EARLY HISTORY OF BELL'S THEOREM THEORY AND EXPERIMENT

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ABSTRACT

Before 1980 it was unfashionable for a physicist to admit that he either did not understand and/or doubted the Truth and/or Orthodoxy of Quantum Mechanics (QM). Contemporary wisdom deemed it impossible that it may lead to incorrect predictions. Thus, it was foolish to suggest that it warranted further testing. Said wisdom proclaimed that nothing would ever be gained by any such pursuit. Bohr had won his debates with Einstein. Von Neumann had proven all other interpretations wrong. That was the end to it! Only an iconoclast dared think otherwise. Here I provide a brief history of some of my encounters with a few fellow iconoclasts, past denizens of a QM doubter's subculture.

David Bohm was an early pioneer and charter member of the quantum orthodoxy doubting subculture, along with deBroglie, Schrödinger and Einstein. Already on the road to Bell's Theorem in the 1950's, Bohm planted three very important seeds for Bell's theorem. These were #1 a formulation of the Einstein - Podolsky - Rosen (EPR) paradox in terms of a coupled system of two particles with spin[1], #2 the Causal Theory of QM[2], and #3 (with Yakir Aharonov) identifying the Wu - Shaknov experiment[3] as the only existing experimental refutation of the Schrödinger - Furry hypothesis for the EPR paradox[4]. In #3 Bohm and Aharonov almost discovered Bell's Theorem, but not quite.

Bell's genius was in actually synthesizing Bohm's three pieces of the puzzle into his remarkable result. Bell pondered Bohm's Causal Theory. Von Neumann purportedly had "proved" the theory to be impossible, but there it was! While deducing von Neumann's oversight, Bell reformulated Bohm's Causal theory and used it to try to explain the EPR paradox. He found that to do so seemed to require non-locality. He wondered why. So astonished when he found the reason, he published it[5] before completing his work on the hidden - variables "impossibility" proofs.[6]

In the late 60's I was a graduate student at Columbia, struggling to understand quantum mechanics (QM). I had read Bohm's work when I encountered Bell's Theorem. Taking the hint from Bohm and Aharonov, I wondered about its experimental status. Bell's paper was a little ambiguous on this point. He echoed Bohm and Aharonov, and suggested performing a second generation experiment involving rotating the analyzers while the particles are in flight, but referenced no first generation (static analyzer) results. My preliminary search of the literature yielded no experimental results other than Wu and Shaknov's.

Looking further, I derived a form of the theorem that could be tested experimentally, as Bell's result was in a form that required ideal systems. With it, I quickly convinced myself that the Wu - Shaknov experiment itself was not a convincing test. I expected that any violation of the QM prediction would manifest itself as a deviation from a sine - squared angular dependence of the coincidence rate and required measurements at angles between the two monitored by Wu and Shaknov. Moreover, even if one were to make such measurements, it seemed clear that their analyzers were far too inefficient to provide a convincing test.

Knowing only the Wu - Shaknov results, I went to Madame Wu at Columbia, told her about Bell's result, and asked if she had made measurements at intermediate angles. She said no, wasn't very interested in Bell's Theorem, and dispatched me to her student Len Kasday. Len was redoing the experiment and needed additional motivation for doing so. He had not seen Bell's result. The idea of measuring at intermediate angles intrigued him and he agreed to do so. He subsequently announced the results in 1970, and eventually published a full discussion of the experiment in 1975[7].

Recognizing that a repeat of the Wu - Shaknov experiment could give suggestive but not conclusive results, I looked further. I had read a paper by Peres and Singer[8] who (following Bohm and Aharonov) had proposed additional tests of the Schrödinger - Furry hypothesis. They suggested 90° scattering as a means for producing the required singlet two - particle entangled state. Hearing that Dan Kleppner and Dave Pritchard at MIT were doing crossed - beam scattering experiments with alkali metals, I went to MIT and gave Kleppner's group a seminar on Bell's theorem. At the end of my talk, Dave Pritchard said to Carl Kocher (a newly arrived postdoc in the group) "Carl wouldn't your experiment test this?" Carl replied "Of course, that's why we did it!" Carl had no reprints on hand, so I returned to New York and eagerly located his article in the library. Good Grief! Kocher and Commins[9] also had only measured coincidences at 0° and 90° relative analyzer orientation. I still had no data!

I next wrote letters to Bell, Bohm and deBroglie, asking (a) did they know of any experiments testing the result, and (b) did they consider that a repeat of the Kocher Commins experiment with improved polarizers at intermediate angles would be convincing. All three courteously replied NO to (a) and YES to (b). Bell himself was particularly enthusiastic about the idea. Thus encouraged, I drafted an abstract for the Washington DC APS Spring Meeting proposing the experiment[10]. As soon as it appeared in print, I received a phone call from Abner Shimony, who said that he and his student Mike Horne had come to the same conclusions. Their pursuit of experimental evidence had led them to Frank Pipkin at Harvard. His student, Dick Holt, was now setting up to do the experiment. After comparing notes, we all agreed to coauthor a Phys. Rev. Letter formally presenting our conclusions[11]. In the process Abner, Mike and I forged a lasting friendship that was to spawn many subsequent collaborations.

Upon receiving my PhD from Columbia, I moved to Berkeley to work as a postdoc for Charlie Townes in radio astronomy. By now I was convinced that QM may well be wrong, with its error having gone undiscovered. There was virtually no experimental evidence plus or minus. The possibility of experimentally discovering a flaw in QM was mind boggling. Since Gene Commins was also at Berkeley, I suggested to both Townes and Commins that they allow me to perform a resurrection of the Kocher - Commins experiment. Townes was intrigued and offered half of my time to this end, 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 far too tolerant. Radio astronomy suffered; Stu and I published our results in 1972[12], and Stu got his PhD with this experiment.

My own (not Stu's) vain hopes of overthrowing QM were shattered by data Stu and I had taken, ourselves! What might I have overlooked? Maybe second generation experiments were warranted, as Bell had suggested. Or, was it that I had assumed the existence of zero'th generation experimental results, that did not, in fact, exist. The CHSH assumption[11] (upon which our cascade photon experiment was based) was motivated by assuming photons to be localizable (spatially boundable) entities. Was there any reason to justify this assumption? Jauch[13] based a similar view on an experiment by Ádám, Jánnosy and Varga[14]. They claimed to show that photons would NOT split at a half - silvered mirror. A critical number in the experiment was the detector efficiency. Upon reading their paper, I was astonished by their claim to have achieved 10% detection efficiency. Stu and I had struggled with much better equipment to get an efficiency of 10^{-3}. They had ignored the solid - angle loss and quoted only the quantum efficiency. Their experiment was not at all convincing!

I thus decided to improve their experiment with convincingly efficient detectors and especially to configure it so that it could not be a null experiment. Upon presenting the idea at the 1972 Rochester Conference on Coherence and Quantum Optics[15], Willis Lamb and Leonard

Mandel queried me "But how are you going to generate a one - photon state?" I told them "Look immediately after the first photon is detected in a cascade decay with weak excitation. Then you know for certain that one and only one photon has been emitted." Although cascade - photon coincidence counting was well known[16], evidently its use to prepare number states was not recognized in quantum optics circles. I published the results in 1974[17]; QM was still alive, and photons did NOT split at a half silvered mirror.

The 1972 Rochester Conference was important for other quantum subculture iconoclasts as well. Ed Jaynes and his students had earlier formulated a semi-classical theory of the Lamb Shift[18]. Unfortunately, when extended to EPR configurations, the theory effectively became an example of the Schrödinger - Furry hypothesis. I thus reviewed the Bohm - Aharanov observation to the conference participants, and noted that both the Kocher - Commins and the Wu - Shaknov experiments refuted the theory[19]. In addition Hyatt Gibbs[20] reported results of an experiment to test the theory's predictions for Rabi flopping against those by QM; QM won again. Shortly thereafter, Jaynes dropped the theory.

I had come to realize that Bell's Theorem had philosophical consequences far beyond the so-called hidden-variables theories that we had earlier advertised it to constrain. Indeed, it constrained even the objectivity of nature itself. My earlier collaborators Abner Shimony and Mike Horne had also come to a similar view, as evidently had John Bell, himself. Mike and I set out to clarify the idea. Abner, although having given us considerable assistance and insight, when offered co-authorship deemed his own contribution insufficient to warrant it. Our paper (we presume) was refereed by Bell who sounded a little hurt that we had overlooked some of his own terse, but salient points. Reflection suggested that we had, and that although Bell had published no details, the seeds of the ideas were contained in his earlier writings. Thus, we rewrote our manuscript from scratch offering full credit to his earlier comments. It was published in 1974[21].

Prompted by our publication, Bell's fuller discussion appeared two years later[22]. He returned our courtesy and acknowledged having profited significantly from our work. Unfortunately, Mike, Abner and I were convinced that he had pushed the arguments too far. He included arguments that we had considered but rejected, because Abner had convinced us we really couldn't justify them. We recognized that Bell really hadn't justified them either. Abner now took the initiative. We three proceeded with a public dialog with Bell over this point in a quantum subculture newspaper called "Epistemological Letters". The interchange has since been formally published in Dialectica[23].

By 1973, the Harvard results were announced in a preprint. Had QM finally failed? No. Eventually, Holt and Pipkin concluded that their result was due to an underlying systematic error. In the mean time, I decided to redo their version of the experiment, and published the results in 1976[24]. I then used the same apparatus to measure the circular polarization correlation[25]. Finally, in 1976 Ed Fry and Richard Thompson published the results of a beautiful experiment[26], and corroborated our earlier results.

In 1976 Antonio Zicchici heard about the growing importance of Bell's Theorem and hosted an International "Ettore Majoranna" Conference in Erice, Sicily. I accepted Bell's flattering invitation to give the introductory address, and summarized my experimental results in a second paper. The sociology of the conference was as interesting as was its physics. The quantum subculture finally had come "out of the closet" and the participants included a wide range of eminent theorists and experimentalists. The consensus was that QM is now far more perplexing than it was before 1964. Soon after the conference, Frank Pipkin[27] and Abner and I[28] wrote reviews of the subject.

A decade after the first Bell's Theorem experimental test by Stu and me, a group in Paris headed by Alain Aspect borrowed the very same interference filters we had used and advanced the two - photon polarization correlation experiments to Bohm and Aharonov's suggested second generation[29], and then repeated a simplified version of my photon splitting experiment[30]. (Perhaps, those filters should be bronzed?) QM survives their efforts also. Unfortunately, all experiments to date have relied on the CH[21] (and/or CHSH[11]) assumptions. Although reasonable and probably innocent, nature may not choose to follow them, as she has already revealed herself to be quite perverse. Thus, in my own opinion (perhaps still a vain one) the important final acid test will be the experiment proposed by Abner and me in our review article and recently given real substance by Ed Fry[31]. Curiously, by the time Ed's new experiment is complete, the time interval between EPR's original paper[32] and Bell's original paper will have approximately doubled - time goes by when you are having fun, and contemporary wisdom does indeed evolve!

[1]D. Bohm, *Quantum Theory*, (Prentis Hall, Englewood Cliffs, NJ, 1952), p. 614.

[2]D. Bohm, *Phys. Rev.* 85 (1952) 169. See also D. Bohm and J. P. Vigier, *Phys. Rev.* 96 (1954) 208.

[3]C. S. Wu and I. Shaknov, *Phys. Rev.* 77 (1950) 136.

[4]D. Bohm and Y. Aharonov, *Phys. Rev.* 108 (1957) 1070.

[5]J. S. Bell, *Physics* 1 (1965) 195.

[6]J. S. Bell, *Rev. Mod. Phys.* **38** (1966) 447.

[7]L. R. Kasday, J. D. Ullman, and C. S. Wu, *Bull. Am. Phys. Soc.* **15** (1970) 586, and *Nuovo Cim.* **25** (1975) B633. See also L. R. Kasday, in *Foundations of Quantum Mechanics*, ed. B. d'Espagnat (Academic, New York) p195.

[8]A. Peres and P. Singer, *Nuovo Cim.* **17** (1960) 964.

[9]C. A. Kocher and E. D. Commins, *Phys. Rev. Lett.* **18** (1967) 575.

[10]J. F. Clauser, *Bull. Am. Phys. Soc.* **14** (1968) 578.

[11]J. F. Clauser, M. A. Horne, A. Shimony, and R. A. Holt, *Phys. Rev. Lett.* **23** (1969) 880.

[12]S. J. Freedman and J. F. Clauser, *Phys. Rev. Lett.* **28** (1972) 938.

[13]J. M. Jauch, in *Foundations of Quantum Mechanics*, ed. B. d'Espagnat (Academic, New York) p. 20, and *Are Quanta Real?*, (Indiana Univ. Press., Bloomington, Indiana, 1973).

[14]A. Ádám, L. Jánossy, and P. Varga, *Acta Phys. Hung.* **4** (1955) 301, and *Ann. Physik* **16** (1955) 408.

[15]J. F. Clauser, in *Coherence and Quantum Optics*, ed. L. Mandel and E. Wolf (Plenum, New York 1973) p. 815.

[16]C. Camhy-Val, M Dreux, A. M. Dumont, and J. Marchal, *Nucl. Instr. and Meth.* **60** (1968) 333; A. M. Dumont and C. Camhy-Val, *Nucl. Instr. and Meth.* **60** (1968) 329.

[17]J. F. Clauser, *Phys. Rev.* **D9** (1974) 853.

[18]M. D. Crisp and E. T. Jaynes, *Phys. Rev.* **179** (1969) 1253, **185** (1969) 2046; C. R. Stroud, Jr. and E. T. Jaynes, *Phys. Rev.* **A1** (1970) 106; E. T. Jaynes, *Phys. Rev.* **A2** (1970) 106, and in *Coherence and Quantum Optics*, ed. L. Mandel and E. Wolf (Plenum, New York 1973) p. 815.

[19]J. F. Clauser, in in *Coherence and Quantum Optics*, ed. L. Mandel and E. Wolf (Plenum, New York 1973) p. 111. See also J. F. Clauser, *Phys. Rev.* **A6** (1979) 49.

[20]H. M. Gibbs in *Coherence and Quantum Optics*, ed. L. Mandel and E. Wolf (Plenum, New York 1973) p. 815.

[21]J. F. Clauser and M. A. Horne, *Phys. Rev.* **D10** (1974) 526.

[22]J. S. Bell, *Communication at the 6th GIFT Conf. Jaca June 1975*, Res Th 2053-CERN, Reprinted in J. S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, (Cambridge Univ. Press, New York, 1987), and also in Ref.23.

[23]J. S. Bell, *Dialectica* **39** (1985) 85,86, A. Shimony, M. A. Horne and J. F. Clauser, *ibid* p. 97, J. S. Bell, *ibid* p. 103, A. Shimony, *ibid* p. 107.

[24]J. F. Clauser, *Phys. Rev. Lett.* **76** (1976) 1223.

[25]J. F. Clauser, *Nuovo Cim.* **33** (1977) 740.

[26]E. S. Fry and R. C. Thompson, *Phys. Rev. Lett.* **37** (1976) 465.

[27]F. M. Pipkin, in *Advances in Atomic and Molecular Physics*, ed. D. R. Bates and B. Bederson (Academic Press, New York, 1978) p, 281.

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

[29]A. Aspect, P. Grangier, and G. Roger, *Phys. Rev. Lett.* **47** (1981) 460, **49** (1982) 1804; A. Aspect, J. Dalibard and G. Roger, **49** *Phys. Rev. Lett.* (1982) 1804.

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

[31]E. Fry, this volume. P. Eberhard (private communication) is similarly considering an "acid" test experiment with photons.

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