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**Selected Papers Of John S Bell
(with commentary)**

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QUANTUM MECHANICS, HIGH ENERGY PHYSICS and ACCELERATORS

**Selected Papers Of John S Bell
(with commentary)**

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John S Bell

*Photograph taken at CERN by Anita Corbin and John O'Grady.
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QUANTUM MECHANICS, HIGH ENERGY PHYSICS AND ACCELERATORS: SELECTED PAPERS OF JOHN S. BELL (WITH COMMENTARY)

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Introduction

This book contains a considerable fraction of the work of J.S. Bell. Very roughly this was split in three parts: accelerator physics, foundations of quantum mechanics and the rest, mainly high energy physics. The accelerator part was organized by Mary Bell, his wife and collaborator on many of these accelerator papers. The part on the foundations of quantum mechanics was selected by Kurt Gottfried, while Martinus Veltman took on the task of considering the remainder. Gottfried and Veltman were actively involved with John Bell both professionally and personally. In preparing this work we were constantly and painfully reminded of the premature end to John's very fruitful life. We truly miss him. He has been a major influence in our lives.

The short description of John's career and honours as well as the introductory notes on section one were written by Mary Bell, those on sections two and three by Veltman and Gottfried respectively. The last part contains a bibliography of published papers. In the notes numbers in square brackets refer to the papers in that list.

Summary of John S. Bell's Scientific Career

John Stewart Bell was born on the 28th of July, 1928, in Belfast, Northern Ireland. On completing his secondary education at the Technical High School, Belfast, in 1944, John started work as a laboratory assistant in the Physics Department of Queen's University, Belfast, under the supervision of Prof. K.G. Emeleus and Dr. R.H. Sloan. After a year, some financial support appeared and he became a physics student. He graduated in 1948 with a B.Sc. in Experimental Physics, and in 1949 with a B.Sc. in Mathematical Physics. He was one of the last students in Belfast of the famous crystallographer Prof. Paul Ewald, a refugee from Germany, to whom

he wrote in 1979: "... your bad fortune in being cast away in Belfast was my very good luck."

In 1949 he joined the Atomic Energy Research Establishment (A.E.R.E.) at Harwell, Great Britain. After a few months he was sent to the theoretical accelerator group of W. Walkinshaw at Malvern. The Walkinshaw group moved to Harwell in 1951, and John remained there till 1953.

In the autumn of that year, 1953, on leave from A.E.R.E., he went to the Department of Mathematical Physics at the University of Birmingham, directed by Prof. Sir Rudolf Peierls. There he profited from the supervision of the late Paul T. Matthews. On his return to Harwell in 1954 he joined the theoretical group of the late T.H.R. Skyrme. He completed his thesis for Birmingham there (Time Reversal in Field Theory and Functional Methods in Field Theory, both contained in this book), and obtained a Ph.D. in 1956. In November 1960, he left A.E.R.E. to join the Theoretical Physics Division at CERN, Geneva. Apart from a leave of absence at SLAC, Stanford in 1963–1964, he remained there until his death on the 1st of October, 1990.

In addition to his work for which he was paid, his "hobby" was the foundations of quantum mechanics. The papers of the late David Bohm on hidden variables in the early fifties rekindled a long-held feeling of dissatisfaction. This interest continued all his life.

In 1972 he was elected a member of the Royal Society of London. He received the Reality Foundation Prize in 1982 (shared with J.F. Clauser). Other awards were:

- 1987 Elected Honorary Foreign member of the American Academy of Arts and Sciences.
- 1988 Dirac Medal of the Institute of Physics (London).
- 1988 D.Sc. h.c. Queen's University, Belfast.
- 1988 Sc.D. h.c. Trinity College, Dublin.
- 1989 Dannie Heineman Prize for Mathematical Physics, American Physical Society.
- 1989 Hughes Medal, Royal Society, London.

Notes on Section 1.¹ Accelerator Physics (*edited by M. Bell*)

John S. Bell wrote a number of papers on accelerators, both linear and circular. He, along with W. Walkinshaw, introduced, for example, mathematical methods for disc-loaded linear accelerators. At the time only mechanical desk-top calculators were available. However, the methods are still useful, either to get a fairly quick answer, or as a starting point for large-scale computer programs. Most of these early papers were A.E.R.E. reports, and are now out of print. We have selected [21], "Linear Accelerator Phase Oscillations" as a representative paper of this early period.

¹ Mary Bell, June 3, 1994.

The papers on electron cooling arose in connection with the “ICE” (Initial Cooling Experiment) project at CERN ([122], [123]). In preparation for the AA (Antiproton Accumulator) machine, both electron and stochastic cooling (which was actually used), were studied.

The interest in Radiation Damping arose because of the LEP (Large Electron Positron) machine at CERN, that in quantum bremsstrahlung because of the studies for linear colliders [126]. The other three papers in this section ([127], [134] and [138]), with co-authors J.M. Leinaas and R.J. Hughes, are on the Unruh effect, which links Hawking radiation from black holes to quantum fluctuations of electrons in storage rings.

Notes on Section 2.² High Energy Physics (*edited by M. Veltman*)

The papers in this section are mainly about high energy physics and field theory. Up to about 1961 John wrote quite a few papers on subjects in the domains of many body physics and nuclear physics. His 1959 lectures on the many body problem in Naples, are perhaps the best reference in this context [39]. His work in nuclear physics is more extensive, among that work with Skyrme (on nucleon anomalous moments) and especially on the nuclear optical model (with Squires). The latter has received quite some attention among nuclear physicists, and we included this important paper [37]. At the same occasion as mentioned above (Naples, 1959) he also lectured on this subject [38], and the text is contained in the same book.

His very first work in the domain of field theory is a very important one and concerns the CPT theorem [22]. It was unfortunate for him that Pauli and Lüders had also tackled the subject and simply dominated it; John’s article (Time Reversal in Field Theory) was, as I remember from personal discussions, conceived independently. It is also of course very different, and perhaps today more relevant, than the rather formal field theory arguments of Lüders. In his introduction John acknowledged the paper of Lüders, and never sought to even suggest that his work had been done “independently.” Problems relating to time reversal invariance were very fascinating to him, and that interest can be seen as a theme in his scientific career. It really reflected his interest in the very foundations of physics. After this first fundamental paper there are several other papers on the subject, notably the very important review paper with Steinberger ([63], reproduced here) and the truly wonderful paper ([59], with Perring, also included) on the idea that the observed CP violation in K decays was due to a long range field coupled to hypercharge. As is the hallmark of a good theory, it was easily destroyed by subsequent experiments, but the paper remains as one of the most daring, beautiful and simple of the many written worldwide during that memorable episode in 1964.

Ever since coming to CERN, in 1960, John felt it part of his responsibility to work on subjects related to the activities at the laboratory. To be sure, he was easily persuaded to engage in the discussion and study of any subject in physics;

² Martinus Veltman, July 8, 1994.

he simply had a voracious appetite in that sense. The collaboration with me on the subject of vector boson production by neutrinos was certainly also motivated by that sense of duty [53],[54]. Even so, it was a very exciting period, as it drew us into intense discussions with the physicists working on the neutrino experiment. Every theorist ought to pass through such an experience; it makes you truly aware of what reality is. We certainly missed a few very great opportunities in that episode as Donald Perkins, a member at the time of that neutrino group, is fond to point out. Three papers relating to that period are included, among that the paper ([52], with Løvseth and myself) summarizing the neutrino experiment at the 1963 Sienna conference. It should perhaps be mentioned that in one of those papers ([54], on the polarization of vector bosons produced by neutrinos, 1963), on John's initiative, the consequences of time reversal invariance were discussed. Unfortunately, vector bosons were never produced in this type of reactions; they are too heavy for that.

Nuclear physics was always an interesting subject to John. After his earlier work on the nuclear optical model, he became very interested in neutrino reactions with nuclei. In particular, the hypothesis of the Partially Conserved Axial Current (PCAC) connects neutrino reactions and pion scattering, and in this context the concept of the "nuclear shadow" may be mentioned. His work with Llewellyn Smith [78], [80], dating from 1970, is contained herein.

Like many of his generation, the development and success of gauge theories, largely within the domain of perturbative methods and Feynman diagrams, did not really suit his taste and did not become part of his activities. Nevertheless, his influence on the subject has been most important, mainly through two papers. The first, on the famous anomaly ([74], with Jackiw, contained in this book), stands today as a classic. His second, much less appreciated, is on the relation between current algebra and gauge theories [69], [72]. Current algebra was the hot topic of 1966, not least through the work of Adler and Weisberger, linking current algebra to observed data. A paper written by me derived these successful relations from purely ad hoc assumed properties of the weak currents, axial and vector. It was John who pointed out that these equations, and in fact current algebra, can be seen as a consequence of gauge invariance [69]. This paper is included here. That analysis was instrumental and led to a renewed interest in gauge theories, with the familiar consequences. Related work, with Sutherland, on current algebra and the decay $\eta \rightarrow 3\pi$, remains an important reference and is contained in this book [70].

After the rise in popularity of gauge theories, in 1971, John attempted to understand the mechanics of those theories in his own way, by relating high energy behavior of amplitudes to the symmetry structure of the theory [87]. That was also done by a number of other authors, among them Llewellyn Smith, who at that time often worked and discussed with John. Apart from this one publication John never ventured any further into the subject.

Another interest that John had for quite some time is the spectrum and structure of low mass hadrons (pions, vector mesons such as the ρ and ω , and baryons such as the proton), and later on hadrons containing heavy quarks. As noted above, one of his early papers, with Skyrme [29], was on the subject of anomalous moments

of nucleons. In the middle Sixties SU(6) was the hot subject for some time, to a large extent because of the relation found (by Pais and Beg) between proton and neutron magnetic moments. That relation is $\mu_n/\mu_p = -\frac{2}{3}$, and it holds quite accurately. Many of us were fascinated by this agreement, and it made SU(6) a very popular and intensively studied theory. However, it became clear that the theory was far from perfect, and specifically that at best the symmetry was restricted to non-relativistic systems with all momenta zero. In that approximation one cannot study magnetic moments, and the truly remarkable agreement could not really be seen as an argument in favor of SU(6). While most of us left the subject, John maintained his interest, in the belief that this could not be an accident. This, or so I think, was his main motivation for pursuing the Melosh transformation and the associated insight in low energy properties of particles. Lectures given on this subject in Schlading, Austria in 1974 have been included in this book [89]. It remains an important and interesting subject. His work on $\widetilde{U}(12)$ and null-plane field theory (with Ruegg) also falls into this category and a selection is included as well [94], [96]. In later years he became interested in modern models of hadrons, based on quantum chromodynamics, and ([113], [118] with Bertlmann) he wrote a number of articles relating to and emphasizing the work of Shifman, Vainshtein and Zakharov. Several of those papers are included here [120], [128], [130].

Notes on Section 3.³ Foundations of Quantum Mechanics (edited by K. Gottfried)

John's 'hobby' — questioning quantum mechanical orthodoxy — yielded the most startling and disturbing insights into the foundations of physics since the great debate that followed the discovery of quantum mechanics. That debate was, in a sense, tabled after the Einstein–Podolsky–Rosen paper of 1935. It was John's re-examination of EPR, fully three decades later, that brought new generations of theoretical *and* experimental physicists, as well as philosophers, into an ongoing examination of what all agree is at once the most successful and most mysterious of physical theories.

Section 3 of this volume contains most of John's papers on the foundations of quantum mechanics.⁴ These papers fall into three major categories:

- (i) those that bear on whether an acceptable theory can be constructed by ascribing the statistical aspect of quantum mechanics to 'hidden variables' not accounted for by the orthodox theory;
- (ii) those that constitute an unrelenting critique of the central role given to the observer and measurement in orthodox quantum mechanics;

³ Kurt Gottfried, July 3, 1994.

⁴ *Speakable and Unspeakable in Quantum Mechanics*, Cambridge University Press, 1987, contains all of John's papers on the foundations of quantum mechanics written up to 1987, except for two preliminary versions of later papers that are in the Cambridge volume.

- (iii) those that comment on attempts by others to construct theories that seek to meet this criticism, and by that token would replace orthodox quantum mechanics.

The two classic papers in the first category were written at about the same time, though published two years apart. In the first of these [57], John reviewed the question of whether the “states” of orthodox quantum mechanics could actually be ensembles of states that are free of dispersion, specified by additional (hidden) variables, with each member of the ensemble giving the exact value of an individual measurement as determined by the values of the additional variables. Following the publication of von Neumann’s famous *The Mathematical Foundations of Quantum Mechanics* in 1932, it was widely believed that this question had been answered in the negative once and for all. John, however, showed that von Neumann’s proof hinged on a physically unjustified assumption. He then went on to supply another no-go theorem which reaches the same conclusion without invoking von Neumann’s unreasonable assumption.⁵ On the other hand, this theorem does *not* apply to theories in which “the result of an observation may ... depend not only on the states of the system (including the hidden variables) but also on the complete disposition of the apparatus.” That, of course, is an unwanted proviso, for if there is one point that quantum mechanics had made so convincingly it is that one cannot ignore the apparatus.

John’s most profound and remarkable contribution to physics, and indeed to knowledge on a far broader front, is “On the Einstein–Podolsky–Rosen paradox,” [60], published in 1964. It is this paper which contains what has since become known as Bell’s Theorem. This too is a no-go theorem, but with a far weaker assumption: the “parameters [hidden variables] added to quantum mechanics to determine the results of individual measurements” must not be such that “the setting of one measuring device can influence the reading of another instrument, however remote.” Indeed, if the hidden variable theory is to reproduce the statistical predictions of quantum mechanics, signals between remote instruments “must propagate instantaneously, so that such a theory cannot be Lorentz invariant.” Or, as it is customarily put, Bell’s Theorem states that no *local* hidden variable theory can reproduce the results of quantum mechanics.

The truly remarkable feature of The Theorem is that it showed that in experiments of the EPR variety the predictions of quantum mechanics and *any local* hidden variable theory disagree, and that in principle that disagreement should be measurable. The conversion of “in principle” into practical and actually done experiments took the efforts of an ingenious community of theorists and experimenters, which confirmed quantum mechanics.

⁵ John called this important result a corollary of a paper by A.M. Gleason, *J.Math. & Mech.* **6**, 885 (1957), which may explain why it is usually referred to as the Kochen–Specker theorem [S. Kochen and E.P. Specker, *ibid.* **17**, 59 (1967)]; however, in [125] John cites the earlier paper E. Specker *Dialectica* **14**, 289 (1960).

Although John was certainly not astonished by this outcome, he remained deeply disturbed that his Theorem and the experiments it had stimulated led to the conclusion that no 'objective' interpretation of quantum mechanics could be devised that would obey the strictures of relativity. He came to hold a very radical view as a result of this conundrum. When asked⁶ whether he "would prefer to retain the notion of objective reality and throw away one of the tenets of relativity," he answered "Yes. ... [I] want to go back to the idea of an aether ... because in these EPR experiments there is a suggestion that behind the scenes something is going faster than the speed of light."

The second major theme that runs through John's writings on quantum mechanics is already evident in 1966 (i.e. at about the same time as those on hidden variables) in an article [65] written with Michael Nauenberg:⁷

We look forward to a new theory which can refer meaningfully to events in a given system without requiring 'observation' by another system ... it seems that the quantum mechanical description will be superseded. In this it is like all theories made by man. But to an unusual extent its ultimate fate is apparent in its internal structure. It carries in itself the seeds of its own destruction.

He elaborated this critique in a sequence of articles spanning a quarter century. Thus he argued [86] that the distinction between system and measuring apparatus — object and subject — is inherently undefined, making the theory "only *approximately* unambiguous, only *approximately* self-consistent," and introduced the concept of *beable* — the assignment of objective properties to objects, independent of any observation on the object, in contrast to the observables of the orthodox theory, which he called [98] "a rather woolly concept."

Later he put the issue forward with still greater sardonic force [117]:

The problem is this: quantum mechanics is fundamentally about 'observations.' It necessarily divides the world into two parts, a part which is observed and a part which is observing. The results depend in detail on just how this division is made, but no definite prescription for it is given. All that we have is a recipe — *put sufficiently much into the quantum system that the inclusion of more would not significantly alter practical predictions* — which, because of practical human limitations is sufficiently unambiguous for practical purposes ...

In my opinion, this view is too complacent. The pragmatic approach which it exemplifies has undoubtedly played an indispensable role in the evolution of contemporary physical theory. However, the notion of the 'real' truth, as distinct from a truth that is presently good enough for us, has also played a positive role in the history of science. Thus Copernicus found a more intelligible pattern by placing

⁶ *The Ghost of the Atom*, P.C.W. Davies and J.R. Brown (eds.), Cambridge University Press, 1986; p. 50.

⁷ Emphasis added.

the sun rather than the earth at the center of the solar system. I can well imagine a future phase in which this happens again, in which the world becomes intelligible to human beings, even to theoretical physicists, when they do not imagine themselves to be at the center of it.

His last paper *Against 'measurement'*, [144], published at the time of his death, is a fervent jeremiad summarizing a lifetime of reflection on what he saw as the fatal flaws of the orthodox theory.⁸ It begins with a lamentation:⁹

Surely, after 62 years, we should have an exact formulation of some serious part of quantum mechanics? By 'exact' I of course do not mean 'exactly true.' I mean only that the theory should be *fully* formulated in mathematical terms, with *nothing left to the discretion of the theoretical physicist* ... until workable approximations are needed in applications.

That orthodox quantum mechanics fails to satisfy this requirement becomes evident when one compares its formulation to classical electrodynamics, which John held up as the model of a respectable theory.¹⁰ For in classical electrodynamics, the new concept — the electromagnetic field — is defined by the Lorentz force and Newton's equations of motion, i.e. the theory is "fully formulated in mathematical terms" for anyone who knows the pre-existing concepts of classical mechanics. That is not so for quantum mechanics: Schrödinger's equation does not specify what someone who only knows classical physics is to make of Schrödinger's new concept Ψ . A separate set of rules, which invokes still other concepts and whose consistency with the Schrödinger equation is at best problematic, must also be known before contact can be made between Ψ and the world about us. It is this logical gap, which has no precedent in successful 'fundamental' physical theories, that John believed held the seeds of destruction within orthodox quantum mechanics.

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⁸ Herein John introduces a pair of superbly sarcastic acronyms, FAPP = for all practical purposes, deployed repeatedly with great relish to castigate sloppy arguments, and QED FAPP, for "proofs" dependent on such means.

⁹ Emphasis added.

¹⁰ For an elaboration on John's views in this regard, see K. Gottfried, *Physics World*, October 1991, pp. 34–40. The same point could be made with Newtonian dynamics: it is fully formulated to anyone who knows the pre-existing concepts of Euclidean geometry and time.

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