The end of physics and other comments

Unedited posts from archives of CSG-L (see INTROCSG.NET):

Date: Fri Dec 24, 1993 11:20 am PST
Subject: Lindley: The End of Physics

[From Bill Powers (931224.1030 MST)] I recommend

Lindley, David; _The End of Physics: the myth of a unified theory_. New York: BasicBooks, a division of Harper-Collins (1993).

Lindley works for _Nature_.

Some of the next-to-last paragraph of the book is worth quoting:

> The ideal theory of everything, in the minds of the physicists searching for it, is a mathematical system of uncommon tidiness and rigor, which may, if all works out correctly, have the ability to accommodate the physical facts we know to be true in our world. The mathematical neatness comes first, the practical explanatory power second. Perhaps physicists will one day find a theory of such compelling beauty that its truth cannot be denied; truth will be beauty and beauty will be truth -- because, in the absence of any means to make practical tests, what is beautiful is declared ipso facto to be the truth. (p. 255)

This book contains a long and careful analysis of the history of physics -- how it got to be the way it is. Lindley emphasizes periodically that the complexities of "fundamental" (i.e., particle) physics have never been invented for the fun of it; physicists have always been trying to find the simplest explanation they could find. The complexity of nature, says Lindley, simply requires a complex theory. This is a very charitable view, but I think it is open to question. I'm not sure that Lindley meant it to be taken seriously.

One hint that he didn't comes in his observation that physicists seem to treat the current state of particle theory as if it is "ground zero" -- that is, to be taken as given without any further attempt to explain why. This puts physics alone among the sciences in declaring deeper questions to be out of bounds.

There were numerous places during the historical discussions where I wished that physicists had spent more time asking why before they settled on an official view. Starting with the troubles that led to the special theory of relativity, some physicists seemed to become suddenly impatient with the slow march of progress; it's as though they wanted to leapfrog the usual way of going, to skip all the explorations of simple problems, to get on with it. This is when the theoretical explorations began to develop their own life, with longer and longer stretches of uninterrupted computation being used to bridge longer and longer gaps between experimental demonstrations. The world of observation and direct experience, which is the only ultimate anchor for any theoretical framework, began to fade into the background. A smaller and smaller percentage of the critical assumptions were put to test; the number and variety of actual phenomena involved got smaller and smaller.

I wish, for example, that when the quantum nature of some phenomena was discovered, physicists had taken more time to ask how this kind of phenomenon might be generated in a continuous universe, instead of instantly giving up on continuity. There are many possibilities; think of standing waves in a string, which occur only in whole-number ratios, yet are completely explainable in terms of continuous relationships. Perhaps physicists were still in shock from the discovery of the constancy of the speed of light; perhaps those who were happy to see the old Newtonian scheme collapse (something of an exaggeration) were just the sort who would seize on other apparent breaks with tradition without asking too closely whether they were also necessary.

On the surface, the ideas that came out of Copenhagen are very much in line with PCT. We know only what we can observe; the universe itself is unknowable.

If we can't simultaneously measure position and momentum, then we must accept that our observed universe is basically uncertain. If we are limited to the calculation of probabilities, then the world we are given to analyze is probabilistic.

The odd thing about this latter assumption is that the main tool of quantum physics, the Schroedinger wave equation, is basically a continuous equation, with continuous derivatives. A conscious decision was made to treat it not as a description of a continuous phenomenon, but as a description of a probability distribution. All at once, physicists started wearing quantized and probability-colored glasses, apparently unaware that the same principle applied: you see the world that is constructed by human perceptions. The view through these spectacles quickly came to dominate physics; it was accepted that the world was fundamentally quantized, not Einsteinian.

Lindley notes one of the penalties for this decision: general relativity (which is about a continuous if distorted universe) and quantum mechanics remain at odds with each other. The Big Bang, according to general relativity, would have had to start with a singularity. Quantum mechanics can't allow that singularity to exist: only a finite probability cloud could have existed.

The way quantum mechanics gets around the problem of singularities is to use a trick that has had to be used often during its development. Well, the physicists say, we know that there was no singularity at t = 10^-24 sec, so we'll just normalize to that time and forget about what happened earlier. This same problem arose in trying to describe the electron in quantum-mechanical terms. When the equations were solved, more or less, infinities immediately cropped up, both in modeling a single electron and in modeling the distributions of multiple electrons around the same atom. So someone decided that if the wave function could be defined at some small distance from the singularity, we could just forget about the infinities. This was "renormalization."

In ordinary physics we have a similar problem. If a gravitation field falls off as the inverse square of distance, what is the gravitational field at the center of a mass? Infinity, of course. For macro phenomena, the solution is easy: you recall that a planet's mass is distributed, so when the distance shrinks to less than the radius of the planet, the amount of mass contributing to the field also shrinks and the field goes to zero at the center of the planet. This leaves the description believable all the way from infinite distance to zero distance.

But in quantum mechanics this solution was not available, or for some reason was not considered. Since everything had to consist of particles, infinities cropped up everywhere (until string theories appeared), and one had to find an excuse for this failing of the theoretical representation, or a way to ignore it.

This, I think, is where physics starting getting (a) sloppy, and (b) mystical. Instead of admitting that there was a problem with the model, physicists started drawing a veil of mathematics across the scene. Renormalization was used basically because without it, the theory failed. The Schroedinger wave equation was transformed from a mathematical expression into an illuminated script on an altar. At that level of analysis, all search for an alternative description that would not bring up those ugly infinities was halted. Nobody ever seemed to think that they might have been created by the theory: by the Schoedinger equation itself.

All these heretical ideas are mine, not Lindley's. Lindley does not address the issue of what might have been or what the critical decisions were in the development of fundamental physics. In fact, Lindley doesn't speak about the influence of the very early adoptions of premises in creating the difficulties that physicists have had ever since. Nor does he remark on the way in which the world of experimental quantum physics has shrunk until all it seems concerned with is the discovery of a new particle at longer and longer intervals.

He does point out that one of the latest gimmicks, supersymmetry, seems to have put an end to experimental particle physics. As soon as supersymmetry was invented, every known particle in existence suddenly acquired an imaginary companion particle. The least energetic of these new particles might possibly be observable using a supercollider. Observing the rest of them would require increasing the collision energy by a factor of trillions. This means that supersymmetry will just have to remain a figment of the imagination -- beautiful in the eyes of the physicist, perhaps, but unverifiable. There is therefore nothing left to prevent physicists from completing the grand unified theory of everything. All that is now required is that it be internally consistent, like any systematic delusion. Nature need no longer be consulted.

Best, Bill P.

Date: Mon Dec 27, 1993 7:17 pm PST
Subject: What physics depends on

[From Bill Powers (931227.1900 MST)] Martin Taylor (931227.1415)

> Bill P has offered for Xmas a rather bleak and despairing view of the state of physics.

It's bleak and despairing only if all hope is tied up in having physics stand forever on the same foundations. I am looking for, and perhaps finding (although real physicists will have to decide that point) cracks in the foundations, not just to enjoy the sight of a pile of rubble but because I think that physicists have become too narrow in their approach to nature -- too narrow, and at the same time too ambitious in trying to extend their theories of matter to the organization of living systems. I think they have become too reliant on mathematics, forgetting that a curve that exactly predicts the positions of well-separated points can still be totally wrong in the regions between the points, and thus convey an erroneous picture of the underlying phenomenon. If the experimental points were closer together in particle physics I would feel less critical and skeptical.

B. F. Skinner could predict certain kinds of behavior with far greater precision than anyone could achieve before. But his explanation of WHY he was able to predict so well was wrong. I can admit and admire the great predictive achievements of physics, where they lie within range of experimental verification, without having to believe the story behind the predictions.

I am put off by the enthusiasm with which physicists try to explain everything at the subatomic level in terms of particles, even the forces that exist between other particles. As Harry Rymer, an old friend from astronomy, wryly remarked, these particles seem to have awfully good aim. And it's difficult to apply that concept while at the same time physicists speak of forces that vary continuously with separation, like the force binding quarks together which increases steeply with their separation. If one particle carries a force between two other particles, what particle carries the interaction between the force-carrying particle and its target particle? And just what kind of interaction is that? Infinite regress lies just around the corner.

I am also put off by the reification of the concept of probability, which in my mind is still just part of the processes we use to make predictions based on previous experience, when we lack data. I think it is far more profitable to assume that if a phenomenon fails to repeat when all initial conditions are the same, we have simply failed to include all necessary initial conditions, or we are unable to discern the degree to which initial conditions that look the same are actually different.

> I have to grant that both then and now it all comes down to what we can see on meter dials or imagery, or otherwise get in through our senses. That doesn't mean that the phenomena of physics was or is restricted to what we see, feel, or hear directly. Those are the phenomena of psychology. Physics helps us to understand how they occur, but it is not about them.

... and ...

> Other sciences explain "why" by reference to supporting sciences, physiology to biochemistry, chemistry to physics. Where is physics to go to ask why? There is no simpler science to support it, is there?

Physics is fundamentally about perception, not about the real world. Even in Copenhagen they recognized this, although they weren't thinking in terms of PCT. They spoke of "observation" and "measurement," thinking of what artificial instruments could reveal, but what they said applies more widely to human perception itself. And even more to the point, it applies not only to the senses, but to higher levels of perception derived from sensory information by transformations that exist in a human brain. At some level of perception, these transformations yield variables amenable to mathematical treatment, which is itself a product of the brain's activities. There is no mystery behind the fit of mathematics to our perceptions of the world: both are products of the same brain, consequences of applying the same transformations. Sums and differences, products and ratios, equalities and inequalities, sequences and series, are all elements of perception, products of a brain's functioning. The functions involved are those that give us a world to experience in the first place, at many levels. Of course the mathematics fits them!

There is no simpler science on which physics rests, but there is another science on which it rests: the science of life and more specifically human life. Perhaps the insight I am waiting for that will put physics on a new foundation can come about only through exploring the organization of human perception and action. Schroedinger's equation is a structure in the human mind. Like all mathematical expressions, it can't be applied to other experiences until the variables have been assigned meanings. It is in this process of assigning meanings that human perceptions get into the picture without necessarily announcing that they are perceptions. Quantum physics, like the rest of physics, is loaded with human perceptions, yet I have never heard a physicist point out this fact. Human perceptions are accepted as given aspects of the world. Under PCT, this can't continue to be the case.

The kind of system that can have and control perceptions is not continuous with other kinds of systems. It is the fundamental kind of system, as far as our knowledge of a universe is concerned. Without it there would be no knowledge, and nobody to know it.

Best, Bill (Scrooge) P.

Date: Sat Jan 22, 1994 10:53 am PST
Subject: Bohm and quantum theory

[From Bill Powers (940122.0930 MST)]

General information of interest:

In _Science_ 263, 14 Jan 94, pp. 254-255, there is a book review by Sheldon Goldstein, Department of Mathematics, Rutgers, of

Holland, Peter R.; _The quantum theory of motion. An account of the de Broglie-Bohm causal interpretation of quantum mechanics. Cambridge University Press, New York, 1993. xx, 598 pp. Illust. $120 or 70 pounds.

The following quotes give the main effect of the review:

> Novel resolutions of the quantum mysteries regularly appear. Most cannot withstand careful scrutiny. It is therefore worth emphasizing that the explanations found in Holland's book are genuine. In particular, they are not evasions, in which the real problems are skirted rather than solved. Moreover, as Holland points out, Bohm's theory is "very much a 'physicist's theory' and indeed puts on a consistent footing the way in which many scientists instinctively think about the world anyway."
>
> One very striking and much discussed implication of quantum theory, that of quantum nonlocality, remains in Bohm's account, not as a mystery but as a natural consequence of the mathematical structure of quantum theory

> itself. In this sense, while the quantum paradoxes are _eliminated_ by Bohm's theory, nonlocality is _explained_ by this theory. Holland quite appropriately devotes much attention to quantum nonlocality, delineating how it emerges in the quantum theory of motion.
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> In fact, one has to do astonishingly little to textbook quantum theory in order to transform it into a theory -- Bohm's theory -- in which the quantum paradoxes are not merely resolved but are eliminated entirely.
>
> .... the essential point of Bohm's account: that the origin of the quantum paradoxes lies neither in quantum phenomena nor in the quantum formalism that governs these phenomena but rather in the quantum philosophy, expressed in the Copenhagen interpretation of quantum theory, with which the quantum formalism has been encumbered. Almost as soon as one dispenses with this philosophy and instead posits that particles have positions regardless of whether or not they are being observed, one arrives at Bohm's theory, which succeeds in accounting for all (nonrelativistic) quantum phenomena while avoiding the quantum paradoxes, not so much because of the detailed character of the trajectories that it defines as because of the mere existence of these trajectories.

I am in no position to pass on the validity of Bohm's theory (I don't even know what it is), but this passage suggests that the story of quantum theory has not yet been finished. The "instinctive" way physicists treat the world is to assume that it is there and continues to behave in the same way whether or not it is being observed.

At first glance this would seem to go against the PCT motto, "It's all perception," which appears to support the Copenhagen interpretation. But on second glance, the message is about how we build models of the physical world, not what the physical world "actually is." A model based on the assumption that the physical world is only and exactly what we perceive it to be -- what we measure of it -- is actually no model at all. It is a crippled model, because it ties us into solipsistic knots whenever we try to use it.

If we applied the Copenhagen interpretation to PCT, we would never know how to interpret an experiment. The cursor is following the target, we would say -- but of course that is only one of my own perceptions, which might be different from what the subject sees the cursor doing. On the other hand, I can only guess at what the subject is seeing -- perhaps the world actually bifurcates into alternate universes, in which the subject sees the cursor moving opposite to the target while I see it moving with the target, the subject considering that kind of relationship to be "tracking." Perhaps the cursor is both following the target and not following it, until I observe it, at which instant it collapses into one or the other state. The subject, observing it from a different framework, might cause it to collapse into the other state, or perhaps my observation leaves only one possible state for the subject to observe.

The problem with this way of making a model is that it entails events and states that are _inherently unobservable_. The pre-observation states of the cursor and target are by definition unknowable, because the act of observation supposedly changes the states. This is not modeling; it is idle speculation. There will never, even in principle, be a way to verify this speculation.

It is not only possible, but it is necessary to construct models as if they describe a world that is actually existent whether or not we observe it, and regardless of who observes it. We could not even write the equations for a model of tracking behavior if we didn't assume that the equations describe a stable world. Even though we know (according to the same model) that all we can observe of that world is in the form of neural signals, we have to assume that the world is there -- not necessarily exactly as we perceive it, but in SOME reliable form.

The basic rule of modeling as I see it is that _every_ part of a model should, in principle, have consequences that we can observe: every variable, every sign, every function, every operator. This implies that down the road (if not immediately) we should be able to find an experimental way to manipulate each element of the model, and prove that changing it has the observable effects that the model predicts. To build models in which elements of any importance are forever beyond this kind of test is futile -- it's fantasy, not science.

I don't mean to imply that this kind of modeling requires that every person see the world in the same way. The model includes not only the outside world, but the perceiving system itself. There can be great differences in experience and organization which lead individuals to see the world very differently from the way others see it. Knowing this, but using a model of an objective reality, we can proceed to distinguish between what is common to all people and what is not: what is outside them, and what it inside. We can know WHERE to place interpretations.

A model that purports to represented an external reality can be mistaken, because it is tested by acting on the external world and observing the perceptual consequences. A model that contains aspects that are inherently unobservable can't really be tested experimentally, because any failure of the model can be attributed to some other unobservable, and as yet unsuspected, feature of reality. Only a model that commits itself to saying that THIS is the way reality works can really fail a test. Only such a model, by passing tests that could be failed, is worthy of belief.

As I said, I don't know what Bohm's model is. But I suspect it is simply a conversion from a totally subjective model to a model that posits a stable reality (maybe that's what Goldstein meant by the importance of the "mere existence of these trajectories"). The contortions of quantum-theoretic explanations result from assumptions that actually lead to solipsism and lead to all kinds of speculations about unobservable processes. I suspect that Bohm, simply by making a commitment to a specific view of an actual world, has found that all these contortions simply go away, leaving a simpler and cleaner picture of what is going on.

Best, Bill P.