Make Physics Real Again

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No other scientific theory can match the depth, range, and accuracy of quantum mechanics. It sheds light on deep theoretical questions—such as why matter doesn't collapse—and abounds with practical applications—transistors, lasers, MRI scans. It has been validated by empirical tests with astonishing precision, comparable to predicting the distance between Los Angeles and New York to within the width of a human hair.

And no other theory is so weird: Light, electrons, and other fundamental constituents of the world sometimes behave as waves, spread out over space, and other times as particles, each localized to a certain place. These models are incompatible, and which one the world seems to reveal will be determined by what question is asked of it. The uncertainty principle says that trying to measure one property of an object more precisely will make measurements of other properties less precise. And the dominant interpretation of quantum mechanics says that those properties don't even exist until they're observed—the observation is what brings them about.

"I think I can safely say," wrote Richard Feynman, one of the subject's masters, "that nobody understands quantum mechanics." He went on to add, "Do not keep saying to yourself, if you can possibly avoid it, 'But how can it be like that?' because you will get 'down the drain,' into a blind alley from which nobody has yet escaped." Understandably, most working scientists would rather apply their highly successful tools than probe the perplexing question of what those tools *mean*.

The prevailing answer to that question has been the so-called Copenhagen interpretation, developed in the circle led by Niels Bohr, one of the founders of quantum mechanics. About this orthodoxy N. David Mermin, some intellectual generations removed from Bohr, famously complained, "If I were forced to sum up in one sentence what the Copenhagen interpretation says to me, it would be 'Shut up and calculate!'" It works. Stop kvetching. Why fix what ain't broke? Mermin later

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regretted sounding snotty, but re-emphasized that the question of meaning is important and remains open. The physicist Roderich Tumulka, as quoted in a 2016 interview, is more pugnacious: "Ptolemy's theory"—of an earth-centered universe—"made perfect sense. It just happened not to be right. But Copenhagen quantum mechanics is incoherent, and thus is not even a reasonable theory to begin with." This, you will not be surprised to learn, has been disputed.

In *What Is Real?* the physicist and science writer Adam Becker offers a history of what his subtitle calls "the unfinished quest for the meaning of quantum physics." Although it is certainly unfinished, it is, as quests go, a few knights short of a Round Table. After the generation of pioneers, foundational work in quantum mechanics became stigmatized as a fringe pursuit, a career killer. So Becker's well-written book is part science, part sociology (a study of the extrascientific forces that helped solidify the orthodoxy), and part drama (a story of the ideas and often vivid personalities of some dissenters and the shabby treatment they have often received).

The publisher's blurb breathlessly promises "the untold story of the heretical thinkers who dared to question the nature of our quantum universe" and a "gripping story of this battle of ideas and the courageous scientists who dared to stand up for truth." But *What Is Real?* doesn't live down to that lurid black-and-white logline. It does make a heartfelt and persuasive case that serious problems with the foundations of quantum mechanics have been persistently, even disgracefully, swept under the carpet.

Why does that matter? Because, as John Stewart Bell, one of the book's heroes, believed, the attempt to provide understanding is a moral matter. Dismissing philosophical criticism is not only philistine, it's bad science. Physics isn't finished. A theory is not only a way to address the problems at hand; it's a jumping off point for the next, better, theory. And quantum mechanics is *the* theory that any new idea in fundamental physics must accommodate.

Quantum mechanics proper began in the mid-1920s with Werner Heisenberg's "matrix mechanics" and Erwin Schrödinger's "wave mechanics." These mathematical formalisms—later shown to be equivalent—unified revolutionary but ad hoc ideas about the atomic and subatomic world that had earlier been advanced by, among others, Bohr and Albert Einstein.

Becker begins his story proper with one of history's most famous scientific gatherings, the Solvay Conference of 1927. Of its twenty-nine attendees, seventeen were or would become Nobel laureates. Their meeting to discuss the new quantum theory inaugurated a long and famous

debate between Bohr and Einstein. Bohr championed the "Copenhagen interpretation," a name generally believed to have been introduced by Heisenberg in 1955. This was never a precisely defined set of beliefs, but Becker says the core that Bohr's circle could agree on can be summarized as follows:

• It is not possible, even in principle, to explain the quantum world independently of observation.

• Physics therefore concerns not what nature is but what we can say about it.

• Quantum mechanics is in essence a tool to predict the outcomes of measurements.

These beliefs in turn accorded with the logical positivism of the Vienna Circle, whose ideas were very much in the air. Positivism was more a philosophical movement than a single doctrine. Its themes included: reductionism and a hostility to metaphysics; various forms of the "verificationist" axiom that the meaning of a statement lies in its method of verification and that statements not capable of verification are meaningless; a strong strain of "antirealism," which held that scientific theories serve only as a way to generate testable assertions and do not imply that the nouns they use denote things in the real world. (Becker goes overboard when he says that positivists ruled out theories referring to entities that could not be directly observed.)

Einstein, by contrast, was a scientific realist: The aim of science is to understand a world that exists independently of our observation; thus, quantum mechanics, despite its impressive success, could not be the whole story. The conventional wisdom about this debate says that a reactionary Einstein, past his prime, couldn't get with the program, lost the argument, and thereby validated the Copenhagen interpretation. That, says Becker, is a fable—because, among other reasons, few of his opponents understood what Einstein was really driving at.

Bohr and his associates corresponded with the philosophical luminaries of the Vienna Circle. But for most physicists, says Becker, the Copenhagen interpretation amounted not to a carefully considered philosophical position but to a permission slip for dismissing questions, a sort of bar room putdown: Why worry about things you can't see—such as whether, when you're not looking, a light beam is made of particles or waves? To respond to this question, it's worth briefly rewinding.

At the end of the nineteenth century, fundamental physics modeled the constituents of the world as particles (discrete lumps of stuff localized in space) and fields (gravity and electromagnetism, continuous and spread throughout space).

Particles traveled through the fields, interacting with them and with each other. Light was a wave rippling through the electromagnetic field.

Quantum mechanics arose when certain puzzling phenomena seemed explicable only by supposing that light, firmly established by Maxwell's theory of electromagnetism as a wave, was acting as if composed of particles. French physicist Louis de Broglie then postulated that all the things believed to be particles could at times behave like waves.

Consider the famous "double-slit" experiment. The experimental apparatus consists of a device that sends electrons, one at a time, toward a barrier with a slit in it and, at some distance behind the barrier, a screen that glows wherever an electron strikes it. The journey of each electron can be usefully thought of in two parts. In the first, the electron either hits the barrier and stops, or it passes through the slit. In the second, if the electron does pass through the slit, it continues on to the screen. The flashes seen on the screen line up with the gun and slit, just as we'd expect from a particle fired like a bullet from the electron gun.

But if we now cut another slit in the barrier, it turns out that its mere existence somehow affects the second part of an electron's journey. The screen lights up in unexpected places, not always lined up with either of the slits—as if, on reaching one slit, an electron checks whether it had the option of going through the other one and, if so, acquires permission to go anywhere it likes. Well, not *quite* anywhere: Although we can't predict where any particular shot will strike the screen, we can statistically predict the overall results of many shots. Their accumulation produces a pattern that looks like the pattern formed by two waves meeting on the surface of a pond. Waves *interfere* with one another: When two crests or two troughs meet, they reinforce by making a taller crest or deeper trough; when a crest meets a trough, they cancel and leave the surface undisturbed. In the pattern that accumulates on the screen, bright places correspond to reinforcement, dim places to cancellation.

We rethink. Perhaps, taking the pattern as a clue, an electron is really like a wave, a ripple in some field. When the electron wave reaches the barrier, part of it passes through one slit, part through the other, and the pattern we see results from their interference.

There's an obvious problem: Maybe a *stream* of electrons can act like a wave (as a stream of water molecules makes up a water wave), but our apparatus sends electrons one at a time. The electron-as-wave model thus requires that firing a single electron causes something to pass through both slits. To check that, we place beside each slit a monitor that will signal when it sees something pass. What we find on firing the

gun is that one monitor or the other may signal, but never both; a single electron doesn't go through both slits. Even worse, when the monitors are in place, no interference pattern forms on the screen. This attempt to observe directly how the pattern arose eliminates what we're trying to explain. We have to rethink again.

At which point Copenhagen says: Stop! This is puzzling enough without creating unnecessary difficulties. All we actually observe is where an electron strikes the screen—or, if the monitors have been installed, which slit it passes through. If we insist on a theory that accounts for the electron's journey—the purely hypothetical track of locations it passes through on the way to where it's actually seen—that theory will be forced to account for where it is when we're not looking. Pascual Jordan, an important member of Bohr's circle, cut the Gordian knot: An electron does not have a position until it is observed; the observation is what compels it to assume one. Quantum mechanics makes statistical predictions about where it is more or less likely to be observed.

That move eliminates some awkward questions but sounds uncomfortably like an old joke: The patient lifts his arm and says, "Doc, it hurts when I do this." The doctor responds, "So don't do that." But Jordan's assertion was not gratuitous. The best available theory did not make it possible to refer to the current location of an unobserved electron, yet that did not prevent it from explaining experimental data or making accurate and testable predictions. Further, there seemed to be no obvious way to incorporate such references, and it was widely believed that it would be impossible to do so (about which more later). It seemed natural, if not quite logically obligatory, to take the leap of asserting that there is no such thing as the location of an electron that is not being observed. For many, this hardened into dogma—that quantum mechanics was a complete and final theory, and attempts to incorporate allegedly missing information were dangerously wrongheaded.

But what is an observation, and what gives it such magical power that it can force a particle to have a location? Is there something special about an observation that distinguishes it from any other physical interaction? Does an observation require an observer? (If so, what was the universe doing before we showed up to observe it?) This constellation of puzzles has come to be called "the measurement problem."

Bohr postulated a distinction between the quantum world and the world of everyday objects. A "classical" object is an object of everyday experience. It has, for example, a definite position and momentum, whether observed or not. A "quantum" object, such as an electron,

has a different status; it's an abstraction. Some properties, such as electrical charge, belong to the electron abstraction intrinsically, but others can be said to exist only when they are measured or observed. An observation is an event that occurs when the two worlds interact: A quantum-mechanical measurement takes place at the boundary, when a (very small) quantum object interacts with a (much larger) classical object such as a measuring device in a lab.

Experiments have steadily pushed the boundary outward, having demonstrated the double-slit experiment not only with photons and electrons, but also with atoms and even with large molecules consisting of hundreds of atoms, thus millions of times more massive than electrons. Why shouldn't the same laws of physics apply even to large, classical objects?

Enter Schrödinger's cat, the famous thought experiment beloved by pop-physics expositors and often deployed to wow (and cow) laymen by demonstrating the deep strangeness of quantum mechanics and the mental might of the Scientists who wield it. In fact, Schrödinger offered it as a *reductio ad absurdum* of the Copenhagen interpretation.

The experiment—buried in a lengthy 1935 paper, "The Present Situation in Quantum Mechanics"—asks us to imagine a sealed box with a tiny amount of radioactive substance, a Geiger counter for detecting its decay, a glass jar of cyanide, a mechanism that controls a hammer, and a cat. If the Geiger counter detects that the radioactive substance has decayed, it activates the hammer, which breaks the jar and poisons the cat. Spontaneous decay is a quantum event about whose occurrence we can make only probabilistic predictions. The amount of the radioactive substance is chosen so that the probability of its decaying within one hour is 50 percent. If we leave this sealed box to itself, what can we say about the radioactive substance, and hence the cat, when the hour is up?

If it's literally true, as the Copenhagen interpretation claims, that an unstable atom is in an "indeterminate" state—neither decayed nor undecayed—until an act of observation compels it to choose which, then the cat is also in an indeterminate state—neither alive nor dead—until we observe it. Someone who takes Copenhagen seriously, Schrödinger says, must say that the cat is neither alive nor dead until we open the box and that, if it is dead, opening the box is what killed it.

Schrödinger describes the thought experiment as a "quite ridiculous case," demonstrating that the attempt to make a principled, radical distinction between a quantum world and a classical world made no sense. Popular accounts don't always make this point clear—*wow!* has a market.

The orthodox account of what happens during a measurement

seems only to dig the hole deeper. In Schrödinger's wave mechanics an electron (or, in general, any quantum system one wishes to model) is represented by a *wave function*, which can be thought of as a kind of machine that evolves in time: If at any time we input to that machine a location, it returns as output (roughly speaking) a probability. We can set up the machine so that this probability represents the likelihood of finding an electron in that location at that time. The famous Schrödinger equation specifies how any wave function will evolve in time.

The probabilities returned by the wave function describe the results expected *if an observation were to occur*. But, according to the Copenhagen interpretation, the instant an observation does occur the situation somehow radically changes. The observation establishes a 100 percent probability that the electron is where we see it and a 0 percent probability that it is anywhere else. The evolution of the wave function stops obeying the Schrödinger equation. Instead the wave function is said to "collapse" by, instantaneously at all points in space, changing to represent these new probabilities. After the collapse, normal physics, following the dictates of the Schrödinger equation, resumes.

This is hard to make sense of, not least because in principle, as some argued, the Schrödinger equation should apply to everything—the electron, the measuring device, and presumably the physicist making the measurement—and measurements themselves cannot be some special category of causation. Becker quotes the late philosopher and mathematician Hilary Putnam: "Measurements are a subclass of physical interactions—no more or less than that.... 'Measurement' can never be an *undefined* term in a satisfactory physical theory, and measurements can never obey any 'ultimate' laws other than the laws 'ultimately' obeyed by *all* physical interactions." And "quantum mechanics must, if correct, apply to systems of arbitrary size."

In addition to the measurement problem, Becker emphasizes the unsettling possibility of events having effects that are not "local," such as an observation causing the wave function to collapse instantaneously everywhere in the universe. Contrary to popular belief, Einstein found nonlocality more problematic than the probabilistic nature of quantum mechanics. His theory of relativity postulates that all effects are local, that an event taking place *here* can't instantaneously affect something *way over there*. Causal influence can't spread faster than the speed of light.

In 1935, Einstein, along with Boris Podolski and Nathan Rosen, proposed a thought experiment—now called, after its three authors, the EPR paradox—that honed the question to a sharp edge. The argument has been reformulated in various ways, but here is its essence:

1. The Copenhagen interpretation asserts that a certain property of a particle—call it property X—doesn't exist before it's measured, and that the act of measuring X causes it to assume a definite value we could not have predicted before performing the measurement.

2. Quantum mechanics predicts—and experiment has confirmed—that we can create two particles that are "entangled" in the sense that measurements of property X will yield the same value for both particles— even though we won't be able to predict before measuring either particle what that value will be. This will be so even if, after the particles are entangled, they become separated and are located arbitrarily far apart.

3. Assertion 1 rules out the simple explanation of entanglement, which is that the particles possess values for X before they're measured and that entangling them gives X the same value in both particles (even if it's unpredictable).

4. If assertion 1 is accepted, it seems that only one possible explanation remains: The result of measuring X for either particle *creates* a value that is instantaneously communicated to its twin, giving it the value that subsequent measurement can disclose. Einstein would later famously call such nonlocal effects "spooky actions at a distance."

Einstein presents Copenhagen's true believers with two unpalatable possibilities. One is that quantum mechanics describes a world that is nonlocal. The other is that they are wrong about measurements causing particles to assume their properties, and therefore the existing formulation of quantum mechanics is incomplete—because it fails to represent the physical reality that particles have properties independent of observation. Loath to give up locality, Einstein concluded that quantum mechanics must be incomplete. Bohr published a reply that Becker says no one really understood, but whose mere existence most physicists took as proof that all must be well with the status quo.

World War II interrupted debates about foundations, and in its aftermath Big Science nearly finished them off. The pre-war funding of scientific research had been small, Becker writes, and little of it came from the government. By 1954, U.S. funding for basic research in the physical sciences had increased twenty-five-fold. Of this funding, 98 percent came from the military or defense-related agencies, which wanted applications, not research in foundations. Universities began to produce physicists on a scale that made it impossible to dwell on philosophical issues ill-suited to the large lecture hall, and textbooks began to ignore these questions. Physics was doing fine. Why fix what wasn't broken?

A concern with philosophy had been part of the education and culture of quantum mechanics' founders. For later generations that would no longer be true. Becker notes with dismay that some contemporary scientists with large public followings—Neil deGrasse Tyson, Lawrence Krauss, the late Stephen Hawking—make pronouncements about philosophy that can only be called "breathtakingly ignorant." (Krauss, I would add, does double duty as a village atheist. The laughably bad antitheist arguments in his book *A Universe from Nothing* make a case study in motivated reasoning.)

The second part of *What Is Real?*, necessarily more episodic than the first, profiles some "dissidents" from the quantum orthodoxy, including David Bohm, a tragic figure, and John Stewart Bell, who emerges as a hero.

David Bohm was ruined by politics. A rising star who did his doctorate under Robert Oppenheimer, he had as a student flirted with joining the Communist Party. As a result, Oppenheimer's attempt to hire him for the Manhattan Project failed; even though the project made use of his work, Bohm could not get a security clearance. Bohm was then recruited by Princeton, where he worked with Einstein and did important research in plasma physics and condensed-matter physics. In 1949, called before the House Un-American Activities Committee, Bohm refused to answer questions "because it might tend to incriminate and degrade me," and was later arrested for contempt of Congress. Though acquitted of any crime, he lost his job at Princeton and wound up in a kind of exile—in Brazil, then Israel, then England.

He was also victimized by someone else's mistake. Bohm had published a textbook on quantum mechanics in 1951 that conformed to the Copenhagen orthodoxy. But the next year, encouraged by Einstein, he developed and published an interpretation of quantum mechanics in which a particle *does* have a definite position, momentum, and so on prior to being measured. The uncertainty principle, however, still applies, and limits how precisely we can measure those values. Models of this kind are called "hidden variable" theories because they explicitly contain dynamic variables that, although determined in the world, are "hidden" from us in this sense. In Bohm's formulation, the wave function is a "pilot wave"—it guides a particle toward the places where, according to the probabilistic interpretation, it is more likely to be found. (This was actually a rediscovery of and improvement upon a suggestion made by Louis de Broglie in 1927.)

But it was widely, and gratefully, believed that in 1932 the great mathematician John von Neumann had already proven hidden-variable interpretations to be impossible and

the Copenhagen interpretation irrefutable. It was therefore assumed that Bohm's work must contain an error and could be safely ignored. Becker says that it was actually von Neumann who had erred. Grete Hermann, who caught his error, was not a physicist but a philosopher and mathematician. Because she was an outsider and a woman, says Becker, her discovery fell on deaf ears. Like Bohr's reply to Einstein, von Neumann's proof was accepted more on authority than on substance. (Eminent physicists have recently taken up again the debate over whether von Neumann erred or was misinterpreted.)

Bohm died in 1992, but interest in his work, including not only physics but neuropsychology and the philosophy of mind, has since revived.

John Stewart Bell was a respected particle physicist with a second string to his bow—"I am a quantum engineer," he once said, "but on Sundays I have principles." He found it scandalous to ignore the murky foundations of quantum mechanics. He also had a laconic sense of humor, once writing with a similarly skeptical colleague, "We emphasize not only that our view is that of a minority, but also that current interest in such questions is small. The typical physicist feels that they have long been answered, and that he will fully understand just how if ever he can spare twenty minutes to think about it."

The most detailed technical discussion in Becker's book, which should nonetheless be accessible to a conscientious lay reader, is his account of Bell's theorem, first presented in the 1964 paper "On the Einstein Podolsky Rosen Paradox."* The mathematical result is not in dispute, though there is a large, and somewhat contentious, literature on what it means.

As Bell wrote, the EPR thought experiment had been intended as "an argument that quantum mechanics could not be a complete theory," that hidden variables were required "to restore to the theory causality and locality." Bell devised an ingenious variant of the EPR thought experiment, then proved that the result predicted by quantum mechanics could not be explained by any theory that is local. Bell's finding raises a troubling question. Relativity insists that nature acts locally—that causes cannot travel faster than the speed of light, that there is no "spooky action at a distance." How could this fit with Bell's theorem?

Bell proposed actually performing his version of the EPR experiment to verify the prediction of quantum mechanics, so that the discussion would concern facts on the ground. But his seminal paper was almost lost to history. In 1964 he

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* Becker actually describes not Bell's proof but his own version of a version formulated by N. David Mermin.

was a visitor at the Stanford Linear Accelerator Center and, not wanting to obligate his host for publication charges, sent the paper to a fledgling journal that disappeared not long after publishing it.

Some years passed before a few physicists with the right mix of interests and experimental skill stumbled across the paper and proposed its first experimental test. John Clauser and Stuart Freedman managed to carry it out with little encouragement and much dumpster diving for equipment, publishing their result in 1972. The experiment confirmed the results predicted by quantum mechanics. Therefore, says, Becker, "quantum mechanics had survived.... and something awfully strange was going on in nature." But the experiment made little impression, and Clauser's doctoral advisor asserted—in a letter of recommendation, no less—that it was "junk science."

Aware that the spectrum of reactions to work on foundational questions ranged from indifference to hostility, Bell advised anyone without tenure not to work on them. It took another decade, Becker says, for such work to become more respectable.

Bell's original paper had noted a possible escape hatch in the experiment that Clauser and Freedman carried out—potentially sneaky forms of communication between the particle detectors that could be eliminated by arranging that certain switches on the detectors not be set until the entangled particles were already flying toward them at the speed of light. In 1982, an ingenious experiment by the French physicist Alain Aspect and his collaborators did just that. Subsequent theoretical and experimental work has refined Bell's theorem and the corresponding experiments, always with the same conclusion. The body of work inspired by Bell's theorem, notes an article in the *Stanford Encyclopedia of Philosophy*, poses "a baffling conceptual problem that at present confronts us: that the prima facie nonlocality of Quantum Mechanics will remain a permanent part of our physical world view, in spite of its apparent tension with Relativistic locality."

From the outset, Becker says, the predominance of the Copenhagen interpretation had much to do with personality—particularly Niels Bohr's. He was not only a great scientist; he was charismatic. For many he became a father figure, almost a cult figure, whom they wished to please and were reluctant to challenge. Bohr also had great institutional power: The Danish government created a research institute "with the sole purpose of giving Bohr an environment to work in," and many of the founding generation passed through its halls. For some, even the obscurity of Bohr's writings bolstered his reputation as a sage.

Becker says there was no "concerted, organized effort" to keep the

Copenhagen interpretation dominant, but, even if not organized in any formal sense, the response of the Copenhagen circle to opposition was often fierce. For example, the distinguished physicist Léon Rosenfeld, Bohr's closest ally, repeatedly pops up in the book as a kind of *consigliere* ready to beat down anyone who strays from orthodoxy—such as Dieter Zeh, who in 1970 originated a now flourishing project to understand how classical behavior (such as a seeming-but-not-actual collapse of the wave function) can emerge from quantum behavior. Zeh was protected by tenure, but, with Rosenfeld's lobbying, his career stalled and his students couldn't get jobs—"something," he said, "I will never be ready to forgive."

The experiment by Alain Aspect eventually helped to revive interest in the foundations of quantum mechanics, because its virtuosity led to speaking invitations at which he could explain its significance. Moreover, some of the philosophical props had been kicked out from under the Copenhagen interpretation. Logical positivism was long dead, and by the 1970s "scientific realism" began to dominate the philosophy of science—sophisticated accounts of the commonsense view that scientific theories provide an approximately true description of an independently existing world, one that is there even when we are not observing it. Realism, said Hilary Putnam, is "the only philosophy that

doesn't make the success of science a miracle." It is difficult now to find a philosopher of science who thinks that the Copenhagen interpretation makes sense.

Yet Copenhagen remains the orthodoxy among practicing physicists. Being first had advantages: It was what everyone was, and still is, taught—and the resulting practice has, after all, been a brilliant success.

One can't claim that Becker's narrative sheds dramatic new light on what science is like or how it is done. The pursuit of science has never been perfectly rational. Typical human motives, better and worse, play their part, albeit in an elevated and exotic setting. People seek money and power and prestige, and all of these are factors in deciding access to jobs and research funds. People build empires; they stake out positions and dig in.

None of that, Becker says, implies that the practice of science is merely the exercise of power or that its results are "bullshit somebody made up." Nor does it offer aid and comfort to advocates of pseudosciences like homeopathy, who try to portray themselves as bold, independent-minded dissenters from a stifling orthodoxy. The debates over quantum mechanics, though sometimes bitter, begin from fundamental agreement on the experimental facts and the practical success of the current theories.

He summarizes the state of quantum mechanics as "a wildly successful theory, an embarrassment of interpretations, and a major challenge in moving past our theory to the next one." The small but vigorous community doing work on foundations is less marginal than it used to be. The book's final section sketches some of its current research and concludes modestly that the wisest course at present is accepting a pluralism of interpretations, or "at least humility." "Quantum physics is at least approximately correct.... We just don't know what that means yet. And it's the job of physics to find out."

But to do that, says Becker, technical ideas about physics must take their place within a larger context. And here he quotes Einstein: "A knowledge of the historic and philosophical background" is necessary for a scientist to become free of the "prejudices of his generation from which most scientists are suffering. This independence created by philosophical insight is—in my opinion—the mark of distinction between a mere artisan or specialist and a real seeker after truth."

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