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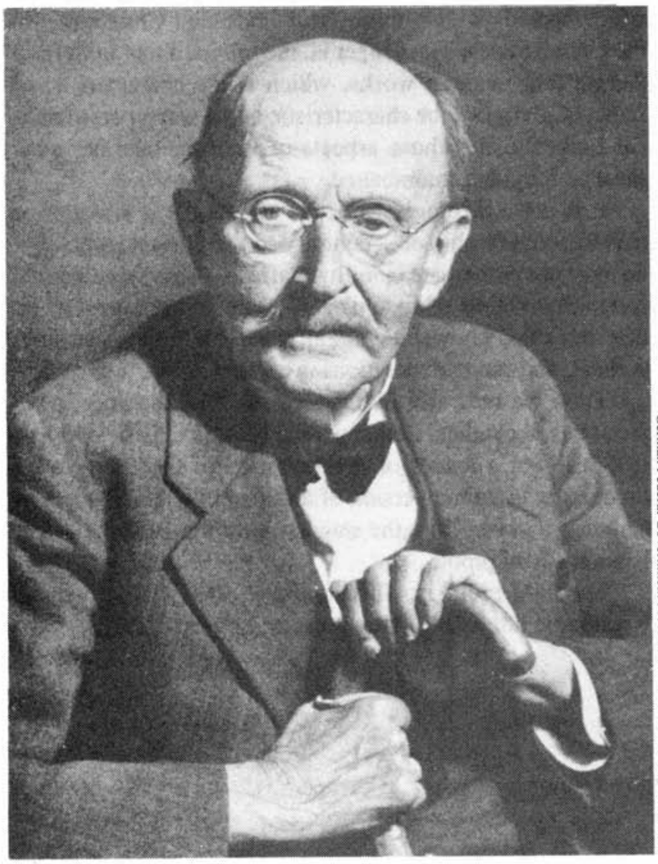
The bankruptcy of 'standard theory'

So far, we have outlined material which should make clear the general significance of the following statement.

What is called, in conversation among physicists and in classrooms, "standard theory," is inherently fallacious, even in its treatment of so-called entities of so-called classical and quantum mechanics, including relativistic physics: to the degree that ontological entity's existence, and function, depends, in any significant degree, upon deductive consistency of a particular mathematical physics, employed to create the relevant array of cumulative experimental material.

Just to restate that in a few sentences, to make the point absolutely clear.

We have, for example, the definition of the quark. The quark has no experimental existence. The quark, and associated features of that kind of theory, arise from the attempt to explain actual experimental evidence from the standpoint of



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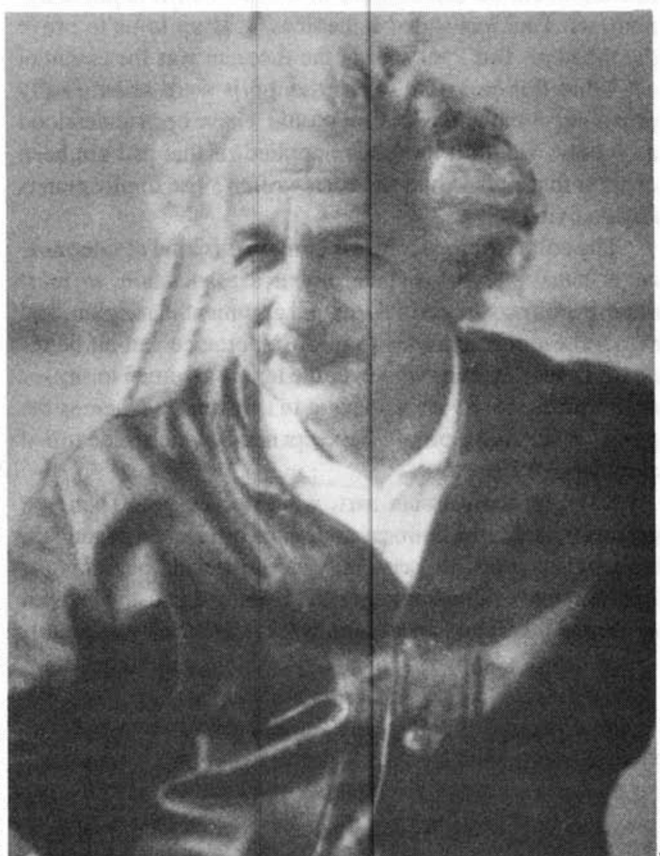

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Max Planck (1858-1947) and Albert Einstein (1879-1955). "The original Planck, and his derivation of the concept [of quantum physics] is rich and exciting . . . as opposed to the dull and arbitrary assumptions . . . of an Einstein."

consistency with standard theory. Thus, quark theory represents the creation of assumed ontological existences, purely on the basis of the requirement of establishing consistency with experimental evidence for a standard theory.

The problem here is that the standard theory is, we know, absurd. That is, any physics which is based on a deductive mathematics, is absurd, to the extent that the physics is dominated by deductive mathematics.

Let us put this another way.

Given a valid experiment, one which is reproducible by almost any standard, irrespective of, say, a deductive design of experiments.

First take an observation, which is agreed to be an anomaly, in which the event is not structured (the observation may not be structured by any experimental design, in which the experimental design might be contaminated by deductive assumptions).

So, in this case, we may use the deductive system to describe the phenomenon. It will not correspond to the phenomenon; that is, the mathematics will not correspond, except as a matter of approximation, in a sense of, shall we say, linear curve-fitting, as the famous four Archimedean propositions deal with that sort of thing.

The improper mathematics has described, in terms of approximation, an experimental result. Fine. The experimental result pertains to something which has ontological significance.

However, suppose we stretch the theory, the mathematical theory, to such a degree, that we attempt to account for the margin of error in curve-fitting, between the curve-fitting construction, i.e., the linear construction, and the actual phenomenon whose description is approximated.

Now, let us suppose that we say, that we must account for the existence of the phenomenon described in respect to the margin of error between itself, and the curve-fitting involved. In that case, we would have created an entity, an apparent, but fictitious entity, which is the margin of error between the object and the approximation. This action, of course, would be subject to experimental verification. One could verify, repeatedly, under repeated experiments, that such a discrepancy exists. Therefore, one might leap, foolishly, to the assumption that the entity has an ontological existence, which it does not.

That is a very crude, simple, but I think effective, illustration of the point. It is the same point which Newton made (and probably he reasoned in a similar way in making it), in warning the reader, in his famous clock-winder treatment of the universe, that the universe was not running down (i.e., the Second Law of Thermodynamics does not exist), but that the appearance of this (that is, that the Second Law of Thermodynamics exists), is merely a product of the superimposition of a defective mathematics upon the process of description of the empirical evidence.

Now, how do we avoid this?

This takes us to the requirement of a different mathematical form of physics, something different than the standard classroom physics, or standard classroom theory, for example.

Let us take the case of the alternative, which I have proposed: *a Riemann-Beltrami surface function* as a general mode of describing all of these anomalous (otherwise called nonlinear), phenomena, which do not precisely fit neat standard physics. Also, looking at some of the things in so-called standard physics, which might appear subtle to some, which are not subtle, but fallacious, because they involve the assumption of entities, where none exist: that is, pseudo-entities like the epicycles of Ptolemy, in order to make the system seem to work.

For example, when I indicated the discrepancy between the attempted curve-fitting approximation, and the actual curvature. That margin of error, and the epicycles, of course, come out very similarly. The existence of the epicycles is based on the margin of error introduced by a bad theory: a bad attempt at description.

In order to escape such bad attempts at description, let us take all the cases, which are really wildly anomalous, obviously nonlinear; and, let us take those which we should be looking at as anomalous, in which the entity, like the quark, comes into existence in our mind, solely as an attempt to reconcile a margin of error, between the events actually observed, and the error of approximation inherent in the method of description employed to represent that event.

So, the Riemann-Beltrami surface function is a very useful way of subsuming the relationship among, and of, weak and strong nuclear forces.

In this case, when we bring that into play, and deal with the relationship of electromagnetic and gravitational phenomena, for example, in these terms of reference, particularly on the nuclear scale, we get a completely different kind of result than we do with, say, the quark theory.

For example, there is a problem which arises in the published version of Wells's model for the solar system, in the sense that he is using a standard classroom theory-approach for describing something which was actually developed from a different standpoint. So, there is a discrepancy. I think that in that case, in Wells's construction, we'd have to go back, away from the standard theory which he is using for the IEEE publications, and so forth, and go back to the source to eliminate the "curve-smoothing errors" which arise from the use of linearity of standard theory, to represent the approximation of the process discussed.

Another part of this, which has to be emphasized, is that among Anglo-Americans, most emphatically (I keep away from the special problem of neo-Cartesianism among the French), there is absolutely lacking, in virtually every case, any understanding of what a strong rigor is. Not only do they show a lack of strong rigor in their work; but, in general, they do not even know what it is that they lack. They do not know what a strong rigor should be. None of them, for example, are trained profoundly in the Socratic method, which virtually all of the great classical discoverers in physics were, including Leibniz, or including all of the leaders of the work in developing the theories of elliptic functions and so forth, during the nineteenth century.

The greats approach these ontological and other questions, and questions of axiomatics, with an understanding of the Socratic method. The average Anglo-American, with terminal degrees of the highest qualifications, is educated *to avoid* any consideration of that sort of material, to avoid any conception of geometry which is inconsistent with that approach.

Thus, the typical American today (I'm talking about the Anglo-American scientist), by a margin of 99.9999%, is incapable of understanding the kind of rigor which is employed by the best scientists, the best continental scientists in particular, of the nineteenth century. This makes it doubly important to shift the emphasis away from standard theory, and to compel some of these scientists, ones who are more viable, and perhaps a bit younger in some cases (if they can rebuild themselves), to take this Platonic approach. Because only on that basis can they become acquainted with a strong rigor. There is no sense in trying to educate people merely in constructive geometry per se. I suppose there is some sense in it, but you are not going to get the student to the kind of desired result from that. You must accomplish what must be done from the Platonic kind of approach, of which I have represented a reflection here.

For example, I would give examples of cases which are relevant, apart from Gauss, Riemann, Beltrami, and so forth. Look at the less profoundly rigorous figures, such as Felix Klein, Max Planck, and so forth. These people were much less rigorous about the turn of the century than their leading predecessors in the same institutions a half-century earlier. They'd gone down in terms of rigor. But still, the rigor of people such as this is overwhelming, astonishing, awesome, compared to the loose, almost gossipy character of standard theory today.

Max Planck, from this standpoint

There are two Max Plancks. One is the Max Planck who derives the concept with which his name is associated; and there is the other Max Planck, the mythical Max Planck, who was created by Albert Einstein in 1917 approximately, with that terrible abomination that Einstein produced at that point on the subject, or as reified through the radical positivist version, which, coming out of Niels Bohr and company and others, seems to be hegemonic, more or less, today. So, we have this multichotomy among the so-called classical version (which is not classical at all) and the positivist version of quantum mechanics, and the positivist version of relativity; these three kinds of things bobbing around, none of them really good physics. Everything has been misunderstood from the attempt to reconcile the irreconcilable among these three things, none of which should exist.

The original Planck, and his derivation of the concept, is rich and exciting; at least it was for me, as opposed to the dull and arbitrary assumptions, not only of an Einstein who was probably one of the better cases among the bunglers later on in this positivist tendency.

You will observe, going to Planck's own published account of his derivation of the concept,[1] that there is a precise affinity between my attack on Euler's attack on the *Monadology* (see above, Chapter 6, and the Appendix) and Planck's method. That is, rather than taking smallest of smallest of smallest, or arbitrary division of line lengths, linearity, one must reduce the thing to action in the form of isoperimetric action, and the question of division of rotation, as the division of an angle, and then the division of that angle. And that is exactly the way the Planck Constant actually develops. So, looking at it in those terms, keep to Planck's original terms, in using the quantum relationships—that is, in this notion of rotation, this isoperimetric motion—and a lot of the nonsense which commonly arises, is averted. Then, put that back into the approach I have outlined to a Riemann-Beltrami surface function, and Planck's concept, as he describes his derivation of it, in his autobiographical note on this, applies beautifully. It lends itself to comprehension, and avoids this terrible, positivist, statistical mysticism, and convolutions which come along commonly in this connection.

Planck made a wonderful, great discovery, and, he made it in an extremely rigorous way. People seem to be deprived of the beauty of that rigorous discovery, and prefer the after-the-fact reification of that from a positivist standpoint; but the discussion of Planck should be situated, as I have recommended it be situated.

1. Max Planck, *A Survey of Physical Theory* (formerly titled *A Survey of Physics*), translated by R. Jones and D.H. Williams, Dover Publications: New York, 1961.