The Shaky Game Einstein Realism *and the* Quantum Theory

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Science and Its Conceptual Foundations

David L. Hull, Editor

Preface

The essays in this volume that have been previously published represent a body of work developed over a long period of time. There is a strong temptation to use this occasion not only to correct and improve them, but also to unify and regularize their style. I have resisted it, deciding instead to let them show the character of their particular times and places, so far as possible, without cosmetic surgery. On that account, however, a few explanatory words may help guide the reader.

When I began to work with the unpublished Einstein materials, they were available on microfilm housed in the Rare Book Room of Princeton University's Firestone Library, indexed at random, and were the legal property of the trustees of the Estate of Albert Einstein. Since then Princeton has built a new and separate manuscript library, the Seeley G. Mudd Library. Photocopies of the materials have been made, moved there, and uniformly indexed. The materials themselves are now the legal property of Hebrew University of Jerusalem. My varying references and acknowledgments in the essays reflect this history.

Those acknowledgments also reflect a history of support for which I should like to express my appreciation once again. I began the Einstein work under a fellowship grant from the National Endowment for the Humanities, continued that and related projects under grants from the National Science Foundation, and was able to complete the work represented here in the lovely community of Port Townsend, Washington thanks to a fellowship from the John S. Guggenheim Foundation. Those fellowship years were made possible by supplements and time off granted by the University of Illinois at Chicago, when I began the work, and Northwestern University, when I finished it. I want to thank these agencies and institutions for their help and confidence.

Many individuals have supported and assisted this work as well. The various essays already acknowledge some of that help. Some acknowledgments bear repeating. This is especially the case for materials discussed in chapter 5, on the Einstein-Schrödinger cor-

respondence. Linda Wessels gave me access to this correspondence, and Dana Fine organized it and made preliminary translations. I was also helped in that chapter by a translation of Schrödinger's *Naturwissenschaften* article prepared by the (now) late John D. Trimmer (1980), who sent it to me at the suggestion of James McGrath. Lest the footnote reference in chapter 7 go unnoticed, I want to acknowledge the coinventor of NOA, Micky Forbes, whose good philosophical and linguistic sense has guided the development of many of the ideas in the later essays, and their expression. I thank you all.

Chapter 1 opens by referring to one of J. L. Borges's tales. In November 1984, Alberto Coffa gave Micky and me the collection of stories from which that is drawn. He did so mumbling something about how some of the stories reminded him somehow of NOA. We were never able to explore the relevance further, for Alberto died a month later. I believe that Alberto may have meant especially for us to read the story called "On Universal Theater," by way of criticism and caricature of NOA's view of the relationship between philosophy and science. Alberto's form of criticism was always supportive. That critical support was instrumental in building my confidence actually to put this volume together. My different use of a Borges story in chapter 1 is my side of a conversation I never got to have with Alberto; it is here in his memory.

Those without whom this volume would never have been published include my editor, David Hull. By all means, you may hold him responsible.

Finally, between my writing it and its first publication, some small but irritating changes were made in my text of the essay of chapter 7. I have changed them back in this printing—surgery, not cosmetic but merely restorative.

Acknowledgments

The following chapters consist of previously published essays, whose provenance is as stated: chapter 2, "The Young Einstein and the Old Einstein," from R. Cohen et al., eds., *Essays in Memory of Imre Lakatos* (Dordrecht: D. Reidel Publishing Company, 1976), 145–59; chapter 3, "Einstein's Critique of Quantum Theory: The Roots and Significance of EPR," from P. Barker and C. G. Shugart, eds., *After Einstein* (Memphis: Memphis State University Press, 1981), 147–59; chapter 4, "What Is Einstein's Statistical Interpretation, or, Is It Einstein for Whom Bell's Theorem Tolls?" from *Topoi* 3 (1984):23–36; chapter 6, "Einstein's Realism," from J. Cushing et al., eds., *Science and Reality* (Notre Dame, Ind.: University of Notre Dame Press, 1984), 106–33; chapter 7, "The Natural Ontological Attitude," From J. Leplin, ed., *Scientific Realism* (Berkeley: University of California Press, 1984), 83–107; chapter 8, "And Not Anti-realism Either," reprinted by permission of the editor from *Noûs* 18 (1984):51–65.

The author wishes to express appreciation to these periodicals and publishers for permission to reprint the above mentioned essays in this volume.

The author is also pleased to acknowledge the cooperation of Hebrew University of Jerusalem, Israel, for permission to quote from the unpublished Einstein papers, and to thank Frau Ruth Braunizer for permission to quote from the unpublished Schrödinger correspondence.

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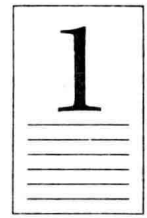
The Shaky Game



"Every absurdity has now a champion." Under this banner J. L. Borges's champion of the awful, H. Bustos Domecq, marches off in defense of modernism. In a hilarious art-critical ramble, Don Bustos recounts the development of Uninhabitables, an architectural movement that began with functionalism and culminated in "Verdussen's" masterly "House of Doors and Windows." The principle that underlies this masterpiece of the absurd is the schizoid idea of utilizing all the basic elements of habitable dwellings—doors, windows, walls, etc.—while abandoning the usual and ordinary connections between them.[1]

The principles and ideas that marked the development of the quantum theory display a curious parallel with those of Borges's tale. As I emphasize in the second chapter, the principle of complementarity that underlies Niels Bohr's influential interpretation of the quantum theory involves precisely "a rational utilization of all possibilities of unambiguous" . . . "use of the classical . . . concepts" (Bohr 1935, pp. 700–701), while also systematically abandoning the usual connections between them. For the policy of complementarity is to segregate the classical concepts into mutually exclusive, complementary pairs. Moreover, where we find Don Bustos describing the Uninhabitables as growing out of functionalism, so we see Bohr, in parallel, describing the quantum theory as a natural generalization of classical physics. Following Borges's example, we would expect the quantum theory to be a monstrous Uninhabitable, which is exactly what Einstein, perhaps its foremost critic, found it to be: "This theory [the present quantum theory] reminds me a little of the system o delusions of an exceedingly intelligent paranoic, concocted of incoherent elements of thoughts."[2]

What bothered Einsten most of all was actually twofold. First, he could not go along with the idea that probability would play an irreducible role in fundamental physics. His famous, "God does

1. See "The Flowering of an Art" in Borges (1976), pp. 77–82.
2. Letter (in English) from Einstein to D. Lipkin, July 5, 1952, Einstein Archives.

not play dice" is a succinct version of this idea, which he also expressed by referring to the quantum theory as a "flight into statistics".[3] But, as I explain in chapter 6, usually he expressed this concern positively, by affirming his interest in pursuing causal (or determinist) theories. Since Einstein's mode of expression on these issues was often elliptical, it may not be surprising to discover that his concerns over the quantum theory had a second focus. It is featured when he says of the quantum theorists, "Most of them simply do not see what sort of risky game they are playing with reality."[4] The risky, or as I like to call it, the *shaky game* puts into jeopardy what Einstein saw as the traditional program of physics, the attempt to construct a model of an observer-independent reality, and one that would stand the test of time.

The image of the shaky game, however, which I have adopted for my title, not only attaches to throwing the dice and toying with reality, Einstein's concerns over quantum physics, it attaches as well, I would urge, to all the constructive work of science and of the philosophical or historical programs that seek to place and understand it. These are games insofar as they involve elements of free construction and play. These are shaky because, without firm foundations or rigid superstructures, their outcome is uncertain. Indeed not even the rules of play are fixed. It follows that at every step we have to be guided by judgment calls. Einstein's use of the risk factor as a rhetorical weapon represents his own judgment about the character of the quantum theory. I think he understood that his dispute over the quantum theory was important precisely because past scientific practice, which he saw as developing a program for causal and realist theories, did not have built-in rules that would fix the character of future science. What he saw, I think, was that just because science is a shaky game, the realist program was at risk.

In chapters 2 through 5 I trace out the development of Einstein's concerns over the quantum theory. In chapter 6 I isolate Einstein's realism for separate analysis and evaluation. These reflections on Einstein's realism form a transition to chapters 7 and 8, which take up the contemporary philosophical dispute over the realism/antirealism issue, especially in the context of science. In these two

3. "I . . . believe . . . that the flight into statistics is to be regarded only as a temporary expedient that bypasses the fundamentals." Letter from Einstein to C. Lanczos, February 14, 1938. Translated in Dukas and Hoffman (1979), p. 68. He repeats this phrase, and sentiment, in his letter of August 2, 1949, to B. Dessau, Einstein Archives.

4. Letter from Einstein to E. Schrödinger, December 22, 1950. Translated in Przibam (1967), p. 39.

chapters I try to show that both houses here are plagued by incurable difficulties. The way out, I suggest, is to adopt what I call *the natural ontological attitude* (NOA), a stance neither realist nor antirealist; indeed the very one that moves us to see science as a shaky game. The last chapter of the book (chapter 9) comes full circle to reconsider the issue of realism in the quantum theory from a contemporary perspective, and in light of NOA.

It may help the reader if something is said about the development of these chapters. Some of it will be by way of setting and highlight, some by way of reflecting on the character of the work itself. The essays that make up the chapters of this book span a long period of time, about a decade in all. The essay in chapter 2 was written some ten years ago. Those of chapters 5 and 9 are brand new. The background to these essays is my own attempt to come to terms with issues in the interpretation of the quantum theory, and in particular with the issue of realism there. While working out the technical defense of both determinate and indeterminate forms of quantum realism (in the face of challenging "no-go" results concerning a classical setting for probabilities, value assignments, and quantum statistics), I gradually began to realize that in some cases Einstein had already pioneered similar ideas and had been severely criticized for so doing.[5] This fact, together with the then recent possibility of access to microfilms of the Einstein *Nachlass*, set me off to find out just what Einstein's ideas were concerning the interpretation of the quantum theory, and to see how viable (or not) they appeared some fifty years after the founding of the theory. The results of that investigation are contained in the essays that comprise chapters 2 through 5.

In chapter 2 I take on a scientific rumor, insinuated by a variety of well-known scientists and historians, that by the time of the development of the quantum theory (1925–27) the old Einstein (then all of 46) was no longer capable of grasping the radically new quantum ideas. His dissent from the quantum theory, then, would appear to be a kind of scientific senility. In the essay I try to put the lie to this, first by recalling how Einstein's scientific work (right up to 1926) not only contributed to the new quantum mechanics, but actually anticipated some of its central features. I then try to tease out of Einstein's scientific papers a general method of his, *the*

5. I use "determinate/indeterminate" for the technical question of whether the quantum magnitudes (the "observables") have definite point-values in noneigenstates. This is not the question of determinism, which has to do with the evolution of things over time. For my involvement with determinate forms of quantum realism see Fine (1973a, 1974), for indeterminate ones see Fine (1971, 1973b).

method of conceptual refinement, that actually requires significant conceptual change as the vehicle for scientific development.[6] Finally, I argue that it was precisely the conceptually conservative elements of the quantum theory that triggered Einstein's dissent. The nasty rumor, then, emerges as an interesting myth, a tall tale whose function was to protect the fledgling quantum theory from the sharp criticism of this century's most illustrious scientist. Thus Einstein's debate over the quantum theory constitutes a significant episode in which what was at issue was nothing less than the very pattern according to which physics was to grow.

The showpiece of Einstein's dissent from the quantum theory is the paradox of Einstein-Podolsky-Rosen (EPR), published in a little four-page paper in 1935. That tiny paper has been the subject of a voluminous secondary literature, including the mass of material that relates to Bell's theorem.[7] In chapter 3 I report my discovery of Einstein's letter to Erwin Schrödinger of June 19, 1935 (right after the paper was published), where Einstein remarks that, following some discussions, the paper was actually written by Podolsky, and that the central issue got buried in Podolsky's text. In that chapter I work out one version of the EPR argument that Einstein points to in his letter as central. It does not have to do with so-called hidden variables, the concept most readers of Podolsky's text seem to associate with EPR, nor even with the simultaneous assignment of positions and momenta. Rather it has to do with undercutting the Bohr-Heisenberg doctrine according to which puzzling features of quantum mechanics can be traced back to an inevitable and uncontrollable physical disturbance brought about by the act of measurement. Thus, in the version I discuss in chapter 3, Einstein's EPR has to do with the measurement of a single variable (not of two incompatible ones). It introduces a plausible principle governing measurement disturbances (the principle of separation) which is then shown to be incompatible with a particular interpretation of the state function, an interpretation that Einstein associated with Bohr, and that he calls "completeness." Thus what EPR intends to challenge is not quantum theory itself, but rather a particular version of the Copenhagen interpretation. And the way this occurs is by taking the doctrine of measurement disturbance seriously enough to formulate physical principles governing it. The "paradox of EPR" is, therefore, supposed to be a paradox

6. Since writing the essay for chapter 2, I have developed the method of conceptual refinement in an article on Imre Lakatos's philosophy of mathematics, Fine (1981a).

7. Chapters 4 and 9 explain "Bell's theorem."

for the Copenhagen stance toward quantum theory, showing that two of its central components (the doctrine of disturbance and the "complete" interpretation of the state function) are incompatible. I think readers familiar with the usual discussions of EPR will find the version in chapter 3 quite novel. In that version one prominent feature of the EPR text falls by the wayside entirely; namely, the notorious "criterion of reality." In line with this reading, one should note that although Einstein himself later published several versions of EPR, none of them makes any reference to or use of that reality criterion.

Einstein's own published accounts of EPR, however, employ a variation on the argument of chapter 3. I discuss that variant, and some of its problems, in chapters 4 and 5. In chapter 4 I move from EPR, Einstein's showpiece puzzle, to discuss the so-called statistical interpretation, his favored resolution. In that chapter I argue that we do not actually know what Einstein's statistical interpretation was. Einstein's various references to it are altogether too brief and sketchy to pin it down. Hence the widespread idea that Bell's theorem refutes Einstein's statistical interpretation is rather on the order of wishful thinking. There is indeed an interpretation that Bell's theorem does refute, the idea of an ensemble representation, but I argue that Einstein's texts suffer considerable strain if we try to fit them into that particular mold. To drive that argument home I show how much better Einstein's texts fit the pattern of prism models, an interpretative idea that I have developed elsewhere (see chapter 4) as a determinate way of bypassing the Bell theorem. But it is not really my intention to make Einstein into a precursor of my prism models. Rather, I want to challenge the conventional wisdom here, which seems to me altogether too ready to foist onto Einstein some simple (and usually refuted) point of view. The salient fact in the history of Einstein's statistical interpretation is its utter vagueness, and this is coupled with Einstein's almost complete indifference to the "no-go" demonstrations known to him (including at least Schrödinger's version of von Neumann's "no-hidden-variables" theorem).[8] To understand this I think we have to see that the statistical interpretation functioned for Einstein as a temporary expedient, like the quantum theory itself. It provided no more than a setting rhetorically apt for calling attention to the incompleteness of the quantum theory; a way, therefore, of motivating a search for a more complete theory. But Einstein's idea of

8. See note 3 of chapter 4 and my discussion in chapter 5 of the development of section 4 of Schrödinger's *Naturwissenschaften* article.

a completion had nothing to do with adding things to the existing theory; for example, in the manner of hidden variable extensions (not even, as I explain in the appendix to chapter 4, local ones). Rather, Einstein's idea of "completing" the quantum theory was an expression his vision of building an altogether different kind of physics.

In the foundational literature, EPR is the paradox associated with the issues of locality, completeness, hidden variables, and realism. The other great foundational issue, the so-called measurement problem, has a paradox all its own, the one affectionately called "Schrödinger's cat." Its genesis is the subject of chapter 5. There I examine the remarkable correspondence between Einstein and Schrödinger that took place during the summer of 1935. That correspondence was stimulated by the publication of EPR. It began with the correspondents agreeing in their opposition to the quantum theory, but not in their diagnoses of the basic disease nor, therefore, in their remedies. The good-natured joisting over criticisms and cures that ensued challenged each of them to try to undo the pet ideas of the other. Thus Einstein set out a refuting example that prompted Schrödinger to respond by attempting to refute Einstein's statistical interpretation. What Einstein had aimed to refute was what he called the "Schrödinger interpretation" of the state function, exactly the Bohr version of quantum completeness that was the target of EPR. Einstein's refutation involved an intriguing example which Schrödinger identified as being "very similar" to one he had just himself constructed. And then Schrödinger proceeded to tell the plight of the poor cat. Thus the correspondence shows Schrödinger's cat and Einstein's similar example not as pointed to the measurement problem but rather as new versions of EPR. They are indeed arguments for incompleteness that bypass the need for an additional premise having to do with locality (or separation). This places the cat somewhat differently from the way it is generally viewed, but it places it exactly as it is found in Schrödinger's published account of it. Nevertheless, I try to argue on the basis of evidence internal to the correspondence that the cat may well have first occurred to Schrödinger in the context of measurement, and only later been adapted to fit its published place as a demonstration of incompleteness. I confess to having considerable fun in this latter argument, playing historical detective on the basis of very few clues. (I do not know whether draft material or editorial correspondence exists to confirm, or refute, my speculations.) However, the dynamics that relate published scientific writing with concurrent informal reflections of a

more private kind is fascinating, even when speculative. In the end I could not resist exploring the possible dynamics and, in a book called *The Shaky Game*, it seemed fair practice to share the material, and the speculations, with my readers.

The issue of completeness, which is at the heart of EPR, Schrödinger's cat, and the statistical interpretation, turns on the idea of realism. As Einstein put it to Schrödinger, "Physics is a kind of metaphysics. . . . All physics is a description of reality; but this description can be either complete or incomplete."[9] The methodological dispute with Bohr, featured in chapter 2, also turns on realism, construed as a program for theory construction. Thus realism links the different subjects of chapters 2 through 5. It is the explicit subject of chapter 6. In this chapter I try to pull together various aspects of Einstein's methodological thinking to construct a detailed account of his realism, which I organize by means of two central concepts: Einstein's "entheorizing" and his holism. Let me just mention two conclusions of that study. The first is that, for Einstein, commitment to realism was commitment to pursuing a specific kind of program for theory contruction, one that already included the idea of causality (or determinism), and one that was to be judged on exactly the same basis as any other scientific program; namely, by judging its empirical success over time. A second conclusion that emerges from examining Einstein's realism is that, for him, realism was not at all a cognitive doctrine, a set of specific beliefs about nature (or the like). Rather, his realism functions as a motivational stance toward one's scientific life, an attitude that makes science seem worth the effort. But it is supposed to do so, somehow, without involving specific cognitive content. At the conclusion of chapter 6 I suggest the psychoanalytic concept of an *imago* as an analogue for this noncognitive, motivational conception of realism.

The work of chapters 2 through 6 represents a sort of hybrid form of scholarship; partly a conceptual (or philosophical) investigation and partly a historical one. Because it traces the conceptual odyssey of a single person, I think of the work as writing conceptual biography. What I have tried to construct is a coherent story line that integrates over time Einstein's various works and thoughts on the conceptual issues that relate to quantum mechanics. In so doing I have pushed hard for Einstein's consistency, some will say too hard. I have not always found it. (See, for example, the discussion

9. Letter from Einstein to Schrodinger, June 19, 1935: "die Physik eine Art Metaphysik ist. . . . Alle Physik ist Beschreibung von Wirklichkeit; aber diese Beschreibung kann "vollständig" oder "unvollständig" sein."

of "bijective completeness" in chapter 5, or, perhaps, the uneasy conception of motivational realism itself in chapter 6.) But I have certainly adopted as a working hypothesis the possibility of basic consistency over time, and I set out to test the hypothesis by making up a basically consistent tale. A kind of historiographical positivism stands opposed even to the effort.[10] It holds that the best one can do, as a historian, is to tell about the various moments in the life-thoughts of an individual. The historian does not, so to speak, have the standing to validate some further integrating scheme. In support of this brand of positivism one might cite, from my very own procedure, the occasions when I have had to select from among the Einstein materials those that go along with my scheme of integration and to demote, or explain away, materials that do not (see, for instance, how I treat the letter to Tanya Ehrenfest over the EPR criterion of reality in chapter 4, note 24; or what I make of Einstein's cognitive-sounding realist language in chapter 6.) This points to the necessity for judgment calls, even on what counts as good data. Moreover, I do not argue that the coherent stories I tell are the only ones that could be told nor, certainly, the best ones. And that shows how loosely the data, even after selection, determine the biographical narrative. But these two elements, the necessity for good judgment and the underdetermination of the theory by the data, are constituents of any creative intellectual endeavor—physics no less than history. I believe that the positivist attempt to use such elements to construct a warrant restricting the character of legitimate intellectual activity rests on a mistake. It is the mistake generally called "epistemology," one of the subjects criticized in chapter 8. In response to the positivist line, I would simply say that in writing conceptual biography I have been trying to tell a true story. Of course, I may have failed to do so satisfactorily, perhaps because some competing story turns out to be better (i.e., truer) or because, as it turns out, there is no true *story* to be told but only discrete historical moments to be catalogued. If so, then that will be a matter of fact and not of divergent historiographical philosophies. Indeed, in coming down strongly on the side of coherence and consistency, I would hope to challenge those skeptical about such accounts to respond and, in this way, to increase our understanding of things.

10. This breed of positivism is by no means restricted to the construction of historical narratives. It flourishes in the area called "critical theory," especially among those who have been influenced by contemporary French thinkers. There it attaches to the entire study of texts. My brief critique below of the historiographical version applies to the whole breed, which is best undone by adopting NOA.

In coming out for the truth of scientific stories (even historical ones) and in rejecting positivist epistemology, I may run the risk of being identified with the opposite philosophical camp, that of the realists. But I have no love for realism, as I hope the charitable reader of chapter 7 will conclude from my opening sentence there, which announces the good news that realism is dead. That chapter develops a "metatheorem" demonstrating how the philosophically fashionable, explanationist strategy for supporting realism simply (and irretrievably) begs the question. It goes on to show how detailed, realist-style, explanation sketches of scientific methods also simply (and irretrievably) fail. This sets the stage for a discussion of the natural ontological attitude (NOA) as a way of saving the concept of truth, while avoiding both the metaphysics of realism and the epistemology of various antirealisms. In its antimetaphysical aspect, NOA is at one with Einstein's motivational realism. But NOA perceives in that realist language and motivational setting a philosophical support system external to science and not actually required for its meaningful pursuit. If we let the scaffolding go, we discover that science stands perfectly well on its own and, indeed, *we* can then see it all the better.

That attitude, to let science stand on its own and to view it without the support of philosophical "isms," is what characterizes NOA. All the *isms* (realism, nominalism, idealism, conventionalism, constructivism, phenomenalism, positivism—and even pragmatism) involve global strategies, approaches to interpreting and providing a setting for science as a whole. Often these strategies derive from an underlying semantics, frequently from a picture or a theory of truth. In chapter 8 I show how the project of an acceptance (or consensus) theory of truth derives from a behaviorist reaction to realism, and how it traps several contemporary antirealisms in a destructive regress. To adopt the realist approach, and construe "truth" as some sort of realist-style correspondence, is not the way out of the trap. The way out is simply not to "construe" *truth* at all. Thus NOA adopts a no-theory attitude toward the concept of truth. It follows that up with an appropriate emphasis on the varieties of local scientific practices, and on the likely nonprojectibility of essentialist reconstructions of these practices. NOA carries this all the way to the practice of science itself, for it opts out of the game of inventing factors whose possession would *make* a practice "scientific" (or "pseudo-scientific", or "nonscientific"). It even opts out of the popular teleological version of this game, the one where you make up aims and goals for science. Here is where good judgment is called for and NOA, if you like, simply judges these games to be altogether *too* shaky!