The Intellectual Journey of an Eminent Logician-Philosopher

-- An Interview With Susan Haack

CHEN Bo, Susan Haack

ABSTRACT In 2002, at University of Miami, USA, Chen Bo made an long interview with Susan Haack who is an eminent logician-philosopher in the world. In this interview, they talked about Haack's intellectual background, and her studies in philosophy of logic, epistemology, metaphysics, pragmatism, philosophy of science, post-modern trends, especially Haack's own philosophy, e.g, logical pluralism, foundherentism, critical common-sensism, innocent realism.

KEY WORDS revisability of logic, logical pluralism, foundherentism, pragmatism, critical common-sensism, innocent realism.

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Susan Haack [hereafter SH], M.A., B.Phil. Oxford, Ph.D. Cambridge, formerly Professor of Philosophy at the University of Warwick (U.K), is currently Cooper Senior Scholar in Arts and Sciences, Professor of Philosophy, and Professor of Law at the University of Miami (U.S.A.). She is the author of Deviant Logic (Cambridge, 1974); Philosophy of Logics (Cambridge, 1978), Evidence and Inquiry: Towards Reconstruction in Epistemology (Blackwell, 1993); Deviant Logic, Fuzzy Logic: Beyond the Formalism, (Chicago, 1996); Manifesto of a Passionate Moderate: Unfashionable Essays (Chicago, 1998), and Defending Science -- Within Reason (forthcoming); and of numerous articles on philosophy of logic and language, epistemology and metaphysics, pragmatism, feminism, philosophy of science, and scientific testimony in court. Her work has been translated into Spanish, French, Portuguese, Italian, German, Polish, Russian, Danish, Korean and Chinese. A volume of essays on her work is in preparation.

Chen Bo [hereafter CB], Ph.D., China Renmin University, is Professor of Philosophy at Peking University, P.R. China. In 2002-3 he held a CCSC fellowship awarded by the American Council of Learned Societies, the Social Sciences Research Council, and the National Academy of Sciences, as a Visiting Scholar in the Department of Philosophy at the University of Miami.

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CB: Professor Haack, I'm very glad to have this opportunity to interview you. Because of your book Philosophy of Logics you are well known in logical circles in China, but we know little about you

personally. Could you tell us something about yourself?

SH: Well, let's see: I was born in England after World War II. I was educated at state primary and grammar schools; at Oxford, where I earned first my B.A. in Philosophy, Politics, and Economics, and then the B.Phil. in Philosophy (I also received the M.A., but at Oxford this is a formality); and then at Cambridge, where I earned my Ph.D. while teaching at New Hall, a Cambridge women's college. I was the first person in my family ever to go to university. Still, looking back, I think my philosophical education probably began with my (maternal) grandparents, who had little formal schooling, but entertained me by teaching me challenging card games and introducing me to the word puzzles published in the newspapers -- which I soon came to relish as much as my grandmother did; perhaps this was the seed that eventually grew into my crossword analogy for the structure of evidence.

At Oxford, where I was a student at St. Hilda's College, my first philosophy teacher was Jean Austin (widow of J. L. Austin); after that, I studied Plato with Gilbert Ryle, and logic with Michael Dummett. I wrote my B.Phil. dissertation under the supervision of David Pears; its topic, ambiguity, foreshadowed my later conviction that many important philosophical mistakes are the result of equivocations. At Cambridge, where I wrote my Ph.D. under the supervision of Timothy Smiley, I was a junior colleague of Elizabeth Anscombe, then newly appointed as Professor of philosophy, and continued my philosophical education by way of our often-heated lunch-time conversations.

After Cambridge, I taught for almost twenty years (1971-90) in the philosophy department at the University of Warwick, one of the new universities set up in Britain in the 1960s. It was at Warwick that I prepared Deviant Logic for publication, wrote Philosophy of Logics, began seriously reading the American Pragmatists, and started work on Evidence and Inquiry. I joined the department of philosophy at the University of Miami in 1990, and a couple of years later completed Evidence and Inquiry. I soon found my interests drawn in two new directions: I began work first on cultural and social issues intersecting with my work in epistemology and with pragmatism; and then on questions about the role of expert, and especially scientific, testimony in the courts. And so I wrote the essays in Manifesto of a Passionate Moderate, including two pieces on the extravagances of self-styled "cultural critics" of science that eventually led to my most recent book, Defending Science -- Within Reason; and began to teach and publish on the interactions of science with the law. These interests are reflected in my present position: as Cooper Senior Scholar I teach an interdisciplinary course each year for the College of Arts and Sciences, and as Professor of Law I teach a course on the law of scientific testimony.

While I was at Warwick I also held visiting positions in Canada, South Africa, Australia, and the

United States. In the last ten years or so, besides extensive travels in the U.S. and Canada, I have made many professional visits to Europe, especially Spain (where I was visiting professor at the University of Santiago de Compostela), and Scandinavia (where I was visiting professor at Aarhus University in Denmark); and also to Brazil. I learned in the 1980s that Lo Yi had translated Philosophy of Logics into Chinese, but I only recently discovered that this book of mine was well-known in China, that translations of several excerpts had already been published, and that Lo Yi's translation of the book is to appear with Commercial Press. Of course I am very pleased that now, with your translation of Evidence and Inquiry under way, and our collaboration as editors-in-chief of Renmin University Press's series, Contemporary Western Philosophy in Translation, I am able to communicate with colleagues in China too.

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CB: Deviant Logic, your first book (listed in Philosophy of Science, Logic and Mathematics in the 20th Century in the Routledge History of Philosophy, Vol.IX, 1996), was published in 1974, and reprinted in a new, enlarged edition in 1996. What do you think were the most important ideas presented in this book?

SH: I would say: my articulation of the distinction between deviant and extended logics; my defense of the idea that logic is revisable; and my detailed studies of the motivation for some proposed revisions of "classical" logic, the two-valued, unified propositional and predicate calculus we inherited from Frege, Peirce, Russell, etc.: logics of vagueness, free logics, three-valued logics for future contingents, Intuitionist logics, and quantum logics. (I learned about fuzzy logic and relevance logics, both of which I discussed in my next book, only after I finished Deviant Logic.)

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CB: In your view, classical logic is revisable. My question is: in what respects could classical logic be, or has it been, revised? What kinds of logical system are genuinely deviant? It has been claimed that deviant logics change the meanings of the logical connectives, so that there is no real conflict, no real rivalry, between supposedly deviant systems and the classical system. What do you think?

SH: Whether apparently deviant logics are genuinely rivals of classical logic, or are merely notational variants of the classical system (a question put on the agenda of philosophy of logic by Quine) was discussed at length in Deviant Logic. I argued that change of meaning of the connectives is insufficient to show that there is no real rivalry; and that, in any case, there is no good general

argument that deviant logics must invariably involve change of meaning.

But as I said in the Introduction to the new, 1996 edition of the book, though I still hold that it is possible that classical logic should turn out to be in need of revision, I wouldn't approach the question of revisability in quite the same way I did earlier, which I now find too superficially linguistic. Rather, I would distinguish the question of the necessity of the laws of logic from the question of our fallibility about what those laws are; and would stress the latter. It is most implausible to suppose that we are immune from mistake in believing exactly these, classical principles to be, let alone to be all, the real laws of logic; especially, I would add, given that the logical system we now call "classical" was arrived at only after a long and arduous history, and that, even as it was achieving its canonical articulation in Frege and Peirce, non-classical systems were already under exploration -- by Hugh McColl, for example, and by Peirce himself.

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CB: You also argued in Deviant Logic that, though logic is revisable, we would need to have good reasons before revising classical logic; and that in many instances the reasons for proposed deviations from classical logic have been quite weak. Why is that?

SH: Well, in some cases the motivations seem to me quite unconvincing. For example, I argued in the original edition of Deviant Logic that Lukasiewicz's argument for a three-valued logic to represent future contingents (which he saw as derived from Aristotle), rests on a modal fallacy; and I argue in the new edition of Deviant Logic that the arguments for fuzzy logic are badly confused, and the proposal that we need a non-classical "feminist logic" is laughable. But in other case the motivations go quite deep. Thanks to the work of my former student Dr. Robert Lane, for example, we now understand Peirce's motivation for his triadic logic (the earliest three-valued system, devised in 1909): he intended his third value to be taken by propositions which predicate of a breach of mathematical or temporal continuity one of the properties which is a boundary-property relative to that breach. And -- though I'm inclined to think that "relevance logics," interesting as some of these developments have been, ultimately rest on a confusion of logical with epistemological issues -- investigations in paraconsistent logic, intended to isolate contradictions so that "p and not-p" no longer entails "q," whatever "q" may be, could conceivably throw light on epistemological issues about inconsistent evidence.

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CB: I very much appreciate the conclusion of your article "The Justification of Deduction" (1976), reprinted in the new edition of Deviant Logic. If we put your conclusion about deduction and Hume's skepticism about induction together, it will follow that there is no absolutely certain knowledge, that the most we can have is highly probable knowledge, well-warranted by evidence, but not infallible. Do you agree?

SH: The conclusion of this paper was expressed quite modestly, in a way that stressed the parallels between deduction and induction. "The moral of this paper," I wrote, "might be put, pessimistically, as that deduction is no less in need of justification than induction, or, optimistically, that induction is no more in need of justification than deduction." But yes, I am indeed a thorough-going fallibilist, about logical as well as empirical knowledge. In Evidence and Inquiry, I tried to articulate what makes empirical evidence better or worse; but I don't yet have a comparably detailed account of what is involved in our (fallible) knowledge of logic.

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CB: Philosophy of Logics (1978) was your second book. It has been translated into Spanish, Italian, Korean, Portuguese, Chinese, and (in part) Polish, and has had great success in logical circles worldwide. As I told you, even though the Chinese translation is still in press, this book has been widely read by Chinese logicians, among whom it has been very influential. In fact, it was after I read this book as a graduate student that I first became interested in philosophy of logic, and gradually began to do my own research in this field -- as I said when I expressed my sincere thanks to you in the preface of my book, Studies in Philosophy of Logic (2002). What do you think were the most important ideas expressed in this book of yours?

SH: Philosophy of Logics was intended as a textbook -- I wrote it, in fact, because I could find no suitable textbook for the course I regularly taught at Warwick on philosophy of logic -- and in consequence much of it is taken up with exposition of logical concepts and philosophical theories about logic. But writing this book was also an opportunity to develop numerous ideas of my own: about the nature and scope of logic, for example, and the relations between formal logical systems and informal arguments, about the metaphysical and epistemological underpinnings of logic, and especially about the philosophical significance of the plurality of logical systems signalled by the plural expression -- Logics -- in my title.

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CB: Can you tell us a bit more about what you meant by speaking of "pluralism" in logic?

SH: Two things, one quite modest and uncontroversial, the other rather bolder. The modest idea was, simply, that there are numerous systems of logic, with different expressive power, notations, theorems, valid inferences, interpretations, and applications; and that thinking about the differences among them can help us understand some of the deepest metaphysical and epistemological questions about logic, such as: is there just one correct system of logic, or could there be several which are equally correct? what could "correct" mean in this context? how do we recognize truths of logic? could we be mistaken in what we take to be such truths?

The more ambitious idea, articulated in the final chapter of Philosophy of Logics, was that (contrary to instrumentalism) it makes sense to speak of a logical system as correct or incorrect, and that (contrary to monism) there is more than one correct logical system. The argument, in brief, was this: Formal systems of logic aspire to represent extra-systematic conceptions of validity and of logical truth. However, there are alternative formal projections of the same informal discourse; and sometimes, when different formal systems give different representations of the same informal arguments, they may be equally good, perhaps for different purposes. (This doesn't mean that we never have to choose between a deviant and classical logic, only that we may not always have to.)

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CB: I assume you are a kind of empiricist in philosophy of logic: you stress the contrasts between formal and informal arguments and between system-relative and extra-systematic conceptions of validity, and think of the former as characterizing the latter; and you argue that, since there may be no uniquely correct characterization, we should accept a kind of logical pluralism. Is this right?

SH: It was a crucial insight of Aristotle's that an argument is valid or invalid in virtue of its form, not its content; and a central aim of the formal systems devised by logicians should be to capture just the valid arguments. But I would prefer to avoid describing this as a form of empiricism, which might suggest, wrongly, that my view is that formal systems of logic should aim simply to characterize people's actual reasoning processes.

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CB: In some sense modern logic originated in Frege's anti-psychologism; and nowadays logic tends

to be thought of as having nothing to do with the processes, methods, and laws of human thinking, but as concerned with language, or perhaps reality. But I'm skeptical of this idea: as I see it, logic is the science of inference and argument; but inference and argument are processes of thinking, so that logic surely is related to our thinking processes. What is your view of psychologism and anti-psychologism in logic?

SH: First, perhaps a historical comment is in order: Frege was indeed strongly anti-psychologistic. It seems clear that his antipathy to psychologism was a result, in part, of the strongly Introspectionist character of the psychology with which he was familiar. And Peirce was scathing in his criticism of the idea -- which he associated with Sigwart -- that validity is a psychological property, a kind of tingling feeling you get when you move from premisses to conclusion. However, Boole, usually seen as the earliest major figure in the development of modern logic, was considerably more sympathetic to psychologism than they.

I would distinguish two interpretations of the claim that logic "has to do with" human thinking processes, a stronger and a weaker. As I said earlier in response to your describing me as an "empiricist" in philosophy of logic, I certainly don't believe that logic simply describes our thought-processes (no-one who has ever taught a logic class could think that!). However, I do think that logic is, in a sense, normative for thinking; for principles of deductive logic tell us that, if you argue like this, you will never move from true premisses to false conclusion, that if you argue like this you will end up contradicting yourself, and so on.

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CB: In what ways would you say that your philosophy of logic is like Quine's, and in what ways unlike?

SH: This is very difficult to answer briefly, in part because Quine's views seem to shift: for example, in some places he stresses the revisability of logic, but in others he argues that genuine revision of logic is impossible (I traced some of his shifts and twists in "Analyticity and Logical Truth in The Roots of Reference," originally published in 1977, and reprinted in the 1996 edition of Deviant Logic). In the end, it seems to me, Quine is much more of a logical conservative than I, more committed to the idea that classical first-order predicate logic really is THE correct logic, and of course more committed to a strict extensionalism.

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CB: I can tell from your two books in philosophy of logic that you have a wide reach and strong background in symbolic logic; but you seem always to have been more interested in its philosophical than its technical side. And after these two books you seem to have turned to epistemology, pragmatism, and other topics. Why was that?

SH: I have no pretensions of being a mathematician, and my interest in logic was always more philosophical than technical; indeed, one reason for the success of Philosophy of Logics was probably that I appreciated the difficulties of understanding logical technicalities, and worked hard to help readers master them.

The shift in emphasis to epistemology more generally was in part a happy accident: I had taught a course on Epistemology and Metaphysics at Warwick for several years; so when Blackwell invited me to write a book in epistemology for them I was intrigued by the challenge.

My interest in pragmatism began, as I recall, when, after reading the critique of Peirce's account of truth in the first chapter of Quine's Word and Object, I began seriously reading in Peirce's Collected Papers, and was soon hooked by the work of this quite remarkable philosophical mind! Peirce himself, I might add -- besides being a formal logician of broad scope and deep penetration -- was always much concerned with philosophical questions about logic, and (though he didn't like or use the term "epistemology") with what he and the other pragmatists called "theory of inquiry."

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CB: Now let me turn to your third book, Evidence and Inquiry: Towards Reconstruction in Epistemology (1993). As you know, with the help of two graduate students, I have translated this book into Chinese, and it should be published in Beijing by the end of 2003. What are the most important ideas developed in this book?

SH: First, of course, the articulation and defense of my new theory of epistemic justification, which I call "foundherentism" because it combines elements from the traditionally-rival theories, foundationalism and coherentism. In this context, my analogy between the structure of evidence and a crossword puzzle has proven particularly fruitful in my own work, and has been found useful by many readers, not only philosophers but also scientists, economists, legal scholars, etc.. Then, besides my analysis and critique of various versions of foundationalism and coherentism, of reliabil-ism, etc., I would mention my articulation and defense of a more modest style of naturalism than Quine's, and my critique of Rorty's (and Stich's) "vulgar pragmatism."

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CB: Can you tell us more about foundherentism?

SH: Let me begin by saying something about the two traditionally rival styles of theory of epistemological justification, foundationalism and coherentism. "Foundationalism" refers to theories which rely on a distinction between basic and derived beliefs, and hold that relations of support always run from the basic to the derived, never in the other direction; "experientialist foundationalism" refers to foundationalist theories which take the basic beliefs to be justified by a subject's sensory and/or introspective experience. "Coherentism" refers to theories which rely on relations of mutual support among beliefs, holding that a belief is justified just in case it belongs to a coherent belief-set. As these theories were developed and refined, some foundationalists acknowledged that not even basic beliefs were infallible, and that there could be mutual support among derived beliefs; and some coherentists suggested that "experiential" beliefs should be given special weight in the calculation of degrees of coherence and hence of justification. So the traditionally rival theories began to lean closer together. But the leaning destabilized them: as moderate foundationalists tried to explain why there couldn't be mutual support among derived and (so-called) "basic" beliefs, they risked falling into coherentism; and as moderate coherentists tried to explain why experiential beliefs should be weighted more heavily than others, they risked falling into foundationalism.

I argue, however, that foundationalism and coherentism don't exhaust the field, and that an intermediate theory is more plausible than either. It is possible to allow the relevance of experience to the justification of empirical beliefs, as experientialist foundationalism does but coherentism does not, and at the same time, instead of requiring a privileged class of "basic" beliefs, to allow for pervasive mutual dependence among beliefs, as coherentism does but foundationalism does not. These are the key ideas of foundherentism. The crossword analogy, by the way, first came to mind as a way of understanding how there can be mutual support among beliefs (as there is mutual support among crossword entries) without vicious circularity; and then I realized that the analogy helped with another problem too -- that the clues to a crossword were the analogue of a person's experiential evidence, and already-completed intersecting entries the analogue of his reasons for a belief.

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CB: In this book you criticize Quine's program of naturalized epistemology. Could you explain your

reasons? Since human beings are natural creatures, and our bodies and our brains are the product of evolution, can't we study human cognitive processes in the same way we study other natural processes or phenomena? Briefly: why can't epistemology be naturalized?

SH: This question is a bit misleading. Yes, I criticize Quine's "epistemology naturalized"; but I also articulate and defend my own, more modest style of naturalism. What is wrong with Quine's position, in brief, is that runs together three different and incompatible ideas: (i) that epistemology is not purely a priori, but depends on assumptions about human beings and their cognitive powers; (ii) that epistemological questions should be turned over to the sciences of cognition to resolve; and (iii) that epistemological questions are illegitimate, and should be displaced in favor of scientific questions about human learning processes. But it is not within the scope of physics, or psychology, or any of the sciences, to tell us what constitutes better or worse evidence, for example, or why true predictions confirm the truth of a theory; and if these characteristically epistemological questions were illegitimate, the scientific enterprise would make no sense. So the second and third forms of naturalism are indefensible. The first style of naturalism, however, I find quite defensible -- in fact, I defend it myself.

I call my kind of epistemological naturalism "reformist aposteriorist naturalism": "reformist" to distinguish it from revolutionary naturalism, which denies the legitimacy of traditional epistemological questions; and "aposteriorist" to distinguish it from scientistic naturalism, which holds that those traditional epistemological questions can be resolved by psychology. Nevertheless, my position is a form of naturalism; for it conceives of epistemology, not as a purely a priori, but as explicating evaluative concepts which depend on presuppositions about human cognitive capacities and limitations.

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CB: It seems to me that your view of induction has changed: in Philosophy of Logics you classified inductive logic under "classical logic," but in Evidence and Inquiry you wrote that if "inductive logic is taken to indicate relations susceptible of purely syntactic characterization, it is prone to paradox at best, perhaps even mythical." However, you add, even if there is no formal inductive logic, there is still such a thing as (objectively) supportive-but-not-conclusive evidence. Why don't you think inductive logic is possible?

SH: No, I didn't classify inductive logic under "classical logic": if you look again at the table on p.4 of Philosophy of Logics you'll see that it classifies traditional, classical, deviant, extended, and inductive

logic under "systems of formal logic." However, you are right that I have come to doubt that there can be a formal, i.e., syntactically characterizable, logic of induction.

My present view is this: how supportive evidence E is with respect to a claim depends on how much adding E increases the explanatory integration of one's reasons with respect to that claim. Explanation, however, requires generals, kinds and laws; and so supportiveness is not simply a matter of form, but depends on the content of the predicates involved. What persuaded me of this initially was Goodman's grue paradox: if our evidence is more supportive of "all emeralds are green" than it is of "all emeralds are grue" -- as I believe it is -- it must be in virtue of some difference between "green" and "grue," for both statements have the same logical form.

These ideas, suggested briefly in Evidence and Inquiry, are worked out in much more detail in Defending Science -- Within Reason, where I argue that the failure of the Old Deferentialism in philosophy of science was in part the result of too narrowly logical a conception of rationality, and suggest what I describe as a "worldly" conception of evidence.

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CB: Now we come to your fourth book, Manifesto of a Passionate Moderate: Unfashionable Essays (1998). My first question, as before, is: what do you regard as the most important ideas expressed in this book?

SH: As the title indicates, this book is neither a text nor an academic monograph, but a collection of essays -- but essays tied together by some common themes. Many of them were written in response to the numerous invitations I received after Evidence and Inquiry was published to lecture on "feminist epistemology," Rorty's neo-pragmatism, etc.. One important unifying theme is my defense of the legitimacy of the concept of truth and the ideal of honest inquiry against the arguments (and the rhetoric) of radical feminists, neo-pragmatists, multiculturalists, the New Cynics in philosophy of science, and so on; which led me into explorations of the similarities, and the differences, between science and literature, the cognitive importance of metaphors, Peirce's distinction between genuine and sham inquiry, the many meanings of "relativism" -- and eventually to such socio-political issues as the many styles of feminism and multiculturalism, affirmative action, and the present condition of philosophy, and the academy itself.

What are the most important ideas in this book? The idea that has proven, thus far, most influential, is the metaphysical theory I began developing in "Reflections on Relativism," Innocent Realism. This

is a position intermediate between Metaphysical Realism, on the one hand, and metaphysical forms of cultural relativism and irrealism on the other. My classification of the many and various types of relativism has also attracted readers' attention. Then there is my distinction between the Old Deferentialism, i.e., philosophy of science focussed on rationality, logic, structure, and the New Cynicism, which focusses instead on power, politics, and rhetoric; and an early articulation of my intermediate position. I would also mention my development of Peirce's distinction between genuine inquiry and sham reasoning into a tripartite distinction: genuine inquiry versus the two varieties of pseudo-inquiry, the sham and the fake; and my identification of the Passes-for fallacy: that ubiquitous argument from the true premiss that what passes for truth, known fact, strong evidence, etc., is often no such thing, but only what the powerful can get accepted as such, to the false conclusion that the concepts of truth, fact, evidence, etc., are nothing but ideological humbug.

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CB: Could you tell us more about Innocent Realism?

SH: It is, I hope, a metaphysical position that can accommodate the most robust realist intuitions to the most sophisticated anti-realist objections. The main ideas are something like this. The world -- the one, real world -- is independent of how we believe it to be. In saying this, obviously, the Innocent Realist repudiates both the irrealist thesis that there is no real world, and the pluralist thesis that there are many. However, she of course allows that human beings intervene in the world, and that we, and our physical and mental activities, are part of the world. The one, real world, in other words, is heterogeneous: there are, besides natural things and events, human artifacts of every kind, social institutions, and the theories, depictions, and imaginative constructions of scientists, artists, poets, novelists. etc..

Adapting an idea from Peirce (who was in turn adapting an idea from Duns Scotus), the Innocent Realist construes "real" as meaning "independent of how you, or I, or anyone believes it to be"; and as contrasting with "fictional, a figment, imaginary." Scientific theories are real; and so are works of fiction. But the explanations scientists imagine, when they are successful, are true, and the laws they imagine real; while fictional characters and events are precisely <u>not</u> real, but imaginary.

Though very fallibly and imperfectly, we humans are able to know something of how the world is. This is possible only because we have sense organs able to detect information about particular things around us, and the intellectual capacity to make generalizations about them; and because the things around us are of kinds and subject to laws.

We describe the world, sometimes truly, sometimes falsely. Whether a synthetic description is true or is false depends on what it says (which is a matter of human convention) and on how the things in the world it describes are. There are many different true descriptions of the world, in different vocabularies. All these many different truths must somehow fit together: there can't be rival, incompatible truths or "knowledges." But this doesn't mean that all the truths about the world must fit together by being reducible to a privileged class of truths in a privileged vocabulary; I see the truths of the social sciences as "fitting together" with the truths of the natural sciences more in the way a road map can be superimposed on a contour map of the same territory.

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CB: You are an outstanding scholar of American pragmatism, and a former President of the Charles S. Peirce Society. In Evidence and Inquiry you included a chapter on Rorty's neo-pragmatist critique of epistemology, and in some of the essays in Manifesto you wrote at length both about classical pragmatism and about contemporary neo-pragmatism. I have translated two of your articles on pragmatism -- "'We pragmatists ...' ; Peirce and Rorty in Conversation," which appears in Manifesto, and "Pragmatism Old and New" (hitherto published only in Spanish) into Chinese; and you have accepted my invitation to edit an anthology of pragmatist writings for Chinese readers [Pragmatism: Selected Writings], to be published by People's Publishing House. What do you think we can learn from the pragmatist tradition?

SH: Let me start by explaining that pragmatism is the only school of philosophy native to the United States: Charles Sanders Peirce (1839-1914) and William James (1842-1910) were its joint originators, and the tradition developed in the work of John Dewey, George Herbert Mead, Sidney Hook, and C.I. Lewis. Peirce always maintained that pragmatism is "not a doctrine but a method" -- the method encapsulated in the Pragmatic Maxim, according to which meaning is a matter of experiential consequences. James too made the pragmatic maxim central, but interpreted it rather differently, in terms of practical consequences. Peirce's pragmatism is scientific, logical, and realist; James's is focussed on religion rather than on science, and is psychological rather than logical, nominalist rather than realist. And as one might expect from its origins, classical pragmatism -- to borrow a fine analogy due to the Italian pragmatist Giovanni Papini -- is like a hotel in each room of which different pragmatists are doing different kinds of work, but all of whom went through the same lobby on the way to his room: I think of Peirce's contributions to logic, semiotics, theory of inquiry, philosophy of science, metaphysics; James's to philosophy of religion, psychology and philosophy of mind, ethics; Dewey's to epistemology, philosophy of education, social and political philosophy; and Mead's to the philosophy of mind, language, and society.

I began my readings in pragmatism with Peirce -- an astonishingly wide-ranging, profound, and original philosophical thinker. And I have been much influenced by him: by his articulation and defense of the ideal of genuine inquiry; by his distinction (derived from Scotus) between the existent and the real, and his defense of the reality of generals; and perhaps especially by his "synechism," the "doctrine of continuity." This idea has of late come to seem to me extraordinarily fertile, as I have explored the continuities not only of scientific inquiry with empirical inquiry generally, but also of social-scientific with natural-scientific inquiry, of philosophy with science, and of inquiry with other human intellectual activities. But I have also been influenced by James, by Dewey -- specially his concern for the relation of science and values -- and, in philosophy of mind and philosophy of the social sciences, by Mead's remarkably insightful work.

James once wrote that the virtue of pragmatism is that it "unstiffens our theories"; and most of all, I think, as time goes by I am more and more grateful to the classical pragmatists for helping to liberate me from the uneasy reluctance of analytic philosophy to stray beyond strictly conceptual, logical, or linguistic issues. That is why I think of Defending Science, which goes far beyond the usual questions tackled by analytic philosophy of science, as in a way the most pragmatist of my books.

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CB: What is your opinion of Rorty's neo-pragmatism?

SH: It's quite a mess! Let me begin by getting the historical picture a bit clearer. At first, Peirce was hesitant to use the word "pragmatism" in print, because he feared readers would confuse his specifically philosophical position with pragmatism in the ordinary sense, meaning going by expediency rather than principle. He never used the word in his published writings until after James had made it famous. And later he came to think he needed to distinguish his style of pragmatism from James's, Dewey's, etc., and especially to dissociate himself from the misunderstandings of pragmatism that were being perpetrated in the literary journals; so he introduced the term "pragmat-icism," hoping it would be "ugly enough to be safe from kidnappers." The point of my "conversation" between Peirce and Rorty was, of course, to bring out how utterly different Rorty's literary-political, anti-metaphysical "pragmatism," with its disdain for logic and repudiation of epistemology, is from Peirce's pragmaticist philosophy. And Rorty's neo-"pragmatism" is not only very different from Peirce's; it is also quite distant from James's, and even from Dewey's. The old pragmatist whom Rorty most resembles is F.C.S. Schiller -- the British philosopher whose radically relativist position James once described as "the butt-end foremost" version of pragmatism.

That's why, in Evidence and Inquiry, I referred to Rorty's (and Stich's) views as "Vulgar Pragmatism." In chapter 9 of E&I I argued that Rorty's critique of epistemology is seriously confused. His repudiation of "foundationalism" runs together three quite distinct ideas: (i) foundationalism (i.e., as I explained earlier, theories of epistemic justification relying on a distinction of basic versus derived beliefs); (ii) foundationalism (i.e., the thesis that epistemology is an a priori discipline); and (iii) FOUNDATIONALISM (i.e., the thesis that epistemological principles are not merely conventional but have some objective grounding). Foundationalism, I agree, is false; foundherentism is the right theory of epistemic justification. And foundationalism is also false; as my reformist aposteriorist naturalism holds, epistemology is not wholly a priori. But FOUNDATIONALISM, I argue, is true. Rorty's argument against FOUNDATIONALISM depends on the assumption that, if truth is not correspondence to Things in Themselves, it can be nothing more than here-and-now agreement; but this, obviously, is a grossly false dichotomy.

In Manifesto of a Passionate Moderate I included the "conversation" between Peirce and Rorty (compiled from their own words) to show how far Rorty's position is from classical pragmatism. In "Confessions of an Old-Fashioned Prig" I discussed Rorty's confusions about truth in more detail. And in "As for that phrase 'studying in a literary spirit ...'" I argued against Rorty's proposal that philosophy be reconceived as a genre of literature, "just a kind of writing." Indeed, I showed, one disastrous consequence of Rorty's strange ideas about truth is to make it impossible not only to understand what genuine inquiry is, but even to grasp that literature has important truths to teach us.

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CB: In Manifesto you are often quite polemical; but also a real intellectual -- enthusiastic, wise, responsible, and marshalling subtle arguments against many kinds of intellectual fashion, such as neo-pragmatism and radical styles of feminism, multi-culturalism, sociology of knowledge, literary theory, etc.. You defend the possibility and importance of genuine inquiry, and stress the dangers of sham and fake reasoning. I have every sympathy with your main themes. But on some topics I have rather different ideas. Could we discuss some of these differences?

SH: Of course.

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CB: In "Multiculturalism and Objectivity" and "Reflections on Relativism" you argue against any kind of cultural relativism, but I'm afraid your position is too strong. True, whatever our race, sex, nationality, and so forth, we are all human beings, with universal characteristics. So it is wrong to see multiculturalism as pulling against objectivity, or against universal human rights. But I think there is another side of things. Human beings live in different countries, with different languages, different histories, traditions, and cultures; they face very different environments. Don't all these elements influence our cognitive processes? I remember, when I was a visiting scholar in Helsinki, Finland, in 1998, attending a seminar on abortion: does abortion mean killing a baby? Should it be legal or illegal? This kind of problem doesn't arise in China, because the problem China faces is a population explosion. The population is already 1.3 billion, and according to traditional Chinese culture, children are very important: it is a terrible thing for your family if you have no children, especially no son. So the Chinese government had to impose the "one couple, one child" policy -- which is often criticized in Western countries, especially the U.S. As a Chinese intellectual, I think the Chinese government is right on this matter; as do most Chinese people. I conclude that it is wrong to maintain an absolute universalism against every form of cultural relativism. How would you answer?

SH: I'd better begin by explaining that you have misread me here. In "Multiculturalism and Objectivity" I distinguish several kinds of multiculturalism, and argue against the kind I call "epistem-ological counter-culturalism." But I don't say, nor do I believe, that all forms of multiculturalism are misguided. Similarly, in "Reflections on Relativism" I distinguish numerous kinds of relativism, and develop a position -- the Innocent Realism mentioned earlier -- incompatible with what I call deep metaphysical-cultural relativism (the thesis that ontological claims make sense only relative to a community or culture). But I don't say, nor do I believe, that all forms of cultural relativism are mistaken.

As I understand it, you are inclined to defend some form of ethical relativism. If your point is just that in different cultures, different ethical norms are accepted (which would be shallow, or anthropological, ethical-cultural relativism in my classification), of course I agree. But obviously it doesn't follow from this that ethical norms make sense only relative to a culture (which would be deep, or philosophical, ethical-cultural relativism in my classification).

Turning now to your example, let me begin by saying that, granting for the sake of argument that the "one couple, one child" policy is morally justifiable, an ethical absolutist could still argue that there is some overriding ethical principle, not itself culturally relative -- for example, that all children should have a reasonable opportunity for a healthy and productive life -- which would justify that policy in certain circumstances, but different policies in other circumstances. In fact, as you say, the policy is much criticized in the West. In part this is because we place a high value on people's freedom to make (what we believe to be) essentially <u>private</u> decisions for themselves: which raises hard

questions about how we should balance freedom and welfare. In part, also, I suspect, it is because our press reports that many Chinese couples are far from happy about the policy; and that, in consequence of its imposition, many female fetuses are aborted, and girl babies left to die. And, of course, in the U.S. (as, apparently, in Finland), abortion is a very controversial topic -- indeed, it is probably one of the most divisive issues in American politics, with feelings running very high on both sides.

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CB: I would like to add that for developing countries with a big population, like China's, the dilemma is whether to place strict controls on the birth rate (and face criticism from the West for doing so), or to let people produce children who will have no hope of a decent life -- of adequate food, education, healthcare, and so on. What do you think about this dilemma?

SH: This is what I was alluding to when I spoke earlier of hard questions about the balance of freedom and welfare. The best compromise, I suppose, would be if people could be made to understand the need to control the birth rate, and limited their families voluntarily. But I don't at all mean to suggest that this would be easy to achieve (its feasibility will depend, for instance, on the infant mortality rate, on whether people have a reasonable expectation of security in their old age, and so on). Beyond this, I don't think I know enough to offer an opinion -- I have no idea, for example, how successful or unsuccessful India has been at trying to control her birth-rate by voluntary means, without such draconian policies as China's.

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CB: You also fiercely criticize affirmative action. I would say, however, that while affirmative action certainly runs against procedural fairness, it is compensation for past unfairness, and so still acceptable from a long-term point of view. In order to correct historical injustice, we have to adopt some policy that will open opportunities to the weak and enable them to become stronger. Besides, I guess preferential hiring should be conditional on "other things being almost equal."

SH: Perhaps I need to say first, in the simplest and most direct way possible, that there is no disagreement <u>with respect to ends</u> between myself and those proponents of affirmative action who want to ensure, as I would, that talented people are not disadvantaged because of irrelevant factors such as their race or their sex; where we disagree is about the <u>best means</u> to achieve this end. And I'm quite surprised that you describe me as "fiercely criticizing" affirmative action: for the strongest thing I said in "The best man for the job may be a woman" is that I fear that preferential hiring of women in the academy, though it has had some good consequences, may have done more harm

than good overall -- I don't think that's exactly "fierce," do you?

However, I must say find the argument about compensation, which you seem inclined to give a lot of weight, very problematic. I might approach this in a personal way: I don't regard it as appropriate compensation to me for the discrimination I encountered as a young woman, if universities now to appoint weaker candidates than they could hire, on the grounds that they are women. More importantly, this isn't appropriate compensation for all those women of my generation who were excluded altogether, or who found themselves in permanently-temporary lecturer jobs, unable to advance in their careers. But the problem is quite general: "compensation" makes sense if you give some benefit to people who were actually harmed; not, however, if you give some benefit to other people who were not themselves victims of the "historical unfairness" of which you speak.

When, in "The best man," I complained about "This-or-Nothingism," what I meant was that we need to try much harder to find better ways to achieve equal opportunities for the talented regardless of race or sex; better ways, that is, either than the old system, or than the new -- which seems to me of dubious benefit even to its supposed beneficiaries, and certainly a corrupting influence on an already depressingly corrupt academic hiring system. There is no doubt in my mind, either, that so far from overcoming sexist (and racist) attitudes in the academy, after thirty-odd years of preferential hiring these attitudes are in some ways even worse, because less overt, and more disguised and distorted by layer upon layer of self-deception and hypocrisy.

As for "other things being almost equal," I can only say that, in my opinion, the present situation is so bad that it just isn't realistic to expect departments to make such subtle distinctions.

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CB: While we are speaking of corruption in the universities, let me turn to your "Preposterism and Its Consequences," a strong critique of the present pressure to publish regardless of the quality or significance of publications. I agree that this is a very general and serious problem in the academy, both in the West and in China. Still, I am more optimistic to this situation than you. I think, when we have to make selection from some candidates, we need at least some of their publications in order to judge their academic level. Moreover, we have no way to judge the academic quality of some works before they are published. This kind of judgment has to be done by academic community only after these works have been published, and usually it needs time. And I always believe, the peer-review system will finally work well enough, and the really valuable work will eventually be recognized.

SH: Well, of course I don't know how well the peer-review process works in China; but I have to say that in the realm of English-language philosophy, I think the system is now rife with corruption and incompetence. Too many reviewers are ignorant, prejudiced, and/or more concerned to advance themselves than to give an honest appraisal. (Much of what is published is weak, uninteresting, blandly conformist, faddy, or sycophantic; and even outright plagiarism too often escapes detection.) Junior people who MUST publish to get tenure, being at the mercy of editors and referees, soon learn that it is much easier to publish bland, inoffensive stuff, and to cater to prejudices, than it is to place work that is truly original or independent-minded.

As for the good stuff being found eventually, let me just say that by now the sheer volume of academic publications is absolutely overwhelming, and the dirty secret we all know but don't admit openly is that most of what's published is never read. It's hard to be optimistic that the gems will eventually be found and valued at their true worth.

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CB: "Preposterism and Its Consequences" is mainly critical; I'd like to know what positive suggestions you may have for overcoming this problem. If you were the president of an important university, what would you do to change the situation? Unless something is done, won't the situation just continue?

SH: The problem is very serious, and by now very deep-rooted; and I don't think it can easily be solved by means of some policy imposed by an administrator, however powerful or influential. It took many decades to get into this mess, and it will take longer, probably, to get out of it. It may even be -- as I think in my most pessimistic moments -- that there will be no change for the better until things have got even worse, and people simply won't tolerate it any longer; perhaps we shall have to wait until students' parents become aware that their children are the real victims of the corrupt culture of the academy.

Still, we can try to figure out how things got so bad. I would mention, among the many interacting causes: credentialism (i.e., a culture that values a diploma more than education, and where employers insist on "qualifications" even when they are quite irrelevant to a person's ability to do the work in question); the enormous growth of the universities, and the rise of a vast, self-interested administrative class, including many people with no real understanding of the demands of serious academic work; and of course the imposition by this new administrative class of a disastrous

business model of "productivity" completely inappropriate to the academy. So, if I were a university president, I guess I would try to get the most intellectually serious of my faculty on my side to do anything we could to get away from that model, and to encourage real, hard, independent thought and real, tough, effective teaching -- and I would do my best to halt any further administrative bloat.

Looking back over the last paragraph, though, I realize that this, no doubt, is why I shall never be president of a university!

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CB: Now we come to your new book, Defending Science -- Within Reason (forthcoming). As I understand it, Defending Science is in a way an extension of the spirit of, and of some topics in, Manifesto of a Passionate Moderate. I have the same question as before: what do you think are the most important ideas presented in this book?

SH: Defending Science is a very ambitious book, in which I have tried not only to understand the epistemology and the metaphysics of science, but also to paint a much more comprehensive picture of the place of the sciences in inquiry, and in our lives. It began, yes, with some ideas from Manifesto, but as the work proceeded it has gone far, far beyond the couple of essays on science in that collection.

This book begins with a diagnosis of a key false assumption shared by the Old Deferentialism and the New Cynicism: that, if science is a rational enterprise, its rationality must be explicable in narrowly logical terms, i.e., it must be syntactically characterizable. Rejecting that assumption, I argue that the rationality of the scientific enterprise can only be captured by an account of evidence and method which is worldly: i.e., takes account not only of form or structure, but also of scientists' interactions with, and of the relations of scientific language to, the world.

By "scientific evidence" I mean the evidence with respect to scientific claims and theories. I call my epistemology of science "Critical Commonsensism," in part because I hold that scientific evidence, in this sense, is like the evidence with respect to empirical claims generally -- only more so. It includes both experiential evidence and reasons, working together in the foundherentist way articulated in Evidence and Inquiry; but it is more dependent than the evidence with respect to ordinary empirical claims on instruments of observation, and is almost always the shared resource of numerous scientists, within and across generations. So I first give an account of the personal sense of warrant (the degree of warrant of a claim at a time for a person); then construct an account of the social

sense (the degree of warrant of a claim at a time for a group of people); and finally construct an account of the impersonal sense (the degree of warrant of a claim at a time, simpliciter.) After illustrating this account by reference to Watson and Crick's evidence for the double-helical structure of DNA, I am able to show how it resolves the raven paradox and the grue paradox, and then to tackle Quine's thesis of "underdetermination."

As for "scientific method," my thesis is that, rather than there being a uniquely rational mode of inference or procedure of inquiry used by all and only scientists, there are the inferences, procedures, desiderata, and constraints common to all serious empirical inquiry, overlaid by a vast array of local and evolving scientific "helps": instruments of observation, models and metaphors, techniques of mathematical and statistical reasoning, and a social organization that enables evidence-sharing, and helps keep most scientists, most of the time, reasonably honest.

After that, I articulate the modestly realist metaphysical assumptions that underpin these e-pistemological ideas -- the Innocent Realism intertwining with my Critical Commonsensism. Briefly and roughly: rejecting instrumentalism and constructive empiricism, I see scientists as seeking substantial, explanatory truths -- truths about the one, real world that they, like other inquirers, investigate. For the scientific enterprise to be possible, I argue, we must have sensory organs capable of detecting some information about particular things and events around us, and the intellectual capacity to make generalizations and test them; moreover, particular things and events in the world must be somewhat accessible to our senses, and must be of kinds, and subject to laws. But unlike some recent realists, I don't build claims about the progress of science into my metaphysics, but rather argue that progress in the sciences, though undeniable, is ragged and uneven, and never guaranteed.

With all these ideas in place I go on to consider the relations of the natural to the social sciences; the epistemological role of sociology of science; the relations between science and literature, and the place of rhetoric of science; the interactions of science with the law; the tensions between science and religion; and finally, predictions of the end of science.

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CB: Could you tell us a bit more about why you call your approach "Critical Common-sensism"?

SH: I borrowed the expression from Peirce, who uses it to refer to his response to Hume, which combined elements from Kant (the "critical" part) and from Reid (the "common-sense* part). What I

mean, however, is something rather different: essentially, that scientific evidence is like the evidence with respect to ordinary empirical claims, and that the methods of the sciences are, as Einstein once put it, "nothing more than a refinement of our everyday thinking." As least as that phrase is sometimes understood, there is no "scientific method" -- no modes of inference or procedures of inquiry used by all and only scientists and guaranteeing, if not truth, at least progress. Rather, by means of all those "helps" to inquiry I mentioned earlier, the natural sciences have amplified and refined the procedures of ordinary inquiry. For example, plumbers and auto mechanics and cooks, as well as scientists, make controlled experiments; but the sciences have refined and developed far more sophisticated kinds of experimental control.

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CB: I find the comprehensive picture you develop, including Critical Commonsensism in epistemology and Innocent Realism in metaphysics, very congenial -- at least close to the truth in many respects. But I'd like to hear more about your view of the similarities and the differences between the natural and the social sciences.

SH: I call my chapter on the social sciences "The Same, Only Different" -- borrowing a phrase my grandmother used to use when she explained a new idea to me: "You know such-and-such?", she would say; "Well, this is the same, only different."

I begin by distinguishing the intentional social sciences (the parts that must appeal to people's beliefs, hopes, fears, etc.) from the rest (such as physical anthropology or physiological psychology). Non-intentional social science is in all essentials just like natural science. But intentional social science, I argue, is not reducible to natural science. However, the intentional social sciences can be integrated with the natural sciences –to use the analogy I mentioned before, rather as a map of the roads and towns in an area can be superimposed on a contour map of the same territory.

The intentional social sciences, like the natural sciences, are kinds of inquiry; but they investigate social rather than natural phenomena. Like all empirical inquiry, natural-scientific inquiry included, social-scientific inquiry requires making explanatory conjectures, checking to see how well they stand up to any evidence you have or can lay hands on, and then using your judgment whether to accept them, modify them, or abandon them and start again; however, the evidence that is relevant is of a rather different kind than in the natural sciences, and so are the explanations sought. Intentional social-scientific inquiry uses the same inferences and procedures, and is subject to the same demands, as all empirical inquiry, natural-scientific inquiry included; but the "helps" to inquiry

appropriate in intentional social science are not the same as those appropriate in the natural sciences: for example, social scientists use questionnaires and interviews, rather than microscopes and telescopes, as instruments of observation. (Unfortunately, however, hoping to share in the prestige of the natural sciences, social scientists have sometimes handicapped themselves by borrowing inappropriate natural-scientific helps. This is one of several reasons why, so far at least, the social sciences haven't made nearly such impressive progress as the natural sciences have done.)

Metaphysically, the situation is similar: like the natural sciences, the social sciences seek significant, explanatory truths, and the possibility of doing this requires that there be kinds and laws; but social kinds are (in a weak sense) socially constructed as well as real, and social laws are historically or locally conditioned. Moreover, even though, in some senses of that multiply-ambiguous phrase, social science isn't value-free, nor would we want it to be, intellectual honesty, respect for evidence, is no less important in the social than in the natural sciences; and social-scientific inquiry should not be confused -- as it has too often been -- with political advocacy.

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CB: You began your career as a logician, but gradually extended your range to epistemology, metaphysics, pragmatism, philosophy of science, even philosophy of literature and law, and so on. It's quite a way from philosophy of logic to philosophy of law, isn't it? Could you tell us something about your intellectual development, and what you hope to achieve in the future?

SH: I don't think the gradual growth in my interests is really so surprising: after all, given my early interest in epistemological and metaphysical questions about logic, it was natural that I should move to epistemology and metaphysics more generally, and then to philosophy of science -- nor is it so surprising that legal scholars, who are themselves concerned with questions of evidence, should have taken an interest in my epistemological work, and so drawn me into their questions too.

This year, I have agreed to write several papers on the law of scientific evidence: I just finished a general-interest paper for Daedalus (the journal of the American Academy of Arts and Sciences); I am now working on another, this time on tensions between inquiry and adversarialism, fallibilism and finality, for a conference at Cardozo Law School in New York; then I shall write a piece on the Supreme Court's (Popperian) philosophy of science for the American Philosophical Association's Newsletter on Law and Philosophy; and after that a plenary address for the International Congress of the Association for Legal and Social Philosophy to be held at Lund University in Sweden in the

summer of 2003 ... so I shall be quite busy with legal philosophy for a while!

As for what comes next -- well, it's never easy to predict, but I find myself more and more drawn to questions in philosophy and literature: I'm especially intrigued by what I think of as the Epistemological Novel, such as George Eliot's Daniel Deronda, with its quite profound refections on the Power of Ignorance, Samuel Butler's The Way of All Flesh, a brilliant portrayal of self-deception and hypocrisy, and Alison Lurie's Imaginary Friends, a hilariously funny novel about cognitive dissonance and the pitfalls of social-scientific research. I like the idea of combining my epistemological interests with my love of literature, and of the flexibility and subtleties of the English language. And of course this would also be an opportunity to think over questions that have long interested me about truth and reference in fiction, the differences between inquiry and literature, and even the old Platonic "quarrel between philosophy and poetry."

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CB: Professor Haack, thank you for agreeing to talk with me -- it feels almost as if, in our conversation, I have been able to accompany you on your intellectual journey as a logician, philosopher, and social critic and educator. May you continue to succeed in your philosophical endeavors!

SH: Thank you. It has been a pleasure talking, and working, with you.

摘要 陈波于2002年在美国迈阿密大学对世界知名的逻辑学家兼哲学家苏珊·哈克做了一长篇访谈，先谈论了苏珊·哈克的理智背景，然后依次谈论了哈克在逻辑哲学、认识论、形而上学、实用主义、科学哲学、后现代思潮等方面的研究，特别是她本人提出的逻辑多元论、基础融贯论、批判常识主义、坦诚实在论等，最后谈到了哈克的近期计划。

关键词 逻辑的可修正性，逻辑多元论，基础融贯论，实用主义，批判常识主义，坦诚实在论