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Quantum [Un]speakables

From Bell
to Quantum Information

With 141 Figures, 4 in Color



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Part II Tests of Bell's Inequalities

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6 Early History of Bell's Theorem

John F. Clauser

This article is dedicated to the memory of John Bell, whose work exerted a profound influence on my own life and professional career as an experimental physicist. In this article I attempt to recount as accurately as possible the important events in the development of one of the most profound results in physics of the twentieth century – Bell's Theorem.

6.1 Introduction

Bell's Theorem provides us with the 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 allows 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, our 1978 conclusion appears to stand.

The present-day heuristic impact of Bell's Theorem is immense. It clearly and explicitly reveals highly peculiar and remarkable properties of quantum-mechanically “entangled” states that were previously not appreciated. Its elucidation of these properties, in turn, has inspired various efforts, both to quantify and to exploit entanglement on a wide variety of physical systems. As a result, it has directly or indirectly fostered many spin-off practical applications and created whole new areas of study, including quantum cryptography, quantum communication, quantum computing, entanglement sharing, quantum cloning, etc. The spin-offs from Bell's Theorem are now so widely ranging that it is difficult to open any recent issue of Physical Review Letters or Nature without finding at least one article that is a legitimate progeny. 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 conceptual tool for promoting revolutionary useful technology.

Bell's Theorem also did one more very important thing. It cleaned up a very important "untidiness" left by the founding fathers of quantum mechanics. The word "untidy" is used here to describe a state of affairs in which the physics community had become bifurcated into two camps, with each camp confidently holding beliefs that were just the opposite of that of the other camp, with neither camp holding any experimental justification for its beliefs, with opposite appropriate future directions for physics research implied by these opposing beliefs, and with any rational discussion by these two groups about their differences prohibited by a ludicrous and debilitating social convention. How did this absurd situation arise, and how did Bell's Theorem end it?

This note provides an overview of the history (through the early 1980s) of the development of Bell's Theorem. As a parallel narrative, I offer some first-hand personal recollections about the conduct of physics during this development period. Young physicists may find it difficult to believe that thirty years ago, most of the above ideas and subject matter represented forbidden thinking for practicing physicists. Indeed, any open inquiry into the wonders and peculiarities of quantum mechanics and quantum entanglement that went outside of a rigorous "party line" was then virtually prohibited by the existence of various religious stigmas and social pressures, that taken together, amounted to an evangelical crusade against such thinking. As a result of this evangelism, much of the early important work on Bell's Theorem was published only in an "underground" newspaper¹, whose circulation was limited to members of a "quantum-subculture", and that probably cannot be found in most physics libraries. The achievement of the results that led to our review article may then be seen to be all the more remarkable, given then existing sociological barriers to normal scientific inquiry.

The reader may soon feel that much of my portrayal of this history resembles a strange mixture of serious physics with a cheap soap opera. While my inclusion of the latter aspect of this history may detract from my portrayal of the former, I beg the indulgence of reader. I include some of

¹ *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.

the sleazy aspects of this history (but still attempt to portray it as accurately as possible), because it had a significant impact on both the workers and on the works of this field, because it may explain why some important ideas were not published in a timely fashion (including much of John Bell's work), because it explains why many important published ideas languished unread for prolonged periods (including John Bell's most important 1964 paper), and finally, because, to my knowledge, it is not described elsewhere. I apologize ahead of time to anyone who is either offended or bored by my sacrilegious approach. Such "sensitive" readers are advised to bypass the two Sections entitled, "Evangelical theoreticians dominate", and "The development of a 'stigma'".

Also, what the heck! How often is a simple experimental physicist (who spends much of his time mucking about in cutting oil and chasing down vacuum-system leaks and electronics and software bugs) given poetic license to offer his own editorial opinions about the sociology of physics? I sincerely hope that John Bell would have approved.

6.2 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 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* 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 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.² 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.³ 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 *necessary* 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 *sufficient* 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!

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

² The Wigner distribution is obviously not such a distribution, since it is not always non-negative, and thus it cannot be viewed as a sampling-frequency distribution.

³ Max Jammer, in Sect. 4.2 of his book, *The Conceptual Development of Quantum Mechanics* (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. Chapter 4 of this book is noteworthy in that it summarizes the important experimental results available when quantum mechanics was originally being formulated, while Chap. 8 provides an abbreviated list of experimental confirmations of the theory that were then promoted universal belief in the "standard religion".

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.

6.3 An 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 were immediately followed by additional works that further sharpened their content.

One of these two important works is by *John von Neumann*. In his book, *Mathematische Grundlagen der Quantenmechanik* [2], 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 eigenvalue 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 differences between two 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 suggests that he believes that the existence of such an alternative theory is "highly unlikely". Unfortunately, many subsequent workers have misinterpreted his work as a "proof" of the non-existence of any such alternative theory.

The other one or the two important works is by *Einstein, Podolsky and Rosen* [3] (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" 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, there are now two urns. At one urn, the ball's weight is measured, and at the other, the ball's color is measured. Knowing say the 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. Correspondingly, it then must be supplemented with "hidden parameters" that provide such a specification for it to become complete.

Bohr then offered an important rebuttal [4] 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.⁴ Thus, despite the aims of von Neumann (and Bohr) and of EPR, their antipodal conclusions left the completeness issue in a very untidy state.

Additional related comments about EPR were then offered by *Schrödinger* [5] and by *Furry* [6]. 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

⁴ 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. Given the careful wording of von Neumann's above-quoted passages, perhaps Einstein saw no compelling reason to publish such criticism, since von Neumann, himself, never actually claimed in his book that hidden variables are impossible.

omits entanglement must be immediately viewed as wrong, since quantum mechanics is “obviously” a correct theory.

6.4 Evangelical Theoreticians Dominate

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.⁵ 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* [7],⁶ and then only to refute the short-lived *Bohr-Kramers-Slater* [8] predecessor theory. But after all, von Neumann was a mathematician. Unfortunately and correspondingly, his totally mathematical approach leaves an

⁵ Whenever I asked anyone during the 1960s 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.

⁶ This experiment is a more precise repetition of an earlier experiment by W. Bothe, H. Geiger: *Z. Phys.*, **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?”.

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 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!

Religious dogmatism then quickly promoted a nearly universal acceptance of quantum theory and its Copenhagen interpretation as gospel, along with a total unwillingness to even mildly question the theory's foundations. Such dogmatism persisted well into the 1970s. A standard phrase that was (and frequently still is) offered in quantum theory's defense, as a logical missal to the "standard religion" (as a nearly direct paraphrasing of von Neumann), is then,

Quantum theory is so successful, it is simply inconceivable that its foundations and/or predictions can be unsettled, misunderstood, or, God forbid, in error.

Given the omnipresent religious zeal, it would seem to be sheer folly for anyone to propose new experimental tests of quantum theory. What if their results disagree with quantum theory's prediction? We certainly will all perish! I offer the two following anecdotes as typical examples of this kind of dogmatism.

While I was a physics graduate student at Columbia University, I went to Prof. Bob Serber (a former student of Oppenheimer) to ask for his opinion of the 2-spin EPR configuration, and also to describe Bell's Theorem to him, along with my opinion of the importance of experimental tests of this configuration. To him, it was inconceivable that there might be any possible difference between the quantum mechanical prediction for this configuration and the experimental results. He dismissed me from his office, saying "Well *that* (the Bell's Theorem prediction) might be worth pointing out in a letter to the editor, but no decent experimentalist would ever go to the effort of actually trying to measure it with *that* in mind!"

As a second example, while I was actually performing the first experimental test of the CHSH-Bell predictions as a postdoc at UC-Berkeley (see below), I visited my undergraduate *alma-mater*, Caltech, and made an appointment with Prof. Richard Feynmann to discuss these same questions. Feynmann was very impatient with me. As soon as I told him that I was performing an experiment to test the predictions of quantum mechanics for the EPR configuration, he immediately threw me out off his office saying "Well, when you have found an error in quantum-theory's experimental predictions, come back then, and we can discuss *your* problem with it."

Given the religious impact of Bohr'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 the completeness problem. To a large extent, further work in this field got put on hold by the intervention of World War II. The atomic bomb, radar, rocketry, and flight from Nazi Germany by many then necessarily occupied much of physicists' attention, and little time was then available to ponder the philosophical implications of quantum mechanics. Following the war, new technology that had seen unprecedented rapid development as a result of the war effort (e.g. sophisticated electronics, particle accelerators, microwaves, high-vacuum technology, etc.) brought the capabilities of experimental physicists to a dramatically higher level. Previously impossible experiments became straightforward. Maybe now would be the time to tidy up the foundations of quantum mechanics, perhaps now with some help from experimentalists? Not yet!

6.5 The Development of a "Stigma"

During the post-war years, the United States quickly became embroiled both in the cold-war and in an internal anti-communist frenzy. Driven by Sen. Joe McCarthy, stigmas then came into vogue. Indeed, McCarthy's stigmatizing attacks on physicist, Robert Oppenheimer, are well known and well documented. Unfortunately, acceptance of stigmas by the populace appears to foster the malignant creation of additional stigmas, especially in an environment that is dominated by religious fanaticism. Thus, in keeping with the

times, a very powerful secondary stigma began to develop within the physics community towards anyone who sacrilegiously was critical of quantum theory's fundamentals. This stigma long outlived the McCarthy era and persisted well into the 1970s and 1980s. Sadly, it effectively kept buried most of the untidiness left behind by quantum theory's founders, and physicists went on about their business in other areas. The net impact of this stigma was that any physicist who openly criticized or even seriously questioned these foundations (or predictions) was immediately branded as a "quack". Quacks naturally found it difficult to find decent jobs within the profession.

To be sure, there remained alive a minority of the theory's founders (notably Einstein, Schrödinger, and de Broglie) who were still critical of the theory's foundations. These men were obviously not quacks. Indeed, they all had Nobel Prizes! Instead, gossip among physicists branded these men "senile". This is not a joke. On many occasions, I was personally told as a student that these men had become senile, and that clearly their opinions could no longer be trusted in this regard. This gossip was repeated to me by a large number of well-known physicists from many different prestigious institutions. Given this branding, their leadership role in charting the course of progress in physics was thereby severely undermined. Under the stigma's unspoken "rules", the worst sin that one might commit was to follow Einstein's teaching and to search for an explanation of quantum mechanics in terms of hidden variables, as Bohm and de Broglie did.

This stigma was very real and had more than just a mild social impact on the conduct of physics. For example, even as late as the early and mid seventies, whenever a manuscript discussing the foundations of quantum mechanics (and especially one discussing hidden variables) was submitted to either the Physical Review or Physical Review Letters, editor Samuel Goudsmit would, in turn, enclose a one-page APS policy statement along with it to the manuscript's referee. That policy, in essence, urged the referee summarily to reject any paper on this subject, unless the paper was both mathematically based *and* gave new quantitative experimental predictions. Bohr's response to EPR certainly could not have been published under Goudsmit's stated criteria.

Religious zeal among physicists prompted an associated powerful proselytism of students. As part of the "common wisdom" taught in typical undergraduate and graduate physics curricula, students were told simply that Bohr was right and Einstein was wrong. That was the end of the story, and the end of the discussion. Of course it was the end, because the concluding chapters of the story were not yet written. Bohm's and de Broglie's alternative theories (see below) were neither taught, nor even cited.⁷ Any student who questioned the theory's foundations, or, God forbid, considered studying the associated problems as a legitimate pursuit in physics was sternly advised that he would ruin his career by doing so. I was given this advice as a student

⁷ John Bell laments this fact in his book.

on many occasions by many famous physicists on my faculty at Columbia, and Dick Holt's faculty at Harvard gave him similar advice.

Fortunately, within the community, there were a few physicists who actually enjoyed challenging this common wisdom. These are the iconoclasts of our profession, who, unfortunately, took the major brunt of the stigma. One important player was David Bohm. Given the era, no one dared trust his opinion since he was openly communist. At one point in his career, the best job Bohm could find was a professorship in far away Brazil, until Oppenheimer brought him back to Princeton. Louis de Broglie, held a prominent seat on the French National Academy, and was thus comparatively immune from the stigma's curse. However, his publications went largely unread, since, of course, everyone already "knew" that he was senile.

An obviously central figure in the development of Bell's Theorem is the hero of our story, John Bell, himself, who appeared to be keenly aware of the power and potential danger available within this stigma. Generally, he appeared to exercise great caution to avoid its wrath. Perhaps, for this reason, not much of his work on Bell's Theorem was formally published (see below) until it much of it was later collected into his book, *Speakable and unspeakable in quantum mechanics* [9]. John Bell clearly evidences his awareness of the stigma and its associated limitation to open discussion, via the double-entendre manifest in this book's title. His article, *Against Measurement* [10], is somewhat more poignant and outspoken on this issue. I offer here three anecdotes further supporting my opinion of his extremely cautious attitude towards the stigma.

In 1982, John Bell and I were announced to be joint winners of "The Reality Foundation Prize", and to share its \$6000 award between us. Before accepting this award, however, he wrote to me and asked whether or not this was a "quack" group that was offering it, and whether if not accepting it might hurt his own reputation as a legitimate scientist. I assured him that such was not the case, and that the money was coming from one of the founders of Federal Express (Charles Brandon), who personally had a great interest in promoting further legitimate study of the foundations of physics. Nonetheless, Bell did not attend the awards ceremony to accept the Prize. Instead, Bernard d'Espagnat, who did attend, accepted it on his behalf.

Another example of John Bell's caution relative to the stigma's wrath is that he never openly discussed his study of the foundations of quantum mechanics with any of his co-workers in the high-energy physics theory group at CERN. The first time that any of these coworkers found that this man of mystery had been leading a double life, is when Bell, Zichichi and d'Espagnat put together an International "Ettore Majorana" Conference in Erice, Sicily on "Experimental Quantum Mechanics". Until then, his coworkers had never even heard the term "Bell's Theorem". I speculate that this double-life was chosen primarily to avoid the stigma's curse!

A final example is from Alain Aspect. When Alain was preparing to do his own experimental tests of the CHSH-Bell predictions in the early 1980s to confirm and extend our earlier work, he visited Bell at CERN. Fearing that Aspect, himself, might fall afoul of the stigma, Bell's first question of him was "Do you have a permanent position?"

6.6 Challenging the Common Wisdom

The next important step in the saga occurred in 1950, when *David Bohm* (a former student of Oppenheimer) published a new textbook, *Quantum Theory* [11]. The book's treatment of the underlying conceptual foundations of quantum mechanics is quite similar to other textbooks of the era. In it, Bohm 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 [12] 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* [13] at Columbia University. 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 [14].⁸ Given the stigma against such work, few people took Bohm’s paper seriously. One who did take it seriously was Louis de Broglie. Inspired by this work, *de Broglie* reentered the arena and published a whole series of books [15] and papers promoting a similar theory. (Bohm and de Broglie’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 clean things up a little, and offers an argument as to how von Neumann’s proof may not apply to his theory.

6.7 John Bell Unravels the Confusion

The stage was finally set for the brilliant iconoclast John Bell to enter the picture to begin unraveling much of the accrued (but largely hushed-up) 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 de Broglie’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 [16], 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

⁸ Fortunately, this work was published before the APS policy had been formulated. Presumably, it would have not passed muster under that policy.

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 de Broglie'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 de Broglie had wanted.⁹ 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 [17], 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! 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

⁹ de Broglie 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.

must be strongly non-sinusoidal if it has at least one point with the maximal correlation value that is required by quantum theory.

6.8 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 University, and was struggling, trying to understand quantum mechanics. I had read EPR's

paper, and also had read Bohm's and de Broglie'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 de Broglie'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.¹⁰ By holding that opinion (given the above-mentioned stigma), 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 de Broglie'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 this 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, Bell's 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

¹⁰ John Bell confesses in the preface to his book, *Speakable and Unsayable in Quantum Mechanics*, to being similarly enamored with these features of pilot-wave theories.

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?

I recalled a paper by *Peres* and *Singer* [18], who (following Bohm and Aharonov) had proposed additional tests of the Schrödinger-Furry hypothesis.¹¹ They had suggested using 90° scattering as a method for producing Bohm's two-particle spin-singlet entangled state. Hearing that Dan Kleppner and Dave Pritchard at MIT were doing crossed-beam scattering experiments with alkali metal atoms, I went to MIT and gave Kleppner's group a seminar on Bell's Theorem, and suggested that their experiments might be reconfigured so as to provide an experimental test. 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. At the end of my talk, Dave Pritchard asked "Carl wouldn't your experiment test this?" Kocher replied, "Of course, that's why we did it!" Kocher had no reprints on hand, so I returned to New York and eagerly located his published results in the library [19]. However, contrary to Kocher's claim, I discovered that their experiment was totally inadequate as a test. Kocher and Commins had only measured coincidences at 0° and 90° relative analyzer orientation. As per Bell's assumption, these data points are located 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 de Broglie, asking all three of them (a) did they know of any experiments

¹¹ The Schrödinger-Furry hypothesis represents probably the simplest form of a hidden-variable theory. It predicts a reduced-amplitude sinusoidal correlation. Strictly speaking, Bell's 1964 proof does not apply to it, since Bell's required perfect correlation (for at least one point) never obtains for it. This hypothesis, however, is constrained by the CHSH and CH inequalities.

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 [20].¹² 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 McCarthy era was now in the distant past. Instead, 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 [21]. 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.¹³ 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 [22].¹⁴ Our collaborative effort is now commonly referred to by

¹² According to David Wick, my 1969 letter was the first response to his 1964 paper that Bell had received.

¹³ 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.

¹⁴ This paper first coined the term “Bell’s Theorem”.

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.

6.9 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. Fortunately, Townes was very tolerant as my radio astronomy projects languished. Stu and I published our results in 1972 [23], 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 quite similar to ours, except that it 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 further 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 [24] 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 [25]. 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, *Fry* and his student *Thompson* [26] 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 [1].

6.10 Generalization of the Bell and CHSH Results to Constrain Local Realism and Space-Time

While I was actively pursuing 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 now 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 [27]. 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 Bell's 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 [28].¹⁵ However, Mike 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 [29], 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 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

¹⁵ 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. However, in doing so he adds no new premises to Bell's Theorem in this new derivation, and similarly explicitly assumes determinism to hold.

system's assumed nature within the Theorem's derivation? Our 1974 paper was the first publication that specifically answers these questions.

Historically, the CHSH result 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 [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).¹⁶ 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 *any* 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.

¹⁶ 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.

It may be noted, in passing, that a single "bit" of information, as may be stored in a computers memory, qualifies as a "quantum object". Bell's Theorem the shows that in a "quantum computer", said bit may not necessarily reside within a single box that sits on one's desk. Indeed , if the quantum computer is part of a quantum-computer network, then the bit's definition may be determined only by joint operations performed on the whole network, but not by any attempt to access it on a single box that is attached to the quantum network.

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 the cascade photon source that we used, 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 [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 photons can no longer, by itself, provide a direct

test of local realism.¹⁷ 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* [30] have recently offered such a design, and its experimental realization is currently in progress. 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 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 radiation theory", see below). 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)

¹⁷ 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.

an analyzer, can then be no greater than the probability would have been if the particle had not passed through the analyzer, i.e. greater 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 “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 “detector-efficiency loophole”.

6.11 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.

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 [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.¹⁸

A four channel experiment was first used by *Aspect, Grangier and Roger* [31]. 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 “detector-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 the Aspect et al. 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.

6.12 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

¹⁸ In the future, authors are hereby requested to please stop misquoting us!

only as a preprint [32].¹⁹ It is titled *The theory of local beables*, and is based on the CH paper's results,²⁰ 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 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" [33].²¹ 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". [34]

6.13 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 1960s and 1970s 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,

¹⁹ 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.

²⁰ 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."

²¹ For a republication of this interchange that also includes Bell's "Beables" paper.

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* [35], and *Peter Franken* [36] 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 common²² 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 physically real electric charge density, (3) Maxwell’s equations (without field quantization) describe the physically real electromagnetic radiation field,²³ 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 [37, 38].

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, de Broglie, and Schrödinger. While Jaynes did not

²² 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.

²³ 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.

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 [39].

The 1972 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 [40], 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.

6.14 Splitting Photons?

To obtain an experimentally testable prediction, CHSH originally assumed that whenever a photon (or at least its detectable component part²⁴) 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

²⁴ 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.

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.²⁵ 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 [41], expresses a view that photons do indeed exhibit an unambiguous particle-like character [42]. 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* [43] 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, Jánosy* and *Varga* (ÁJV) [44] 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 they are purely wave-like (in this classical sense), then the two detectors will show coincidences when a single temporally localized photon is directed

²⁵ 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 Unsayable in Quantum Mechanics*.

at said 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.²⁶ 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 the efficiency of their photon detectors. 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 above-mentioned 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 *not* 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 detector 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 efficient detectors, 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 [45].

²⁶ 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.

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 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 ÁJV’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 yet another critical gap between theorists and experimentalists.)

The issue arises from the fact that there are really two independent definitions of electromagnetic energy – one from quantum mechanics ($h\nu$) 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* [46] have since performed a somewhat similar experiment that also observes sub-Poissonian statistics and thereby confirms my results.

6.15 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 opposite 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 paranoia²⁷ 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.

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. [47] 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.

²⁷ By similar reasoning, the simple fact that you are paranoid doesn't mean that *they* are not actually trying to kill you!

6.16 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.

My own final conclusion is that the only real loser here has been the "stigma", itself. I hope that John Bell would have agreed.

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