Early History of Bell's Theorem

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Bell's Theorem is one of the most profound results in physics of the twentieth century. Not only does it have a significant impact on natural philosophy and on the true meaning of quantum mechanics, it also has stimulated important and practical new research in quantum optics. In 1972 at the CQ03 Rochester conference, in response to a number of disturbing issues and challenges then raised by Ed Jaynes concerning the foundations of quantum electrodynamics, I introduced the quantum optics community to Bell's Theorem and a few of the associated mysteries manifest in quantum entanglement. Given the widespread belief that the foundations of quantum mechanics were then well understood, needless to say, my 1972 talks were then met with considerable skepticism. Eventually, however, the importance of Bell's Theorem gained full acceptance, so that now it is discussed in many recent quantum mechanics and quantum optics textbooks. Similarly, my first experimental test (with Stuart Freedman in 1972) of the Bell-Clauser-Horne-Shimony-Holt prediction has since then been repeated and confirmed literally dozens of times, and that prediction now provides a standard quantitative measure of entanglement. Moreover, the fundamental ideas underlying Bell's Theorem have been found to be sufficiently useful and important, that it is doubtful that the parallel conference ICQI-2001 would have occurred without them. This article recounts the important historical events behind the development of Bell's Theorem.

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Bell's Theorem provides us with a somewhat rare opportunity for experimental physics to answer questions in natural philosophy. Correspondingly, Abner Shimony and I open our 1978 review article, *Bell's theorem: experimental tests and implications*[1], with the sentence,

> *"Realism is a philosophical view in which external reality is assumed to exist and have definite properties, whether or not they are observed by someone."*

In that review we describe how the experimental evidence in hand in 1978 allowed us reasonably to assert that

> *"The conclusions* [from Bell's Theorem] *are philosophically startling; either one must totally abandon the realistic philosophy of most working scientists, or dramatically revise our concept of space-time."*

Despite numerous repetitions and extensions of the reviewed experiments and theory, along with considerable scrutiny of both since then, our 1978 conclusion appears to stand.

Bell's Theorem explicitly reveals highly peculiar and remarkable properties of quantum-mechanically "entangled" states that were previously not appreciated. Improved understanding of entanglement, in turn, has allowed it to be exploited on a wide variety of physical systems. As a result, Bell's Theorem has directly or indirectly fostered many spin-off practical applications and created whole new fields of study, including quantum cryptography, quantum communication, quantum computing, entanglement sharing, quantum cloning, etc. Its concepts are now common in undergraduate physics textbooks, as well as in a large body of scientific literature for the layman. One colossal recent spin-off proposal is by the US National Security Agency, who now envisions a quantum Internet that is absolutely secure against eavesdropping, and that is linked together by an international network of quantum computers, wherein quantum entanglement is then shared world-wide via a network of satellites. Thus, not only has Bell's Theorem deepened our understanding of quantum mechanics and the foundations of physics and natural philosophy, its full impact far exceeds the realm of natural philosophy, and has become an important heuristic tool for promoting revolutionary useful technology.

This note provides an overview of the history (through the early 80's) of the development of Bell's Theorem, along with its relation to the foundations of quantum mechanics. Prior to 1972, that development had proceeded quite independently from the study of quantum optics and quantum electrodynamics. However, the residual problems in quantum mechanics spilled over into quantum electrodynamics. In the late 60's and early 70's, Ed Jaynes had raised a number of related and disturbing parallel issues. Unlike many of quantum theory's founder's, however, Jaynes challenged the quantum optics community to provide hard experimental evidence to resolve these problems. In 1972 at the CQ03 Rochester conference, in response to Ed Jaynes' challenge, I brought these independent areas of study together, and introduced the quantum optics community to Bell's Theorem, its experimental tests, and a few of the profound associated mysteries manifest in quantum entanglement.

Acceptance of the importance of Bell's Theorem did not come easily. From the post-war years well into the 70's there was a widespread belief that the foundations of both quantum mechanics and quantum electrodynamics were then well understood. Needless to say, my CQ03 talks were met with considerable skepticism. That belief was so prevalent that it provided what then amounted to stigma against any challenges to or criticism of those foundations, and especially against any associated discussion of the notion of hidden variables in quantum mechanics. Nonetheless, quantum theory's founders had, in fact, left many significant gaps in the theory's conceptual foundations. More importantly, they also had promoted many incorrect, unfounded, and/or misleading conclusions. Much of the history behind Bell's Theorem involved filling in these gaps and correcting these errors, and furthermore, doing so in an unfriendly climate for a politically unreceptive audience. As a result of this stigma, much of John Bell's early work was published only as preprints and in

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[1] J. F. Clauser and A. Shimony, Rep. Prog. Phys. (1978) **41** 1881-1927.

an "underground" newspaper[2], whose circulation was limited to members of a "quantum-subculture", and that probably cannot be found in most physics libraries. Only much later, it was collected to form his book *Speakable and unspeakable in quantum mechanics*[3].

Eventually, however, the importance of Bell's Theorem gained full acceptance, so that now it is discussed in many standard quantum mechanics and quantum optics textbooks. Similarly, my first experimental test (with Stuart Freedman in 1972) of the Bell-Clauser-Horne-Shimony-Holt prediction has since been repeated and confirmed literally dozens of times, and that prediction now provides a standard quantitative measure of entanglement.

An unresolved issue left by the founding fathers

While quantum mechanics was being initially formulated, there were many discussions about its meaning, its self-consistency, and about whether or not it provides a complete description of physical reality. Albert Einstein's famous debate with Niels Bohr at the 1927 Solvay Conference was primarily focused on the self-consistency of quantum mechanics *vis-à-vis* Heisenberg's uncertainty principle and the Copenhagen interpretation. At the end of the debate, Einstein conceded that quantum mechanics is indeed self-consistent. However, he remained adamantly undeterred in his belief that quantum mechanics, as a framework for physics, is incomplete. In his view, additional "hidden parameters" are needed to supplement quantum mechanics, and thereby to provide a full and complete description of physical reality.

If quantum mechanics is an incomplete theory, then the probabilities that one calculates from it are similar to those defined by Bernoulli and Pascal, i.e. they are quantitative measures of ignorance. Via classical probability theory, probabilities are modeled classically as sampling frequencies, in a conceptual parallel to the sampling of colored balls with varying weights from an urn. That is, given a blind choice of a ball from an urn, one estimates, for example, the probability (i.e. the sampling frequency) for choosing a ball with say a prescribed color. Alternatively, since the balls have weight as a second distinguishing characteristic among them, one may also estimate a probability for obtaining a prescribed weight. If the ball's weights are somehow correlated with their colors, one may also estimate a joint probability distribution of weight and color for these balls. In classical probability theory, however, no one ever doubts the existence of some actual set of balls within the urn, immediately prior to a sampling. That set correspondingly still has some preexisting color distribution, weight distribution, and joint color and weight distribution, although these distributions' properties may or may not be known. Thus, even when, "weight" and "color" are not quantum-mechanically "commuting observables", Einstein still expected that probabilities arise within quantum mechanics in exactly the same way that they do in classical probability theory, i.e. through ignorance of details about the sampling process, and/or ignorance of details of the parent distribution.

The measurement of some characteristic feature of a quantum-mechanical system is, however, not quite as simple as simply weighing a ball and/or observing its color. During Einstein's initial discussions with Bohr, Bohr talked about a "physical disturbance" that occurs during a measuring operation that is caused by the measuring apparatus, itself. Given only a limited set of then-known apparatus choices, Bohr argued that this disturbance is unavoidable in a quantum-mechanical measurement. In effect, Bohr was saying that one can measure a ball's weight, or its color, but not both, and somehow a measurement of a ball's weight can change its color. Nonetheless, the existence of such a physical disturbance mechanism is fully consistent with Einstein's view, and does not contradict any classical

[2] *Epistemological Letters* (Association Ferdinand Gonseth, Institut de la Methode, Case Postale 1081, CH-2501, Bienne.) This newspaper was somewhat unique for its time, in that it openly proclaimed that the usual stigma against hidden-variable theories, and the like, was to be absent for publications within it.

[3] J. S. Bell, *Speakable and unspeakable in quantum mechanics*, (Cambridge Univ. Press, Cambridge, 1987).

probability concepts. In Einstein's view, random results occur here because one is ignorant about both the details of the physical disturbance mechanism and the details of the *à priori* probability distribution of any physical variable or set of variables being measured. If one somehow were given a hypothetical "improved" apparatus that produces only a negligible disturbance, then the preexisting value of a variable could be measured in a single trial, and the *à priori* parent probability distribution could be determined with a large set of trials. In effect, such a hypothetical apparatus can still weigh the ball without thereby changing its color. Equivalently, given such a hypothetical apparatus, one can then measure simultaneously the preexisting values of two non-commuting variables, along with their *à priori* joint probability distribution. Bohr's initial arguments thus offered nothing to Einstein to counter his notion about the origins of randomness within quantum theory, and Einstein continued to believe that the uncertainty principle sets only practical limits to measurement precision, and that it is not an inherent fundamental limitation to the precision of quantum measurements.

Bohr, however, also maintained that quantum mechanics is a complete theory. If so, then the randomness observed in experimental outcomes is something very different from the probabilistic behavior of events envisaged by Bernoulli and Pascal, and that the non-existence of said hypothetical apparatus is highly relevant. In Bohr's view, probabilities arise within quantum mechanics, not through ignorance of the internal workings of nature, but in a fundamentally new and different way. In a complete theory, there is no preexisting *à priori* joint distribution of actual values that one may attempt to measure, since that complete distribution is not prescribed by quantum mechanics[4]. The prohibition against peeking inside the urn prior to sampling is now one of principle, and is not due to a limitation of the availability of suitable measuring devices. Bohr thus insisted that there is no such distribution, nor are there any details available in principle about the disturbance mechanism. In such case, one may not ask questions about details of said distributions, nor may one ask questions about details of the measurement process for any given trial.

A moment's reflection will convince the reader that Bohr's insistence on the nonexistence of an *à priori* joint distribution of actual values, is also a denial of the preexistence of any one value in any one trial. In essence, Bohr is denying the whole realistic philosophy, as defined above in the opening paragraph[5]. The random results observed in a quantum-mechanical measurement of a specified parameter are somehow a manifestation of the lack of a preexistence of a value for that parameter. Einstein evidently recognized that Bohr was denying realism. At one point in their discussions, Einstein reportedly asked Bohr "Do you really believe that the moon doesn't exist when no one is looking at it?" A recognition that the basic difference between their opposite views is simply acceptance versus denial of realism, then clarifies both of their logically following conclusions. Einstein is often quoted as saying that "God does not play at dice!" A more cogent metaphoric response that describes Bohr's view is then "Of course not; God has no dice to play with!" The apparent nonexistence of the abovementioned hypothetical "improved" apparatus is a <u>necessary</u> condition for Bohr's view to hold. However, from Einstein's view, issues about the existence or nonexistence of such an apparatus are all red herrings, since the apparent nonexistence of said hypothetical apparatus is, by no means, a <u>sufficient</u> condition for a denial of realism. Neither man, however, expected that the whole realistic philosophy would become amenable to experimental testing. It is thus probable that if Bell's Theorem had been available to them, their discussions and debates would have been remarkably different, and that both men would have taken keen interest in Bell's Theorem!

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[4] The Wigner distribution is obviously not such a distribution, since it is not always non-negative.

[5] Max Jammer, in Sect. 4.2 of his book, *The Conceptual Development of Quantum Mechancs*, (McGraw-Hill, New York, 1966) argues that Bohr's denial of realism was in response to his being strongly influenced by the contemporary philosophers, Kierkegaard and Høffding.

Whether or not quantum mechanics is a complete theory is a nontrivial question, and its answer significantly determines the appropriate direction of future research in physics. Clearly, if quantum mechanics is incomplete, then a complete theory presumably exists that should be sought. On the other hand, if it is complete, then one may ask Einstein's questions, but one can never hope to find their answers, nor can one even hope to understand why it is vain to ask these questions.

Untidy legacy left by the founders of quantum mechanics

Soon after the Solvay Conference debates had ended, two heuristically important works were published. Both were written for the purpose of settling the completeness issue, once and for all. Unfortunately, the opposite result occurred, since these two works reached exactly opposite conclusions. In turn, these two were immediately followed by additional works that further sharpened their content.

One of these two important works is by John von Neumann (a former student of the mathematician David Hilbert). In his book, *Mathematische Grundlagen der Quantenmechanic*[6], von Neumann offers a "proof" of the non-existence of "hidden variables" within quantum mechanics. As a basis for this mathematical proof, he assumes that a linear operator (on a Hilbert space) is associated with each measurement apparatus, and that any measured result from this apparatus is an eigenvalues of this operator. He then proves that, within the mathematical structure of quantum mechanics, there cannot exist any additional (hidden) variables that then may be used to distinguish any further statistical differences between particles in the same "pure" state. Thereby, he shows that quantum mechanics is a complete theory. First, he concludes that

> "... *The only formal theory existing at the present time which orders and summarizes our experiences in this area in a half-way satisfactory manner, i.e. quantum mechanics, is in compelling logical contradiction with causality. Of course it would be an exaggeration to maintain that causality has thereby been done away with: quantum mechanics has, in its present form, several serious lacunae, and it may be even that it is false, although this latter possibility is highly unlikely, in the face of its startling capacity in the qualitative explanation of general problems, and in the quantitative calculation of special ones. ...* "

Further on, he strengthens this conclusion by stating that

> "*... we may say that there is at present no occasion and no reason to speak of causality in nature – because no experiment indicates its presence, since the macroscopic are unsuitable in principle, and the only known theory which is compatible with our experiences relative to elementary processes, quantum mechanics, contradicts it. ... Under such circumstances, is it sensible to sacrifice a reasonable physical theory for its sake?* "

Given the careful wording of these passages it should be noted in all fairness to von Neumann, that he does not prove, nor does he claim to prove the nonexistence of some other alternative theory that does not use his linear operator calculus, and whose predictions are equivalent to those of quantum mechanics. He just states that he believes that the existence of such an alternative theory is "highly unlikely". Unfortunately, many subsequent workers misinterpreted his work as a "proof" of the nonexistence of any such alternative theory.

The other one or the two important works is by Einstein, Podolsky and Rosen[7] (EPR). In their paper, they construct a hypothetical configuration that involves a pair of spatially separated but quantum-mechanically correlated particles. They further specify that these two particles are sufficiently far apart that it is impossible for the particles to physically interact with each other in any way. In this manner, EPR evade Bohr's "physical-disturbance"

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[6] J. von Neumann, *Mathematische Grundlagen der Quantenmechanic*, (Springer-Verlag, 1932). English translation: *Mathematical Foundations of Quantum Mechanics*, (Princeton University Press, 1955).

[7] A. Einstein, B. Podolsky, and N. Rosen (1935), Phys. Rev. **47**, 777-80.

argument. EPR contend that a measurement at one location on one particle of the pair cannot "physically disturb" the other particle, nor can it disturb the other particle's measurement outcome. In the above ball and urn analogy, for a given trial there are now two urns, each containing one ball. At one urn, the ball's weight is measured, and at the other, the ball's color is measured. Knowing say the *à priori* color correlation between the two balls, one can then simultaneously determine each ball's weight and color. Quantum mechanics, however, does not simultaneously specify these two parameters, and therefore it must be considered incomplete. Corrèspondingly, it then must be supplemented with "hidden parameters" that provide such a specification for it to become complete.

Bohr then offered an important rebutal[8] to EPR that further complicated the saga. It significantly diminished the elegance of EPR's careful reasoning by its providing an important counter-argument. Bohr therein contends that, depending on how one words the "completeness" question, EPR's configuration can be viewed in a manner that escapes their conclusion. While many physicists view Bohr's arguments as a "refutation" of EPR's arguments, in fact, Bohr simply shows that his own view may be held consistently, even for EPR's configuration. First he concedes a lack of full generality to his own earlier arguments regarding a (presumably local) physical disturbance of either particle by its associated apparatus. Thus, he generalizes his earlier idea of a local physical disturbance now to include a possible global disturbance to the two-particle composite system by the two-component composite apparatus. If either of the component apparatuses that comprise the global apparatus is changed, then the net global disturbance by the composite apparatus is also changed. Since this global disturbance interacts with the composite two-particle system, then the predicted outcome for the joint measurement is also changed.

To anyone intent on obtaining a microscopic view of details of this interaction process, then action-at-a-distance clearly seems to be necessary for Bohr's global disturbance to occur. Bohr's argument is more readily understood, once that one recognizes that it is based on his denial of realism. That is, it is impossible to physically disturb something that doesn't exist! Thus, if there is no physical-space description of either the quantum mechanically entangled individual systems or of the actual disturbance process, then the required associated existence of non-local action-at-a-distance is a non-issue. The objectionable aspect of Bohr's description (action-at-a-distance) is then, consistently with Bohr's assertion of completeness, simply nonexistent, by definition!

To an avowed realist (such as Einstein), Bohr's "refutation" of the EPR argument seems like simply playing with words. Thus, Einstein remained adamantly unconvinced by Bohr's argument, saying,

> *"To believe this is logically possible without contradiction, but it is so very contrary to my scientific instinct that I cannot forego the search for a more complete conception."*

Reportedly, Einstein was also unconvinced by von Neumann's argument, but never published a critique of it.[9] Thus, despite the aims of von Neumann (and Bohr) and of EPR, their antipodal conclusions left the completeness issue in a very untidy state.

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[8] N. Bohr, Phys. Rev. **48**, 696 (1935); Nature **136**, 65 (1935).

[9] Unfortunately, there does not appear to be any evidence that von Neumann and Einstein (whose offices at Princeton were just down the hall from each other) ever directly discussed the validity of von Neumann's "completeness proof". However, Abner Shimony relates a story, told to him by Peter Bergmann, about a time when he (Bergmann) asked Einstein for an opinion of this proof. Reportedly, Einstein was quite familiar with it. In response to Bergmann, he fetched and opened von Neumann's book, and then pointed to one of von Neumann's assumptions, upon which the proof is based. He then said that he saw absolutely no reason to believe that this assumption should hold in general for all alternative theories. Sadly, Einstein never published his criticism of von Neumann's proof. Evidently, the assumption that he then fingered is the same one (linearity) that is later criticized by Bell in his 1965 paper, and found there to be untenable in general.

Additional related comments about EPR were then offered by Erwin Schrödinger[10] and by Wendell Furry[11]. They both considered what would happen if the quantum-mechanical correlation between the particles somehow ceased to exist, once the two particles became sufficiently separated. Both men arrive at exactly the same conclusion. If this were to happen, then the specific experimental predictions by quantum mechanics would be altered significantly. Schrödinger's paper is also noteworthy in that it introduces the term, "entanglement", into our vocabulary for describing the two-particle correlation. Interestingly, these two authors also offer exactly opposite personal impressions of entanglement. Schrödinger calls entanglement "sinister", and views its presence as indicating a fatal flaw in quantum mechanics. Furry, on the other hand, views these result as indicating that any modified theory that omits entanglement must be immediately viewed as wrong, since quantum mechanics is "obviously" a correct theory.

The neglected experimental foundations of quantum mechanics

The untidy legacy left by the above authors was particularly acute, since neither side attempted to justify its position with hard experimental data. It was automatically assumed by them that, without any doubt, quantum mechanics is "obviously" the correct theory[12]. Indeed, none of the above authors ever cite any experimental evidence at all to back up their arguments. Instead, hard experimental evidence is replaced in the above arguments by a simple reliance on the self-consistency of the theory, whereupon only *Gedankenexperiments* are then needed for these arguments to proceed. Thus, it seems that all of these authors believed in what I will call here the "standard religion", that may be summarized as follows:

> *If a theory is self-consistent and elegant, and if it explains a significant body of experimental data that are inconsistent with previously held theories, then the theory must be accepted as gospel. Correspondingly, the theory needs no further testing, even in areas where its predictions may seem to be surprising and/or paradoxical.*

In later years, the phrase "it needs no further testing" became reinterpreted to mean, *it is sacrilege to even suggest that further tests should be performed.*

Said "religion" follows, in turn, directly from the above-quoted passages by von Neumann. Unfortunately, von Neumann's work evidences a total lack of any direct contact with any actual experiments. His "proof" is based only on the self-consistency of the operator calculus used by quantum mechanics. Throughout his book, he cites only one experiment, that by Compton and Simon[13], and then only to refute the short-lived Bohr- Kramers-Slater[14] predecessor theory. But after all, von Neumann was a mathematician. Unfortunately and correspondingly, his totally mathematical approach leaves an experimental physicist in the very awkward position of having no specifically defined experiment (or set of experiments) on which quantum mechanics is purportedly based! Needless to say, von Neumann also does

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[10] E. Schrödinger, Proc. Camb. Phil. Soc. **31**, 555 (1935).

[11] W. H. Furry, Phys. Rev. **49**, 393 (1936); Phys. Rev. **49**, 476 (1936). The second paper emphasizes the differences between his and Schrödinger's views of this result.

[12] Whenever I asked anyone during the '60s about the experimental justification for using quantum mechanics to describe entangled-state systems, I was told that its accurate prediction of the spectrum of helium proved it to be universally correct for all such systems. However, not much sophistication is required (especially now in hindsight) to recognize that the energy levels exhibited by two electrons bound in a helium atom do not provide, by themselves, a very unconvincing example of the full range of remarkable phenomenology exhibited by an EPR system.

[13] A. H. Compton and A. W. Simon, Phys. Rev. **26**, 289-299 (1925). This experiment is a more precise repetition of an earlier experiment by W. Bothe and H. Geiger, Zeits. für Physik, **26**, 44 (1924). Schrödinger, however, subsequently found a semi-classical explanation of this experiment, and went on to propose an important relevant experiment. See "Splitting photons?".

[14] N. Bohr, H. A. Kramers, and J. C. Slater, Phil. Mag. [6] **47**, 785-802 (1924).

not specify any allowed error bounds for these unspecified experiments, which, if violated, might overturn or help confirm the far reaching conclusions that he draws.

Von Neumann is not alone in this regard. Bohr, EPR, Furry and Schrödinger also cite no actual experiments. Perhaps these authors all tacitly accepted and promoted the aforementioned religious belief because they were all theorists and felt that correspondingly they should act as clergymen. At the very least, they clearly show negligible respect for experimental physics as the final arbiter in physics. To their discredit, none of these authors even suggests in the above-cited works that experimental testing of the predictions for their *Gedankenexperiments* is in order. Perhaps their tacit reliance on and promotion of this religion was due to the fact that in the thirties, experimental physics was still in a rather crude state of development, and theorists then had no choice but to base their arguments on *Gedankenexperiments*, and then to stop at that point, because realizations of these *Gedankenexperiments* were well beyond the technology of the day.

Whatever justification these men may have had in this regard is perhaps unimportant now. Given their ecumenical leadership, and especially given Bohr's strong leadership, the net legacy of their arguments is that the overwhelming majority of the physics community accepted Bohr's "Copenhagen" interpretation as gospel, and totally rejected Einstein's viewpoint. Also, given von Neumann's daunting intellect, his proof was held to be sacrosanct, even to those who had never even perused its details. Moreover, since its details were rarely perused, over time, von Neumann's proof became commonly (and incorrectly) misinterpreted to imply that

> *No theory based on hidden variables is possible that gives the same experimental predictions as quantum mechanics!*

Correspondingly, the standard religion became hardened to hold that

> *Quantum theory is so successful, it is simply inconceivable that its foundations and/or predictions can be unsettled, misunderstood, or, God forbid, in error. Therefore, it warrants no further testing!*

as a nearly direct rephrasing of von Neumann's above claims. Unfortunately, this religious belief was held dogmatically by most physicists well into the '70's.

Challenging the common wisdom

Given the effectively religious impact of Bohr and von Neumann's teaching, little progress was made during the subsequent two to three decades towards any further development of our fundamental understanding of the meaning of quantum mechanics, or especially towards a resolution of the completeness problem. Given that the religion warrants against further direct testing of quantum theory's foundations, negligible effort was made in this area also, even after the state-of-the-art of experimental physics had advanced to the point where many new direct tests had become possible.

This neglect of the theory's foundations is quite unfortunate, in my opinion, because (as will be documented below) a large number of "killer" details have frequently slipped through cracks that exist between a theorist's view of a fundamental experiment and an experimentalist's view of that same experiment. Moreover, such "killer" details are frequently sufficiently important so as to totally invalidate many of the conclusions that are then commonly drawn from these experiments, even though those conclusions have become entrenched as an inseparable part of the physics community's common wisdom. John Bell's article, *Against Measurement*[15], is likewise critical of our community's blind acceptance of this religion, and is quite outspoken in suggesting that outstanding problems remain in our understanding of quantum theory, and that further careful study of the theory's foundations is warranted. Fortunately, within our community there were a few physicists who actually enjoy(ed) challenging this common wisdom. These are the iconoclasts of our profession.

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[15] J. S. Bell, *Phys. World*, Aug. 33-40 (1990).

The next important step in the saga occurred in 1950, when David Bohm (a former student of Oppenheimer) published a new textbook, *Quantum Theory*[16]. The book's treatment of the underlying conceptual foundations of quantum mechanics is quite similar to other textbooks of the era. In it, Bohm follows von Neumann and expresses strong doubts about the existence of hidden parameters within quantum mechanics. However, he stops short of asserting that their existence is impossible. While he does cite von Neumann's book as a reference, he does not mention its "impossibility proof". Instead, he discusses the completeness of quantum mechanics via reference to the EPR configuration, and argues for completeness by defending Bohr's criticism of EPR's argument. However, in the process, Bohm adds one very important new idea to the discussion that significantly sharpens both the EPR configuration and its associated argument.

Originally, EPR had discussed entanglement in terms of the continuous variables, momentum and position. In this book, Bohm reformulates the configuration in terms of entangled discrete-state systems, i.e. in terms of the spin-component correlations of two spin-1/2 atoms in a spin-singlet entangled state. In a historical context, this sharpening is crucial for the work that follows.

One problem with writing a textbook is that it makes you think carefully about the book's subject matter. Following publication of this book, Bohm broke with tradition in two important ways. First, with his new student Yakir Aharonov, he published a paper[17] where, for the first time, they sought real experimental evidence for the peculiar properties of entanglement that are exhibited by the EPR configuration. Using a two-photon analogy of the two-spin configuration outlined in Bohm's book, they noted that the polarization correlation of gamma-ray pairs emitted during positronium annihilation is a real-life example of EPR's configuration. That correlation's magnitude had, in fact, recently been measured by Wu and Shaknov[18] at Columbia Univ. Bohm and Aharonov then point out that the correlation's measured magnitude agrees with quantum theory's prediction (with entanglement present), and significantly disagrees with the prediction by the Schrödinger-Furry hypothesis (with entanglement absent, once the particles become significantly separated). By publishing this paper, Bohm and Aharonov transcend the "standard religion", and actually consult an experiment! They also prove here what Furry could only assume but could not prove, i.e. that the Schrödinger-Furry hypothesis cannot be held as a correct description for nature.

Bohm, however, was just beginning his iconoclastic endeavors. Despite the fact that in his book he expresses strong doubts that hidden variables might exist, he now adds extreme untidiness to the existing situation by formulating a new "causal" theory that actually incorporates hidden parameters within it[19]. Given the stigma against such work, few people took Bohm's paper seriously. One who did take it seriously was Louis deBroglie. Inspired by this work, deBroglie reentered the arena and published a whole series of books[20] and papers promoting a similar theory. (Bohm and deBroglie's theories are both examples of what are commonly referred to as "pilot-wave theories".) The state of untidiness surrounding the issue of quantum theory's completeness had now blossomed to become a real mess, let alone a now seriously important skeleton in quantum theory's closet. Von Neumann's "proof" purportedly asserts that these theories cannot exist. But they obviously now do! Something clearly must be wrong, somewhere. Aware of von Neumann's "impossibility proof" Bohm attempts to

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[16] D. Bohm, *Quantum Theory*, (Prentis Hall, Englewood Cliffs, NJ, 1950).
[17] D. Bohm and Y. Aharonov, Phys. Rev. **108**, 1070 (1957).
[18] C. S. Wu and I. Shaknov, Phys. Rev. 77, 136 (1950).
[19] D. Bohm, Phys. Rev. **85**, 169 (1952). See also, D. Bohm and J. P. Vigier, Phys. Rev. **96**, 208 (1954). Fortunately, this work was published before the APS policy had been formulated. Presumably, it would have not passed muster under that policy.
[20] L. deBroglie, *Nonlinear Wave Mechanics*, (Elsevier, Amsterdam, 1960); *Ondes Electromagnetiques et Photons*, (Gauthier-Villars, Paris, 1968); and *Introduction to the Vigier Theory of Elementary Particles*, (Elsevier, Amsterdam, 1963).

clean things up a little, and offers an argument as to how von Neumann's proof may not apply to his theory.

John Bell unravels the confusion

The stage was finally set for John Bell to enter the picture to begin unraveling much of the accrued (but largely ignored) mess. While both von Neumann's and EPR's arguments are directed towards settling the issue about the possibility (or impossibility) of hidden variables within quantum mechanics, ironically Bell now shows, in a sense, that both arguments are effectively "wrong", at least in terms of their effective conclusions.

Bell first critically looked at von Neumann's argument. Given the existence of Bohm and deBroglie's theories, von Neumann's conclusion (or at least his perceived conclusion) obviously must be wrong! Bohm had given one argument as to how that might be so. However, Bell is neither very complimentary towards nor impressed by Bohm's counter to von Neumann's argument. In a masterfully written article that was published in a 1965[21], Bell scrutinizes von Neumann's requirement for a linear superposability of eigenvalues along with the linear superposability their associated operators. He then points out that this assumed requirement has no *à priori* reason to hold, in general, for a hidden-variables theory, especially when different operators correspond to physically different measuring apparatuses, and when these eigenvalues are also to be taken as the possible measurement outcomes for the associated apparatus. Furthermore, without this linearity assumption, von Neumann's argument neither obtains, nor constrains hidden-variable theories. Correspondingly, hidden variables are then no longer found to be "impossible" for alternative theories (to quantum mechanics) by von Neumann's argument.

At that time, many physicists who had looked carefully at these issues generally put much more reliance on von Neumann's rigorous mathematical argument than on Bohr's somewhat vague philosophical argument in casting their votes against hidden variables. However, given that von Neumann's argument can no longer be relied upon, Bell next wondered if EPR's precisely worded argument might be relied upon instead. Given that Bohm and deBroglie's theories had provided good test cases for von Neumann's argument, Bell (in his 1965 paper) offers a generalization of these theories to allow them to include the two-particle case needed by the EPR configuration. Bell's generalization does not follow exactly along the lines of what Bohm and deBroglie had wanted.[22] In fact, Bell found that no matter how hard he tried to formulate a two-particle generalization of these theories while simultaneously maintaining agreement with quantum theory's predictions, he could only succeed by incorporating non-local interactions between the separated particles into the generalization. But it is just these non-local interactions that EPR reasonably had insisted cannot and do not exist, given the arbitrarily wide separation of the two particles. Given Bell's difficulties in generalizing these theories in a purely local way, he then began to speculate that this difficulty might be generic to any possible hidden-variables generalization operating in the EPR configuration. Following this line of thinking further, Bell discovered an astonishing new result. This result (along with its many subsequent generalizations by others) is (are collectively) now referred to as Bell's Theorem. Bell evidently considered his new result so important that he published it in 1964[23], before publishing his 1965 review of von Neumann's impossibility proof.

Recall that EPR had argued earlier that considerations of their configuration show that hidden variables must exist. With supreme irony, Bell shows in his 1964 paper that further considerations of EPR's configuration indicate that exactly the opposite is true!

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[21] J. S. Bell, Revs. Mod. Phys. **38**, 447 (1965).

[22] DeBroglie had earlier admitted to having encountered serious difficulties in generalizing his theory to describe more than one particle, and in affecting a transition from the configuration space in which quantum theory is formulated, to physical space, in which his and Bohm's theories are formulated.

[23] J. S. Bell, Physics, **1**, 195 (1964).

Therein he proves that for Bohm's two-spin configuration, the prediction by any deterministic local hidden variable theory for the polarization correlation coincidence rate's normalized angular dependence will necessarily be different from that rate's angular dependence predicted by quantum theory. Quantum mechanics predicts that the dependence will be precisely sinusoidal with unit amplitude. Bell's paper shows that for any deterministic local theory based on hidden variables, this dependence must be strongly non-sinusoidal if it has at least one point with the maximal correlation value that is required by quantum theory.

Theoretical truth versus experimental truth

Initially, Bell expected that his new result would be adopted as a straightforward replacement for von Neumann's argument. With this expectation, however, he initially fell into the trap of accepting the "standard religion". His 1964 paper cites no experimental data as to what the observed dependence actually is. Of course, he couldn't cite any such data, because none had yet been measured. Still, he appears to assume tacitly in that paper that experimental evidence for this rather dramatic prediction is already in hand. After concluding that quantum mechanics and a theory with local hidden variables give different statistical predictions, he further states

> *"Of course, the situation is different if the quantum mechanical predictions are of limited validity. Conceivably they might apply only to experiments in which the settings of the instruments are made sufficiently in advance to allow them to reach some mutual rapport by exchanging signals with velocity less than the speed of light. In this connection, experiments of the type proposed by Bohm and Aharonov, in which the settings are changed during the flight of the particles, are crucial."*

Bell's wording of this quoted passage has been very carefully chosen. Its converse conclusion is that it is correspondingly inconceivable that the quantum-mechanical predictions do not correctly predict experiments with static instrument settings, i.e. that the quantum-mechanical predictions for static instrument settings are indeed correct, and have been verified experimentally to be correct. While the passage does not actually assert that this converse conclusion holds true, it strongly suggests that it does. Thus, the passage tacitly implies and gives the misimpression that the existence of hidden variables (at least in situations with static analyzers) is, *ipso-facto*, refuted by his proof.

Nonetheless, Bell hedges his bets. Given that his arguments only consider the predictions for non-relativistic quantum theory, he appears to suggest that new keys to the hidden-variables mystery will be revealed only via considerations of relativistic corrections to quantum theory. Correspondingly, he goes on to promote Bohm and Aharonov's second-generation experiment with rapidly changed analyzers. However, such a proposal is clearly silly, unless one acknowledges the existence of first-generation experimental results with static analyzers. He was clearly bluffing, and did not really know, at this point, what the experimental status of his result really was. I personally will claim much of the credit for calling his bluff.

In the late sixties I was a graduate student at Columbia Univ., and was struggling, trying to understand quantum mechanics. I had read EPR's paper, and also had read Bohm's and deBroglie's work. While I had difficulty understanding the Copenhagen interpretation, the arguments by its critics seemed far more reasonable to me at that time. Since quantum mechanics is formulated in a configuration space, it then provides no physical-space model to consult, whereupon it is then difficult to visualize what might actually be happening, especially in a two- (or more-) particle system. I found Bohm's and deBroglie's works refreshing, since they do give real physical-space models of what is happening. I also found EPR's arguments much more persuasive than Bohr's, and was only vaguely aware of von Neumann's "proof" at this time. Hidden variables thus seemed (to me then) to be a perfectly logical solution to the problem[24]. By holding that politically incorrect opinion, however, I was then certainly branded as a heretic by many, and undoubtedly as a quack by others.

Then, I read Bell's 1965 and 1964 papers. Given my familiarity with Bohm and deBroglie's work, I was not particularly surprised by Bell's 1965 result. On the other hand, once I comprehended what the 1964 paper actually said, I was astounded by its result. However, I was not yet willing to accept the paper's far-reaching implications until I finally saw some experimental evidence that decided between its two significantly different predictions. Since Bell's paper is conspicuously vague on the experimental status of its prediction (but crystal clear on everything else), I suspected that Bell was bluffing and started to search through the literature for experimental results. I found none, other than that by Wu and Shaknov.

With regard to experimental testing, I saw at least one serious problem with Bell's 1964 result. That problem is that Bell's form for his result applies only to his idealized configuration, and thus cannot be directly tested experimentally. Bell (being a theorist) assumes an ideal apparatus and an ideal two-particle entangled-state preparation. However, his proof then relies on this idealization, and further uses it to show that a perfect correlation between the two particle's polarizations occurs for at least one setting of the polarization analyzers. Unfortunately, even an infinitesimal departure by the considered system from this idealized behavior then invalidates the applicability of Bell's proof to it, since then, the required single point with a maximal correlation no longer exists. Correspondingly, his result then applies only to ideal systems and not to real ones! (Damn theorists!) Obviously, in any real experiment, the required perfect correlation cannot and will not obtain.

Does my observation here then imply that Bell's exciting new result is no more applicable to the completeness problem in real-life situations than is von Neumann's result? I thus asked myself whether or not Bell's assumption of an idealized apparatus and system is, in fact, necessary for an alternative proof of his Theorem to obtain. Looking further, I found that this assumption can indeed be avoided. Thus, I succeeded in deriving a new more general form for his result that then can be tested experimentally. Thus holding an experimentally testable prediction, I then returned to the question about the result's experimental status. It became immediately clear that no definitive conclusion then obtains when this testable prediction is compared with the data available from the Wu-Shaknov experiment. Moreover, I found that it is straightforward to build a simple *ad-hoc* hidden-variables counter-example that yields predictions that are exactly the same as those from the quantum-mechanical prediction for the Wu-Shaknov experiment. Thus, I concluded that something beyond a simple extension of the Wu-Shaknov experiment is needed for a valid experimental test. What can it be?

While I was still searching for an appropriate experimental test of these new predictions, I gave a seminar on Bell's Theorem to Dan Kleppner's group at MIT. Dave Pritchard's thesis work in this group involved doing crossed-beam scattering experiments with alkali metal atoms. I suggested to Pritchard and Kleppner that 90° - COM scattering is a possible method for producing Bohm's two-particle spin-singlet entangled state, and that their experiments might be reconfigured thusly to allow measurement of the polarization correlation of the scattered atoms as a test of the Bell's Theorem prediction. A newly arrived postdoc in the group, Carl Kocher, attended the seminar. For his PhD thesis at UC Berkeley, he had just finished performing an experiment with Gene Commins, wherein they had measured the polarization correlation of photon pairs emitted in an atomic cascade[25]. My attention was called to this experiment during questions that followed my talk, whereupon I then realized that an experiment similar to it could provide a far easier and much better test. Unfortunately, Kocher and Commins had only measured two-photon coincidences at 0° and 90° relative analyzer orientation. As per Bell's assumption, these data points are located

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[24] John Bell confesses in the preface to his book, *Speakable and unspeakale in quantum mechanics*, to being similarly enamored with these features of pilot-wave theories.

[25] C. A. Kocher and E. D. Commins, Phys. Rev. Lett., **18**, 575 (1969).

exactly where the two theories can give the same prediction. Also, as per my own generalization of Bell's result to provide an actual experimental prediction, I noted that their polarizer efficiencies were nowhere high enough for the experiment to be conclusive, in any case. Thus, their experiment provides no data at all for a test of hidden-variable theories. What is clearly needed at that point are data taken at angles intermediate between their measured points with an experiment that is similar to theirs, but that uses much better polarizers.

Finally, I decided to call Bell's bluff. I wrote letters to Bell, Bohm and deBroglie, asking all three of them (a) did they know of any experiments testing the result, and (b) did they agree that a repeat of the Kocher-Commins experiment with improved polarizers at intermediate angles would be convincing, and (c) how did they view the importance of such tests[26]. All three courteously responded and replied NO to (a) and YES to (b). Bell was particularly enthusiastic about the idea, and simultaneously also revealed his own agnostic religious beliefs about quantum theory. In response to (c), he said

> *"In view of the general success of quantum mechanics, it is very hard for me to doubt the outcome of such experiments. However, I would prefer these experiments, in which the crucial concepts are very directly tested, to have been done and the results on record. Moreover, there is always the slim chance of an unexpected result, which would shake the world!"*

The Vietnam War dominated the political thoughts of my generation. Being a young student living in this era of revolutionary thinking, I naturally wanted to "shake the world". Since I already believed that hidden variables may indeed exist, I figured that this was obviously the crucial experiment for finally revealing their existence. But if they do exist, then quantum mechanics must be verifiably wrong here, with its error having gone undiscovered heretofore. However, since there was virtually no experimental evidence then available for this configuration (plus or minus), that "slim" possibility could not be discounted. To me, the possibility of actually experimentally discovering a flaw in quantum mechanics was mind-boggling. Thus, I drafted an abstract for the Washington DC Spring Meeting of the American Physical Society proposing this experiment[27]. Immediately as it appeared in print in the APS Bulletin, I received a phone call from Abner Shimony (a former student of Eugene Wigner). He said that he and his student Mike Horne had come to the same conclusions[28]. Their pursuit of experimental evidence had led them to Frank Pipkin at Harvard, who had just performed an experiment that was similar to the Kocher-Commins experiment, in that it observed two-photon coincidences from an atomic cascade, but that did not include any polarization analysis. Pipkin's PhD student, Dick Holt, was now setting up to perform the experiment that I had just proposed. After comparing notes, we all agreed to coauthor a Phys. Rev. Letter formally presenting our conclusions[29]. Our collaborative effort is now commonly referred to by the initials, CHSH, with the paper's experimental prediction contained in what is now correspondingly known as the CHSH inequality. In the process of writing this paper, Abner, Mike and I forged a long lasting friendship that was to spawn many subsequent collaborations.

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[26] According to David Wick [*The Infamous Boundary – Seven Decades of Controversy in Quantum Physics*, Birkhäuser, Boston, 1995], my 1969 letter was the first response to his 1964 paper that Bell had received

[27] J. F. Clauser, Bull. Amer. Phys. Soc. **14**, 578 (1969).

[28] Shimony's name was already familiar to me when he called, because I had earlier read some of his work on the "measurement problem" in quantum mechanics. This problem then leads to the Schrödinger's-Cat and Wigner's-Friend paradoxes, that are another untidy aspect of the foundations of quantum mechanics. Both paradoxes result from the peculiar properties of quantum entanglements.

[29] J, F, Clauser, M. A. Horne, A. Shimony, and R. A. Holt, Phys. Rev. Lett., **23**, 880 (1969). This paper first coined the term "Bell's Theorem".

Beware of the experimentalists lurking about

Upon receiving my PhD from Columbia, I moved to UC Berkeley to work in radio astronomy as a postdoc for Charlie Townes. Since Gene Commins was also at UC Berkeley, I suggested to both Townes and Commins that they allow me to resurrect the Kocher-Commins experimental apparatus, and to use it to test the CHSH-Bell prediction. Townes was intrigued by the idea, took it seriously, and offered half of my time to this project, while the other half was to be spent on radio astronomy. Commins offered Stuart Freedman, his new graduate student, to work with me on the project. Said "resurrection" actually involved building a whole new mammoth-size apparatus. Fortunately, Townes was very tolerant as my radio astronomy projects languished. Stu and I published our results in 1972[30], and Stu got his PhD with this experiment.

Our results agree exactly with the quantum-mechanical prediction, but exclude theories based on local hidden variables by about five standard deviations. This experiment is noteworthy in that it is the first to actually be able to draw such a conclusion! Its count rate was so low that the results are effectively unchanged, even if "accidental" (background) coincidences are not subtracted from the totals used in a comparison with the CHSH inequality.

Meanwhile, in competition with our experiment at Berkeley, Dick Holt and Frank Pipkin at Harvard had been pursuing their parallel experiment. It was similar to ours, except that it was much smaller and used a different atomic species and a different kind of polarizers. While our experiment was still in progress, they actually got the first results. Unfortunately, their results disagreed with the quantum-mechanical prediction, but did agree with the local hidden-variables prediction by the CHSH inequality. They decided to be cautious, however, and not to publish their results until they saw what our experiment at Berkeley revealed. Once we announced our results, they then opted not to publish theirs formally. Instead, they distributed an informal preprint that outlined what they had done and observed. In the end, they concluded that their result was due to an underlying systematic error.

By declining publication of their experimental results, they left ours as the only published results available. Given the importance of the issues at hand, our own results obviously needed confirmation. Obviously, I was no longer actively working in radio astronomy at this point. Fortunately, Howard Shugart allowed me to continue my work at UC Berkeley as a member of his atomic-beams group. Following my photon-splitting experiment (described below) I proceeded to repeat Holt and Pipkin's experiment with only a few minor modifications. Again, my new result[31] agrees with quantum theory's prediction, but significantly disagrees with the CHSH-Bell prediction. I also went further and tested the predictions of quantum theory for the circular polarization correlation[32]. These results also agree with quantum theory's prediction.

It should be noted that when we began our experiments at Berkeley, tunable lasers were not yet commercially available. Thus, all of the Berkeley experiments were done without their benefit. As a result, the count rates were very low, and the experiments were very slow and tedious. Finally, Ed Fry and his student Randal Thompson[33] performed a test of the CHSH prediction using one of the first commercially available dye lasers. Given the short counting periods afforded by their use of this laser, they could more readily search for possible systematic errors. Their experiment confirmed our "Berkeley" results, but disagreed with Harvard's results. The score was now three to one, in favor of quantum mechanics. That score was sufficiently convincing to allow Abner Shimony and me to draw the above-described conclusions in our review paper [Ref. 1].

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[30] S. J. Freedman and J. F. Clauser, Phys. Rev. Lett. **28**, 938 (1972).
[31] J. F. Clauser, Phys. Rev. Lett., **36**, 1223, (1976).
[32] J. F. Clauser, Il Nuovo Cimento, **33B**, 740 (1976).
[33] E. S. Fry and R. C. Thompson, Phys. Rev. Lett. **37**, 465 (1976).

Generalization of the Bell and CHSH results to constrain local realism and space-time

While I was actively pursing my experimental efforts at UC-Berkeley, in parallel with that work, I also pondered some remaining details that still had not yet been addressed by CHSH and Bell. There were then at least four important unanswered questions:

(1) What are the fundamental assumptions underlying Bell's Theorem?
(2) What is the affect of the large number of unobserved particles (especially when the particles are optical photons) associated with a finite detector solid-angles and finite detector efficiencies? Also, given unobserved particles, what are the requirements for an experiment's design that then still allow its results to provide conclusive arguments to be made regarding the truth or falsity of the fundamental assumptions that, in fact, underlie Bell's Theorem?
(3) If these experimental requirements cannot readily be met in practice, then what is the least objectionable supplementary assumption that one can make to allow reasonably convincing inferences to be drawn from experiments that, for example, can be performed with optical photons?
(4) Also, if these requirements are not met in practice, is there at least one counterexample consistent with the fundamental assumptions underlying Bell's Theorem for experiments performed with say optical photons? Unfortunately, if such a counterexample exists, then only an experiment performed within these limits can be considered to be fully conclusive as a test of the fundamental assumptions underlying Bell's Theorem.

Mike Horne and I addressed and answered all four of these questions in our 1974 paper[34]. That paper is now commonly referred to by the initials, CH.

Regarding question (1), it may seem surprising and strange, at this point in the development, that the fundamental assumptions underlying Bell's Theorem were still not yet well defined. As a parallel problem to the gap between Bell's 1964 result and the CHSH result, this lack of definition again arises from differences between the idealized configuration considered by Bell, and the realizable configuration considered by CHSH. Here and elsewhere, it represents an example of crucial details falling through a gap that exists between a theorist's view of an experiment and an experimentalist's view of the same experiment.

Following EPR's lead, both Bell and CHSH initially had assumed that determinism is the basic underlying assumption for Bell's Theorem. This notion follows from EPR's definition of an "element of reality", as per

> *"If, without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity."*

While predictions with certainty are possible for idealized systems, unfortunately, no such predictions can be made for realizable systems (except, of course, for death and taxes). Following EPR, Bell effectively derives determinism in his 1964 paper for the EPR particles, via a use of the at least one data point with a perfect correlation. Unfortunately and conversely, determinism cannot be derived via this reasoning for any real system, which, of course, has no such point with said perfect correlation, and thus no predictions with probability equal to unity. Given this fact but still following EPR's ideas, CHSH simply and explicitly assumed determinism to hold for the purposes of their derivation[35]. However, Mike

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[34] J. F. Clauser and M. A. Horne, Phys. Rev. D **10**, 526-535 (1974).

[35] Shortly following CHSH, Bell did extend the range of applicability of the CHSH result further to include the effects of additional hidden variables in each apparatus. [J. S. Bell, *Introduction to the hidden-variables question*, in *Foundations of Quantum Mechanics, Proceedings of the International School of Physics, "Enrico Fermi"*, B. d'Espagnat, ed. (Academic Press, New York, 1971), pp. 170-194]. However, in doing so he adds no new premises to Bell's Theorem in this new derivation, and similarly explicitly assumes determinism to hold.

Horne and I noticed the fact that the CHSH prediction also appears to hold for models in which determinism is neither assumed, nor in fact, even holds. This fact suggested to us that an assumption of determinism is not really necessary for Bell's Theorem to apply. Independently, John Bell had also noticed this fact and mentions it in a book review[36], wherein he comments that a hidden-variables model cannot give the probabilistic quantum-mechanical prediction for the two-spin system, even if a random-noise generator at every point in space-time[37] is assumed to influence the space-time evolution of the hidden variables. Such a system, by definition, does not have a deterministic evolution.

Mike and I thus asked ourselves the following questions: If determinism is not required of a system for Bell's Theorem's constraints to hold for it, what then is the fundamental characteristic of a physical system, such that it is constrained by Bell's Theorem. Also, how does one go about specifying said system's assumed nature within the Theorem's derivation? Our 1974 paper was the first publication that specifically answers these questions.

Historically, the CH results can be seen as a logical continuation of a sequence of quandaries faced by Einstein during his development of special relativity. In formulating special relativity, Einstein noted that one cannot ask the universal questions "When" and "Where" in a precise fashion, without first defining the associated primitive entities, "time" and "distance". He found that, at best, he could only provide purely operational definitions for these entities. That is, he defines "time" to be the stuff you measure with a clock, and "distance" to be the stuff you measure with a ruler. Similarly, to allow Einstein to ask the universal question "What" in a similarly precise fashion, EPR's definition of an "element of reality" effectively provides his initial attempt to help one to define the associated primitive entity, "object". But, as noted above, that definition does not apply to realizable systems. CH thus found it necessary to offer an improved and explicit operational definition for the notion of an "object". The historical development of Bell's Theorem is then seen to have a direct parallel to the development of special relativity. Once that a suitably precise definition is given for some fundamental "stuff" of nature, then and only then can new testable physics emerge.

In their 1974 paper, CH operationally define an "object" (within a local realistic theory) as stuff with measurable properties that also can be put in a box, i.e. stuff (along with an associated measuring apparatus) that can be spatially surrounded by a space-time Gaussian surface. CH supplement this definition with an associated definition of an "objective local theory" (subsequently also called a "local realistic theory" and/or a "theory of local realism" by Clauser and Shimony in Ref. [1]), as a theory that describes such objects, wherein action-at-a-distance is precluded, and wherein a measurement reveals local properties of an object (i.e. of said stuff within said box)[38]. Moreover, which properties of an object are to be measured may be arbitrarily chosen at the free will of the experimenter who operates the apparatus. Thus, for objects within such a theory, if the correlated properties of two objects in two disjoint boxes are measured at space-like separated measurement events, then absence of action-at-a-distance prevents the experimenter's choice of property measured in the first box (e.g. his choice of color vs. weight, in the earlier examples) from affecting the results of a measurement made on/in the second arbitrarily distant box. CH found that these very simple and naïve premises and definitions for <u>any</u> such local realistic theory of nature, then lead to a very general formulation of Bell's Theorem, that, in turn, then constrains the experimental predictions for said theory. The CH specific experimental predictions are contained in a new inequality that then must be satisfied by any such theory within local realism. That inequality is now commonly referred to as the CH inequality.

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[36] J. S. Bell, Science, **177**, 880 (1972).

[37] Presumably, Bell may have had vacuum-fluctuations in mind for his random noise source, as a parallel to some of John Wheeler's space-time models,

[38] Under the CH definition, a "deterministic local hidden variables theory" then becomes a special case of the more general class of "local realistic theories".

Under CH's definition of an object, Bell's Theorem then not only provides hard new experimental predictions for a local realistic theory, but it also provides surprising and profound implications for quantum mechanics as well. One first surprise is that these two different theories (local realism and quantum mechanics) give measurably different predictions for realizable experiments. A second surprise is that Bell's Theorem shows that quantum mechanics does not describe objects that fall within the scope of CH's naïve definition. Thus, it appears that within quantum mechanics, a "quantum" object (however else one may choose to define it) is not something that can be put in a box! Unfortunately, it does not appear that Bohr (or anyone else) has ever provided a clear concise definition for an "object" within quantum mechanics. On the contrary (as is often noted by Bell in his book), the distinction between and definitions of the terms "observer", "apparatus", and "system" (where "system" presumably means "object"), all remain quite vague under the Copenhagen interpretation.

Question (2) naturally arises when one tries to test the CHSH experimental predictions using optical photons, as Stu Freedman and I had just done. In such an experiment, only about one emitted photon pair in a million is actually detected in coincidence. Most photon pairs emitted by the source are not detected at all. For most of the observed events from our cascade photon source, only one photon of the pair is actually detected. This situation is clearly quite far removed from that of the idealized configuration originally considered by Bell in his 1964 paper, wherein both particles of each and every emitted pair are subsequently detected, and where the pair is known to have been emitted (as per the discussion of "event-ready" detectors in Ref. [1]). Mike Horne and I realized that the associated enormous unobserved volume of phase space in such experiments leaves a lot of room for significant sampling biases to occur. Unlike the CHSH inequality, however, the CH inequality's prediction is carefully derived so as to render it unaffected by these biases. The experimental requirements for testing the CH inequality are also derived in the CH paper. Unfortunately, the consequence of using the CH inequality (instead of the CHSH inequality) for experimental tests is that an experiment performed with optical photons can no longer, by itself, provide a direct test of local realism[39]. Moreover, designing and carrying out a practical experiment that actually meets the CH inequality's experimental requirements is not an easy task. Fry, Walther, and Li[40] have recently offered such a design. As of this writing, no direct tests of the full CH inequality have yet been performed. Experimental tests of the CHSH inequality, rather than the CH inequality, are now commonly said to leave open the "detector-efficiency loophole".

Question (3) concerns the relationship between the experimental predictions made by the CHSH and CH inequalities. Since experiments performed with optical photons, as a general rule, do not violate the CH inequality, but can and do violate the CHSH inequality, it is natural to ask what additional assumptions can (or must) be added to allow the CHSH inequality and experiments using photons to still give reasonably persuasive (but perhaps not totally compelling) conclusions about the truth or falsity of local realism. That is, what is the best we can do with contemporary technology? To a similar end, CHSH had earlier derived their inequality by making an auxiliary assumption regarding the behavior of a photon at a polarizer that then allows an experimentally testable prediction to obtain. Thus, CHSH assume that whenever a photon encounters a polarizer, then the photon is either transmitted by the polarizer or not transmitted by the polarizer. Furthermore, given that two photons have emerged from their respective polarizers, their detection probability is assumed by CHSH to be then independent of the analyzer-pair settings. Here, CHSH have taken the concepts of "passage" and "non-passage" to be primitive, and thereby effectively also assume that photons are particle-like, for the purposes of the experiment. The CHSH auxiliary assumption obviously does not apply to a semi-classical radiation theory (such as Jaynes' "neo-classical

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[39] This fact is still true, even for photon pairs that are generated by a parametric down-conversion source, as with most recent experimental tests of Bell's Theorem.
[40] E. S. Fry, T. Walther, and S. Li, Phys. Rev. A, **52**, 4381-4395 (1995).

radiation theory", see below) that is necessarily an example of the Schrödinger-Furry hypothesis[41]. In such a theory, each of the individual "photons" that comprise a pair of "coincident photons", must then be represented as a short pulse (wave packet) of classical electromagnetic radiation. Since each pulse is now classical, each "photon" is then partially passed and partially transmitted by a polarizer. Nonetheless, "photons" in such a theory are clearly "objects", under the CH definition.

The CHSH auxiliary assumption is thus deficient, in that it does not apply to this important class of local hidden-variable theories. CH noted, however, that even though the CHSH assumption does not apply to these theories, the CHSH inequality apparently does still apply, and Bell's Theorem clearly does constrain semi-classical radiation theories. To remedy this deficiency, CH provide a significantly improved auxiliary assumption that is also much weaker than the CHSH auxiliary assumption. It is called the "no-enhancement" assumption. It simply requires that the probability of an object's detection, after it has passed through (or partially passed through) an analyzer, can then be no greater than the probability would have been if the particle had not passed through the analyzer, i.e. than if the analyzer had been absent. This assumption clearly holds, for example, for semi-classical radiation theories when the objects are classical electromagnetic pulses and the analyzers are optical polarizers. Furthermore, it seems to be highly plausible and very difficult to avoid for almost any local realistic theory. Then, for all local realistic theories for which the CH "no-enhancement" assumption also holds, the CH inequality's prediction reduces to the CHSH inequality's prediction. In such cases, (except as noted below) experiments such as that by Freedman and Clauser then convincingly refute all such theories.

Question (4) arises in a quest for finding both necessary and sufficient conditions for the CH inequality to constrain local realism. Towards this end, CH provide an *ad hoc* counterexample, that is consistent with local realism, that does not violate the CH inequality, and that does violate the CHSH inequality. While the existence of this counterexample does not prove that the "no-enhancement" assumption is "necessary", it does show that at least some additional assumption is required for one to use the CHSH inequality to test local realism. Use of the CHSH inequality (as opposed to the CH inequality) is now commonly said to leave open the "detection-efficiency loophole".

Common confusion about count-rate normalization

For testing either the CH or CHSH inequalities, a normalization is required for the coincidence count rates as a function of relative polarizer orientation. A valid test of the CHSH inequality's prediction (as obtained from either the original CHSH auxiliary assumption and/or the CH no-enhancement assumption) calls for coincidence rate measurements to be taken, both with polarizers in place and with polarizers absent. The latter measurements then provide a normalization for the former. Here one calculates ratios of coincidence rates to coincidence rates, and since both rates have the same order of magnitude in photon counting experiments, a valid test is straightforward to obtain. The primary reason that direct tests of the CH inequality are much more difficult is that here, the polarizers are not removed. Instead, this normalization is done via the detector singles count rates (with the polarizers in place). For low detector efficiencies and/or a high particle loss rate via the broad angular correlation associated with a three-body-decay emission, the singles rates are generally much larger than the coincidence rates, the associated ratios are very small, and no violation of the CH inequality, by itself, can then occur. For this reason, a two-body-decay is required for such a test.

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[41] The Schrödinger-Furry hypothesis represents probably the simplest form of a hidden-variable theory. It predicts a reduced-amplitude sinusoidal correlation. Bell's 1964 proof does not directly apply to it, since Bell's required perfect correlation (for at least one point) never obtains for it. This hypothesis, however, is constrained both by the CH inequality and by the CHSH inequality with the no-enhancement assumption.

Thus, obtaining a correct normalization for the coincidence count rates is always crucial for a proper and valid test to be performed. Unfortunately, this requirement is often either overlooked or significantly downplayed in its importance in many recent experiments. Furthermore, unless "event-ready detectors" are used, testing of either the CH or CHSH inequality is most readily done with only one detector following each analyzer. Indeed, the superiority of two-channel experiments (relative to four-channel experiments) is discussed, at length, in both the CH paper (Appendix B), and in the Clauser-Shimony review article [Ref. 1].

Note that various other more recent workers have been tempted to use an alternative scheme that is more along the lines of Bohm's original *Gedankenexperiment*, and thus also along the lines of the idealized experiment considered by Bell in his original 1964 paper. In these four-channel schemes, two counting channels follow each polarizer. Workers who succumb to this temptation generally ignore the need for event ready detectors, and commonly reason that an alternative and easier to implement normalization method will suffice. In such schemes, the polarizers are never removed. Instead, the observed coincidence rates are normalized to the sum of the four possible coincidence rates in all such channels. This alternative normalization method is adamantly not the CHSH (nor the CH) normalization method, although it is frequently (and incorrectly) attributed as such[42].

A four channel experiment was first used by Aspect, Grangier and Roger[43]. For that experiment, Aspect *et al.* necessarily rely on a very different set of auxiliary assumptions to allow a comparison of their results to Bell's Theorem. However, their set of assumptions is clearly much stronger than the CH no-enhancement assumption, and thus, allow a much larger "detection-efficiency loophole". The relative strength of their alternative auxiliary assumptions with the no-enhancement assumption is readily seen by noting that it is straightforward to build a rather simple counterexample that violates the CHSH inequality (configured as per the no-enhancement assumption), but that does not violate the count-rate inequality used by Aspect *et al.* For example, consider a simple local hidden-variable theory that is similar to the CH counterexample, but that does not include the enhancement used by that counterexample. This theory then predicts an angular dependence that has a reduced coincidence-rate sinusoidal amplitude along with a proportionately reduced constant offset from zero. An experiment that uses Aspect *et al.*'s normalization will then incorrectly conclude that this hidden-variable theory is refuted, and that quantum mechanics is correct. However, an experiment performed using the CHSH/CH normalization method will reach the opposite (but correct) conclusion.

Bell's response to CH and "local Beables"

Prompted by the CH paper's results, Bell's own fuller discussion of the fundamental basis for Bell's Theorem appeared two years later, but then only as a preprint[44]. It is titled *The theory of local beables*, and is based on the CH paper's results[45], which Bell therein attempts to extend. In this extension, Bell follows a line of reasoning that is essentially the same as that of an early rejected draft of the CH paper. In preparing the CH manuscript, Mike and I had been convinced by our discussions with Abner Shimony that such an extension does not

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[42] In the future, authors are hereby requested to please stop misquoting us!

[43] A. Aspect, P. Grangier and G. Roger, Phys. Rev. Lett., **49**, 91 (1982).

[44] J. S. Bell, *Communication at the 6th Gift Conf. Jaca, June 1975*, Res Th 2053-CERN. Perhaps, Bell's reason for not publishing this result in 1975 is that he may have been afraid that doing so might warrant his being branded a quack, given the then existing stigma against such work.

[45] Bell therein acknowledges our prior work, by stating
"As regards the literature on the subject, I am particularly conscious of having profited from the paper of Clauser, Horne Holt, and Shimony which gave the prototype of (16), and from that of Clauser and Horne. As well as a general analysis of this topic this last paper contains a valuable discussion of how best to apply the inequality in practice."

work, and correspondingly we did not use it. Thus, Abner, Mike and I were together confident that Bell had pushed his arguments too far. Whereas the CH paper maintains the observer as free-willed and as a separate entity from the "objects" being observed, in the "Beables" paper, Bell further includes the observer as part of the objective reality being constrained. So doing, he is then able to hang the whole argument on locality considerations alone, whereupon considerations of objective reality are then no longer needed. But in doing so, the observer's free will to choose analyzer orientations is lost. Given that the observer unwittingly may be a preprogrammed automaton, his programming then defeats the locality considerations, and the arguments then leading to Bell's Theorem no longer follow. With Abner Shimony now taking the initiative, we three proceeded with a public dialog with Bell over this point in the abovementioned "quantum subculture" newspaper, "Epistemological Letters".[46] In this dialog, Bell effectively concedes our point. The dialog is also noteworthy in that many other important related issues are also discussed in detail therein, including the important concepts of "parameter independence" and "outcome independence".[47]

The quantum-optics community encounters related problems.

Despite the grand successes of quantum electrodynamics (QED) in accurately predicting the Lamb Shift, g-2 for an electron, etc. it was quite common in the 60's and 70's for atomic physicists to use semi-classical radiation theory (SCT) to explain their results, since doing so was much easier than using QED. However, any author who did so, also generally included an apology, conceding, "of course a correct treatment of this problem requires QED". Nonetheless, atomic physicists found that, with very few exceptions, SCT accurately predicts observed experimental results. As time progressed, the number of exceptions kept shrinking. It then became sort of a game among these workers to see just how often QED effects can be ignored. For example, as part of this game, Willis Lamb and Marlan Scully[48], and Peter Franken [49] showed that important observable aspects of the photoelectric effect can be derived without quantizing the radiation field, and Franken made a similar demonstration for resonance fluorescence. In fact, the use of SCT became so common[50] and gave results in agreement with atomic physics experiments so often, that various workers such as Franken began to jest that QED may be totally unnecessary! The game began to get serious, however, when iconoclast Ed Jaynes said that he was not joking, and formulated a new semi-classical theory as a serious alternative to QED. He referred to it as neoclassical radiation theory (NCT). Furthermore, he formally challenged the quantum-optics community to refute NCT with direct experimental evidence. Talk about a break from the "standard religion" (regarding acceptance of a theory without compelling experimental evidence) – Wow!

Jaynes' NCT assumes that (1) Schrödinger's equation governs the evolution of an atom's wave function, (2) the absolute square of an atom's wave function provides a

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[46] See, J. S. Bell, A. Shimony, M. A. Horne, and J. F. Clauser, Dialectica, **39**, pp.85-110, (1985), for a republication of this interchange that also includes Bell's "Beables" paper.
[47] See also, A. Shimony, *Search for a Naturalistic World*, (Cambridge Univ. Press, Cambridge, 1993). More recently, the importance of these concepts was independently rediscovered by J. Jarrett [Noûs, **18**, 569 (1984)].
[48] W. E. Lamb and Scully, in *Polarization: Matière et Rayonnement*, (Presses Universitaires de France, Paris, 1969), edited by Société Française de de Physique.
[49] P. A. Franken, in *Atomic Physics: Pooceedings of the 1st International Conference on Atomic Physics,* (Plenum, New York, 1968) V. Hughes *et al.* eds., p.377.
[50] Strictly speaking a semi-classical treatment of such systems is a "hidden-variables" treatment of these systems, and, of course, such a treatment represents "forbidden thinking" under the canons of the "standard religion". However, it is well known in matters of both politics and religion, that issues of practicality commonly override such canons, via the use of "creative ambiguity", especially when appropriate obsequious apologies are also offered. Indeed, "creative ambiguity" allows the present existence only one China in the world.

physically real electric charge density, (3) Maxwell's equations (without field quantization) describe the physically real electromagnetic radiation field[51], and (4) radiation reaction effects are included, so that radiation emitted by an atom affects the atom's own evolution. Using only these elements, Jaynes and his students found that (without QED) they could predict absorption of radiation, spontaneous and stimulated emission of radiation, the Lamb shift, and the black body radiation spectrum[52].

Jaynes' work, however, had crossed a magic line in the sand and obviously had to be viewed as heresy. Heretofore, it was a firmly held belief that all of these latter effects do require a quantization of the radiation field, as is done in QED. But Jayne's' work now cast serious doubts on these cherished beliefs. Nonetheless, it would seem that if quantum theory rests on a solid foundation, then Jaynes' challenge should be easy and straightforward to meet. But Jaynes' challenge went unanswered for several years. A small crisis was thus at hand within the quantum-optics community.

In a historical context, Jaynes was iconoclastically following a path similar to that taken by Bohm, deBroglie, and Schrödinger. While Jaynes did not refer to NCT as a hidden-variables theory (perhaps because of the stigma against such theories), he did recognize that he was indeed tinkering with very fundamental issues within quantum theory. Unlike the theory's founders, however, Jaynes based his work on a very broad range of experimental evidence, most of which postdated the work by the theory's founders. Unfortunately, he was not yet familiar with the power of Bell's Theorem and the associated peculiarities of entanglement.

Upon reading Jaynes' work, I quickly realized that NCT is clearly a local realistic theory. The electromagnetic field and the charge density in NCT are, in principle, all measurable objectively real quantities. Photons (or whatever the stuff is that gives rise to the coincidences in the Freedman-Clauser experiment) within NCT thus qualify as objects, as per the above discussion. Moreover, to refute NCT, the full power of Bell's Theorem is not even needed, and a much simpler argument is sufficient to do so. I thus accepted Jayne's challenge[53].

The 1972 CQ03 - Third Rochester Conference on Coherence and Quantum Optics became a watershed for discussions of this issue, and many of the papers and discussions at this conference were directed toward resolving the issues that Jaynes had raised. At this conference, I pointed out to the participants that if NCT is applied to the EPR configuration, it essentially becomes an example of the Schrödinger-Furry hypothesis. Given the above-mentioned argument by Bohm and Aharonov, it is clear that the Wu-Shacknov experiment already refutes this hypothesis. I also pointed out that the Kocher-Commins experiment provides additional refutation, and offered a slightly more general proof of this fact than that given by Bohm and Aharonov. In his own written contribution to the Conference's Proceedings[54], Jaynes' publicly concedes that my arguments do indeed refute NCT. He soon dropped any further pursuit of this theory, and in a personal letter to me, both praised my work and admitted that his own efforts now "lay in ruins". In defense of Jaynes' valiant efforts, I contend that his efforts provide a heuristically valuable and therefore worthwhile exercise that is highly beneficial to our overall basic understanding of physics.

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[51] Elements (1) - (3) will be recognized as together constituting the so-called "old Schrödinger interpretation". It is noteworthy that these elements are still essential primitives for x-ray crystallography.

[52] E, T. Jaynes and Cummings, Proc. IEEE **51**, 89 (1963); C. R. Stroud, Jr. and E. T. Jaynes, Phys. Rev. A **1**, 106 (1970); M. D. Crisp and E. T. Jaynes, Phys. Rev. **179**, 1253 (1969); **185**, 2046 (1969). See also, P. A. Nesbet, Phys. Rev. A **4**, 259 (1971).

[53] J. F. Clauser, Phys. Rev. A, **6**, 49 (1972).

[54] The proceedings of this conference are in *Coherence and Quantum Optics*, (Plenum Press, New York, 1973) L. Mandel and E. Wolf, eds.

Splitting photons?

To obtain an experimentally testable prediction, CHSH originally assumed that whenever a photon (or at least its detectable component part[55]) encounters a polarizer, then it is either transmitted by the polarizer or not transmitted by the polarizer. That is, CHSH, with their auxiliary assumption require that photons are always particle-like for the purposes of the experiment. While the CH no-enhancement assumption now obviates the CHSH auxiliary assumption, it was then still an open and relevant question as to whether or not there was any compelling *à priori* experimental evidence that justifies the CHSH assumption. That is, do photons clearly demonstrate an unambiguous particle-like behavior, at any time? If so, then under the CH definition of an object, this behavior may be used to constrain further the dimensions of the box that is needed to surround them, and thereby to limit further the number of viable counterexamples to the Freedman-Clauser experiment[56]. It is clear from the simple fact that one observes temporal coincidences between a sequentially emitted pair of cascade photons, that at least the second photon of a cascade must be temporally localized during its propagation. However, this observation, by itself, does not imply that the same photon is also spatially localized (i.e. confined within a box that does not always also still include the emitting atom), and especially spatially localized in directions perpendicular to its propagation direction.

Josef Jauch, who had earlier coauthored a standard textbook on QED[57], expresses a view that photons do indeed exhibit an unambiguous particle-like character[58]. He bases this view on an experiment that was originally proposed by Schrödinger. Earlier, Schrödinger had provided a causal interpretation of his own equation -- the so-called "old" Schrödinger interpretation – wherein a propagating electron therein has objectively real charge density waves associated with its propagating wave function. For the Compton scattering process, Schrödinger[59] shows that these waves can act as moving matter-wave gratings that Bragg-scatter classical electromagnetic wave packets in a manner that exactly predicts the results observed by Compton and Simon. Schrödinger was thus not convinced of the pivotal importance of the Compton-Simon experiment (as von Neumann apparently was) in compelling an acceptance of the Copenhagen interpretation. As an original heretic to the standard religion, he thus persuaded Ádám, Jánnosy and Varga (ÁJV)[60] to actually perform his proposed experiment. In their experiment, two independent photo-detectors are placed respectively in the transmitted and reflected beams of a half-silvered mirror. If photons have a particle-like character, i.e. if their detectable components are always spatially bounded and well localized, then photons impinging on the half-silvered mirror will not be split in two at this mirror. On the other hand, if they are purely wave-like in nature, (as with classical waves) then they can and will be split into two independent classical wave packets at this mirror. This fact then implies that if photons are purely wave-like (in this classical sense), then the two detectors will show coincidences when a single temporally localized photon is directed at said

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[55] In a "pilot-wave" theory, separate component parts of a quantum particle exist that then respectively account for the particle-like and the wave-like aspects of its propagation.

[56] The huge detector efficiency loss that is due to the small solid-angles subtended by photon detectors in typical cascade photon experiments, can then be rendered effectively harmless by said significantly reduced box volume. This fact is used explicitly (but without citing my experimental justification for it) by Bell in the *Gedankenexperiment* that he depicts on p.107 of his book, *Speakable and unspeakable in quantum mechanics.*

[57] J. M. Jauch and F. Rohrlich, *The Theory of Photons and Electrons* (Addison-Wesley, Reading, MA, 1955).

[58] J. M. Jauch, *Dialogue on the Question ARE QUANTA REAL?* (Univ. of Geneva, preprint, 1971).

[59] E. Schrödinger, Physikalische Zeits. **23**, 301-303 (1922); Die Naturwissenschaften **12**, 720-724 (1924); Il Nuovo Cimento **9**, 162-170 (1958).

[60] A. Ádám, L. Jánnosy and P. Varga, Acta Phys. Hung., **4**, 301 (1955); Ann. Physik, **16**, 408, (1956).

mirror. One of these independent wave packets will be transmitted to illuminate the first detector, and the other will be reflected to illuminate the second detector, and both detectors will then have a finite probability of detecting the same photon (classical wave packet). This latter possibility, however, violates the predictions by QED, which prohibits such coincidences. ÁJV thus searched for anomalous coincidences between photomultiplier tubes that viewed the reflected and transmitted beams behind a half-silvered mirror. Unfortunately, their mirror was illuminated by a temporally continuous beam of light, and not by "tagged" single photons[61]. They found no such anomalous coincidences, and thereby concluded that, consistently with QED, photons do not split at the mirror and thus do exhibit a particle-like character.

A critical parameter in the ÁJV experiment is their photon detection efficiency. Its value then determines whether or not the sought-after anomalous coincidences can be distinguished from the large background of omnipresent "accidental" coincidences. Upon reading their paper, I was astonished by their claim to have achieved 10% photon detection efficiency. Stu Freedman and I had struggled with much better equipment to get an efficiency of about 0.1%. In fact, we actually measured this efficiency using the abovementioned photon-tagging scheme, by calculating the ratio of the coincidence count rate to the singles count rate for first photons of the cascade. How could ÁJV possibly claim to have achieved this absurdly high efficiency? To do so, they had ignored the solid-angle loss, and had quoted only the so-called photomultiplier quantum efficiency. But it is clear that for a worst-case spherically expanding wave, it is the overall photon detection efficiency that matters to the argument, and <u>not</u> the photomultiplier's quantum efficiency. Further study of their result and of similar repetitions of their experiment convinced me that none of these experiments convincingly demonstrated a particle-like behavior for photons. With low detection efficiencies, all of these experiments fell far short of having the required precision to do so. At best, they could only observe coincidences between two photons that accidentally were temporally overlapped. Viewed in this light, the ÁJV experiment was effectively a null experiment that, at best, sets an upper limit to the anomalous coincidence rate that might be present. Moreover, the upper limit that they obtain does not then even usefully constrain semi-classical radiation theories.

I thus decided to perform an experiment that has convincingly high photon detection efficiency, and also that is configured, not as a null experiment, but instead as one that gives a definitive answer. Its results were published in 1974[62]. This experiment, unlike that by ÁJV, exploits the temporal localization already exhibited by "tagged" photons. Also, unlike the ÁJV experiment, the experiment employs four photomultiplier tubes, operating pair-wise in coincidence, and two half-silvered mirrors. By a judicious application of the Cauchy-Schwarz inequality to four different measured coincidence rates, it is then possible to obtain the desired definitive (non-null) prediction for this configuration. The experiment's results show that both quantum mechanics and quantum electrodynamics hold true, and photons do not split at a half-silvered mirror. This experiment is further noteworthy, in that it represents the first observed violation of a Cauchy-Schwarz inequality, as derived from classical optics principles, and thus also represents the first observation of sub-Poissonian statistics for light.

Curiously, the results of this experiment also resolve another untidy issue left behind by quantum theory's founders. That issue is whether or not energy conservation in the photoelectric effect is point-wise exact, or only holds as a statistical average, as suggested earlier by Bohr, Kramers and Slater. (Recall that von Neumann cited the Compton-Simon

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[61] A "tagged" single photon is readily produced by a cascade photon source, by using a coincidence gate to only look for a sequentially emitted second single photon from an atom that is undergoing a cascade decay, that immediately follows the detection of the first photon of the decay. Given a detection of the former, one then knows with a very high probability that the associated second photon has been emitted by the atom, within roughly an intermediate-state lifetime, following the first photon's emission.

[62] J. F. Clauser, Phys. Rev. D, **9**, 853 (1974).

experiment as a refutation of this theory.) Schrödinger, in his above-cited works, is critical of this conclusion, and reopens the question. As a logical follow-on, Schrödinger then worried about the particle-like character of photons and, as noted above, went on to propose the ÁJV experiment as a crucial test. Schrödinger (as a theorist), however, never carefully scrutinized ÁJV's experimental assumptions and parameters to note the difference between AJV's incorrect use of the photomultiplier quantum efficiency in place of the (correct) photon detection efficiency. As a result, no prior experiments had ever carefully and conclusively resolved this issue, that instead, heretofore had been simply dismissed by others on purely theoretical grounds. (Here we see more "killer" details falling through a critical gap between theorists and experimentalists.)

An ambiguity concerning energy conservation arises from the fact that there are really two independent definitions of electromagnetic energy – one from quantum mechanics (hv) and one from classical mechanics (integral over all space of the electromagnetic field energy). Quantum electrodynamics is formally based on an assumed strict equality of these two different independently defined quantities, and thus requires a strict point-wise conservation of energy. But, prior to my experimental result, this crucial fundamental assumption had not yet been directly experimentally demonstrated. My improved version of the ÁJV experiment now finally does just that. Grangier, Roger, and Aspect[63] have since performed a somewhat similar experiment that also observes sub-Poissonian statistics and thereby confirms my results.

Remaining locality loopholes

It should be noted that an experiment that is shown to violate the CH inequality then rules out all local realistic theories, except for one remaining special class of theories. These are theories for which Bell (following Bohm and Aharonov) noted in his original 1964 paper that an experiment performed with static analyzers is, itself, logically flawed, since it does not strictly enforce locality. Indeed, it is logically possible that in such an experiment the two different particle detectors, located on opposites sides of one's laboratory, are conspiratorially communicating with each other, with the specific motive of defeating the experimental test. To close this so-called "locality loophole", Bohm and Aharonov proposed that one should rapidly reorient the analyzers while the photons are in flight, and Bell echoes and promotes this suggestion in his 1964 paper. However, Bohm and Aharonov's artifice clearly does not close this loophole! Instead, it simply shifts the burden of perpetrating said conspiracy from one apparatus component to another.

Although logically, it must be admitted that such theories are indeed possible, in a sense they seem to be rather improbable, indeed almost "pathological". To accept the locality loophole as a reasonable possibility, it is first necessary to believe that a pair of detectors and analyzers that are several meters apart are somehow conspiring with each other, so as to defeat the experimenter, and thus somehow are converting a non-sinusoidal coincidence rate dependence on relative polarizer orientation into a sinusoidal dependence. However, once that one is willing to concede that these apparatus components are indeed capable of perpetrating such a conspiracy, then it seems to be equally reasonable (and similarly logically possible) to assume further that the electronics that control a pair of rapidly reorienting analyzers are (perhaps unwittingly) also equally participating in the same conspiracy, also then to defeat the experimenter's efforts with similarly devious tricks. To be sure, it may seem that a belief in such logically possible conspiracies requires a certain degree of paranoia. A logical consequence of admitting to this paranoia is then an associated recognition that every piece of equipment that has been in your laboratory for more than a few nanoseconds might possibly participate in such a grand conspiracy against you, whereupon no effort on your part can rule out this logical possibility! However, if all experimentalists were similarly paranoid, then experimental physics, in general, would seem to be a pointless endeavor.

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[63] P. Grangier, G. Roger, and A. Aspect, Europhys. Lett . **20**, 1061 (1986).

participate in such a grand conspiracy against you, whereupon no effort on your part can rule out this logical possibility! However, if all experimentalists were similarly paranoid, then experimental physics, in general, would seem to be a pointless endeavor.

Without attributing paranoid tendencies for these workers, it is noteworthy that nearly a decade after the publication of the Freedman-Clauser results, Aspect *et al.*[64] took the possible existence of such conspiracies seriously, and following Bohm and Aharonov's original suggestion, performed a second-generation experiment with dynamic (time-varying) analyzers. Their experiment then rules out detector-analyzer conspiracies. Other workers are currently considering performing experiments that might rule out even more insidious higher-level conspiracies.

Conclusions

Clearly, quantum theory's predictive power and Bohr's Complementarity have both proven to be exceedingly robust while under attack by what is probably the most powerful challenge they have ever had to face – Bell's Theorem. History now shows that Bohr's views are probably the correct ones to hold. However, in retrospect, those who held to Einstein's view can, with reasonable justification, claim that Bohr, himself, was just lucky, in that he then had no real compelling (experimentally justifiable) reason to hold his view. Similarly, those who smugly held to Bohr's view now can say, also with a similarly reasonable justification, "See, we told you so!" Nonetheless, it appears reasonable to believe that neither camp can justifiably argue that the resolution, via Bell's Theorem, of the issues separating them was a waste of time. A lot of new physics was discovered in the process.

Given historical hindsight, I assert that our basic understanding of quantum theory has been significantly improved via Bell's Theorem and via its associated experimental testing, long after it was confidently asserted by many textbooks to be well understood. It is truly amazing that so many "killer" details slipped through cracks that existed between experimentalists and theorists. It is clearly of continuing importance for experimentalists and theorists to scrutinize each other's work with great care to try to eliminate such cracks. Given such hindsight, I also assert that it is clearly counterproductive to scientific progress for one camp smugly to hold to a belief that all problems are solved in any given area. It is even more counterproductive for this camp then further to rely on this belief to formulate a religious stigma against others who do not share their cherished belief. Indeed, history also shows that a prohibition against open discussion and experimental testing of the foundations of quantum theory, in turn, led to a significant delay of the discovery of important new applications of these foundations. Quantum cryptography, distributed entanglement, etc. undoubtedly would never have been envisioned without the intellectual challenges posed by Bell's Theorem.

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[64] A. Aspect, J. Dalibard and G. Roger, Phys. Rev. Lett., **49**, 1804 (1982).