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DISTURBING THE MEMORY

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This is a collection of some weird thoughts, inspired by reading "Disturbing the Universe" by Freeman Dyson (1979), which I found in a bookstore in Cambridge. He reminisces about the history of theoretical physics in the period 1946–1950, particularly interesting to me because as a graduate student at just that time, I knew almost every person he mentions.

From the first part of Dyson's book we can learn about some incidents of this important period in the development of theoretical physics, in which the present writer happened to be a close and interested onlooker (but, regrettably, not a participant). Dyson's account filled in several gaps in my own knowledge, and in so doing disturbed my memory into realizing that I in turn may be in a position to fill in some gaps in Dyson's account.

Perhaps it would have been better had I merely added my own reminiscences to Dyson's and left it at that. But like Dyson in the last part of his book, I found it more fun to build a structure of conjectures on the rather loose framework of facts at hand.

So the following is offered only as a conjecture about how things might have been; *i.e.* it fits all the facts known to me, and seems highly plausible from some vague impressions that I have retained over the years. Inevitably, it will contradict some facts known to others, and I should be grateful to learn of them.

I first met Julian Schwinger, Robert Dicke, and Donald Hamilton during the War when we were all engaged in developing microwave theory, measurement techniques, and applications to pulsed and Doppler radar; Schwinger and Dicke at the MIT Radiation Laboratory, Hamilton and I at the Sperry Gyroscope Laboratories on Long Island. Bill Hansen (for whom the W. W. Hansen Laboratories at Stanford are now named) was running back and forth weekly, giving lectures at MIT and bringing us back the notes on the Schwinger lectures as they appeared, and I accompanied him on a few of those trips.

I first met Edward Teller when he visited Stanford in the Summer of 1946 and Hansen, Teller, and I discussed the design of the first Stanford LINAC, then underway. After some months of correspondence I first met J. R. Oppenheimer in September 1946, when I arrived at Berkeley as a beginning graduate student, to learn quantum theory from him – the result of Bill Hansen having recommended us strongly to each other. When in the Summer of 1947 Oppy moved to Princeton to take over the Institute for Advanced Study, I was one of four students that he took along. The plan was that we would enroll as graduate students at Princeton University, finish our theses under Oppy although he was not officially a Princeton University faculty member; and turn them in to Princeton (which had agreed to this somewhat unusual arrangement in view of the somewhat unusual circumstances). My thesis was to be on Quantum Electrodynamics.

I first met Richard Feynman at Princeton during the 1947–48 school year, when he came down from Cornell University to give us a seminar talk on his path–integral method, while his famous 1948 paper was in the publication pipeline; and still have my notes on that talk. The nonrelativistic Schrödinger equation part seemed analytically messy but easy to understand; the Dirac equation part with the backward trajectories a little cleaner mathematically but impossible to understand. To this day it seems, from the standpoint of physical mechanism, like a lucky mathematical accident rather than a rationally motivated argument (however, from the standpoint of statistical inference I was finally able to see, 35 years later, the beginnings of a rationale for it;

perhaps what is traveling faster than light and backward in time is not a physical causal influence, but only a logical inference).

That same year the Pocono conference took place, and I was one of the graduate students who were recruited to copy the voluminous notes by John Wheeler on Schwinger's famous 8 hour lecture, with its functional equations of motion on arbitrary space–like manifolds, onto purple hectograph sheets and run off copies. I still have that rather faded lecture, in the handwriting of almost every theoretical graduate student in Princeton at the time; and I still like it.

Unlike so many physicists, I never found Schwinger hard to read; our common microwave background made his style seem familiar and "right" to me from the start. Best of all, at that time everything he wrote was clear and well motivated. But the trouble was that you had to read Schwinger for an awfully long time before getting to a real application; and at the time I just didn't have the staying power to hang on until the experimental numbers finally emerged. So both the Feynman and Schwinger approaches had for me – and a few hundred others – some elements of arcane mystery.

At this point, Freeman Dyson enters the scene to help us. I knew of him and a little of his work as soon as he arrived at the Institute in Princeton; but do not recall meeting him personally until a few years after I had left Princeton and returned to Stanford.

But Dyson's 1951 Cornell course notes on Quantum Electrodynamics were the original basis of the teaching I have done ever since. For a generation of physicists they were the happy medium; clearer and better motivated than Feynman, and getting to the point faster than Schwinger. All the textbooks that have appeared since have not made them obsolete. Of course, this is to be expected since Dyson is probably, to this day, best known among physicists as the man who first explained the unity of the Schwinger and Feynman approaches.

Now let Dyson explain the circumstances of this work. He had arrived from England as a graduate student at Cornell for the 1947-48 school year, and was in contact with Feynman there, then with Schwinger in the Summer of 1948 at the University of Wisconsin. Before returning to Cornell he took a two–week vacation to explore the American West, Utah and California, and it is his homeward trip that interests us.

SCHWINGER AND FEYNMAN

Dyson (p. 67) recalls his discovery of the unification of the Schwinger and Feynman systems of Quantum Electrodynamics (QED) while riding across Nebraska in a Greyhound Bus, in September 1948:

> "For two weeks I had not thought about physics, and now it came bursting into my consciousness like an explosion. Feynman's pictures and Schwinger's equations began sorting themselves out in my head with a clarity they had never had before. For the first time I was able to put them all together. For an hour or two I arranged and rearranged the pieces. Then I knew that they all fitted. I had no pencil or paper but everything was so clear I did not need to write it down. Feynman and Schwinger were just looking at the same set of ideas from two different sides."

The analogy to Poincaré's account of his discovery of the unity of two different classes of functions at the moment of stepping on a bus, when he too had no pencil or paper, but needed none, is striking. I have had similar experiences four times, in which my best ideas in probability theory – always involving recognition of a unifying principle, a common pattern in things which had seemed different – have come to me suddenly, after long and frustrating failure, while driving in complete isolation across some boring place like Kansas, or riding a tractor on my farm.

The inspiration does not come unless one has prepared for it by intense mental effort beforehand; but it seems that some kind of change of scenery or activity helps to nudge it out of the subconscious into the conscious mind. It is as if, by analogy with chemical reaction theory, the path between the two brains has a barrier on it that is raised by attempts to use it too much; but it lowers again if the conscious mind is allowed to relax by turning to something entirely different. Could this barrier serve some other useful purpose? I refrain here from that interesting line of speculation.

Dyson, tantalizingly, does not reveal to us explicitly what he saw in that moment of enlightenment; but just for that reason he makes it interesting for us to speculate about this. First, we note the background of the problem:

Schwinger had formulated a set of coupled differential equations based on the conventional quantum theory of the time; essentially the 1927 model Quantum Electodynamics of Dirac (a contemporary of the 1927 Model T Ford). But Schwinger set up the mathematics so elegantly that the relativistic invariance of the theory was made manifest at every stage of a calculation. This made it possible to eliminate the mathematical divergences in a surer and safer way than previously, so he could calculate such things as the Lamb shift, vacuum polarization, and the anomalous moment of the electron, free of some of the ambiguities that had plagued previous attempts.

Feynman, on the other hand, simply threw away the differential equations of previous quantum theory and, retaining only the notion of probability amplitudes, invented his own "path integral" approach in which every conceivable space–time trajectory of a particle between an "initial" space–time point and a "final" one – whether or not it made sense in terms of any equations of motion and including even trajectories where the particle moved "backwards" in time – was taken into account. For the n'th path he found the classical action integral $S(n) = \int L dt$ from the Lagrangian L, and mysteriously combined them into a "propagator" $f = \sum \exp(iS(n)/\hbar)$.

Passing in the limit from a summation over a discrete set of paths to an integral over a continuum of paths, he took the limit of $|f|^2$ as the probability that the particle will go from the initial to the final point. His electrodynamics consisted largely of elaborating this, forming the product of many such propagators for the electrons and photons, chosen and tied together by certain "vertex" rules; and finally squaring the entire expression to find the probability of a complicated physical process.

The insight that came to Dyson on that bus ride through Nebraska may have been, we conjecture, the realization that Feynman's propagators were nothing but the Green's functions of Schwinger's differential equations; and these were in turn two–point covariance functions of the vacuum (*i.e.* vacuum expectation values of the product of two field operators at two different space–time points).

This would not be obvious to a reader immediately, not only because the ideas were expressed in such different language, but because they were Green's functions for very different boundary conditions than we had been accustomed to up till then. They did not give the advanced or the retarded solution, or even any linear combination of them; instead the waves went outward from the source in both space and time, thus seeming to propagate "forward" in the future, "backward" in the past.

But if this conjecture is right, we face the puzzle that this connection must have been well known to both Schwinger and Feynman before Dyson arrived on the scene. At any rate, the record shows that Schwinger was an acknowledged master of the use of Green's functions in solving electromagnetic boundary–value problems; and Feynman had of course demonstrated at the very beginning that his propagators are solutions of the conventional Schrödinger and Dirac equations (results that John Wheeler gleefully took to Einstein in the hope that this might convert him to quantum theory – of course, it could not because as we shall see presently, these mathematical relations could hardly surprise him and in any event his objections were not to the mathematics but to the Copenhagen interpretation of it).

[Note added in May 1986: On further thought, this conjecture seems oversimplified; it must have been not merely the relation of propagators to Green's functions, but more likely the details of connectivity of diagrams with many propagators, in relation to Schwinger's method, that Dyson saw.]

Perhaps it was already clear to both Schwinger and Feynman that they were not expounding different theories, but only different mathematical algorithms for finding solutions. If so, neither chose to stress this; and so it was left for Dyson to point it out and show the advantage of recognizing it.

Furthermore, the nature of the mathematical connection between the two viewpoints was hardly new, having appeared already in Einstein's 1905 treatment of the Brownian motion, where the Green's function for the diffusion equation could be interpreted also in two other ways; as the spreading Gaussian probability distribution for propagation of a single particle, and as a two–point covariance function. In the Fokker–Planck equation we had a differential equation whose Green's function was a spreading probability distribution in momentum space. From a mathematical standpoint, what was new in Feynman's work was that one was now to do this with complex probability amplitudes.

Indeed, Feynman's limiting process led to a relative weighting of paths that was, in the nonrelativistic case, essentially the "Wiener measure" introduced some twenty years before in the theory of Brownian motion; and Einstein's works of 1935 and 1949 showed that he thought of the Copenhagen interpretation of the Schrödinger equation as a physically incomplete, but mathematically analogous, version of his Brownian motion theory. So Feynman's path integral results could hardly have surprised Einstein; this was the third time that relations of that general type had appeared in the kind of physics that Einstein had pioneered.

Three appearances is enough to bring a result to the attention of mathematicians; and Mark Kac then became a pioneer in founding a whole general theory of the relation between differential equations and stochastic equations, that has now grown unrecognizably beyond that beginning. Indeed, any differential equation whose Green's function is nonnegative can be given a stochastic interpretation immediately; and both parabolic and hyperbolic equations have random walk solutions of the Feynman genre.

OPPY

Dyson (p. 73) reports that at the aforementioned 1947 Pocono conference Oppy accepted Schwinger's theory, and criticized Feynman's; but a few months later Oppy was not interested in Dyson's unification of them because he had come to believe that the QED of both Schwinger and Feynman was "just another misguided attempt to patch up old ideas with fancy mathematics." It appears from Dyson's next few pages that it was Bethe who changed Oppy's mind; and then Oppy heaped praise on Dyson in his Presidential Address to the American Physical Society in 1949.

Our conjectures on those events: Although it seems rather extreme to call work which has had such importance in theoretical physics "misguided", Schwinger at least was indeed trying to patch up old ideas with fancy mathematics. Feynman was probably not doing this consciously and intentionally (although for reasons just explained, his new ideas turned out not to be as radical as he may have first thought). And Oppy was quite correct in seeing that new ideas were needed; Dirac, Wigner, Pauli, and Weisskopf all expressed the same view at about the same time. But Oppy could never have accepted the *kind* of new ideas that were needed.

The real trouble had been pointed out earlier by Einstein, Schrödinger, Planck, Wien, and others; but they were not listened to. As we now know from the source–field theory that has developed in quantum optics, the equations that Schwinger had made more elegant, contained far more – infinitely more – mathematical degrees of freedom than were needed to represent the physical phenomena that he was calculating.

That is, a classical Electromagnetic field is fully described by specifying two coefficients for each field mode. But a full description of the state of a quantized field requires an infinite number of coefficients for each of an infinite number of field modes. Small wonder that such a theory led to some infinite results; indeed, it seems remarkable that any finite and physically sensible results could have emerged from a theory based on point interactions between fields which were held to have infinite random fluctuations!

But at the time it seemed to everybody (except perhaps Feynman) that the QED formalism – whose purpose was to extend the very successful Schrödinger equation for electrons to EM fields – required all those degrees of freedom, not to represent the observable physics, but for logical consistency with the Copenhagen interpretation of the Schrödinger equation. Many years later (1966), in private conversations, Roy Glauber impressed this point on me very strongly, which pleased me because it is what I had always believed but had never found a receptive audience for.

So Schwinger, believing firmly in the Copenhagen interpretation (or at least in Complementarity, as he usually chose to put it) did not at this time seek to correct the basic defect, but only to express it relativistically, so that when an infinity had to be swept under the rug, it could at least be done in a relativistically invariant way. Then one could hide modestly behind a high–sounding word, "renormalization", to conceal the disreputable nature of what he was doing.

But, as this writer learned from attending a year of Oppy's lectures (1946–47) at Berkeley, and eagerly studying his printed and spoken words for several years thereafter, Oppy would never countenance any retreat from the Copenhagen position, of the kind advocated by Schrödinger and Einstein. He derived some great emotional satisfaction from just those elements of mysticism that Schrödinger and Einstein had deplored, and always wanted to make the world still more mystical, and less rational.

This desire was expressed strongly in his 1955 BBC Reith lectures (of which I still have some cherished tape recordings which recall his style of delivery at its best). Some have seen this as a fine humanist trait. I saw it increasingly as an anomaly – a basically anti–scientific attitude in a person posing as a scientist – that explains so much of the contradictions in his character.

As a more practical matter, it presented me with a problem in carrying out my plan to write a thesis under Oppy's supervision, quite aside from the fact that his travel and other activities made it so hard to see him. Mathematically, the Feynman electromagnetic propagator made no use of those superfluous degrees of freedom; it was equally well a Green's function for an unquantized EM field. So I wanted to reformulate electrodynamics from the ground up without using field quantization. The physical picture would be very different; but since the successful Feynman rules used so little of that physical picture anyway, I did not think that the physical predictions would be appreciably different; at least, if the idea was wrong, I wanted to understand in detail why it was wrong.

If this meant standing in contradiction with the Copenhagen interpretation, so be it; I would be delighted to see it gone anyway, for the same reason that Einstein and Schrödinger would. But I sensed that Oppy would never tolerate a grain of this; he would crush me like an eggshell if I dared to express a word of such subversive ideas. I could do a thesis with Oppy only if it was his thesis, not mine.

But I found that Eugene Wigner was not at all shocked by such radical ideas; instead of making instant value judgments on emotional grounds, he would talk them over calmly and objectively, always trying first to see what their consequences would be; and offering criticisms only when he could show that they had unacceptable consequences.

This feeling about Oppy bothered me so much that by 1948, a year after arriving in Princeton, the contrast between what I saw as the flighty mysticism of Oppy and the sober – but at the same time far more receptive – rationality of Wigner, forced me into an agonizing decision; and Eugene

Wigner became my thesis advisor. My main interest then shifted from QED to group theory and statistical mechanics.

The same feeling bothered Edward Teller to the point of inducing him to make his statement against Oppenheimer in the infamous Washington hearing of 1954 which, as Dyson notes, hurt Teller far more than it did Oppy. In view of some later distortions of Teller's position, Dyson (p. 87) took care to quote Teller's exact words:

> "I thoroughly disagreed with him in numerous issues and his actions frankly appeared to me confused and complicated. To this extent I feel that I would like to see the vital interests of this country in hands which I understand better, and therefore trust more."

It was not Oppy's loyalty or his competence, but his unstable personality, that bothered Teller.

Unlike those who were motivated by doctrinaire political considerations, Dyson remained a friend of both Oppy and Teller afterward, and recounts socializing with Teller in Berkeley a year later, in 1955.

But still there was something very powerful in Oppy's personality, that many others have noted without being able to explain. His effect on me was such that, in spite of the fact that I was much closer to Teller than Oppy politically, agreed with Teller's judgment of Oppy, and had acted accordingly in my choice of thesis advisor, I still felt such a loyalty to Oppy that I refused to shake Teller's hand when we met again at Livermore in 1955.

In searching for words to describe Oppy's quality, I think "hypnotic" is as close as we shall come. Over and over again, I emerged from one of his lectures feeling that I understood clearly and perfectly what he had been telling us; but the next day, reading over what I knew to be verbatim transcriptions of his words, I could not make any sense out of them. He did not need to be consistent, for he had the power to make you think he was talking sense, even when he was not.

BISHOP WILBERFORCE AND THE PURPOSE OF NATURE

Science, fundamentally concerned as it is with matters of cause and effect, has had to contend with two diametrically opposite attitudes that want to abolish causality; the religious–teleological kind, that wants to replace it with explanations based on God's purpose, and the positivist–Copenhagean kind that wants to replace it with nothing but blind chance. It is curious that both kinds of mysticism are readily expressed in mathematical terms.

Our equations of motion are invariant under time–reversal, and so as far as the mathematics is concerned, solutions are determined as well by specifying teleological final conditions (presumably expressing God's ultimate purpose for the universe) as they are by specifying causal initial conditions. The advanced and retarded solutions satisfy Maxwell's equations equally well; it is only our judgment of their different physical meanings that leads us to choose one over the other.

Blind chance is equally well fitted into a mathematical description; indeed, the theory of stochastic equations, whose very basis would be destroyed by introducing a causal mechanism, is one of the most highly developed areas of probability theory.

So both attitudes are easily given the form of mathematical respectability; and both kinds have the same effect when they succeed in getting a foothold in science. They put a halt to progress in understanding, because a person who does not believe in cause and effect explanations has no motivation to seek them, and is almost certain not to make basic conceptual advances in his field (although he may greatly advance its calculational techniques). Such a person may happily spend a lifetime doing mathematical analysis based on a single form of mysticism, without ever breaking out of it or feeling any discomfort about what he is doing.

It seems that Dyson, having contributed to the mathematics of one kind of mysticism, feels also a predilection for the other. Somehow (p. 246ff) the topic gets around to the famous debate of Bishop Wilberforce vs Thomas Huxley on evolution.

Today it is hard to understand how nineteenth century theologians could have seen a contradiction between Darwinian evolution and their religion. Surely, there is no passage in the Scriptures stating explicitly that God intended every species to be forever immutable; if there were, we would have it constantly drummed into our ears today by the religious fundamentalists.

So the attack must be indirect; Darwin's offense must be taken to be, not that species evolve, but that by providing a causal mechanism to account for it, Darwin rendered a teleological God unnecessary. But that argument is clearly irrational; for if a simple mechanism were available, why would not God make use of it to achieve his ends? Indeed, if the mechanism exists, would not a consistent theologian be obliged to conclude that God must have made that mechanism to carry out his purpose? The Darwin mechanism is a far more efficient, labor–saving means of achieving that purpose than is meticulous attention to every tiny detail of events. (I hasten to add that these are not my own views; only points that I think should have bothered every theologian who tried to attack Darwin).

The theologian's argument is perhaps stated most defensibly in the analogy: the existence of a watch implies the existence of a watchmaker. Indeed so; but then Darwin's evolution is an essential part of that watch. We need not deny such an argument. But neither can we use it constructively in science, for the reason why science does not use theology is neither the presence nor the lack of religious faith; but rather that theological arguments can always explain everything, with zero intellectual effort.

Unfortunately Wilberforce, lacking rational arguments, stooped to personal ridicule (Do you claim descent from a monkey on your Grandfather's or Grandmother's side?), for which Dyson says "The biologists never forgave him or forgot him. The battle left scars which are still not healed."

Steven Weinberg and Jacques Monod, writing in our own time and noting the seeming absence of purpose (in the case of Monod, forbidding even the mention of purpose) in the universe, were chided by Dyson as "still fighting the ghost of Bishop Wilberforce."

My reinterpretation of all this: I doubt whether Wilberforce ever had that much notoriety, or even visibility, outside of England and outside of biology. In December 1983 I had the honor of dining in the elegant Wilberforce Room at St. John's College, Cambridge, with authentic furnishings of the time and the portrait of Bishop Wilberforce's father (an 18'th Century student at St. John's) looking down at our little group of scientists and historians. To paraphrase our American cowboy song, never was heard a disparaging word about his son. I think all of us took it for granted, if unconsciously, that the good he did must have outweighed and outlasted the bad, even though we knew more about the latter than the former.

But to return to the defense of Jacques Monod; I do not interpret his prohibition of purpose as an expression of anti–religious feeling, or even as a misguided, emotional over–reaction to one long gone. It seems to me rather a positive, constructive recognition of what we have just noted: whether or not we believe privately that there is an ultimate purpose to the universe, we have nothing to gain and a great deal to lose by introducing it into science – because it blocks progress by explaining everything *post facto*, but predicting nothing.

THE UNFINISHED PART

(1) Dyson notes that "Quantitative predictions by mere extrapolation of the past quickly become obsolete from qualitative changes in the rules of the game." Yes – this is a perfect description of what happens in Economics – but for Physics we need to elaborate on this with specific examples.

(2) "Description should be independent of the mode of observation (249)." Yes – in fact, this ought to be a major goal of theoretical physics – but the Copenhagen interpretation sneers at it.

(3) "Chance cannot be defined except as a measure of the observer's ignorance of the future." (p. 249). Hooray! Dyson, like Laplace, Maxwell, and Jeffreys, sees the falsity of the notion of "physical probability", which is just a euphemism for "denying the existence of causal mechanisms".

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