The present status of general relativity

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Abstract. The present status of research in classical general relativity, cosmology, and quantum gravity is discussed, and some prospects for future developments in these fields are indicated.

1. Introductory Remarks

It is customary at the end of a meeting of this sort to have a "Conference Summary" talk, to aid the participants in distilling the key new ideas that have been presented here. To give a comprehensive, balanced Conference Summary is an extremely difficult task. Fortunately, I have the advantage at this meeting that my presentation does not even pretend to be a Conference Summary. Rather, this contribution represents my best attempt to summarize the current status and trends of research in the broad subject area covered by this meeting. This will be done from my own personal perspective. I state this obvious fact to emphasize that, although none of my comments are intended to be frivilous, there is no reason for anyone else to take them seriously. In particular, if I felt it likely that any of my remarks would be used to give a stamp of approval (or disapproval) to any given line of research, I probably would have refused to give this talk.

At least one other disclaimer should be made before I begin. I cannot pretend to keep up with all developments even in the areas in which I have done substantial research, no less the very broad area I am attempting to review here. My intention is to review only the basic trends, with an eye toward developments in the field which may be expected in the not too distant future. Names of individual researchers will be kept to an absolute minimum, and references will be limited to other plenary talks at this meeting. The reader who wishes to obtain a comprehensive survey of recent research results in general relativity would be much better served by systematically browsing through all the other contributions to this volume than by reading this contribution.

For the purposes of this talk, I define the term "general relativity" to mean the topics that the people who come to a GR meeting do research on. This probably is best reflected by the subjects covered in the workshops at this meeting. I shall organize this review by dividing it into three main categories: (1) Classical general relativity, (2) Cosmology, and (3) Quantum gravitational physics.

2. Classical General Relativity

General relativity has had a rather strange history. It was formulated more than 75 years ago, and was immediately recognized as being both "beautiful" and "deep". Within a very short time after its formulation, some of its key predictions were confirmed; specifically, the 1919 eclipse expedition confirmed the "light-bending" prediction, and the calculation of the perihelion precession of Mercury accounted for the previously observed "residual" precession of that planet. Furthermore, some key exact solutions (particularly, the Schwarzschild solution) were discovered almost immediately, which could have enabled researchers to investigate some of the new "strong field" phenomena predicted by the theory. With all of these factors working in its favor, it seems remarkable, in retrospect, that so little attention was paid to the theory over the next forty years or so. There were, of course, several notable investigations concerning gravitational collapse and the nature of (homogeneous, isotropic) cosmology, but there appears to have been very little attempt to really take the theory seriously by working out its predictions and consequences in a systematic way. Indeed, general relativity appears to have had more the status of a mathematical curiosity than of a theory of the physical world during this period, and vestiges of this attitude persist even today.

One of the reasons contributing to the unusual status of general relativity in physics is the unusual relationship general relativity has had throughout most of its history between theory and experiment/observations. Most of the dramatic and exciting advances in physics occur when the experimentalists are one step ahead of the theorists, finding new phenomena that challenge the theorists either to find an explanation within existing theory (thereby probing its structure more deeply) or to modify the existing theory. Such a relationship between theory and experiment is particularly healthy when the existing theory is only a partial one, since experiments and observations then serve to define the limits of the theory and suggest appropriate generalizations. The decades of research in particle physics preceding the formulation of the present-day "standard model" of electroweak and strong interactions provides an excellent example of this kind of vigorous interplay.

However, general relativity was "born whole" and – unless it is wrong – its limits, presumably, are defined by the Planck scale, which is entirely inaccessible to direct observation. Throughout most of its history, the contact of general relativity with experiment and observation (apart from cosmology, to be discussed in the next section) has been limited to "solar system" tests: light bending, Mercury's precession, the gravitational redshift, and (within the past 20 years) the gravitational time delay. These tests have been extremely important for validating the theory. The ever increasing precision of these observations – such as the confirmation of the light bending prediction of general relativity to within .3% achieved with long baseline interferometry – should be appreciated and applauded by all general relativists. New experiments planned for the future – such as the measurement of geodetic precession – will provide further tests. However, these "solar system tests" have not, as yet, posed any significant challenges to the theorists: The approximation schemes needed to derive the predictions are quite straightforward, and the data has been in beautiful accord with the theory. Unless they eventually demonstrate that general relativity is wrong, I do not believe that these tests are likely to have much impact upon the direction and progress of the field in the foreseeable future.

However, in other arenas, there is a good chance that theorists will be challenged by experiments and observations in the not to distant future, even assuming that all future experiments and observations will yield results consistent with the predictions of general relativity. To some extent, this is happening already with the high precision observations of binary pulsar systems. Observations of the original Hulse-Taylor binary pulsar system dramatically confirmed the existence of gravitational radiation as predicted by general relativity. Perhaps even more significantly for the interplay of theory and observation, the effects being measured for these systems are of a sufficiently "strong field" nature that the approximation schemes needed to derive them are not straightforward. Indeed, even the approximation which leads to the standard "quadrupole formula" for gravitational radiation (and corresponding back-reaction) is nontrivial to justify rigorously in the case of self-gravitating systems. The binary pulsar observations should continue to provide a stimulus to theorists to develop better and more rigorously justified approximation schemes.

Observations of millisecond pulsars also may result in significant interplay with general relativity theory in the foreseeable future. Already, the rotation rates of the fastest pulsars – when interpreted within the framework of general relativity – are not far from providing significant restrictions on the equation of state of matter composing neutron stars. It is quite possible that with better statistics afforded by observation of more millisecond pulsars, an upper limit on rotation rates (as expected from the instability of rotating stars in general relativity) will be deduced. When combined with additional information about neutron stars obtained from other observations, we stand a good chance of learning new things about the strong field behavior of general relativity, as well as about the properties of matter at nuclear densities.

As I shall comment further upon in the next section, observational astronomy has undergone a revolution in the past decade, and new and higher quality information about astrophysical systems is likely to continue to be obtained at a rapid rate. In particular, it seems safe to predict that in the foreseeable future, important new observations will be made of binary X-ray sources within our galaxy, of the central regions of the nucleus of our galaxy and nearby galaxies, and/or of quasars and other active galactic nuclei. Such observations may yield some stringent tests to the models for these systems which involve black holes. The discovery of some strikingly new phenomenon in these systems would probably afford us the best opportunity we have of learning more about phenomena occurring in the strong field regime of general relativity. Indeed, it was the original discovery of quasars in the early 1960's that provided the first real stimulus to systematically study the strong field predictions of general relativity. This stimulus probably was largely responsible for what I consider to be the "golden era" of classical general relativity – the period from the mid-1960's to the early 1970's when "global methods" were formulated, the singularity theorems were proven, and the theory of black holes was developed.

Perhaps the best opportunity of all for vigorous interaction of general relativity theory with experiment will be attained if the new generation of gravitational wave detectors – presently under construction – succeed in achieving the sensitivity for which they are ultimately designed. The goal of obtaining an unambiguous detection of gravitational radiation would then easily be met. However, for the same reason as the solar sytem tests, by itself this probably would not have a much impact upon the field – unless, of course, the observed characteristics of the radiation differ measureably from the predictions of general relativity. Indeed, the binary pulsar observations have already confirmed the existence of gravitational radiation as predicted by general relativity, with a quantitative precision much greater than any direct detection is likely to attain. Rather, the true potential impact upon the field arises from the possibility of doing "gravitational wave astronomy". Observations of the wave forms of gravitational radiation signals – possibly in conjunction with observations of electromagnetic signals emitted by the same sources – would provide the kind of challenges and stimuli to theorists that could lead to major advances in our understanding of phenomena involving strong gravitational fields.

However, despite the above remarks, it would be quite optimistic to believe that any significant interplay between general relativity theory and experiments and observations will occur within this decade. Thus, for the foreseeable future, the development of classical general relativity is likely to continue to be driven mainly by the study of "old problems", as well as by new developments in cosmology and quantum gravity.

Of the "old problems", there is one which, in my view, stands out as dominant both on account of its fundamental importance and because of the possibility that some significant progress can be made within the coming decade: the nature of singularities. During the "golden era" of classical general relativity alluded to above, mathematical techniques of differential geometry were used to establish the existence of singularities in solutions to Einstein's equations in a wide variety of circumstances relevant to gravitational collapse phenomena and to cosmology. In these theorems, Einstein's equation (together with energy conditions on matter) is used only to obtain inequalities on the Ricci curvature. The fact that the detailed properties of Einstein's equation do not play much role in the singularity theorems is largely responsible for their great power and generality: The occurrence of singularities cannot be evaded by modifying the matter content (provided that the energy conditions are still satisfied) or even by a wide class of modifications of Einstein's equation itself. However, this generality is probably the root cause of the one significant deficiency of the singularity theorems: For the most part, they say nothing about the nature of the singularities they predict apart from the fact that some inextendible causal geodesic must be incomplete.

The study of the nature of singularities in classical general relativity is of fundamental importance for at least two reasons. First, it is crucial for understanding strong field behavior. The physical relevance of black holes is entirely premised upon the hypothesis that the singularities resulting from gravitational collapse are confined to black holes, i.e., that "naked singularities" do not occur. If this "cosmic censor hypothesis" should turn out to be false, our present beliefs concerning strong field phenomena in general relativity would undergo drastic modifications; indeed, even my statement above that Planck scale phenomena are inaccessible to direct observation probably would be wrong. It should be recalled that at the present time, support for belief in the validity of the cosmic censor hypothesis comes entirely from some linear perturbation analyses and from the beauty and internal consistency of the theory of black holes, rather than from analysis of the general behavior of solutions to Einstein's equation in situations corresponding to gravitational collapse. Similarly, in cosmology one would like to understand whether initial singularities generically have a "spacelike character" (so that horizons are present in the early universe) and the manner in which the Ricci and/or Weyl curvatures diverge near an initial cosmological singularity.

Secondly, an analysis of the nature of singularities would provide a major step toward our understanding of the manner in which classical general relativity breaks down. It could provide some important clues as to the kinds of new phenomena that might occur in a quantum theory of gravitation.

Singularities are present and can be studied in detail in some of the simplest and most basic solutions in general relativity, such as the Schwarzschild solution and the Robertson-Walker models. However, it is quite possible that these simple solutions may give a very misleading picture of the general properties of singularities. Clearly, what is required is a very general analysis of properties of solutions to Einstein's equation. As already indicated above, it does not appear that the "global methods" used to prove the singularity theorems can be pushed much further to enable a detailed description of their properties. However, I believe it likely that progress can be made on this issue on two broad fronts.

First, as described in the contribution of Evans, it appears that "numerical relativity" is finally coming of age. It now is feasible to reliably study via numerical simulation the dynamical evolution of nonspherical spacetimes in which gravitational collapse to a singularity occurs. Some interesting examples already have been obtained by Shapiro and Teukolsky, as reported in Teukolsky's contribution. The study of the evolution of spacetimes without any symmetries imposed (i.e., with "4-dimensional codes") may be possible in the foreseeable future. Of course, numerical codes tend to break down near singularities at a much earlier stage than the breakdown of classical general relativity itself, so it is not likely that we will be able to learn about the detailed structure of singularities from numerical experiments. However, at the very least, numerical experiments should be able to provide strong hints concerning strong field behavior near singularities. They also should be able to stringently probe the validity of the cosmic censor hypothesis.

Secondly, the global properties of solutions to Einstein's equation are now being studied with modern methods of partial differential equations. As discussed further in the contribution of Klainerman, the main results obtained thus far are "nonsingularity theorems" – in particular, a proof that globally nonsingular solutions to Einstein's equation exist for all initial data sufficiently close to flat spacetime (i.e., that singularities cannot be created starting with weak gravitational waves). These methods will have to be developed considerably further in order to have a chance at obtaining general results on the properties of singularities in general relativity. However, the advances in this area which have been made in the past decade are quite encouraging, and they hold out hope that analysis of such issues as the validity of cosmic censorship may be possible in the not too distant future.

My extended discussion of the issue of the nature of singularities should not be interpreted as indicating a belief that there are no other issues in classical general relativity worthy of intensive study. However, I do not have space here to discuss these issues, and I fear that any short list of problems which I might attempt to compile would end up being most notable for its inadvertant omissions. Thus, I will simply remark that a substantial portion of my own recent research has been on other issues in classical general relativity, and I have every expectation that this will continue in the future.

The most promising source of new ideas and issues in classical general relativity is the research efforts in the "border areas" of cosmology and quantum gravity. I now turn to a discussion of the first of these topics.

3. Cosmology

The field of cosmology has undergone very significant development in the past two decades. In my view, the most remarkable – and certainly the most solid – of its achievements has been the (in my opinion, convincing) demonstration of the success of the "standard cosmological model" – i.e., the Friedman-Robertson-Walker (FRW) solution with matter in thermal equilibrium in the early universe – in accounting for the basic features of our universe from the era of nucleosynthesis onwards. Thirty years ago, the main reasons for believing in the validity of the "standard model" were the following: (1) Its assumed homogeneity and isotropy appeared to be in good (rough) agreement with the observed distribution of the galaxies. (2) It accounted nicely for the Hubble expansion. (3) The relationships between the observed values of Hubble's constant, the "deceleration parameter", the age of the universe, and the mass density of the universe were in (very rough) accord with the predictions of the model – at least, after serious errors in the determination of Hubble's constant were corrected.

The discovery of the cosmic microwave background in 1965 provided dramatic further evidence in favor of the "standard model." Such "relic" radiation is naturally predicted by the model, and it is very difficult to find other plausible explanations for its existence, Planckian spectrum, and isotropy.

Another strong piece of evidence in favor of the standard model became evident by the late 1960's. The standard model predicts that a substantial amount of He^4 should have been synthesized in nuclear reactions beginning several seconds after the "big bang" singularity of the model, and ending about 15 minutes later. The predicted abundance of He^4 produced in this manner is in excellent agreement with observations. Since far more He^4 is produced in this manner than plausibly could have been produced in stars, the agreement of the predictions with observed helium abundance is not something that could be easily accounted for in other ways.

Today, we probably have, if anything, less grounds than thirty years ago for advancing reason (1) above in support of the FRW models. As discussed further below, redshift surveys during the past decade have enabled the determination of the "three dimensional" distribution of galaxies, and very significant departures from homogeneity have been observed on much larger scales (at least $\sim$50 megaparsecs) than anticipated thirty years ago. The status of the observational support for reasons (2) and (3) above has not changed significantly in the past thirty years.

Nevertheless as already indicated, evidence in support of the standard model has been enormously strengthened in the past two decades. One reason is the greatly improved precision of the measurements of the microwave background radiation. As far as can be determined by COBE, the spectrum of the microwave background radiation is exactly Planckian, and only very recently has COBE finally has detected some tiny departures from exact isotropy. This has provided strong support for believing that this radiation is, indeed, the "relic radiation" predicted by the standard model.

However, perhaps the strongest new evidence for the standard model has come from new observations (and experiments!) related to nucleosynthesis occurring in the early universe. In addition to He^4, trace amounts of H^2, He^3, and Li^7 also are predicted to be synthesized. The predicted abundance of these elements depends sensitively on the baryon density, and the first measurements of deuterium abundance twenty years ago were used primarily to estimate the baryon density – yielding the result that baryons provide about 5% of the mass density that a flat FRW model would have. However, the measurement of additional light element abundances provides a test of the model itself. The fact that the primordial abundances of these elements inferred from observations during the past decade also agree with the predictions of nucleosynthetic calculations is strong support for the standard model (as well as for baryon density $\sim$5% of the "closure density", i.e., $\Omega_B \sim .05$) . Second, the percentage of He^4 which is synthesized is not very sensitive to the baryon density but is sensitive to the expansion rate of the universe during the era of nucleosynthesis. This expansion rate, in turn, depends upon the matter content, and calculations within the standard model showed that the presence of more than 3 light neutrino species in thermal equilibrium in the early universe would yield too high a helium abundance. The existence of only 3 light neutrino species has now been confirmed by the experiments on the decay of the Z^0 conducted at CERN.

Of course, if some discrepancies between calculations and observations had been found, the theorists surely would have found plausible ways of modifying the standard model so as to reproduce the observations (or would have found plausible reasons for rejecting or re-interpreting the observations). However, the fact that the model now has stood up to quite a number of nontrivial, quantitatively precise tests with little or no "fudging" should be taken very seriously. Any present or future difficulties associated with supplementary hypotheses to the standard model – particularly with regard to the origin of the departures from homogeneity – should not be confused with the remarkable success of the essential features of the model. To repeat the advice of a particle experimentalist who had just paid off his wager with a cosmologist on the number of neutrino species: "Don't bet against the big bang!"

It is worth mentioning that the success of the standard cosmological model has had an unfortunate side effect for general relativists. It does not require much knowledge of general relativity to write down the Robertson-Walker line element. Had difficulties with the standard model arisen, the possibility of curing them by going to anisotropic or inhomogeneous models would have been explored, and general relativists undoubtedly would have played a leading role in these efforts. Since such efforts have not been necessary, general relativists have stayed largely on the sidelines, and not much stimulation to the field of general relativity has resulted from these developments in cosmology.

Given the success of the standard model, it is only natural that attempts would be made to extrapolate its description of the universe back to earlier epochs, when the energy and density of matter was much higher than accessible or testable in laboratory conditions. Such research necessarily is of a speculative nature, and most of the research in cosmology during the past decade has been of this speculative kind, concerned primarily with phenomena which may have occurred at times corresponding to the grand unification scale (i.e., $t \sim 10^{-35}$ sec.) or even the Planck scale ($t \sim 10^{-43}$ sec.). Some innovative ideas have been introduced, and some interesting developments have taken place, most notably in theory of inflation and ideas concerning the formation of cosmic strings and other "topological defects."

It would take me too far afield to discuss these ideas here. However, I do wish to briefly comment upon what I view as a shortcoming of the nature of some of the theoretical research activity in this area. In my opinion, insufficient distinction often is drawn between "physical issues" and "metaphysical issues." (Here, by "metaphysical issue" I mean simply an issue lying outside the nature and scope of present-day theories of physics; I do not intend the negative connotation usually implicit in the use of that term by physicists.) To illustrate this point, consider the following two widely discussed problems which often are discussed as though they were on the same footing: the "monopole problem" and the "flatness problem." The "monopole problem" refers to the prediction of grossly overabundant monopole production in the early universe, assuming that matter is described by a grand unified field theory. It is very much a "physical problem", i.e., from well defined initial assumptions and well defined physical laws, one obtains a prediction inconsistent with observation. Its solution must be sought in abandonment or modification of the grand unified model, or in a mechanism to dilute the monopole density (such as inflation), or in the modification of other cosmological assumptions. On the other hand, the "flatness problem" refers to the fact that in standard FRW models, in the early universe the spatial curvature must have been enormously smaller than the energy density of matter; equivalently, the lifetime of our universe is enormously larger than the Planck time or any of the fundamental timescales of elementary particle theory. This problem has much more of a metaphysical character. It is not at all clear that there is any "problem" at all, except possibly "naturalness" (and the creator of the universe might well have a rather different concept of "naturalness" than we do!). A "solution" to this "problem" presumably consists of a model where the conditions of the early universe arise in a manner which seems less artificial to us. Many of the other widely discussed problems of cosmology also have a similar metaphysical component. Our civilization has made enormous progress in the development of physical theories, but I am not at all confident that we are better equipped than the ancient Greek philosophers or medieval scholars to deal with metaphysical issues. It is very important to the progress and direction of science to raise metaphysical issues; important breakthroughs can be made by attempting to address issues which lie outside the scope of present physical laws. Furthermore, the distinction between physical and metaphysical issues is not always entirely clear cut. However, in my view, a serious effort to draw these distinctions as sharply as possible would be very helpful for clarifying the goals and aims of research in cosmology.

Without question, the area of research in cosmology which presently is undergoing the most active development and where many further exciting developments can be anticipated in the near future concerns the "origin of structure", i.e., the processes which led to the formation of galaxies and clusters of galaxies. These developments have been fueled by what can best be termed a revolution in observational astronomy. Without question, the most important single technological advance involved in this revolution was the replacement of the photographic plate by charge-coupled devices (CCD's), which came into wide use beginning about a decade ago. Present day CCD's have a photon detection efficiency of order 75% (as compared with $\sim$1% for photographic plates), and their digital character makes it possible to do accurate and efficient subtractions of the night sky background. As a direct consequence of the advent of CCD's, several orders of magnitude less observing time is required to obtain galactic redshifts than was possible using photographic plates. This has made it possible to take redshift surveys which are much more complete, much deeper, and not nearly as subject to selection effects as prior surveys. As already mentioned above, this enables one to obtain reliable "3-dimensional" maps of the distribution of galaxies, and the surveys taken thus far have shown significant departures from homogeneity and the presence of coherent structures in galactic clustering on much larger scales than had been anticipated. A large number of very ambitious new redshift surveys are currently in progress or planned for the near future, so in the coming decade, we will obtain a great deal of new information concerning the large scale clustering of galaxies.

Other major advances also are occurring in our ability to make observations relevant to cosmology. A new generation of telescopes is being built with adaptive optics, which will greatly improve resolution and could enable phenomena like supernovae in progenitor galaxies to be studied. A large number of satellites have been launched or will soon be launched, giving us a capacity to probe the universe much more fully and in more detail in the microwave, infra-red, visible, ultraviolet, X-ray, and gamma ray regimes. It would be rather surprising if some dramatic new discoveries do not arise from the wealth of information we will obtain from these sources in the coming decade.

This new observational data undoubtedly will provide stringent tests for theories of the origin of structure. The two major new theoretical ideas of the past decade – inflation and cosmic strings – presently provide competing explanations for the origin of structure. Inflationary models provide a simple mechanism for amplifying quantum fluctuations to a magnitude and power and fluctuation spectrum suitable for formation of the observed large scale structure. Strings (or other topological defects) could provide seeds for structure formation either directly through gravitational attraction or via "wakes" resulting from their motion. Models based upon these ideas have provided us with valuable theoretical lampposts under which to search for the keys to the origin of structure. It is certain that much will be learned from the confrontation between these models and new observations, such as the microwave temperature anisotropy observations recently reported by COBE.

The wealth of new observational data also is likely to finally provide convincing evidence as to whether our universe is closed (as had traditionally been favored by theorists prior to inflationary models), very nearly flat (as predicted by inflationary models), or open (as favored by some present observational evidence and as indicated by the observed light element abundances arising from "big bang nucleosynthesis" – unless the present mass density of the universe is dominated by matter in non-baryonic form).

In summary, it seems likely that we are in the midst of a "golden age" in cosmology.

4. Quantum Gravity

Without question, the key issue in quantum gravity is the determination of the fundamental character of the formulation of the theory itself. In all non-gravitational theories, the causal and metrical structure of spacetime are fixed in advance. The notion of an "instant of time" is represented by a spacelike hypersurface in a given, background spacetime. In an ordinary quantum field theory, the fundamental observables of the theory are the values of the field and its correlation functions – modulo gauge if the field is a gauge field – at any instant of time. Methods exist for calculating these field correlation functions in perturbation theory. Although many of the formal expressions for terms occurring in the perturbative expansion are infinite, for renormalizable theories well defined rules exist for extracting finite results without introducing any new parameters into the theory. Furthermore, it should be noted that in most applications, one is interested only in the behavior of the field at asymptotically early and late times, when it can be treated as "free." A particle interpretation of the states is available in these asymptotic regimes, and, in most applications, the relevant information is encoded in the S-matrix, for which (in renormalizable theories) a well defined perturbative expansion exists.

The key phrase in the preceding paragraph is "the field . . . modulo gauge . . . at any instant of time." In general relativity or other gravitational theories based upon a spacetime metric, the gauge group is (or includes) the diffeomorphism group of the spacetime manifold. Unlike other gauge theories, this gauge group includes all "time translations," so in order for a quantity defined at an "instant of time" to be "gauge invariant", it is necessary for it to be "time independent". However, quantities of this sort – referred to as "perennials" in Kuchař's contribution – would appear to be essentially trivial, and do not encompass the usual dynamical variables of general relativity, such as the induced metric and extrinsic curvature of a spacelike hypersurface. Thus, we have what appears to be an essential conflict between general relativity and quantum theory: General relativity demands that only "histories" are well defined (i.e., "gauge invariant"), whereas the fundamental structure of quantum theory requires that only observables defined at an instant of time be well defined, (i.e., "histories" are ill defined in quantum theory – except in an idealized limit of perfect decoherence).

Parametrized theories of particles or fields have formal properties very similar to general relativity. A well defined quantum theory of these systems can be obtained by "de-parametrization", i.e., by explicitly identifying the variable which (secretly, in the initial formulation) plays the role of time in these theories and interpreting the constraint equation as a time evolution equation in this variable. A sensible Hilbert space structure on states also can be defined by making use of this distinction between the "time variable" and the "true dynamical degrees of freedom". However, although the issues involved have been discussed for more than two decades, very little, if any, progress has been made in the direction of "de-parametrizing" general relativity, or, for that matter, in precisely defining the states and observables of the theory by any other means. This is the "problem of time".

There are additional difficulties which any proposed quantum theory of gravity must overcome. One difficulty (undoubtedly closely related to the "problem of time") has to do with the lack of a background causal structure of spacetime. The fact that quantum fields commute (or anticommute) at spacelike separated events is a fundamental property of all non-gravitational quantum field theories, but it is far from clear whether a similar idea can even be expressed in quantum gravity, since the notion of whether two points on the spacetime manifold are "spacelike related" depends upon the (quantum) metric and is not sharply defined (and also is state-dependent). An additional serious difficulty (whose exact relationship, if any, to the "problem of time" is unclear) is that a simple dimensional argument indicates that even if a quantum theory of gravity could be written down, it would be nonrenormalizable. Hence, either each term in its perturbative expansion would have to be finite or (presumably, infinitely many) new parameters would have to be introduced to define the quantum theory.

Two decades ago, the (rather small number of) researchers in quantum gravity divided into two main groups. The first group – comprised largely of theorists with substantial background in particle theory – favored the "covariant approach" to formulating a quantum theory of general relativity. In this approach, one writes the spacetime metric, g_{ab}, as a flat metric, η_{ab}, plus a remainder, h_{ab}, and treats h_{ab} as a quantum "nonlinear spin-2 field" in a flat spacetime. One makes free use of the causal structure of η_{ab} in the formulation of the theory, and treats the theory as though the fundamental observables were the correlation functions of h_{ab} (or the S-matrix elements for graviton-graviton scattering). Eventually, in this approach, one should have to confront the fact that the causal structure of η_{ab} should play no role in the theory, and that η_{ab} and h_{ab} should not have any physical significance as separate entities, since the theory should depend only upon g_{ab}. In other words, initially one does not attempt to impose the condition that the theory one obtains should be independent of the choice of the (artificially introduced, physically unmeasurable) flat metric, η_{ab}, and that h_{ab} is not an observable. In this manner, one can postpone dealing directly with some of the difficult formulational issues of quantum gravity, and focus attention upon issues involving the nonrenormalizability of the theory. One then immediately confronts the difficulty that, although S-matrix elements are finite at one-loop order in perturbation theory, they are infinite at two-loop order (and, presumably, all higher orders); S-matrix elements for typical theories of gravity coupled to matter are infinite at one loop order.

The second group of quantum gravity researchers of twenty years ago – comprised largely by theorists with a substantial background in classical general relativity – favored the "canonical" approach to formulating a quantum theory of general relativity. As discussed further in the contribution of Kuchař, the idea here is to express classical general relativity in Hamiltonian form (with constraints), write down the fundamental canonical commutation relations for the configuration and momentum observables, and then impose the constraints as conditions on the state vector. The quantum version of the classical Hamiltonian constraint equation yields the Wheeler-DeWitt equation. Since this equation enforces the gauge invariance of the state vector under time translation diffeomorphisms, the problem of time is confronted head-on in this approach, and has caused severe difficulties in the definition of the Hilbert space of states and the observables of the theory. Even if these difficulties can be overcome, the difficulties associated with the nonrenormalizability of the theory presumably would remain to be confronted.

Today, there still are two rather distinct groups of researchers investigating the formulation of quantum gravity: one – comprised mainly by particle physicists – taking an approach in much the same spirit as the covariant approach, and the other – comprised mainly by general relativists – taking the canonical approach. Many of the fundamental issues in both approaches remain unresolved. Nevertheless, some interesting new ideas have been introduced, and some notable progress has been made.

The covariant approach has evolved through higher derivative gravity theories (which cure nonrenormalizability but introduce new serious problems), supergravity theories (which yield finite scattering amplitudes to higher loop order in perturbation theory than ordinary general relativity, but apparently yield no fundamental advantages), and on to superstring theory. Superstring theory has the substantial achievement of providing us with a finite theory of gravity in the following sense: It is "finite" in that there is every expectation that the scattering amplitudes for ("first quantized") strings in a background flat (10-dimensional) spacetime are finite at each order in perturbation theory. It is a "theory of gravity" in the sense that one of the modes of oscillation of the string corresponds to a massless, spin-2 field, and there are arguments indicating that the theory can be reinterpreted as a theory involving a metric field which satisfies Einstein's equation in the "low energy limit." Nevertheless, many significant (and, undoubtedly, fundamental) difficulties remain regarding the interpretation of the theory and how to extract predictions from it for quantities other than string scattering amplitudes. In particular, it is not clear what local observables (as opposed to S-matrix quantities) are defined in the theory or how they are to be calculated. Some attempts have been made to formulate a "string field theory" (which, however, would appear to involve "local observables" on an abstract loop space, not on spacetime), but I am not aware of much progress in this direction. The initial surge of optimism in the mid-1980's that superstrings could provide the ultimate "theory of everything" appears to have been replaced in the past several years by a compensating pessimism – based primarily on the non-uniqueness of string theory and the difficulty of doing calculations rather than the uncovering of any inconsistency or wrong predictions of the theory. I have not followed developments in superstring theory closely and, thus, am not in a position even to speculate upon the future directions of research in this area. However, it is clear that the wealth of new ideas and mathematical tools introduced by superstring theory will have a lasting impact upon research in quantum gravity.

Research in canonical quantum gravity has gotten a significant boost in the past five years from Ashtekar's introduction of new Hamiltonian variables for general relativity. Instead of using the induced metric and extrinsic curvature of a hypersurface as the fundamental, canonically conjugate variables on phase space, Ashtekar takes a (complex) SU(2) connection and a "soldering form" as the fundamental variables. The constraint equations of general relativity simplify considerably in terms of these variables, and take a form closely analogous to that of Yang-Mills theory. Probably the most promising new idea toward formulating a quantum theory of gravity to arise from this approach is the "loop quantization" program, reviewed in the contribution of Smolin. This approach has not, as yet, provided a solution to the "problem of time" and issues related to regularization and renormalization of the theory remain to be confronted. However, this approach is still very much in its developing phase, and considerable further progress can be anticipated. At the very least, the "Ashtekar variables" have provided some new life to an approach to quantum gravity that for nearly 20 years had contributed very little in the way of new ideas toward solving the fundamental problems of the quantum gravity.

The direct attacks upon the problem of formulating a quantum theory of gravitation discussed above are by no means the only research activity related to quantum gravity presently being pursued. The theory of linear quantum fields in curved spacetime has been developed to a mathematically complete and precise theory, though some issues with regard to treating "back-reaction" remain to be resolved. Some additional insights into quantum phenomena occurring in strong gravitational fields – in particular, in the early universe – can be expected to result from further research in this area. Research continues in "quantum cosmology", i.e., quantum gravity with all but finitely many degrees of freedom eliminated by symmetry restrictions. Quantum cosmology provides an excellent testing ground for ideas related to the interpretation of quantum gravity, since the usual infinities of quantum field theory are absent, but the "problem of time" remains present. Proposals for selecting a preferred "wavefunction of the universe" also can be formulated and tested in the context of quantum cosmology. Such proposals make a serious attempt – for the first time in the era of modern physics – to provide us with a theory of initial conditions.

Some of the most penetrating insights into the nature of quantum gravity have come from the analysis of particle creation near black holes and its implications for black hole thermodynamics and the loss of quantum coherence. In the past year, research in this area has been revitalized by the study of a two-dimensional ("string-inspired") field theory which appears to have the necessary properties to model a situation corresponding to the gravitational collapse of a body which subsequently emits Hawking radiation. The model is sufficiently tractable that, in the semiclassical approximation, it should be possible to study in detail the nature of singularities produced by the collapse and quantum back-reaction, as well as issues related to the loss of quantum coherence. Unless the model turns out to be seriously flawed, it is likely that some new insights into the nature of the black hole formation and evaporation process will be achieved.

In summary, it undoubtedly is much too early to assess how far down the road we have come toward obtaining a quantum theory of gravitation. However, at least it is encouraging that some progress down that road presently seems to be taking place.

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