-instances to Chisholm's definitions rather than on the general subject of intentionality. I confess that I was one of the people offering a counter instance to one of Chisholm's definitions. Doing this was a lot easier than addressing Sellars' complicated paper.

About a week after the conference I got a telephone call from Sellars. I was at my office at UMass and Sellars was at the University of Arizona, where he regularly visited in the winter. Sellars was upset about the way Chisholm had responded to his paper at the Albany conference. "The trip from Tucson to Albany was long and awkward," Sellars said, "but I went through with it because I wanted to get Rod's [Roderick Chisholm's] reaction to my latest ideas on intentionality. Yet Rod paid no attention to what I said at all. It was terribly disappointing." I am not sure what Sellars hoped I would say, but my response was essentially this: "I know it was disappointing for you, Wilfrid, and I know you came here thinking that if you could express your ideas in the right way, you could bring him around to your way of thinking. But I think you have to accept the fact that he would rather die than agree with you on intentionality or any other subject of importance to him. He sees you as a rival, and he has absolutely no interest in finding agreement with you on any philosophical issue.<sup>69</sup> If you want a meeting of the minds on intentionality, you should forget about Chisholm and find someone else." Sellars seemed astounded at what I said. It never occurred to him that Chisholm was not interested in what he had to say about a philosophical subject.<sup>70</sup> I think he accepted my assessment, because after that he never, to my knowledge, discussed anything with Chisholm again.

As I noted earlier, Wilfrid's father, Roy Wood Sellars, was a distinguished philosopher in his day. Like Wilfrid he viewed philosophy as a lofty subject that should be pursued selflessly—with an eye on the truth rather than on personal victory or accomplishment. I have to say that I view it their way, too: after all, I was Wilfrid's student. But I suspect that this way of viewing philosophy—or viewing one's own work on the subject—is not widely shared. Carnap shared it, and so, I believe, did Feigl and Hempel. But many philosophers seem to view it differently. It is only recently that I have come to suspect (how naïve could I have been all these years?) that most philosophers, instead of seeking philosophical truth and basing their philosophical convictions on the *results* of their inquiries, seek ways of vindicating beliefs they already have. Such philosophers are not

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<sup>69</sup> In reporting this anecdote, I don't mean to imply or even suggest that Chisholm was never willing to respond positively to criticism. In fact, he encouraged his students and friends to respond critically to the views (usually definitions) that he expressed in papers and talks. As far as I know, it was only rivals (or those critical of his philosophical strategy) that provoked his hostility or indifference.

<sup>70</sup> At that time I was unaware of the opinion Chisholm would express years later at the Dartmouth dinner. See footnote 28 above.

particularly interested in what other philosophers think; in fact, they commonly view rival philosophers with contempt or something like it. They rarely, if ever, read the work of their rivals, and they commonly discourage their students from reading it, too. No wonder that philosophy departments are so often sites (as it were) of religious wars. Each faction wants its ideas to prevail and has no interest in any meeting of rival minds.

Sellars was not just different in wanting fruitful discussion with other philosophers; he also wanted to know what they and other thinkers thought about most important subjects. As he said in "Philosophy and the Scientific Image of Man,"

the aim of philosophy, abstractly formulated, is to understand how things in the broadest possible sense of the term hang together in the broadest possible sense of the term. Under things [so understood]...I include not only "cabbages and kings," but numbers and duties, possibilities and finger snaps, aesthetic experience and death. To achieve [ideal] success in philosophy would be..."to know one's way around" with respect to all these things...in that reflective way which means that no intellectual holds are barred (p. 1).

In expanding upon this conception of philosophy he emphasized that one should ideally know one's way around the special disciplines such as mathematics, physics, and theoretical psychology; in addition, as he emphasized to me, one should have a good idea of what other philosophers are doing, and this includes such forbidden figures (for typical analytic philosophers) as Martin Heidegger. I am sure he would have agreed that if, in reading a given philosopher, you see that he or she is clearly taking nonsense, you don't have to keep slogging on until you reach the bitter end. But you ought ideally to read enough to know what is going on—at least if the philosopher is influential or has had a flock of followers. I have long agreed with Sellars on this matter, but I have always kept in mind the qualification "ideally." It is physically impossible to read every philosopher of note (I have never succeeded in reading Heidegger), and it is also impossible to become knowledgeable in every respectable field of knowledge or informed opinion. The most we can reasonably do is to try to become widely informed in philosophy and the related sciences, and do the reading—the work—that that purpose requires.

I want to add a final comment about philosophical correspondence. I said earlier that Sellars' practice of writing philosophical letters and responding to the answers they occasioned has long impressed me as an indispensable way of moving philosophy along. But I now wonder if this impression is actually correct. Have I ever changed another philosopher's views by my correspondence or have my views been changed by the correspondence of another philosopher? I know that I have had my views changed in small ways, but I am not certain about other philosophers. I had a very long correspondence with Sellars on the logic of practical reasoning, and although I was critical of his views on the subject, I was never able to convince him that I was right. His views did change in certain ways as a result of the correspondence, and so did mine, but these changes were not the result of persuasion by the other correspondent. On the other hand, persuasion by the other need not, on reflection, be required for philosophical progress. I know that the activity of responding to criticisms and at-

tempting to persuade another that you are right has greatly improved my understanding of certain subjects, and the adjustments another may make in response to what I say may amount to philosophical progress for him or her. So philosophy can clearly move along without one or the other party being persuaded by the other. Philosophical egos are very delicate, and the experience of being persuaded by another philosopher may feel too much like the experience of being vanquished in a dispute, which is often hard to bear even for a philosopher who, like me, is genuinely bored and even repelled by the omnipresent demands and displays of philosophical egos. But considerate, thoughtful correspondence with a willing correspondent still seems one of the best ways to improve one's philosophical ideas.

When I returned to Pitt from Minnesota in the spring semester of AY 1965-66, I encountered a philosophy department with even more new members. Alan W. Anderson was now there, and so was Joseph Kocklemans, a specialist in phenomenology and existentialism, and an esthetician, Francis Coleman. James Cornman, then a member of the philosophy department at the University of Rochester, was present as a visiting fellow, and so was Charles Chihara, who was visiting from UCal Berkeley. To me, the department was still an intellectually exciting place, even though I had decided to leave it and strike out on my own.

Partly because he used an office opposite mine, James Cornman and I struck up a friendship, which persisted until his tragic death a dozen years later. Jim had a very unusual way of writing: he would sit in his office with his door open and work out his ideas with, as I seem to recall, pencil and paper. What astonished me was his ability to concentrate on his writing while people passed his office, some waving or just saying hello. Jim did have one alarming peculiarity, however: he had a terrible temper. "Terrible" is the right word here, even if "terrible temper" is an obvious *cliché*. Jim was a large, powerful man who had been a football player in college. When he became angry with you, he would come right at you, and he was frightening if you didn't know him. I remember one time when he and I were in Alan Anderson's office and Alan made some pejorative remark about the material conditional. Jim said "What do you mean?" and appeared so angry that Alan ran behind his desk, apparently quite fearful of what Jim might do. Jim was actually a kind man who wouldn't deliberately hurt anyone, at least any good person. (I don't know what he would have done with a mugger.) He was often embarrassed by his temper, as I know from an instance that took place some years later when he had moved on to the University of Pennsylvania and I was there to give a lecture. My family and Jim's became very friendly and took trips to visit each other. He was a very good, clearheaded philosopher and a very good man to know. I had many rewarding conversations with him. I reviewed his book, *Materialism and Sensations*, and although I made some serious criticisms of the position he defended, he replied in a genial way, saying that he thought the review was perceptive and fair. We could differ without rancor, something many philosophers find it very difficult to do.

I have a very indistinct memory of the two trimesters I spent at Pitt that year, one in the winter and the other in the summer. Neither was memorable. In one I taught a seminar on thought and action, a topic I would take up in my third book. In the other I discussed theories of and controversies about truth. I can recall few details about either seminar, except that Jay Rosenberg attended the sec-

ond one. Along with a group of other students, he had been to my house for a cook-out: I remember that I put a chicken on the rotisserie and that cooked so slowly that the students felt starved before it was served. Jay, who loved food, probably complained the most about the delay. Apart from these few details, the year is mostly a blank in my memory. I know I was very busy the whole time. I was still having my short period of fame, so I was often out of town giving papers at conferences or philosophy clubs. I was also still at work writing articles for the *Encyclopedia of Philosophy* (I had committed myself to doing six of them) and rewriting the manuscript of my book. When you are busy like this, time goes very fast: you can slow it down, in my experience, as you are eager to do late in life, only by deliberately doing things that make you bored. Almost before I knew it, then, I was getting ready for my big move to New England. I will talk about this in my next part of this memoir.

