

Sisyphian task of teasing a theory of meaning out of Tarski's theory of truth."

Quine was to receive an honorary degree from Oxford that June, but I had to leave for Israel before Encaenia. Nor long afterwards, however, Quine and his family met me and my family in Asmara, which was then in Ethiopia. After a trip to torrid Massawa on the narrow gauge railroad the Italians had built, we planned to fly to Aksum. When it turned out that our flight had been canceled, we suggested they drive us there, but they protested it was dangerous: "Bandits", we were told. We persisted, they shrugged and produced a small van. Half way to Aksum the "bandits", who certainly were well armed, stopped us, inspected our passports, and wished us well. Eritrea was then still part of Ethiopia; the "bandits" were separatists, and their war was not with us. We spent a good part of the summer touring more of Ethiopia, and then Kenya, Tanzania, and Uganda, with an illegal sortie into what was then Zaire. We drove ourselves through and between the game parks. One evening after dinner my wife, daughter, and I had an encounter on foot with a noisy and irritated elephant, and on another occasion we were threatened by a spear-bearing Masai warrior who was offended because we tried to photograph him without paying the usual ransom; we had no serious trouble.

During my year as chairman at Princeton I had received an offer from the Rockefeller University in New York. The position seemed too good to be true: no teaching unless one elected to do it, in which case the university would purchase the graduate students you wanted, no departmental chores because there was no department (each full professor had his own "laboratory"), total freedom to travel or teach elsewhere. I quizzed two philosophers who had left this Eden, Sidney Shoemaker and Bob Nozick. What, I asked, made you want to leave? Sidney plausibly mentioned the difficulty of raising a child in Manhattan; Nozick had felt deprived of the company of other humanists and people in the social sciences. I knew that Ernest Nagel had returned to Columbia after a very short stay. I consulted my conscience, which told me it was unfair to leave Princeton after only three years (one of which was to be at the Center in Stanford), and my imagination, which told me that I could stand the strain of a position where the only demands were inner. The decision was eased by Princeton's generous response. The department and administration (and board of trustees) offered to create a new permanent position called "lecturer with the rank of professor", with the understanding that I would teach a course or seminar each year, and direct dissertations if asked. I gratefully accepted, and in September of 1970 I moved with my family to New York. The house we rented in the Village was owned by Leonard Boudin, the splendid constitutional lawyer. Elizabeth slept in the bedroom of Kathy Boudin who was at the time on the FBI's most-wanted list.

My new senior colleagues at Rockefeller University were Harry Frankfurt,

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who had been there almost from the start, and had been the main agent in the formation of the philosophy group, Joel Feinberg, and Saul Kripke. There were several non-tenured people, including my ex-students John Dolan and John Wallace. It was good company, and I enjoyed the freedom to pursue what I pleased at my own pace. The small amount of tutoring at Rockefeller, the teaching at Princeton, and a fair amount of travel to lecture satisfied my need to stay in touch with students and to exchange views with other philosophers. A group at Rockefeller which included people from physics, logic, biology, mathematics, and other disciplines met regularly, and as in my graduate student days, I enjoyed the contact with serious science. I had a few talks with Saul Kripke. His was the quickest philosophical mind I have ever encountered. I don't now remember what problem I presented to him one day, but I do remember it was something I had been puzzling over for more than a year. In about fifteen minutes Saul thought of just about every promising solution I had entertained and almost as quickly saw the objections to each. I wasn't too discouraged. I have never thought one had to be particularly brilliant to do good philosophy (not that it hurts). Michael Bratman, Scott Weinstein, Norton Batkin, Jonathan Lear, and Howard Burdick were among the students with whom I worked.

In January of 1971 I was invited to give two lectures at University College, London, and I lectured on the topic I had touched on in my final John Locke Lecture: the notion of conceptual relativism. I had been persuaded by Wilfrid Sellars of the absurdity of an unconceptualized "given", and by Quine of the impossibility of cleanly separating the aspect of our thinking that constitutes the conceptual framework from its empirical content. Quine had made the point by attacking Carnap's distinction between a framework or language which we can choose on pragmatic grounds, and the science that then fills the framework with empirical matter. Another example of the dualism of scheme and content could be found in C.I. Lewis's neo-Kantian *Mind and the World Order*. These variously described dualisms had a common origin in Kant, who clearly had the distinction though he thought there could be only one conceptual scheme. What I argued against was not just the idea of an unconceptualized given which might serve as the foundation of empirical knowledge. If one agreed with Kant that "Thoughts without content are empty, intuitions without concepts are blind", one could not conceive of intuitions as occurring separately and so serving as a basis for empirical knowledge. But in saying this, one would still be assuming that it is possible to distinguish between the conceptual element and the experiential element in thought, and it is the distinction itself I questioned. Without the distinction, conceptual relativism is a position that can't be formulated, and the idea of thought as "representing" reality becomes a misleading metaphor.

When I was at the Center for Advanced Study in Stanford, I had, along

with Gil Harman, organized a small conference on the semantics of natural language. About half the participants were linguists and half were philosophers, some of whom were also logicians: Dana Scott, Richard Montague, Jaakko Hintikka, Pat Suppes, and Quine. A book containing many of the papers read at the conference resulted (Davidson and Harman 1971). It also contained some new material by Paul Ziff, John R. Ross, Jim McCawley, and Saul Kripke's "Naming and Necessity". In the introduction Harman and I pointed out that the convergence of linguists and philosophers on the application of formal methods to the semantics of natural language which I had predicted in "Truth and Meaning" was actually coming to pass. "The purpose of the volume", we wrote, "is the same as that of the conference: to encourage the active exchange of ideas among logicians, philosophers, and linguists who are working on semantics for natural languages. We trust it will be agreed that there is more to this than the usual business of rubbing two or more disciplines together in the expectation of heat and the hope of light."

I had been one of the original group of philosophers who, at the instigation of Stuart Hampshire and Gregory Vlastos, had formed the Council for Philosophical Studies. The idea was primarily to provide a body that could encourage foundations and government agencies to support research and other enterprises in philosophy. One good thing the Council did was supply funds to enable the American Philosophical Association to set up a national office for the first time. The Council also attracted funds to support summer institutes for philosophers who were already teaching and doing research in a particular area. I was asked to organize such an institute, and I in turn once more asked Gil Harman to join me. The topic was again philosophy of language, and though most of the participants were philosophers, a number of linguists, most of whom had been at the conference two years before, gave courses of lectures. Many philosophers who have since become well known participated. It was an exciting six weeks at the new campus of the University of California at Irvine.

Earlier that same summer (1971), there had been a conference at Berkeley in honor of Tarski. I felt out of place among logicians. I guess I was invited because I had been promoting Tarski's treatment of truth as a powerful tool for philosophers. I gave a talk on "Coherence, Correspondence, and Convention T". It was a busy summer. After Berkeley I went to the Fourth International Congress for Logic, Methodology, and Philosophy of Science in Bucharest, my first peek behind the Iron Curtain. I had flown to Vienna with the intention of taking a Soviet ship down the Danube. I tried to buy a ticket before I left the United States, but was told to try London. In London I was told to try Vienna; in Vienna I was informed that all the spaces had been sold. This gave me an unexpected four or five days in Vienna. Remembering that

Schubert had written glorious music sitting in an open-air cafe, I decided to see if inspiration would strike me under the same circumstances. I had planned to talk without a paper at the Congress, but with time on my hands I settled at a table, ordered a coffee, and started writing. I had never before written in public, but I discovered that being observable provided a strong incentive to keep busy. Just as the books say, the waiters left me unmolested for hours, and in three days I had produced "The Material Mind". It is probably the only paper I ever wrote without footnotes. I then took the Orient Express—by now a slow, dilapidated train—to Bucharest. The sole other passenger in my compartment turned out to be reading a paper of mine. The passenger was Vincent Hope, a philosopher from Edinburgh. After Bucharest I dashed to Canterbury to a philosophy of psychology conference, and then to a week-long Balliol reading party in a chalet on a shoulder of Mt. Blanc. The party had been arranged by Tony Kenny, and after we had finished our work we had planned to climb to the top. Alas, it was not to be; bad weather. Years later, at a meeting in Rome, he told me he had subsequently reached the summit.

I spent the academic year 1973–74 as a visiting fellow at All Souls College, Oxford. It was a good year to be at Oxford: Quine was there for the whole year, and Dagfinn Føllesdal was there for much of the year. I had the opportunity to reestablish relations with people I had first met when I gave the John Locke Lectures, or when lecturing in London: John McDowell, Colin McGinn, Derek Parfit, Chris Peacocke, and Gareth Evans. I saw a good deal of Gareth, and came to admire the serious and original philosophy that emerged from his tempestuous personality. Gareth and John McDowell were determined to prove to the "Americans" that they had learned how to apply formal methods to the study of natural language, and they put together a fine volume, *Truth and Meaning*. This volume contained, along with much else of value, the long second part of Michael Dummett's "What is a Theory of Meaning?" This essay had been produced in the course of a seminar on truth that Michael and I gave during that year. The seminar was apparently viewed by the Oxford community as a sort of gladiatorial contest, and a large crowd turned out. At the first session I sketched the gist of my "Truth and Meaning". The burden of Dummett's two articles on what a theory of meaning should be were his response, and this took up most of the rest of the seminar. He contended that to specify what someone who knew a language knew by referring to a theory of truth in Tarski's style was to miss the real problem. It missed the real problem, according to Michael, because it assumed that the interpreter already had a language of his own in which he could state, if he wanted to, the truth conditions of the language he had mastered. Michael wanted an account that went much deeper: he wanted to specify in a non-circular way what behavior would count as showing that an agent commanded

the relevant concepts. Over the years that have followed, my position has developed in various ways, and we no longer are as far apart as we were then, or so it seems to me.

I had come to know Sam Guttenplan, who had helped organize a series of lectures during my year at All Souls: Føllesdal, Anscombe, Quine, Dummett, Geach, and I were the speakers. My paper was "Thought and Talk" in which I made the first of a number of attempts to provide a persuasive argument for a conclusion I was sure was true: that thought depends on language. This is a theme to which I have recurred a number of times. Sam and I played tennis on the grass courts in beautiful University Parks, and he took me to a nearby airfield where he belonged to a gliding club. Gliding was something I had always wanted to try, so I joined. I soon soloed, and spent many exhilarating hours playing with the cumulus clouds that drift over England during the summer. When I returned to the United States I added my new competence to my license for power flight. Over the years I have flown gliders in Switzerland, Australia, and various parts of the United States.

I was president of the Eastern Division of the American Philosophical Society, and this required me to return home several times, and to Atlanta in December to give the presidential address. This caused me to put into final form my criticism of conceptual relativism (Davidson 1974). Frequent trips back home suited me perfectly, for I had fallen in love with Nancy Hirshberg, who was a professor of psychology at the University of Illinois. Nancy had been an undergraduate at Stanford, and we had not lost touch during the years she was in graduate school at Stanford, and then taught, with her psychologist husband Jerry Wiggins, in Champaign. In the spring of 1972 Jerry suddenly decamped. Virginia and I had separated, to everyone's relief, shortly after I went to Rockefeller University, so Nancy and I became reacquainted. We spent a month or so in Portugal and Spain the summer of 1972, several times went skiing with my daughter Elizabeth in Aspen, and after my stint in Atlanta we spent a couple of weeks in Puerto Rico. There was a friendly divorce from Virginia, and Nancy and I were married in 1975. We were able to spend that year together, for Nancy had a visiting job at the Bell Laboratories in Murray Hill, but we needed to get permanent jobs in the same city. Serendipitously, the Rockefeller had decided to pay its philosophers to leave. I was offered a University Professorship at the University of Chicago, and Nancy moved from the University of Illinois campus in Champaign to its Chicago campus.

My new position was in many ways similar to the one I had just left. I was completely free for half of each year, and had no duties I did not choose to assume. I elected to become a voting member of the philosophy department, but from an administrative point of view I was independent of it. There was much to admire about the university and the city. The administration was committed to fostering scholarship; it is the only place I know of that would