The Ephemeris

Focus and books

Keeping Theoretical Physics on Track

Selections from recent discussions of particle physics and cosmology suggest that theoretical physicists are playing fast and loose with some basic rules of science.

Introduction

During the early 1980s theoretical physicists were so impressed with the perceived potential of Grand Unified Theories in particle physics, and the Inflationary Scenario in cosmology, that some were envisioning an "end of physics" and were wondering what to do after the basic foundations of theoretical physics were "finished." Rest assured, their jobs were never in jeopardy, and many theoretical physicists have backed away from such grandiose claims. However, from the way that phrases like "theory of everything" and "wavefunction of the Universe" are still popping up, it appears that the disease—unreasonable confidence in highly speculative models—has not been eradicated, but rather is only in partial remission.

The remarkable thing is how easy it appears to be for many theoretical physicists to overlook the fact that in recent decades observational work has consistently served up compelling reasons for humility. For example, there have been major problems and/or failed predictions regarding free quarks, magnetic monopoles, axions, proton "decay", solar neutrinos, galactic streaming, the Hubble constant, the cosmological constant and cosmological homogeneity. Most poignantly, we have learned that at least 90% (by mass) of matter is in an unknown "dark" form. This humbling discovery was not predicted by any of the pre-existing theories, such as the Standard Model of particle physics or the Big Bang cosmological paradigm. Moreover, currently prevailing, and carefully adjusted, theories seem to avoid specific predictions of what the universe is primarily made up of. Theoretical physicists tend to invoke WIMPs (weakly interacting massive particles) to account for the dark matter, but they cannot even say which members of this ever-growing stable of mythical (none have ever actually been observed) particles represent their choice for the basic stuff of the universe.

This situation gives pause to those who seriously wonder if we really have the foggiest idea of the actual composition, structure and dynamics of the cosmos. This essay explores the possibility that theoretical physics has begun to circumvent some very basic rules of science, and that this trend may be stunting scientific progress.

The present author is certainly not the only scientist to draw attention to this potentially serious problem. For example, Michael J. Moravcsik of the Institute of Theoretical Science (University of Oregon), in a recent paper that was reprinted in *Current Contents* (Vol. 30, No. 2, pp. 7-12, 1990), eloquently makes the case that theoretical physics has increasingly outpaced observational capabilities, and that the growing inability to test theoretical ideas has given theoretical physicists the opportunity to run amok. Because fundamental physics is widely regarded, rightly or wrongly, as the premier science from which other fields take their lead, this situation calls for determined efforts to identify trends that appear to be unhealthy for science.

In this essay, quotations from the recent literature of cosmology and particle physics are used to illustrate main points, but authors are not identified because we are interested in the general attitudes of the theoretical physics *community*, rather than in criticizing individuals. Cited material comes from a variety of formal (*e.g.*, papers in technical journals) and semi-formal (*e.g.*, "Commentaries" in *Nature*) sources. The latter are useful because we seek to emphasize candid expressions of prevalent theoretical viewpoints and attitudes.

Assumptions and uncertainties must be acknowledged

A very basic rule of science is that when reasoning involves assumptions and uncertainties, which is virtually always the case, then those assumptions and uncertainties should be identified and overtly acknowledged. If this rule is chronically neglected, then what begins as a speculative hypothesis slowly metamorphoses into "common sense", and equally reasonable alternative hypotheses become "crank ideas." It is stylistically ungainly, and tiring, to be continually qualifying one's arguments, but good science requires this extra effort. It seems to me that theoretical physicists have become rather cavalier about this rule, except when using it in their criticism of ideas that are not fashionable. Let us consider a few illustrative examples.

In a recent essay (*New York Times*) an eminent particle physicist candidly expressed his views on the reality of quarks.

> *But our experimental skills and our theoretical understanding have advanced really quite dramatically to the point where we really can see the quark—if you grant us some leeway in what we mean by see... Quarks are neither more or less real than apples or atoms.*

But in truth quarks are *not* observed. Typically, what are observed are particles x and y that are inferred to be the decay products of particle *z* which is inferred to have been produced by some quark interaction. Quarks may be as real as apples or atoms, or they may be purely fictional devices used to interpret uncomprehended phenomena, just as were Ptolemaic epicycles. I know of no experimental results that uniquely require the quark hypothesis for an explanation, yet most theoretical physicists portray the hypothetical existence of quarks as indisputable fact. Had experiments revealed particles with non-integer charge in multiples of $\frac{1}{3}$, that would have been impressive, but of course that prediction failed.

The author of a paper in *The Astronomical Journal* states:

> *The local density of dark matter in the galactic disk is 10–12 times that of the dark halo.*

However, others have argued that observations indicate that there may be very little dark matter in the galactic disk. Clearly, a basic uncertainty is being ignored in the quoted paper. The same author goes on to remark that

> *...the general prehistory from overdensities to galaxies is well accepted and well simulated.*

In reality, galaxy formation remains a problem that does not appear to be close to a solution; competing models are highly speculative and over-simplified. What can the author be thinking when he makes such a statement?

A non-technical book on modern physics confidently asserts that

> *...we understand the interior of the Sun better than the interior of the Earth, and the early stages of the big bang best of all.*

Pity the poor reader who usually must assume that the authors are being accurate. A skeptic's interpretation of this statement might be that the authors' confidence in the three theoretical models is inversely proportional to the degree to which they can be tested observationally. Readers of *Scientific American* were assured that

> *Physicists believe they are on the verge of a complete theory of matter. The theory accounts well for the forces that hold together nuclei, atoms and molecules. It is so successful and so widely accepted that it is simply called the Standard Model.*

This remarkably overconfident manifesto is regularly seen in technical and non-technical literature. Usually the reader is not burdened with unpleasant facts such as that there are about 20 adjustable parameters that are basic to the Standard Model and can be freely chosen to assure agreement with observations, that the quantum chromodynamics model of strong interactions has always been quite shaky, that the Standard Model is vitally dependent upon the existence of the mythical Higgs boson which has never been observed and whose mass cannot even be predicted, *etc.*

A recent comment in *Science* described the lengths to which cosmologists will go to retain their standard paradigm. If three separate theories do not individually solve current cosmological problems, then how about splicing them together?

> *According to [one cosmologist],... a mix of one-fifth cold dark matter, four-fifths cosmological constant and a dash of baryonic matter also gives just right type of galaxy clustering. "I'm extremely happy, because the model seems to explain everything," he says.*

Another disgruntled cosmologist came closer to the truth:

> *In some of the newer theories, we are inventing a new physical principle for every new observational fact.*

In the authoritative pages of *Physical Review Letters* we read that:

> *These studies are important because the primary emission process from SN 1987A is nucleon-nucleon axion bremsstrahlung (NNAB) while direct evidence of the invisible axion from laboratory experiments is still lacking.*

Throughout the quoted paper the author gives the impression that the reality of axions is a virtual certainty, whereas experimental evidence would seem to require a robust skepticism about the hypothetical existence of axions.

In the next quotation, one of our most well-known theoretical physicists, writing in *Modern Physics Letters A*, displays an intellectual over-confidence that is increasingly common.

> *However, we now all know that quantum gravity has to be formulated in the Euclidean domain. There it is no problem: it is just a question of plumbing.*

A little lightheartedness is often a welcome respite in a difficult paper, but one gets the feeling that the author is really not kidding at all.

Another author comments in *Nature*:

> *The excitement now is that nuclear abundances might eventually be used to probe not just the mean density of baryons, but also events at a much earlier time—not just the first three minutes, but even the first three microseconds, when the Universe turned from quark soup into ordinary hadronic plasma.*

Again, I worry that the author genuinely believes this! Any theoretical extrapolation from *present* nuclear abundances to a hypothetical "quark soup" existing some 15 billion years ago is going to be extremely uncertain. I, for one, am not so excited about this prospect.

Finally, from a "News and Views" article in *Nature* come the following comments.

> *It is only comparatively recently that cosmologists have been sufficiently well-equipped to address in a quantitative fashion the question "how did the universe begin". Although one might have thought that such an esoteric issue lies outside the realm of physics, it has*

become possible to address it precisely in the field of quantum cosmology, in which quantum mechanics is applied to the whole Universe. There it becomes the essentially mathematical question "what are the boundary conditions on the wavefunction of the Universe?" ...Using elements of an as-yet incomplete quantum theory of gravity, the object is to calculate a wavefunction—the wavefunction of the Universe—which is a function of the geometry of space and of the distribution of matter ... In principle, it contains information about the entire Universe and all its material contents, including ourselves.

So theoretical physicists, who have not been able to predict the dark matter, large-scale galactic flows, recently discovered patterns in galactic clustering, *etc.*, can foresee the development of a "wavefunction of the Universe" which can "in principle" predict what you are going to have for breakfast next Wednesday. Are these guys for real? And why do the editors and referees of prestigious journals allow them to publish this stuff? I can hardly wait to see this 'wavefunction of everything' put to the test, but that would involve actual predictions, a somewhat outdated formality that is discussed next.

Scientific hypotheses must lead to unique, testable predictions

If there is one crucial step in the scientific method that keeps the whole business honest, surely it is the requirement that hypotheses must generate *unique, testable* predictions. Without these predictions, and subsequent empirical testing, science reverts to pseudo-science, and then to applied fantasy. In the essay by Moravcsik mentioned above, concerns about just such a reversion were cogently expressed. But given the potential danger to the scientific process, it is surprising how few scientists have formally expressed concern about the situation in theoretical physics.

Let us consider some current problems with testing popular theories. Two of the most fashionable hypotheses of theoretical physics, string theory and cosmological inflation, cannot be scientifically tested. String theory, which is sometimes referred to as the "theory of everything" and is the subject of countless papers, simply has not yielded testable predictions. The hypothesized inflationary epoch, which rescues the Big Bang paradigm from several serious problems, is supposed to have taken place in the unobservable past. Its major "prediction" that does involve an extrapolation to observable phenomena—that the cosmological density will equal the critical density for "closure" of the universe—is certainly not unique and appears to be contradicted by observational results. How can we ever decide whether these hypotheses are brilliant improvements in our understanding of how nature actually works, or whether they are merely elegant fictions?

Equally distressing is the recent trend toward hypotheses that are so readily adjustable that "predictions" based upon them are of little value. For example, empirical searches for magnetic monopoles have failed to find them for 40 years. Do theoretical physicists accept the most likely verdict of nature? No, they merely modify their hypotheses so that the "predicted" monopoles have revised properties that are no longer in conflict with existing observations. The latest gambit in keeping the magnetic monopole hypothesis alive is an awesome bit of bluff and arm-waving. In the "News and Views" pages of *Nature* we read:

One expects, however, only one monopole per universe on average in [grand unified theories]...

So you see, don't you, there is a magnetic monopole out there somewhere, but it is silly to expect to observe it.

Another candidate for the most slippery of the high fashion hypotheses is the Cold Dark Matter model, which purports to help us understand galaxy formation. The CDM is supposedly composed of WIMPs, which have never been observed, and the CDM hypothesis cannot specify which of the hypothetical WIMPs are involved. Moreover, every time observational astronomers come up with discoveries that conflict with the CDM model, *e.g.*, the cellular nature of galactic clustering, structure on scales much larger than expected, or galactic streaming at velocities higher than expected in the CDM model, its proponents find a way to resurrect the hypothesis and then contend that "now its agreement with observations is stronger than ever".

An archetypal comment from a recent interview (*Science News*) with a leading CDM proponent really gets to the crux of the problem. In defending the CDM hypothesis against conflicts with new observations, he states,

Some people might say, 'If a theory can't describe [an observation], drop it'. I'm more practical. I say, try and revise it.

But does he not see that if failed predictions are routinely circumvented, then predictions are valueless and theoretical physics is in real trouble. Another CDM advocate takes the opposite approach in an interview for *Science*:

A lot of the observations challenging the theory are actually wrong.

Hear no evil, see no evil, speak no evil.

I am not saying that a major hypothesis must be totally scrapped after a failed prediction. What I am saying is that in such a situation proponents of the hypothesis should adopt an attitude of genuine scientific skepticism until such time that the hypothesis redeems itself in a very convincing manner, and by this I mean something that goes well beyond *ad hoc* fixes.

A willingness to take "no" for an answer

As noted above, one can short circuit the scientific method by adjusting a theory whenever its predictions fail;

one might call this the Ptolemaic Method, *i.e.*, adding epicycles. Another problematic response to observations that conflict with one's theoretical beliefs is to ignore the empirical results, or alternatively to convince oneself that the data are incorrect. Below are some recently published statements that suggest an unwillingness on the part of some theoretical physicists to take "no" for an answer, even from nature itself.

Consider the following three opinions of the "agreement" between observations and the cold dark matter model.

> *...we remain impressed by the remarkable agreement between the predictions of this simple model [CDM] and observations of objects ranging from galaxies to galaxy clusters. (The Astrophysical Journal)*

> *Cold dark matter has a relatively easy time forming galaxies,... (Nature)*

> *The cold dark matter model has been remarkably successful in reproducing many of the salient features of the large-scale galaxy distribution. (Science)*

Now remember, this is the 'if at first you don't succeed, try, try again' theory of galactic phenomena. These three glowing statements and the rather bad track record of the CDM hypothesis seem very difficult to reconcile, unless the authors are ignoring or discounting important observational results.

A very well-known theoretical astrophysicist, writing in *Physica D* states:

> *...I think the evidence for [large-scale homogeneity in galaxy distributions] is now close to compelling, though, it is fair to say, not definitive.*

There is, and has always been, *empirical* evidence that seriously calls into question the *theoretical* bias towards large-scale homogeneity in the distribution of matter. Unfortunately, this evidence is usually ignored or belittled.

In a *Nature* "News and Views" we are assured that

> *Conventional cosmology [Big Bang plus Inflation] has been very successful in predicting many features of the observed Universe.*

Conventional cosmology has had some success in predicting the microwave background and retrodicting nuclear abundances. However, the original prediction for the former was considerably off and there are at least three reasonable alternative mechanisms for generating the microwave background. Nuclear abundance retrodictions have been molded to fit the observed abundances, and still there are well-known problems with these retrodictions. Moreover, conventional cosmology has failed to predict the dark matter, the cellular structure of galaxy clustering, galactic streaming, structure on scales $\geq$ 100 Mpc, *etc.* "Very successful in predicting many features"? Much is being ignored when such statements are routinely made.

It is understandable that we are reluctant to abandon our hard won theories, but scientific progress requires that we always be willing to see "with fresh eyes", especially when prodded by unexpected new discoveries.

Science does not deal in absolutes

A basic tenet of science, and one that distinguishes it from religions and other non-scientific systems of knowledge, is the rule that all scientific knowledge is open to question. Scientific theories and observational results are based on limited information, and therefore the scientist does not claim that scientific knowledge is absolute truth. At best, our models and data approach the reality of nature by successive approximations. This is such an important and widely acknowledged principle of science that scientific authors are usually very careful to avoid absolute claims. But from the absolute statements that do slip by the review process, and from the knowledge that we are less cautious in our thinking than in the papers that we submit to refereed journals, one gets the feeling that scientists engage in absolutism much more often than is desirable.

In *Comments On Astrophysics* we read that

> *Although the halo and possibly cluster dark matter may be baryonic, it cannot be in the form of ordinary gas else it would generate too many X-rays. The gas must therefore have been processed into dark remnants of a generation of pregalactic or protogalactic "Population III" stars.*

This is not a very worrisome example of absolutism, and the qualifying "may" in the first sentence certainly helps. However, given the very speculative nature of the ideas involved in this argument, the use of "must" in the second sentence is surely too strong.

A well-known particle physicist commented in *Nature* (Review Article):

> *There is no confirmed experiment contradicting the Standard Model, but it has not been fully verified...*

However, according to quantum chromodynamics which is the theory for strong interactions in the Standard Model, proton spin orientations should not play a significant role in high energy p^+p^+ scattering experiments. Yet this prediction has been clearly contradicted by repeated, and well known, experiments showing that proton spin orientations do make a big difference.

For this and other reasons the above statement is not literally accurate, and the extent to which it is almost accurate is largely due to the fact that the Standard Model has been repeatedly adjusted to come into agreement with contradictory experiments. In another particle physics paper published in *Physics Reports* we are told that

> *...quarks and gluons unmistakably are the underlying degrees of freedom in nuclei...*

It seems to me that this statement exemplifies the kind of absolute certainty that science seeks to avoid. Particle physicists tend to be far too sure of their assumptions, and ironically it is in this realm that empirical uncertainty is the highest and testability is the lowest.

From a Commentary in *Nature* comes the following assurance

> *...we know that [the universe] has order at at least one extremity, namely the extremity we think of as the beginning of time.*

Well, some would like to think they know exactly what was going on about 15 billion years ago, or whenever the present expansion apparently began, but I seriously doubt that our knowledge of that "temporal extremity" is at all certain.

These typical statements are hardly glaring violations of the scientific restriction against absolute certainty, but one might worry about whether theorists are allowing too much "certainty" to creep into their private and communal thinking, and into the peer review process. Because this restriction is truly a *sine qua non* of science, we must be especially fastidious on this issue.

It's OK not to know everything

There is a general human tendency to have "the answer" or at least a very strong opinion on virtually every question. Physicists, as a group, tend to be especially prone to this form of arrogance. No sooner has a new and unexpected finding been made than it is reinterpreted as support for existing theory, or theory is quickly adjust so that we "understand" the new phenomenon. On the one hand, some theorists believe that we have just about figured everything out; on the other hand, we do not even know what the basic stuff of the universe is, *i.e.*, the dark matter objects. How preposterous is the former attitude in light of the latter fact. Scientific goals would be far better served if we were more willing to acknowledge that many phenomena are not really understood, and if we were to treat speculative models as no more than that. Conversely, when we insist that we know just about everything, the mind closes and scientific progress falters.

I have always been particularly fond of Einstein's comment:

> *...all our science, measured against reality, is primitive and childlike - and yet it is the most precious thing we have.*

In my opinion it is no coincidence that a person with this viewpoint was the one who created the foundations for the most important theoretical physics of the century. Many scientists know what they know, but it is a rare individual who can accurately recognize and freely admit what he does not know. There is nothing insurmountable that prevents more scientists from entering this latter category.

Conclusions

Theoretical physicists must renew their dedication to acknowledging speculative assumptions and uncertainties, insisting on unique predictions that can be tested, abiding by the empirical verdicts on those predictions, avoiding absolutism, and generally being more humble with respect to nature and alternative ideas.

Current attitudes of the theoretical physics community have been characterized by Moravcsik (cited above) as follows.

> *...the interaction between theory and experiment weakens and becomes very slow, because experiments take a long time and are indecisive, and because theories become overly mathematically motivated and tend also to be the "slippery" kind that can evade verification or falsification...*
>
> *Intolerance reigns vis-à-vis those who do not believe in the latest fashion and advocate different approaches. By now many of the traits of the "normal" scientific method discussed earlier have fallen by the wayside, and the picture that we are viewing is more similar to that of a religious group or of a political party.*

Those who feel that our scientific knowledge of nature is "the most precious thing we have" should insist, loudly and clearly, verbally and in writing, that present trends in theoretical physics are cause for alarm.

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