

Can there be Physics without Experiments? Challenges and Pitfalls[†]

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Summary

Physicists investigating space, time and matter at the Planck scale will probably have to work with much less guidance from experimental input than has ever happened before in the history of Physics. This may imply that we should insist on much higher demands of logical and mathematical rigour than before. Working with long chains of arguments linking theories to experiment, we must be able to rely on logical precision when and where experimental checks cannot be provided.

Introduction.

During the last few decades our view upon matter, the fundamental forces and the geometry of space-time have undergone a metamorphosis, enabling us to extrapolate our knowledge towards time- and distance scales that in fact cannot be directly probed by experiment. This extrapolation leads us towards the so-called Planck scale, where lengths are of the order of 10^{-33} cm, time intervals of the order of 10^{-44} sec, and masses of the order of $20 \mu g$. Here, gravitational effects cause curvature in space and time, and today's physical models, the "superstring theories" and related ideas, tend to become extremely complex. In general, even our book-keeping procedures, needed to formulate the physical degrees of freedom, their coordinates in space and time, and the disciplines for their evolution, present fundamental difficulties.

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Physicists will not easily accept any new theory unless it has been extensively tested experimentally and all conceivable alternatives have been ruled out. The extremely successful Standard Model is a perfect example. Being the first complete synthesis of the theories of Special Relativity and Quantum Mechanics, it came into being by a close collaboration between theoreticians and experimentalists.

But with String Theory ¹, the situation will be vastly different. Experimentally, this theory will be extremely difficult to assess. Most of our evidence will be extremely indirect at best: the theory shows a slight preference for predicting the emergence of super symmetry at the TeV scale, although this prediction actually was made long before String Theory took its present form. Some cosmological models inspired by String Theory will be experimentally testable but here also the input from these theories is indirect. The most direct predictions refer to the nature of space, time, and the particle spectrum at the Planck scale, where no experiment will ever be able to check any of their claims.

Yet, String Theory is of extreme importance. It is considered by many to be as yet the only detailed avenue towards a better understanding of the synthesis of general Relativity with Quantum Mechanics. One day, we hope to be able to understand the spectral and numerical details of the Standard Model itself. Presently, there are about 26 fundamental constants of nature, whose numerical values we are unable to derive from present theories. It is hoped that String Theory or any other similar construction will do this for us. The question to be discussed now is: Will there be any chance to succeed, if experimental support will be as indirect as we all expect?

To address this question, let us look at the development of important theories in the past, and estimate in what way they owe their existence to the numerous and essential experimental checks.

Theory and Experiment in Physics.

Physics is an experimental science. One studies phenomena observed in our world, ranging from the mundane ones to the extremely esoteric. Obvious, but nevertheless of essential importance are observations such as the fact that our space is three dimensional, and that there exists such a force as the gravitational one, pulling us towards the ground; we see that our universe has matter in it, divided into little particles. It seems that the properties of matter can be reduced to the properties of these particles, which appear to be point like, and their properties, such as the forces acting between them, are universal. Our entire universe appears to be built out of the same material.

The most esoteric observations, requiring the most sophisticated techniques to be realized at all, are for instance the fact that our universe has curvature in it, and that some particles long thought to be massless, actually turn out to have mass after all ². Much of the progress in modern physics results from such eminent achievements of the scientific method.

It is the theoretician's task to try to find order in these phenomena. We have experienced that the phenomena obey rules, which we call "laws of physics". It appears that the laws are obeyed with extreme precision, and, most importantly, it seems that some rules are obeyed everywhere, under the most extreme circumstances. They appear to be universal.

An essential complication however is that, at atomic distance and time scales, a new kind of 'fuzziness' arises, allowing us only to apply certain rules of statistics, rather than deterministic rules. This is what we call Quantum Mechanics. But apart from this important side remark, law and order are everywhere.

I am not the first to be surprised by humanity's continuing progress in unraveling these laws, in spite of the fact that these laws do not seem to have been made by humans. Indeed, they appear to be far superior

to the man-made laws. History nevertheless demonstrated that we can get into grips with them.

How does this process work? Philosophers of science sometimes give answers of the following sort:

- (1) At a given stage of our understanding, there may be competing theories for the explanation of an observed phenomenon, theory #1 and theory #2, say.
- (2) Theory #1 leads to prediction #1, and theory #2 leads to prediction #2.
- (3) And now an experiment is done. If it agrees with one of the two predictions this helps us to select the right theory, until new phenomena are observed that require further refinements. Go back to (1).

According to this picture, the role of theoreticians is to come with a number of alternative scenarios. They are tested by the experiments. Experiment has the last word. But this picture, often taught at high schools, is not quite the way physicists themselves experience what is really happening. It is far too simplistic to suggest that theoreticians are just sitting there producing theories, preparing them just such that they can be confronted with experiment. The point is that in most cases, the different theories are not “equivalent” at all. They are not equally probable or equally credible. Very often, the choice is between

1) a theory, and 2) no theory.

Or else, the theories are of very different quality. I am talking now of highly ‘fundamental’ branches of physics, where the laws at the very end of the scales are searched for: either at extremely tiny distance, at the highest conceivable energies, or, conversely, at the largest distance scale. Nowadays we have a very detailed theoretical picture of what is going on, but there are uncertainties, in regions of physics that have been inaccessible in the past.

We try to produce a theoretical scheme that is more advanced, more refined or more ambitious than the more established versions. To find out whether the new version makes sense or not, one naturally first asks whether experiments or new astronomical observations can be envisioned that can give us further guidance. If this is not the case, at least in the foreseeable future, should we then abandon such theoretical research, or do we have guidelines other than experimental verifications?

It turns out that we do. New theories for space, time, matter and forces are being proposed, which, in practice, appear to be such that they may be subjected to tests of the following kinds:

(i) A theory should be *logically coherent*. This demand is far from trivial or obvious. Often, theories are formulated in terms of abstract mathematical equations that ought to control infinitely many degrees of freedom. If these equations display a clear causal structure, and if wild oscillations can be seen to be subject to constraints derived from a bounded quantity such as energy, one might conclude that the theory makes sense logically. However, many complications can arise that imperil logical consistency. If point like particles are replaced by line like objects, locality of the equations is no longer obvious, so that the causal structure may get lost[‡]; if gravitational effects remove the lower bound to the energy, then energy can no longer be used to show the absence of run-away solutions, and if the theory is only formulated in perturbative terms, the existence and internal consistency of a corresponding non-perturbative system may be questioned. And so on.

(ii) The theory should *be capable of* making predictions. Even if the predictions cannot be tested

[‡] In the *perturbative* formulation of String Theory, causality appears to be obeyed perfectly, including its coupling to gravity. It is in the circular nature of any attempt at a non perturbative formulation where reference to time ordering is often lost, so that causality may be questionable.

experimentally, the mere fact that meaningful statements are made concerning the outcome of some *Gedanken* experiment could distinguish a theory from less viable competitors. In short: a theory should at least be capable of predicting coherently the outcome of experiments done in our imagination. Requiring such outcomes to be informative and coherent is an important criterion for our esoteric theory.

(iii) The theory should agree with older theories that are well-established. Thus, most advanced particle theories such as String Theory, *M*-theory and the like are demanded to agree at least with Quantum Mechanics, and Special and General Relativity. They should allow for the existence of fermions, and a four-dimensional spacetime evolving into something resembling our present universe.

(iv) There should be *agreement* with already observed phenomena. This means that, even if no *new* experiments can be thought of to test our theory, we can still make use of experiments done long ago. For instance, the fact that a gravitational force exists, the fact that there are three generations of quarks and leptons, that the effective gauge forces at low energy exhibit the mathematical structure of $SU(3) \otimes SU(2) \otimes U(1)$, all these should be consistent with our theory, and in practice these are very important constraints. Some string theorists proclaim that their theory “predicts” gravity. This is an absurd statement, of course, but it indicates how stringent this requirement appears to be in the eyes of these physicists.

(v) Eventually, a successful theory must be able to make *real* predictions. For string theory, or other considerations concerning physics at the Planck scale, this may mean that we hope to be able, one day, to ‘predict’ the value of fundamental constants of nature that as yet have eluded any genuine prediction, such as the fine structure constant. Even if this constant could be reproduced with less accuracy than its presently known experimental value, this would be a tremendous achievement. It would imply that, at least in principle, such numbers could be computed with arbitrary precision, so that definitely predictions of the conventional type can be expected. This is surely what we are aiming at eventually. But long before this situation is arrived at, a theory may have become acceptable and respected by a fairly large community, simply because it may have provided more and deeper understanding.

Examples from the past.

It is difficult to compare this situation with our experiences in the past, because physicists always have been so fortunate that clever experiments could be devised to check *directly* whether the equations described in a theory worked the way they were expected to.

Consider the Maxwell equations. James Clark Maxwell discovered around 1864 that the descriptions existing in his time were inconsistent. His equations for electricity and magnetism were a mathematical completion of existing knowledge. He quickly realized that light propagation had to be closely related to electromagnetism, and as the theory agreed with everything known before, it was soon accepted as obeying the first few of the criteria written down above.

Needless to say however, that the theory was tested at numerous occasions when electromagnetic phenomena were further studied. Would we have accepted the theory if such tests had not been there? Clearly, the theory would not have enjoyed the same status as it does today, but the mere fact that, at one stroke, the theory of optics was unified together with electricity and magnetism would have made it highly respectable.

Einstein’s theory of relativity was just such a case. The theory was not primarily intended to “explain” observed phenomena. Although Einstein presumably knew about the Michelson-Morley experiments, his primary concern was to give a more satisfactory description of the geometry of space and time in its relation to electro-magnetism. His analysis was a purely mathematical one. His successful attempts to include the

gravitational force, led to the generalized theory of relativity, and this happened to provide for an unexpected explanation of an observation that had already been made but was not understood until then: the anomalous shift of the orbit of the planet Mercury. It was also very fortunate that Einstein’s theory gave rise to an opportunity for further experimental tests, notably the famous Eddington expeditions.

In this case, the role of the experiment was not to discriminate between Einstein’s theory and some would-be alternative; there hardly were alternatives[§], but the theory could have been wrong or incomplete for other reasons. The test was made in order to find new confirmations of the fact that Physics here was on the right track. This is a situation that we see occur quite often in our field of science. Historically, these tests were generally thought to be of extreme importance. What this means, is that theories that are so novel as relativity was at that time, cannot flourish without as much support as possible from independent sources of information.

Many of our present theories are to be compared to relativity *without* such checks. The builders of these theories are facing hard times; yet I believe that we *can* make progress, provided our techniques, as well as our critical attitude regarding the required logical consistence, improve.

A milestone in physics was Paul Adrien Maurice Dirac’s improved equation for the electron. Previously, Wolfgang Pauli had the following (non-relativistic) equation:

$$H\psi = \left(\frac{1}{2m}(\hat{\mathbf{p}} - e\mathbf{A})^2 - \frac{ge}{2m}(\mathbf{B} \cdot \boldsymbol{\sigma}) \right) \psi ,$$

where the last term describes the interaction between the electron’s magnetic moment with the magnetic field \mathbf{B} . It features an unknown constant, the electron’s gyro-magnetic ratio, g . Now Dirac’s equation⁴ was

$$H\psi = \left(\boldsymbol{\alpha} \cdot (\hat{\mathbf{p}} - e\mathbf{A}) + \beta m \right) \psi ,$$

where $\boldsymbol{\alpha}$ and β are the Dirac matrices.

In the non-relativistic limit, Dirac’s equation turns into Pauli’s equation, but it offers a bonus: the gyromagnetic ratio g is predicted to be exactly 2, which was very close to the experimentally observed value. However, Dirac realized very well that there also was a difficulty with his equation. His electron is described by 4 components, not 2, as in Pauli’s equation. The two extra degrees of freedom could be interpreted as describing fermions with electric charge $+e$ instead of $-e$, as in the electron. About this episode, A. Pais writes⁵:

While in Leipzig, June 1928, Dirac visited his friend and colleague Werner Heisenberg, who had recently been appointed there. Heisenberg was also well aware of the difficulties. In May he had written to Pauli: “In order not to be forever irritated with Dirac, I have done something else for a change” (the something else was his quantum theory of ferromagnetism). Dirac and Heisenberg discussed several aspects of the new theory. Shortly thereafter, Heisenberg wrote again to Pauli: “The saddest chapter of modern physics is and remains the Dirac theory.” He mentioned some of his own work which demonstrated the difficulties, and added that the magnetic electron had made Jordan “trübsinnig” (melancholic). At about the same time, Dirac, not feeling so good either,

[§] There *do* exist various kinds of *modifications* of Einstein’s theory, such as Dicke’s scalar-tensor theory, which could be tested by these experiments — they were all but ruled out — but even these were based on Einstein’s principle of invariance under general coordinate transformations. There actually also existed an alternative explanation for Mercury’s perihelion shift: an unknown planet very near to the sun...

wrote to Oskar Klein: “I have not met with any success in my attempts to solve the $\pm e$ difficulty. Heisenberg (whom I met in Leipzig) thinks the problem will not be solved until one has a theory of the proton and the electron together.”

Indeed, during some time, Dirac and his colleagues suspected that the new components had to be describing the proton, but this gave rise to even more difficulties: Oppenheimer⁶ and Tamm⁷ correctly objected that, if this were true, it would make the hydrogen atom unstable against decay into two or more photons. Finally, in 1931, Dirac had to admit, much against his will, that his equation predicted a particle not observed until then. Just before the year’s end, Carl Anderson made the first announcement of experimental evidence for the anti-electron⁸.

What this history tells us is that the relation between theory and experiment is quite a lot more subtle than a game of differentiation and rejection of false theories. Theories are more often than not rejected solely on the grounds that they do not seem to be reconcilable with known facts.

Another case of interest is Pauli’s theory of the existence of a “neutrino” to account for the missing energy in β decay. In February 1929, Pauli complained in a letter to Oskar Klein that Bohr was serving him up all kinds of ideas about beta decay

“...by appealing to the Cambridge authorities but without reference to the literature”, and he said: “With his considerations about a violation of the energy law Bohr is on a *completely wrong track*”⁵.

The existence of neutrinos was confirmed by Frederick Reines’ experiment⁹ at the Savannah River reactor, in 1956. When he announced the discovery in a telegram to Pauli, the latter reacted not the least surprised: “I knew already that they exist”.

Again a different situation occurred when the new theories for the weak interaction were proposed. In the late ’60s the problem was that the existing theories for weak interactions were simply too diffuse to be believable.

When C.N. Yang and R.L. Mills did their work on the generalization of QED equations, in 1954, the paper they published¹⁰ contained a theory that *did not seem to apply to any known situation* in particle physics. Yet, the theory had such a high degree of elegance that they believed it to contain some deep truth. Although this new theory was ignored by many physicists, it did provide inspiration to some of the deeper thinkers. Richard Feynman used it at various occasions: the Feynman-Gell-Mann expression¹¹ for an effective weak interaction theory (a refinement of an earlier proposal by Enrico Fermi), carried some elements of the Yang-Mills idea, and somewhat later when Feynman made an attempt to understand the quantization of the gravitational force, he was made aware by Gell-Mann of the deep mathematical relations between gravity and Yang-Mills¹². So a theory without any experimental verification nevertheless played a role. Insignificant as it seemed to be at the time, this role would later turn out to be an essential one.

In 1961, C.N. Yang was asked to lecture about “The Future of Physics”¹³, at a panel discussion at the Celebration of the MIT Centennial, April 8. At that time, this is what Yang dared to say about his expectations:

“[What I know now, does] enable us to say with some certainty that great clarification will come in the field of the weak interactions in the next few years. With luck on our side, we might even hope to see some integration of the various manifestations of the weak interactions.”

Yang hoped, *with luck on his side*, to bring all *weak* interactions under one denominator. The fact that,

within ten years, one single theory, based on his own ideas, would not only achieve that, but also include electromagnetism, apparently was way beyond his hopes.

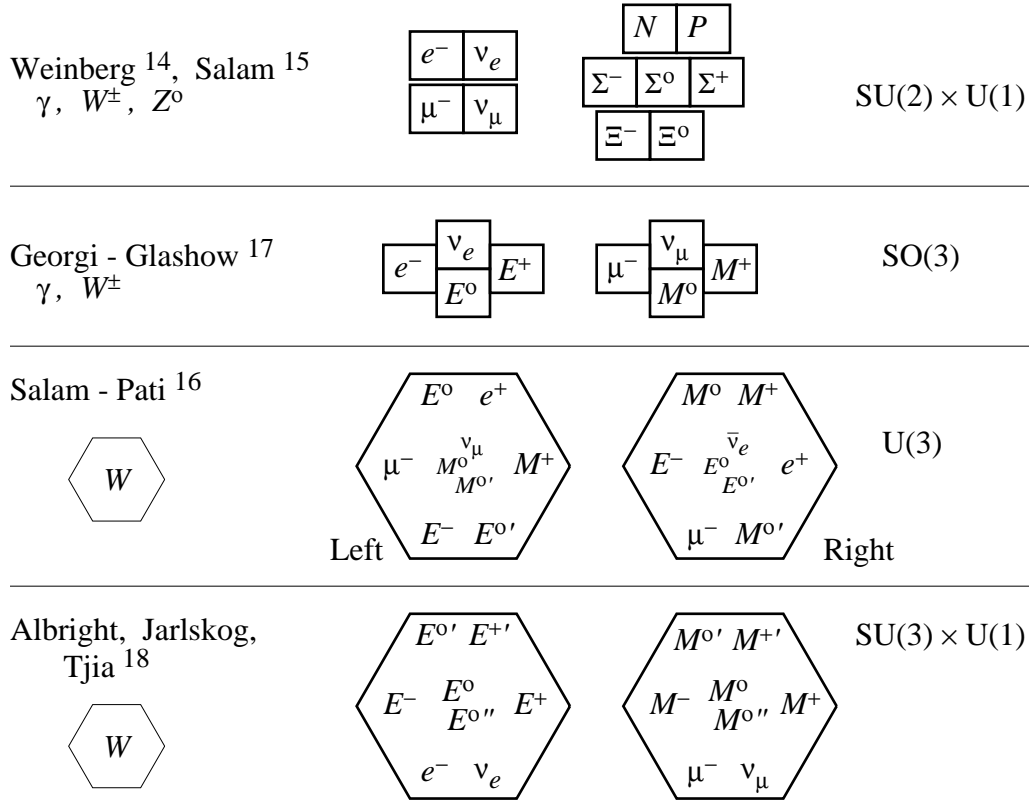


Fig. 1. A few weak interaction models of the early '70's, showing some of the proposed multiplet structures. E and M are proposed heavy leptons. Also indicated are the predicted vector bosons. The hexagons labeled “ W ” indicate nonets of charged and neutral vector bosons. For the last three models only the leptonic sectors are displayed.

In the early 70's it was discovered that *theories of this type were indeed logically fully consistent*, and superior to any of their alternatives. But it was also discovered that a whole class of such theories could be constructed, all having the Yang-Mills equations as a back bone. The earliest, already existing model was due to Steven Weinberg ¹⁴ and Abdus Salam ¹⁵, and it was based on $SU(2) \otimes U(1)$. But among the alternatives was a model proposed by Pati and Salam ¹⁶, whose version was based on $SU(3)$, and a more interesting alternative theory was proposed by H. Georgi and S.L. Glashow ¹⁷, based on the group $SO(3)$. See Fig. 1, where some of the models that were proposed are illustrated.

Most of the models predicted not only the charged vector bosons that intermediate the observed weak interactions, but also one or more neutral components, called Z^0 . Such particles would cause interactions between neutral components of the currents in hadrons and leptons. Such interactions had not yet been detected. What made the Georgi-Glashow model interesting was that, having no Z^0 boson, it would predict the *absence* of any such effects. In the '70's, a combined effort by several experimental teams (CERN ¹⁹, Fermilab ²⁰, Argonne ²¹, Brookhaven ²²) reinvestigated the spurious weak interaction events associated to the neutral current interactions. Notably, according to most of the new theories, a neutrino should be able

to hit against hadrons as well as leptons without itself changing into a charged lepton. Actually, such events had been searched for earlier, with apparently negative results: common wisdom had been that these events do not occur. But it was also agreed upon that these investigations were not totally conclusive, as the events searched for could easily have been mistaken for background noise.

Around 1974 the results of these experimentations became clear: the events were there, more or less as they were predicted by the simplest of all models: the Weinberg-Salam scheme. For theoreticians and experimentalists alike, this was decisive for selecting the original Weinberg-Salam scheme as the most likely theory for the electro-weak interactions. Could we have *derived* from purely theoretical arguments that the model was superior?

The Weinberg-Salam scheme did require a special mechanism, called the GIM mechanism²³, to cancel out all those weak interaction effects that are associated to a change of strangeness, without the exchange of electric charge ('strangeness changing neutral current events'). It was well-known that such interactions, if they existed at all, were very much weaker than the other weak interactions. Since this mechanism required the existence of a new quark species, the charmed quark, the search for this object began, leading to its discovery about a year later. What would have happened if we had not had all these experimental clues?

The alternative models did not describe the hadronic sector very well. In particular, they would have been very difficult to apply to the quark constituents of hadrons. Only for the Weinberg-Salam model did we have a scheme, the GIM mechanism. This had given the model an edge, even before the results of the experiments were known.

One crucial prediction was essentially missed: the new hadronic objects that can consist of only charmed quarks would turn out to have quite conspicuous properties. Experimenters justifiably sometimes ignore theory, by just searching for interesting things even if not explicitly predicted. Some theoreticians did indeed urge to search for narrow resonances, but these were not really considered to be so pronounced or special that they would have striking properties, that would stand out so much that a new field of physics would emerge. But we could have known. When these objects were discovered, called J (by S. Ting²⁴) and ψ (by B. Richter²⁵), in 1974, they were striking indeed. Most remarkable was the fact that their strong interaction properties were far weaker than normal. It was only during a short time that several "alternative theories" were considered to explain J/ψ . For instance, it was suggested that they might be the bosons mediating the weak interactions, or maybe they were totally new objects of physics. Several alternative theories were concocted, completely in Popperian style. Yet the charm theory stood out from the beginning. After half a year it was the only theory.

Since that time, theoretical physics has enormously gained in importance. What we have presently is an extremely detailed description of all particles and forces known to us. It is called the "Standard Model". There is actually only a limited amount of information that is still waiting to be provided by experiments in the near future: more details about the neutrino masses and their mixings², more precise determination of the mixing angles between quarks, responsible for the violation of PC invariance and top and bottom decays, and the mass (and possibly the spectrum) of the Higgs particle.

Of course we hope that accelerators in the near future will allow us to find further extensions of the Standard Model. Indeed, extensions are expected: supersymmetric versions of the theory (the Minimally Supersymmetric Standard Model, or MSSM) require the existence of much more elementary particle types than the ones known presently: the "super partners". Again, this is the presently preferred scenario, to be confirmed, or to be falsified, in which latter case we have not much to put in its place, other than "new and unexpected physics". Since I do have certain objections against the super symmetric scenario, I would

love to see a more complicated physical scene than the one predicted by the MSSM, but detailed predictions cannot be provided.

The reason for giving this talk, and for emphasizing the important shifts in the relations between theory and experiment, is my concern over the situation my field of research will be in, after these facts may or may not have been checked by experiments. We are rapidly proceeding with our theories. Our theoretical view of the particle scenery is presently so clear that we can project much further than the supersymmetry domain. Most importantly, we want to understand the gravitational force.

It is here that possibilities for experiment are severely limited. The Planck scale is determined by the combinations of the constants c (the velocity of light), \hbar (Planck's constant divided by 2π), and G (Newton's constant) that are listed in Table 1.

$$\begin{aligned}\sqrt{\frac{G\hbar}{c^3}} &= 1.616 \times 10^{-33} \text{ cm} \\ \sqrt{\frac{G\hbar}{c^5}} &= 5.39 \times 10^{-44} \text{ sec} \\ \sqrt{\frac{\hbar c}{G}} &= 21.77 \text{ } \mu\text{g} \rightarrow 1.22 \times 10^{28} \text{ eV} .\end{aligned}$$

Table 1. The Planck Units.

Several theoretical approaches are addressing the fundamental questions we are confronted with here, but of these, Superstring Theory, and its successors (D -brane theory and M -theory), are taking the lead. String Theory started as an attempt to understand the strong interactions, but its mathematical elegance became as striking as that of Yang-Mills theory, 20 years earlier. Here again, theoreticians were dealing with a general scheme that inspires us towards finding deeper connections between space, time and matter. The theories are gaining in power now that various extremely essential difficulties are being confronted and new resolutions are being discovered. The theoretical scheme now being devised is generally believed to be the “only credible theory that unifies the gravitational forces with other forms of matter in a quantum theory”.

Most important here is the fact that the gravitational force is a *cumulative force*, which means that, as it acts, it may draw large amounts of matter closer together, and in so doing create regions of space-time where the field becomes increasingly stronger. This makes gravity itself *fundamentally unstable*: it may give rise to “gravitational collapse”, or in more popular terms: a black hole.

All theoretical scenarios agree that, at least in principle, initial state conditions can be realized that inevitably lead to gravitational collapse: black holes, or objects very closely related to them, must be part of the matter spectrum at the Planck scale.

The black hole is a prototype of a physical system that can only be theorized about, but not be studied experimentally; nobody ever saw a black hole up close. Here in particular, one must ask the question whether the existing theories describing their behaviour are fully self-consistent, and whether these descriptions give rise to any kind of logical conflict. Indeed, attempts to arrive at a consistent description using well-known and accepted ingredients of physics, have led us to quite peculiar results.

A black hole appears to behave as if it is a “ball of material” that carries one “bit of information” in every $0.724 \times 10^{-65} \text{ cm}^2$ of its surface area. At the same time, however, a region of space-time that undergoes gravitational collapse into a black hole must also be able to describe ordinary particles living there, and, at first sight, it seems as if these particles are not constrained by a limit on the information they carry. This at

least is what we appear to arrive at when applying Einstein's theory of general relativity. On the other hand, black holes appear to drain away information from space-time, whereas in general such behaviour would be in conflict with standard formulations of quantum mechanics ²⁶.

How can one incorporate these apparently conflicting features correctly in a theory? Remarkably, string theoreticians recently succeeded in describing black holes (of certain types) in such a way that their results agree completely with everything just said above, so that their theory appears to be in good shape. ²⁷

Yet there are still numerous difficulties. Not the least of these is that theories of this nature do not provide simple answers to the question as to how they would predict the observed features of the Standard Model, or how in general any detailed calculation should be done to confront the theory with experiment at low energy scales.

There is no *direct* experiment to test any of the more esoteric ideas. This is not to say that there would not be *indirect* clues. There are still quite a few possibilities for future experiments and observations. The determination of the neutrino mass spectrum is just one of these. Clarification of the supersymmetric domain (if it exists!) and possible detection of superweakly interacting massive particles (if they exist!) are other possibilities. Furthermore, we may hope that astrophysical observations such as the spectrum and fluctuations of the cosmic microwave radiation will yield further data. But, to extrapolate from any of such observations all the way to a string theoretical description at the Planck scale, will continue to be a task of gargantuan proportions.

The only instrument we can use to study the Planck length in detail, is our minds.

In previous years we had experiment to guide us and to protect us against our numerous mistakes, so the correct theories could be identified and confirmed. This, as it turns out, was a luxury that we may have to do without in the future. Does such physics make sense?

Well, we have seen that, more often than not, experiment was not the only way to check a theory. It was logical consistence, general esthetics, agreement with previously known facts, and general rigour, that we could use as criteria in addition.

Unfortunately, history has also shown how easily we can be misled into wrong theories, and how important it was, throughout the ages, that we could ask Nature to settle our disputes. Numerous publications on the new esoteric theories show quite clearly that the moderating control of experiment has been absent for some time. The mathematics is often not as rigorous as one should wish. What disturbs me most is that main progress seems to be coming from theories that take the form of "conjectures". Failing to falsify the conjectures is often presented as a proof of their validity. In reality, these conjectures are essentially impossible to prove (or to disprove) with any kind of rigor since the theories are not sufficiently well defined. How sure are we that our logic is correct? In the past, such a situation has often showed to be detrimental for a theory.

On the other hand, if the logic in an argument appears to be less than perfect, this is not at all a sufficient reason to abandon further pursuit along the lines given. Remember that renormalization theory appeared to have serious shortcomings, yet it worked, just because the difficulties could be kept under control.

Do we have the difficulties of string theory under control? My presently most favored approach is that the most celebrated edifice of Physics, the theory of Quantum Mechanics, must undergo revision. ²⁸ I am not the only one saying this, but I do think that I have a better way to write improved theories and to show why they are superior, and furthermore that I have indications as to *why* Nature chose to run in accordance with laws that feature the remarkable principles of statistics that characterize Quantum Mechanics.

It was a momentous achievement of a century of Physics to find how the dynamics of molecules, atoms and subatomic particles could be described in detail using the principles of Quantum Mechanics. It is conceivable that the next century will tell us how to reconcile these findings with a deterministic world view. All that is needed is a description of a *primordial basis* of states,

$$|\psi_1\rangle, |\psi_2\rangle, \dots$$

in terms of which the evolution operators U behave as pure permutation operators, not just any set of unitary operators. A primordial basis may be seen to be generated by operators $\mathcal{O}(\mathbf{x}, t)$ which commute at all times:

$$[\mathcal{O}(\mathbf{x}, t), \mathcal{O}(\mathbf{x}', t')] = 0, \quad \forall \mathbf{x}, \mathbf{x}', t, t',$$

in contrast with more conventional fields, which do not commute within the light cones. Physicists are presently used to the idea that such operators \mathcal{O} cannot possibly exist, but this is not totally obvious. When gravitational forces are taken into account, any ‘no-go’ theorem would be extremely difficult to prove.

Naturally, my critical audience asks what, in such a theory, would be the cause and the meaning of interference effects. A possible answer is that interference is only recognized as such *after* having assumed some law of physics describing particles in semi classical terms as objects that can be detected in an experiment. These laws of physics may in reality be extremely simplified versions of a statistical analysis using only a very small subspace of Hilbert space generated by the primordial states: the states with very low energies. This subspace is built from superpositions of the primordial basis elements, being the low-energy eigenstates of the hamiltonian (defined to be the true operator generating time shifts). Since these are *not* primordial states, physicists experience interference effects all the time.

The theory, of which the above is only a very brief outline, leads to several difficulties that are as yet far from being resolved. A primary difficulty is the simple observation that, in the real world, the hamiltonian happens to have a lower bound. If I define the hamiltonian to be the generator of time shifts, then the presence of a lower bound is difficult to reconcile with determinism. A possible way out of this dilemma was recently identified: information loss.

If ideas of this sort could be combined with string theory, this might lead to the insights still needed to produce the necessary logical coherence in our theories. But will it be enough?

Without much guidance from experiment, obtaining better understanding of Nature may perhaps be prohibitively difficult. But my confidence in human ingenuity is tremendous. Even if experiments will be difficult, smart new ideas for experiments will still be proposed, but they will require the utmost skill for a correct interpretation. To do our job, to reach the ultimate equations of physics, with the given limitations for experimental evidence, will probably take much more time than the 20 years that were S.W. Hawking’s estimate²⁹. During this time, probably many new generations of young physicists will enjoy the fine taste of new discoveries. We are all full of optimism for the new century.

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