Chapter 2
John Stewart Bell, Quantum Information and Quantum Information Theory

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It is traditional to take virtually for granted [1, 2] that John Bell's work on the foundations of quantum theory led fairly directly to the founding of the discipline of quantum information theory, and thus it is natural to give Bell credit perhaps for the very existence of this subject.

This tradition obviously provides a massive boost to anyone, in the present or in the future, who has the task of describing Bell's importance and demonstrating his stature to non-scientists as well as to scientists. Anybody not interested in such arcane matters as determinism, locality and realism may be excited by his having provided the means of breaking codes, of running safe and efficient elections, or even of teleportation.

Of course looking at events historically there is every reason to take the influence of Bell on quantum information theory as obvious and beyond question. Many of the people involved in foundational studies moved on seamlessly to work on quantum information, though they were joined there, particularly in the study of quantum computation, by many with little genuine interest or understanding of Bell's work, and Mermin [3] has pointed out in his introduction to quantum computation how little physics is needed to work in this area.

Experimental and theoretical techniques designed for one or other of the areas of quantum foundations and quantum information theory were often capable of being adapted to be used in work on the other. For example experimental methods used in the study of quantum teleportation [4] were the basis of those used to demonstrate the existence of the GHZ states [5].

And of course it is also natural to bring up the matter of entanglement. It is surely fair to take note of Bell's importance in the full realisation of the significance of entanglement. Its importance was probably first pointed out by EPR and was stressed by Schrödinger, particularly in his famous statement that it was 'not *one*

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but *the* characteristic feature of quantum mechanics, the one that enforces the entire departure from classical thought' [6].

Subsequently, however, it was not discussed in a substantial way until Bell used it in his important discoveries [7, 8]. Then, of course, and almost entirely as a result of his influence and his theorems, it became central in at least all the standard examples met in both quantum foundational studies and quantum information theory. It must surely be fair to give him credit for that. So we may feel quite happy in talking about Bell's major part in the development of quantum information theory. But it is still slightly different to claim that his work was its main theoretical stimulus or underpinning.

We may remember David Deutsch, who was, at least in many people's views, the genuine founder of the study of quantum computation [9]. His important insight, obvious enough in retrospect, was that, not only must there be a subject of quantum computation, with its own theoretical basis, but that, at least from a rigorous point of view, we do not have classical and quantum computation on an equal footing, any more than classical and quantum physics are on an equal footing.

Rather quantum physics and quantum computation are fundamental, and classical physics and classical computation, are again, in principle, merely good approximations, often extremely good approximations, to the quantum versions.

Thus Deutsch declared that the classical theory of computation, unchallenged for more than fifty years, was obsolete, and it was necessary to rewrite the Church-Turing argument to meet quantum-mechanical requirements [10]. (Turing, Deutsch said, had restricted his argument to paper, thinking, or at least acting as though he thought, that paper was classical, but it wasn't—it was quantum.)

Incidentally Mermin [3] has effectively made a reply to Deutsch's point about the primacy of the quantum computer. To say that a quantum computer is one that obeys the laws of quantum mechanics, he says, is a temptation to be resisted. It would imply that any laptop or even a mainframe computer is a quantum computer, and that is just not how we think of things. Rather: 'A quantum computer is one whose operation exploits certain very special transformations of its internal state'.

For the present argument, the important and obvious point is that Deutsch's argument had nothing to do with the work of Bell. Rather in terms of analysis of quantum theory it had a close, perhaps a symbiotic, relationship with one of Deutsch's other core beliefs, that in many worlds [10, 11]. For Deutsch, the existence of many worlds could be read straight off from quantum theory by any unprejudiced student, but, if that argument was questioned, he could argue that they certainly explained the possible vast speed-up of a quantum computer—the calculations were being carried out in the many worlds; this is known, of course, as *parallelism* [9, 10].

Deutsch's claim that quantum computation relies on many worlds and parallelism has been strongly criticised by Steane [12], who argues that a quantum computer requires only a single universe, and that is misleading to argue that quantum computers perform more operations than those allowed by a single universe.

Steane himself has suggested that the real source of speed-up in quantum computation is entanglement. As we have already said entanglement is indeed present, and indeed seemingly centrally important, in the examples normally considered, so it seems to be a very natural suggestion. However various arguments have shown that quantum computation may proceed with the usual speed-up without any entanglement at all [13–15].

As well as parallelism and entanglement, the combination of superposition and interference has been suggested, but certainly does not *guarantee* any speed-up as compared to classical computation.

Another contender is contextuality, which was essentially Bell's response [7] to the demonstration by Kochen and Specker [16] that without contextuality hidden variables could not exist. Contextuality says that measurement depends on context—the result of measuring physical quantity A depends on whether it is measured together with physical quantity B or C. Again it may be relevant for the speed-up in quantum computation in some cases [17].

Indeed every attempt to isolate 'the' ingredient of effective quantum computation, essential in every case, seems doomed to failure. Overall none of these factors seem to be either necessary or sufficient for quantum computational speed-up. It may, in the end, be best just to say that the power of quantum computation is a result of a fusion of every aspect of quantum theory, different elements of quantum computation relying on different aspects of quantum theory [18].

While on the question of the centrality of entanglement in quantum information theory, we will remember, of course, that one of the central methods of quantum cryptography, BB84, of course, at least in its basic form [19], does not rely at all on entanglement. Indeed it requires only the most elementary aspects of quantum theory, well-known in fact from the 1920s.

In fact when we analyse the extent to which John Bell's work led to quantum information theory, we have two big difficulties. First, as we have seen, it is difficult or impossible to understand what the essential features of quantum information theory actually are. But secondly, over and above the actual mathematical logic of Bell's inequalities, there are very different understandings of what the conclusions of the experiments will be, once all loopholes are removed. Some, perhaps most, believe they show that local realism is impossible [20]. Some believe that non-locality has actually been demonstrated [21, 22]. Still others just say they show that Copenhagen was right all along [23].

However first we will try to discover Bell's actual views on the idea of information. To what extent might he have found palatable the views of those, such as Brukner and Zeilinger [24], Vedral [25], Wheeler [26], Lloyd [27], and many others, who stress the primacy of information? While those mentioned have a wide range of views, they might be summed up by saying that they regard information as the fundamental quantity in the universe, and that using this idea to derive the basic laws of physics makes clear the reason for their quantum nature.

At first sight it is unlikely that Bell would have given much support to this set of ideas, as it seems that his only comment on information comes in his famous or notorious paper *Against "measurement*", originally presented at the Erice conference *62 years of uncertainty* in August 1989, and published in *Physics World* in August 1990 [28], sadly at about the time of Bell's sudden death.

This paper, of course, was a diatribe against a list of words that Bell claimed were used illegitimately to 'explain' the results of quantum mechanical experiments. Most of the words were criticised either for implying an artificial division of the world between measuring and measured systems, with the intention of being cavalier about interactions across the divide, or alternatively for defying precise definition. Of course 'measurement' was, in his view, the 'worst', and most of the rest of the paper is used to savage various attempts to use this word to explain how the results of experiments are produced, those of Dirac [29], Landau and Lifshitz [30], Gottfried [31] and van Kampen [32].

All of the words except one—'system', 'apparatus', 'environment', 'microscopic, 'macroscopic', 'reversible', 'irreversible', 'observable' and 'measurement' itself clearly relate to Bell's bugbear about conventional approaches to quantum theory—the 'shifty split' between apparatus and system. Bell had been suspicious of this division from his earliest days as a student of quantum theory, and had been actively hostile to it for many years, so it was scarcely surprising that all these words appeared on his hit list [33, 34].

Much more surprising was the inclusion of the word 'information'. The only comment is: 'Information? *Whose* information? Information about *what*?' The inclusion of this word is indeed surprising because there is little other evidence that Bell had been particularly concerned with the use or misuse of this word, or its (possible) synonym, 'knowledge'.

There is no doubt that these words had often been used in a way that was quite capable of annoying Bell. They were often used, by quite a variety of writers, to provide what seemed to be an easy 'explanation' of the conceptual problems of quantum theory, but which in fact explained nothing and avoided all the real problems.

Perhaps the simplest misunderstanding is just to assume that *all* a measurement or an experiment does is to provide information about a property of an observed system that we may regard as existing before, during and after the measurement process. There is every temptation to regard the actual system just as conventionally or classically as we wish, with all observables having precise values at all times—in a sense the measured system is not itself really part of the quantum world, which is *just* the information.

That may perhaps apply just when we gather information for the first time. Collapse of wave-function occurs when we have some prior knowledge but perform an observation or experiment to bring our knowledge up to date. Naturally our information changes. So in this approach collapse appears to be altogether a straightforward process, merely representing the alterations in our brain when we take in factual information. Collapse need not imply any change of any type in the observed system.

Of course there may be complications. It may be that we initially know the value of s_x and come to know the value of s_y. We must then recognise that our knowledge of s_x is defunct. But a knowledge or information interpretation certainly does not necessitate, but may often encourage, the belief that all properties of a system (such as s_x and s_y) *have* precise values at all times even though our information about these values is necessarily limited.

Bell may well have seen such interpretations claiming all the conceptual gains of hidden variables without accepting any of the accompanying difficulties of such theories, the labour in their creation, or, of course, the struggle in analysing and making sense of their own properties.

While it has been said that Bell had no particular target in mind, it may be suggested that his criticism was rather obliquely on the practically universal belief that quantum theory is about the results of measurements, rather than what actually 'exists', and just a little less obliquely on the well-known views of John von Neumann and Eugene Wigner.

For von Neumann, [35] final collapse was in the 'abstract ego' or perhaps mind of the observer, not too far from talking in terms of knowledge. Bell was, of course, a great admirer of Wigner, but he did make Wigner's idea of consciousness [36] performing the required collapse of wave-function one of his three 'romantic', and hence in Bell's view 'bad' worlds of quantum mechanics. (The kind of interpretation that brought in the same type of stochastic terms mathematically and so in what Bell took to be a more professional or 'unromantic' way, and was thus a 'good' world became exemplified in GRW [37].) Thus Wigner may also have been in his sights in this paper.

Another suggestion is that of a paper of Cavalcanti and Wiseman [38], that studies two of Bell's papers in which he analyses his theorem in a little more depth than when it was originally presented. These are 'The theory of local beables' [39] from 1976, and 'La novelle cuisine' [40], published after his death in 1990.

In these papers, having reached the general existence of non-locality, Bell asks whether this implies that 'we' can signal faster than light. He produces an argument showing that this is not possible, and his argument in itself is not much different from that produced by others. He divides his 'beables' into two classes, 'controllables' and 'uncontrollables'. Controllables may send or receive signals, but uncontrollables may only receive them. What he calls an 'exercise' in quantum mechanics shows that a change in a controllable variable cannot result in a change in a spacelike separated region.

However his words show a lot less enthusiasm for the analysis and the very idea of signal locality. To give a proper answer to the question, he says, or in other words to discuss signal locality, actually requires at least a schematic theory of 'what "we" can do', or in other words 'a fragment of a theory of human beings'. Clearly he is unhappy about the use of such anthropocentric ideas as 'controllability' and, in the background, 'information'.

In the later paper he questions whether 'no signalling faster than light' can be an expression of the fundamental causal structure of theoretical physics, but he rejects the idea. 'No signalling', he says, should really be expressed as 'We cannot signal faster than light', which, he says, immediately provokes the question: 'Who do we think "we" are?'

Do 'we' include chemists or just physicists?
Plants or just animals?
Calculators or just computers?

The 'we' who can 'signal', he says are the same 'we' who can manipulate 'external fields', and, in particular, the same 'we' who take 'measurements'.

So in this paper, written at about the same time as 'Against "measurement"', we do seem to have reached the closest connection between 'information' and the other words on Bell's banned list.

Of Bell's questions, the first—'Whose information?' may have genuinely been a request for a coherent answer, one which was actually to be supplied over the next years. Probably more likely is that it was intended to be pointing out that Bell considered to be an obvious inconsistency of the idea—surely different people must have different amounts of information. As we shall see, this was not necessarily a defect of the theory.

The other question—'Knowledge about what?'—perhaps brought out Bell's main frustration. Information, he assumed, must be about something, in which case, why not discuss what it is actually about? In other words return the discussion to atoms, molecules, electrons and discuss, for example, how they behave at a measurement, and which if their properties may have values simultaneously. Bell may have felt that talk of information or knowledge may not have actually been wrong, but rather unhelpful; it may have failed to distinguish between things that we are prohibited from knowing by the laws of quantum theory, and those that we could know but have not bothered to find out!

In terms of the later development of quantum information theory it may be remarked that there are perhaps two different definitions of information being used. What we may call information^1 is by definition telling you about something—it has some content. Thus a parent might see their child's scribble—it has no meaning, no information. When the parent is told that it is in fact a picture of him or her, it immediately becomes information^1. Bell, it must be assumed, was thinking of information^1. Information as in information theory, classical or quantum, is information^2.

Bell's paper was regarded as a polemic, and it was not surprising that quite a few replies were sent to *Physics World*, among them letters from Gottfried [41] and van Kampen [42] defending their arguments, and from Squires [43] supporting Bell.

The most interesting was from Peierls [44], which managed to include, in a totally non-contrived manner, both of Bell's targets, measurement *and* information. Peierls was a great admirer of Bell. He had given Bell his first chance to enter genuinely mainstream physics by advocating his move at Harwell to a division devoted to tackling such problems as quantum field theory and particle physics [34], and, for the rest of Bell's career, Peierls had probably been split between admiration of his mathematical ability and his honesty, and horror at his constant attacks on the conventional interpretations of quantum theory [45]. A conspiracy theorist might think that Bell had deliberately included the attack on 'information' to allow Peierls to give full rein to his beliefs.

Peierls regarded himself as a complete believer in Bohr's views, to the extent that he rejected the term 'Copenhagen interpretation'. For him, using this term implied that there were several interpretations of quantum theory, of which Copenhagen was just one. For Peierls, there was only one interpretation, so if you say 'Copenhagen interpretation' you really mean 'quantum mechanics' [46].

Yet Peierls' reply to Bell, titled 'In defence of "measurement"', seemed to be very different from what would come from the mouth of Bohr or Heisenberg, being, as stated above, in terms of knowledge, indeed very much along the lines sketched above in a rather cavalier way but with care taken to avoid the obvious problems. In our previous terms, Peierls' knowledge was presumably still information[1].

If our knowledge is complete, by which Peierls meant the greatest that could be allowed by the laws of quantum theory, in particular the uncertainty principle, we may represent this knowledge with a wave-function. However for less knowledge we must use a density-matrix. Uncontrolled disturbances may reduce our knowledge. Measurement may increase it. If we start with the wave-function case and gain new information, some of the previous information must be lost, and so on.

When there is a change of knowledge, Peierls says that the density-matrix must obviously change, but this is not a physical process so we should certainly not expect the change to follow the Schrödinger equation. This argument does indeed seem an excellent way of giving some explanation of von Neumann's poser of the contrast between type 2 processes, processes outside of measurement, which follow the Schrödinger equation, and type 1 processes, measurements, which follow completely different dynamics such as collapse.

On the first of Bell's questions—whose information?—rather than regarding the question as a means of ridiculing the whole idea, Peierls produced a intelligent and convincing answer. His basic point is that the knowledge of different observers must not contradict each other. Contradiction would occur if one observer 'knew' that s_z was +½, while another 'knew' that it was −½, but it would also occur if one 'knew' that s_z was +½ while another 'knew' that s_y was +½. Mermin [47] says that Peierls uses two conditions. A strong one is that the density-matrices of the two observers must commute, while a weaker one is just that the product of the two density-matrices must not be zero.

Mermin also points out that Peierls does not answer Bell's other question—information about what? He may have felt that the whole point of the Bohr approach was that one need not, could not and should not answer it. It might be surmised, though, that if an observer *knows* that s_z is +½, then even an orthodox interpretation would admit that there is a system with s_z equal to +½. It would probably prefer, though, not to comment on the values of s_y or s_x.

Mermin reports that he was initially on Bell's side in his clash with Peierls, but his view was changed by sustained interaction with those involved in quantum computation, who were convinced that quantum theory was 'self-evidently and unproblematically' a theory of information. In our previous terms, this is, of course, information[2].

Like Peierls, Mermin was keen to answer the question—Whose information? By demonstrating a subtlety of entanglement, he was able to demonstrate a weakness in Peierls' strong condition and to suggest improvements, thus again demonstrating that, if Bell thought the question showed up the weakness of the whole position, he was definitely wide of the mark.

However Mermin rejected altogether Bell's other question—Information about what? He described this as a fundamentally metaphysical question and considered that it should not distract 'tough-minded physicists'. It is not possible, he says, to discover whether information is about something objective or just about other information, and one certainly should not waste time trying to do so. Of course this is information^2.

And of course once one recognises the primacy of information, the whole argument from the quantum to information may be reversed. Rather one argues from information to the physical Universe. For Brukner and Zeilinger [24], the obvious quantisation of information is the cause of the quantisation we see in the Universe. For Vedral [25], information is the only entity on which we may base our most fundamental theories; for example evolution is *purely* the inheritance of information with occasional changes of the basic units, the genes. For Wheeler [26] too, the concept of information may unlock some of the most basic mysteries of the Universe. For Lloyd [27], the Universe is just a quantum computer, and what it computes is just its own behaviour. Smolin [48] believed that quantum information is a possible alternative to string theory as an attempt to solve the most basic problems of physics. And so on.

Now let us return to Bell. It must seem bizarre to allocate credit to him for this development, when his only contribution consisted of seven words, of which four were the same—'information', and also when he seemed to end up on the wrong side of the argument. Yet it may also be said that Bell did not believe in wasting words. He took an issue that had maybe been under the radar for half a century, challenged some basic assumptions, and asked precisely the telling questions, the questions that would take others so far, even if they took them to regions which would have surprised him.

I now want to go back to the beginning of quantum computation and the work of Richard Feynman. If you take the founder of quantum computation as David Deutsch, as I have done, it is natural, following Brown [49], to think of Feynman's earlier work as rather analogous to 'the old quantum theory', the period between 1900 and 1925 when many important results were obtained but without any rigorous foundation to the work.

Feynman published two important papers, the first, a conference paper from 1981, published in 1982 under the title 'Simulating physics with computers' [50], and the second, 'Quantum mechanical computers' [51], published in 1985.

In the first he asks a number of important questions about simulations. In each case the simulation must be exact, and the computational work required must increase only in proportion to the size of the system being studied, not exponentially.

First he shows that a classical system may simulate a classical system, but it cannot simulate a quantum system. He then asks if it can simulate the quantum system probabilistically. The answer is again—no, but in this case Feynman has to provide a detailed argument. He examines in some detail an EPR system, and calculates the probabilities all the way through. Everything works out well—with the exception that some probabilities used in the course of the analysis, not in the answers, have to be negative. Feynman now demonstrates why negative probabilities cannot be avoided, and he effectively works through a proof of Bell's theorem.

Tony Hey, who has edited both Feynman's own work in this area (with Robin Allen) [52], and also more recent papers by his collaborators [53], comments [53] that 'Only Feynman could discuss "hidden variables", the Einstein-Podolsky-Rosen paradox and produce a proof of Bell's Theorem, without mentioning John Bell'!

Hey appears to assume that Feynman had encountered Bell's work, but had perhaps forgotten the name of the originator and worked through the analysis himself.

It must be said that Feynman did have form for this kind of sloppiness. Most significantly, in 1957 George Sudarshan, as a research student being supervised by Robert Marshak, had demonstrated in great detail that the structure of the weak interaction was of a V-A type (vector minus axial vector). However Feynman and Murray Gell-Mann were privately informed of this result and thought about the matter themselves, but forgetting their source of information, published the result in their own names and gained priority over Sudarshan and Marshak. Feynman did write later that: 'The V-A theory was discovered by Sudarshan and Marshak, and published by Feynman and Gell-Mann.' [54–56].

But it is, of course, quite possible that Feynman did produce the Bell-type analysis independently of Bell. Gottfried and Mermin [57] do say that the actual analysis is 'extraordinarily elementary'. Presumably it is having the motivation to think about the matters that is requires a special intellect [57, 58], and Bell and Feynman would both come into that category.

It may be stressed then that for Feynman it is Bell's theorem that makes going beyond classical computers inevitable, and he discusses very briefly the possibility of quantum computers or 'universal quantum simulators'.

However more interestingly he remarks that he often has fun trying to squeeze the difficulty of quantum mechanics into a smaller and smaller place—to isolate the essential difficulty, so as to give the possibility of analysing it is detail. He feels that he has located it in the contrast between two numbers—one required by quantum theory, the other the demand of classical theory—a direct result of a Bell type of analysis.

Thus the significance of Bell's work, according to Feynman, can scarcely be over-exaggerated. It is the core element of quantum theory!

Let us briefly turn to Quantum Key Distribution (QKD). It is well-known that Nicolas Gisin, Antonio Acín and co-workers have produced much detailed analysis of the part that the Bell Inequalities play in many aspects and many variants of QKD.

Here I just want to pick up on one important point that they make [59]. It would perhaps normally be said that QKD relies on the fact that, if Alice and Bob are sufficiently entangled, then Eve is effectively factorised out. Yet it could be the case that Alice and Bob share a space of higher dimension that is separable, and thus the method for QKD becomes insecure. This is easily shown to be the case for BB84.

Usual proofs of the security of QKD rely on entanglement theory, and thus they assume that Alice and Bob share knowledge of the fixed dimension of the Hilbert space of their system. If this assumption, which is in fact rather arbitrary, cannot be made, QKD must involve the violation of some Bell inequality. Thus yet again we see the crucial role played by Bell's theorem.

We are seeing that, while quantum theory is obviously different from classical theory in many ways, in many cases at least the core of the difference or the essential discrimination is just Bell's inequality or a result of Bell's inequality.

We may think of Holland's comment [60] in his book on Bohmian mechanics. He was answering a complaint that Bohm theory was trying to restore classicality to quantum theory. His reply was that Bohm theory fully recognised the great differences between quantum and classical theory. In fact, he said, it gave a possibility of discussing classical and quantum mechanics in the same language, but not of reducing one to the other. But it might be said that the task could not be a total success, precisely because of non-locality, or in other words yet again a result of Bell's theorem.

We might well contemplate adjusting Schrödinger's comment mentioned at the beginning of this discussion to obtain the rather striking statement that it is not (just) entanglement but actually Bell's theorem and its implications that are 'not one but the characteristic trait of quantum mechanics, the one that enforces the entire departure from classical thought.'

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