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Statement

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SYMMETRY AND QUANTUM MECHANICS

Quantum mechanics was born closely allied with symmetry and group theory*; to many of us, it seems to be merely an applied form of group theory. In fact, I have an imaginary scenario in which I dream that Schrodinger had talked a little more professionally with his friend Weyl in March 1926 and asked him, "Hermann, how could I make a mechanics out of your continuous groups of symmetries of space-time?" Many authors have made the point (a typical sample is to be found in the volume[1] based on the Wheeler symposium organized by the Templeton foundation) that quantum mechanics and space-time—continuous—symmetries are uniquely bound up with each other. For this reason I have never taken theories which involve discretization of space seriously. Of course, if Q M had been discovered this way rather than semiempirically as it was, the problem of how to interpret measurements would have stood out even more starkly; Bohr's Complementarity Principle (his version of the adage "never trouble trouble till trouble troubles you") might have seemed even more shaky than it was. But we would have come more quickly to the realization that the real difficulty in the theory of quantum measurement was bound up in understanding how really large, complicated bodies like ourselves can be understood as obeying quantum laws.

The content of Schrodinger's quantum theory, then, could be summarized as: assign a mathematical object we call a wave-function Ψ to the contents of the system under study, and demand that the mathematical operators which are the generators of the symmetries of space and time act

* This statement can be taken several ways: at the time (1926) of Schrodinger's great discoveries, Hermann Weyl, the author of the influential "Gruppentheorie und Quantenmechanik" (1928) was having an affair with Annie Schrodinger; of course Schrodinger was, at the crucial moment, in Ancona, with other ladies.

appropriately on that object. For instance, if we describe the object as a function Ψ(x,t) in space x and time t, to translate Ψ by δx we must add δx×∂Ψ/∂x to it:

$$T(\delta x)\Psi = (1 + \delta x\,\partial/\partial x)\Psi \cong e^{(i)(-i\partial/\partial x)}\Psi \quad ,$$

and hence introduce the familiar operator for momentum p.
In this simple way we can build up the familiar structure of quantum mechanics, adding only the dimensional constant h to tell us how our scales of space and time relate to the natural ones. We need equations to tell us how the time derivative of our function relates to its momentum p=mv:

$$i\hbar\,\partial\Psi/\partial t = H(p,x)\Psi$$

and the assumption, seemingly self-evident, that Ψ lives in a perfectly symmetrical vacuum completes our theory.

Broken Symmetry in CM

I now jump to what seems a totally different subject. In the 40's and early 50's of the last century the attention of theoretical physicists began to be drawn to the subject of thermal phase transitions in condensed matter systems, and how these could be related to the very successful program of understanding bulk phases of matter in terms of the quantum theory. Landau, and independently Tisza, created a general formalism for such phase transitions in terms of a concept they called "order parameter": they observed that when such phase transitions occurred, they were almost always from a more ordered state at low temperature to a less ordered one at higher temperature, consistent with the idea that less order means more entropy. Typical such transitions were ferromagnetism, ferroelectricity, and the liquid-solid freezing point. Logically, though contrary to laymen's intuition, more order means less symmetry, so one can also describe such a transition as the appearance of a "Broken Symmetry". Who invented that phrase I haven't traced, but it seems to be Landau who first observed that it is difficult to remove a symmetry continuously, so such a transition is bound to be singular.

It is a separate exercise, though fascinating, to explore the crucial role which broken symmetry plays in constructing classical physics out of a purely quantum universe. But our purpose here is to follow the lines of thought which led eventually to the Higgs mechanism, and to that this is irrelevant.

Starting two decades before Schrodinger's discovery the first quantum theory of a broken symmetry state was invented by Einstein, and then improved upon by Debye, and made more realistic by Born and Huang; in the form of the quantum theory of lattice oscillations. Debye and Waller even derived the effect of the quantum zero-point motions on the ground quantum state of the crystal lattice.

The first concept of this kind of thing as a general structure was perhaps contained in remarks I added to my paper on the quantum theory of the antiferromagnetic state in 1953,[2] about the fact that the zero-point motion necessarily diverges at long wavelengths in order to restore the broken symmetry to the true ground state—because, in fact, eigenstates of the Hamiltonian must be eigenstates of the symmetries of that Hamiltonian. That means that there is necessarily a zero-energy excitation –a phonon in the case of the crystalline solid, the antiferromagnetic spin wave in that case, etcetera—associated with a broken continuous symmetry. These came to be known as "Goldstone bosons" because Goldstone discovered the same phenomenon in later work on the field theory analogue. There is also the property I came to call "rigidity" of the order parameter from its analogue in the crystalline solid case[3]. I certainly cannot claim to have invented Broken Symmetry, but I am at least among the originators of the idea that it has dynamical consequences.

Now we have to talk about one more crucial connection between symmetry and quantum mechanics: the gauge principle. Weyl, among his many other contributions, first noticed and named this peculiar symmetry of classical electromagnetism: that one could add a (relativistic) gradient of an arbitrary scalar to the potentials V, A without changing any experimental result. When quantum mechanics came along, London, Fock and Weyl realized that this scalar could be identified with the phase of the complex function Ψ; and that what is more, invariance of the energy under local gauge transformations was necessary and sufficient to ensure conservation of charge! From this idea blossomed the Gauge Principle of the Standard Model: that symmetry underlies not only the conservation laws but the interactions of quantum physics.

During the quarter century after the discovery of quantum mechanics many theoretical physicists came to consider the physics of ordinary matter a finished subject, and to specialize in higher energy and smaller scale phenomena—nuclear and particle physics. But there was left behind for the rest of us (who called ourselves "solid state", or later "condensed matter" physicists) at least one abiding mystery: the theory of superconductivity, the property of almost all metals that at low temperatures, and therefore in their ground states, their electrical resistivity dropped to exactly zero.

In 1957 this dilemma was solved by a group led by a solid stater, John Bardeen, which produced the famous BCS theory. [4] An immediate effect was a healthy convergence of the two streams of quantum theory, at least for a decade or so. Since the solution they had found involved novel forms of quantum field theory, it became essential for us to take the quantum field theories our colleagues had been inventing seriously, and to learn them; and at the same time, the elementary particle types were led to take more seriously our experimentally-based discoveries about the nature of quantum fields: and in particular, that they could exhibit the phenomenon of spontaneously broken symmetry.

BCS AND GAUGE SYMMETRY

To begin with, there was the question of the simple gauge symmetry of electromagnetism. Bardeen, Cooper and Schrieffer (BCS) had made the conventional (in condensed matter) assumption that their order parameter chose to be at rest in their particular coordinate system. This led to howls of "unfair" from more sophisticated theorists such as Gregor Wentzel[5] of Chicago, and also a group in Australia, who pointed out that the resulting equations were not gauge invariant. For example, the equation for the current response to an electromagnetic field came out J=(constant)xA (A the vector potential) rather than curl J= constant xH. This was also noted very early by the field theorist Yoichiro Nambu, Wentzel's colleague at Chicago, who questioned Schrieffer about it when he gave a talk at Chicago. Nambu, I, who had also heard Wentzel's objections (he was an occasional visitor to Bell Labs as a friend of Bernd Matthias), and, as it happened, N N Bogoliubov in Russia, all set out independently to remedy the gauge invariance problem.

I was in very fast company, for me. Field theory had matured a great deal since my graduate student years, a decade before. Perhaps a hint of my naivete comes from the fact that in my first paper I started out misspelling "gauge" as "guage" and was only corrected by my wife, an English major, who asked "how do you pronounce that?" I had made a firm commitment to follow the low energy road in graduate school. But I was also very excited, because I knew that I had an instinctive feel for what were not yet called "broken symmetry states" that conventionally trained field theorists might not have. You can tell I was excited from the fact that I submitted three versions of the work in rapid succession, in January, February and July 1958.[6]

In fact, I won the race, if race there was, in several senses. In the first place, the January version contains an acknowledgement of extensive discussions with John Bardeen, and there is a reciprocal acknowledgement by him in the final BCS paper that they were aware of the gauge problem and that I had told them it was solved. In the second place, the papers by Nambu [7]and by Bogoliubov and Shirkov [8] which claimed to have solved it were both submitted after July '58. But, most important, neither of the other papers solved it correctly!

The key, all three of the sets of authors decided, was in the distinction between longitudinal and transverse responses. Superconductors notoriously reject transverse ("pair-breaking") perturbations but respond normally to longitudinal ones, which includes gauge changes; and the means by which this normal response is maintained—in the straightforward BCS kind of theory-- is the existence of Goldstone boson type excitations in the energy gap. We would nowadays call them "zero sound" waves. But there are no such waves for transverse polarization; no sum rule requires them to exist and they don't. They would amount to Cooper pairs with finite angular momentum, which don't satisfy the BCS gap equation.

In calculating the current response of a system of nonrelativistic electrons there appear two terms which come from the two terms of the dependence of the Hamiltonian on A:
H is proportional to $(p-eA/c)^2$ so there is a term in A^2 and a term in pA+Ap. The former gives the superconducting response, proportional to A, but in metals and insulators the optical sum rule requires the latter term to exactly cancel that term; but, as I said above, the superconductor does not satisfy the optical sum rule, because transverse and longitudinal spectra are sharply different, and the requisite singular term is missing.

What my final paper in this series (Phys Rev 112, 1900) had that was unique (aside from the earliest submission date) was the fact, experimentally verifiable, that the gap was empty of excitations. Nambu mentioned plasmons in an aside, but did not compute the spectrum for the case where the Coulomb interactions were included. I was very determined to inform my competitors that I had really solved the gauge problem AND that the gap was empty, and by happenstance I was in Moscow in Dec 1958, where I tried very hard to visit Bogoliubov but was blocked by our Intourist (actually, KGB)
"guides"; on a side trip to Dubna I had a surreptitious 15 minutes with Shirkov and I think got my point over to him. But somehow Nambu, with whom I had and have quite friendly relations, has never quite conceded the point.

I should mention that these papers contained other nuggets that made it into the superconductivity literature. Bogoliubov of course used his already popular Fermion representation; and I, and then in a more sophisticated form Nambu, introduced the spinor notation which Schrieffer and Josephson popularized later. All three of us discussed the possibility of collective modes of the superconducting Fermion gas, which came to be known as "Anderson-Bogoliubov modes". I even calculated a few, including the one which in that model corresponded directly to the Higgson. These became important when we got to superfluidity of He3.

But what the gauge problem had done that was really important was to catch the imagination of Nambu, who had the spectacular idea that the particle vacuum could be a broken symmetry state. We condensed matter physicists—particularly Landau—adapted field theory to our purposes by treating the ground state of our systems as a field-theoretic vacuum for our "elementary excitations"—quasiparticles, spin waves, phonons etc—and Nambu reciprocated this borrowing by supposing that his vacuum for the observed physical particles is some broken symmetry state which has uundergone a phase transition from the "real" vacuum, and whose excitations are not those of the vacuum with all of the fundamental symmetries: some of the latter are "broken".

Nambu introduced this idea[9] (with his colleague Jona-Lasinio) in a BCS theory of the nucleon, with the mass of the nucleon almost entirely caused by interactions, and the pion as a Goldstone boson of a somewhat imperfect chiral symmetry. We didn't have quantum chromodynamics at this early date, and this theory made some useful connections we would later understand in other ways, so it was not exactly wrong; but the idea of spontaneously broken symmetry as the source of a large energy gap, which in field theory translates into a mass, was golden. Nambu came to Bell and talked about the paper, and it was at this point I began to realize the relevance of my coalescing ideas about broken symmetry to the field of particle physics—much aided by a series of discussions with Nambu himself. Although I saw no opportunity to contribute, I began to pay more attention to what was going on there. John Ward also was around Bell Labs during this time and I discussed broken symmetry with him—and also with Bob Brout who was an occasional visitor.

I spent the academic year '61-'62 as a visiting fellow of Churchill College, Cambridge and a visiting lecturer at the Cavendish Laboratory.. It was an eventful year in many ways. Neville Mott put me in Hartree's old office next to him, convenient to the tea room, and as a result at tea I would occasionally hear scuttlebutt from the particle world—Steve Weinberg was an occasional visitor—though that field mostly lived in DAMTP, a piece of the math department with which we shared teaching duties for "Part III Maths". But I think I'm really indebted to J G Taylor, who visited Bell Labs in the summer '62, and to his host John Klauder, for bringing sharply to my attention that the absence of Goldstone modes, i e massless bosons, was a serious obstacle to various schemes that were hatching to use the broken symmetry idea. I think the very first time I heard this my response was "oh, but that's wrong, real superconductors have a completely empty gap! And I've shown exactly why."

John Taylor also left me with a dilemma in the form of a very cryptic paper just published by Julius Schwinger[10] claiming that there is no necessity for massless Goldstone bosons accompanying a conservation law. Had he discovered the same phenomenon? I puzzled over his paper for several weeks, learning a great deal in the process; but in the end I think the great generality in which he expressed himself left him open to the argument that, while my mechanism satisfied his very general criterion, he had not discussed any specific mechanism; and in particular, he made absolutely no mention of broken symmetry.

So, I set out to write my first and last particle physics paper[11], neglecting for some reason to refer to the basic source of the results, my third paper on the gauge invariance problem[12]. Instead, in an attempt to communicate in a language not entirely strange to the particle physicists, I emphasized the prior work of Schwinger, only describing the physical meaning of the manipulations toward the end. The physical meaning was this: In metals there are both longitudinal and transverse plasmon poles at the same high plasma frequency (I was familiar with the latter because of having studied spectroscopy in metals). If we imagine a superconductor with a really large gap—as in the Nambu theory, for instance—these 3 excitations become the 3 components of a vector boson, and the Goldstone boson disappears into the longitudinal component, just as in my 1958 paper.

In my paper there is no Higgson; nor mention of "universal" mass generation. Peter Higgs is the only one of the six supposed inventors of the mechanism who actually investigated the bosons. They are of course "Anderson-Bogoliubov " modes and, as I said above, by happenstance I looked at the one which would become the Higgson in my third 1958 paper, and which for that assumed interaction just turned up at twice the gap. And much, much later Peter Littlewood and Chandra Varma reinterpreted some Raman data on the superconductor NbSe2[13], showing that a rather mysterious Raman-active peak was indeed the Higgs boson equivalent for that substance.

The publicity about the Higgs focuses very much on the idea that the Higgs field is finite throughout the vacuum. The Higgs field is a superfluid, and like liquid He or a Bose-Einstein condensate of cold atoms might be expected to have a condensate, a measurable density of ordered fluid. It has been relatively recently realized[14] that superconductors actually do not have a condensate, a finite density of zero-momentum electron pairs which can serve as a tangible local "order parameter", precisely because of the Anderson-Higgs mechanism. Such a condensate would have charge, and charge cannot exist in the interior of a superconductor. Therefore the true order parameter has only a "topological" meaning and cannot be formally defined locally. Similarly, electroweak charge cannot exist in the "interior" of the vacuum, which is everywhere. The ordering of the Higgs field, then, is, like the superconductor, not local but what in condensed matter we call "topological" order, measurable only by creating boundaries or defects such as vortices where the Higgs amplitude at the core vanishes. Or, like that of the superconductor, by its effect on the other excitations.

If I may insert a note from the future here: It appears likely that there is a third category of non-local topological ordering of the same kind (which in the Higgs case is called a "B-F" model), namely the boson supersolid. Here again there is rigidity in the form of superflow, but no condensate: the atoms flow through the fixed sites without moving or altering the sites. This is certainly true of the Bose-Hubbard model[15] but likely for solid He4.[16]

The reason I put quote marks on "universal" mass generation is that the masses of the actual physical hadrons, n and p, are generally accepted to come mostly from color gauge interactions, the u and d quarks being themselves very light, and consequently the mass at least of the visible universe is not mostly Higgs-related.

The book, "The God Particle" and the quite reasonable publicity about the Higgson search focused on mass generation, but none of the many parents of the Higgs mechanism actually mentioned mass generation in their 1963 and 1964 papers[17] except for the massive gauge bosons.

The superconductor gives mass (ie an energy gap) to every momentum state on the entire Fermi surface; and the Higgs field can give a mass term to every particle which couples to the electroweak field. I feel extraordinarily stupid for not realizing that analogy, whose consequences only surfaced when Peter Higgs took it upon himself to write a full paper, which came out in 1966.[18] (And there it is not emphasized.) I was very puzzled because I anticipated, in my imagination, the modern conclusions about topological order—that superconductivity shows itself directly only when one looks at it from outside the sample—and could not see how to make the broken symmetry manifest. The answer was, if I had seen it, exactly as with superconductivity: in the excitation spectrum! All the Fermions are gapped, i e massive.

I was stymied at this point, particularly by the absence physically of any hint of the binding energy I supposed the condensate might have—and not yet consciously realizing that there was no true condensate. The entire further development of the electroweak symmetry-breaking mechanism took place without any participation by me: I was kept busy with superconductivity and magnetism for the rest of that decade, and when my interests broadened out it was in the direction of complexity science rather than fundamental particles. Not that I didn't keep up with developments; that would not have been easy sitting in the Princeton chair I ended up occupying. But during that half-century I was asked only once to relate my version of the above story, and I did.[19] For the present version I have done my homework a little better, otherwise the message is unchanged. Long live broken symmetry!

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