The Last 50 Years of General Relativity & Gravitation: From GR3 to GR20 Warsaw Conferences

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This article has a dual purpose: i) to provide a flavor of the scientific highlights of the landmark conference, GR3, held in July 1962 at Jablonna, near Warsaw; and, ii) to present a bird's eye view of the tremendous advances that have occurred over the half century that separates GR3 and GR20, which was again held in Warsaw in July 2013.

I. INTRODUCTION

The 1962 GR3 conference in Warsaw/Jablona was the last GRG event before the semi-centennial of Einstein's discovery of general relativity and this conference, GR20, will be the last one before the centennial. Therefore, the organizers of GR 20 thought it would be appropriate to open this conference with a reminder of GR3 and a brief assessment of the evolution of our field since then. Marek Demianski provided a vivid portrait of GR3 itself. I will discuss some scientific highlights of GR3 and contrast what we know now with what we knew then.

GR3 was a scientific milestone in that, thanks to participants like Peter Bergmann, Hermann Bondi, Subrahmanyan Chandrasekhar, Bryce DeWitt, Paul Dirac, Jürgen Ehlers, Richard Feynman, Vladimir Fock, Vitaly Ginzburg, Leopold Infeld, André Lichnerowicz, Achilles Papapetrou, Nathan Rosen, Dennis Sciama, John Synge, Joseph Weber and John Wheeler, it sparked new directions of research in mathematical general relativity, gravitational waves, quantum gravity and relativistic astrophysics. At GR20, it is hard to compete with this illustrious gallery of names. But we are doing better in two respects. First, our field as a whole has evolved tremendously, becoming prominent in a variety of disciplines; physics, astronomy, mathematics, computer science and even some technologies. Second, GR3 had no plenary talk by a female scientist while 20% of our plenary speakers are women, a significantly larger fraction than the membership of the International Society on General relativity and Gravitation. I hope this striking success will make our field even more attractive to all under-represented groups.

The proceedings of GR3 [1] are truly outstanding because they contain not only what was presented in the main talks and seminars, but also the tape-recorded exchanges that took place between the participants after these talks and during special discussion sessions. When I was a graduate student at Chicago, I borrowed them from the library and kept them for two full quarters. Although the proceedings were already a decade old, I learned a lot especially from the stimulating questions, answers and comments during discussions.

Let me provide a few examples of these exchanges to illustrate their general flavor, especially because the younger participants of GR20 would be surprised that so many of the issues that were discussed have continued to be important —and in some cases even central— to our field during the intervening 50 years. On the mathematical side, Rainer Sachs discussed the characteristic initial value problem in general relativity, and this was the first confer-

(a) (b)

FIG. 1: (a) Dirac, Fock and Infeld, and, (b) Dirac and Feynman, at GR3.
Credits: Photographs by Marek Holzman (1962).

ence proceedings in which Penrose diagrams appeared. Interestingly, Weber asked why one uses asymptotically Minkowskian boundary conditions in the study of gravitational waves, rather than asymptotically Friedmann type. Bergmann and Bondi answered that this is the best they had been able to do: "[we] regret that we haven't got to the point of doing the Friedmann universe." On the experimental side, Ginzburg and Leonard Schiff proposed the gyroscope experiments to test the effects of 'dragging of inertial frames'. Weber spoke of the 1000 kg aluminium cylindrical antenna that had just started taking data at the University of Maryland, with a sensitivity of 10^{-14} cm in displacements. Feynman asked for the frequency of gravitational waves that the antenna could detect (and Weber answered: "1600 Hz"). Ginzburg was interested in knowing if the antenna could detect gravitational waves emitted by a binary star system (and Weber answered: "For known double stars, No"). On the quantum front, this was the first conference proceedings in which Feynman diagrams with gravitons appeared; Stanley Mandelstam argued that *"Quantization [of gravity] in flat space can only be regarded as provisional solution ... one would like to formulate the equations of the theory exactly, even though approximations have to be made in their solutions";* and DeWitt spoke of 'quantization of geometry'.

I will divide my remarks into four parts in which the scientific program of GRG conferences has been traditionally divided for several decades: General relativity proper, Applications to cosmology and relativistic astrophysics, Experimental gravity and Quantum aspects of gravitation.

II. GENERAL RELATIVITY, PROPER

As one might expect, this area dominated GR3. Four of the main talks were devoted to asymptotics and conservation laws and three to gravitational waves. The subject of gravitational waves and the energy they carry drew considerable attention: this was the first conference in which the reality of gravitational waves in full general relativity was firmly established.

Since the younger participants at GR20 grew up in the LIGO-VIRGO era, it may come as a surprise to them that the scientific community was ambivalent about gravitational waves for half a century after general relativity was discovered. Let me therefore go back in the years and briefly explain the situation. Einstein did analyze gravitational waves in the weak field approximation around Minkowski space just a year after his discovery of general relativity. However, there was considerable confusion on the subject largely because people could not separate coordinate effects from true physics. Eddington for example did not believe in gravitational waves in full general relativity and famously said that "they traveled with the speed of thought". Even Einstein contributed to the confusion because he first misinterpreted his results with Rosen on what is now known as the Einstein-Rosen cylindrical waves![1] Indeed, he wrote to Max Born in mid-1936:

> *"Together with a young collaborator I arrived at the interesting result that gravitational waves do not exist, though they had been assumed to be a certainty to the first approximation. This shows that non-linear gravitational wave field equations tell us more or, rather, limit us more than we had believed up to now."*

Such misunderstandings were set to rest in the early 1960's by a group of researchers, many of whom were associated with Bondi's group in London. Bondi considered space-times describing isolated systems that represent sources of gravitational waves and recognized that the physical properties of gravitational waves could be teased out of these geometries by moving away from the sources in (future pointing) *null directions.* By encoding the energy carried away by gravitational waves in an invariant field —now called the Bondi news— he dispelled the confusion between coordinate effects and true physics. As he is said to have it later, 'gravitational waves are real; one can boil water with them!'

But even after these announcements, some confusion still persisted at GR3. In particular Bergmann raised the question as to *"whether an n body system held together by purely gravitational forces will radiate or not."* Feynman provided an affirmative answer using perturbative, quantum scattering theory. But not every one was convinced, again because of the concern that Feynman's weak field calculations ignored the infinite dimensional diffeomorphism freedom of the full theory. Also, in his final summary of the conference as a whole, Bergmann emphasized that "the rate of energy (or linear momentum) radiation will depend on the choice of coordinate system, and change under supertranslations in an involved manner". If this were the case then the energy carried away by gravitational waves would not be unambiguous.

Fortunately, thanks to further work by Ted Newman, Roger Penrose, Sachs and others this concern turned out to be unfounded. A clear theory of gravitational waves in *full general relativity* emerged in the asymptotically Minkowskian context. Together with subsequent work on approximation methods, such as the post-Newtonian scheme and increasingly reliable analysis of equations of motion, this theory provides the foundation for much of the current work in numerical relativity as well as data analysis in the gravitational wave science.

Since GR3, the area of mathematical general relativity has made tremendous advances in many other directions as well. I can only include a few highlights to illustrate the diversity of these successes since an inclusive summary of even the major areas will exceed the allocated space.

First, at the time of GR3, there was still controversy as to whether singularities in the known exact solutions were artifacts of high symmetries. The Khalatnikov-Lifshitz school

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[1] For a step by step account of this fascinating episode, see Kennefick's 2005 talk [2].

had a program to show that the general solution of Einstein's equations would be singularity free generically. What cleared up the situation was the introduction and astute use of global techniques by Penrose. The first singularity theorems by him, Stephen Hawking, Robert Geroch and others established that singularities are not restricted to symmetric solutions but arise generically in physically interesting situations. However, these theorems assumed that matter satisfies certain energy conditions, which are violated by the scalar fields used in inflation. So there was a hope that they could be evaded and in the inflationary scenario there would be no big bang in GR. But subsequently Arvind Borde, Alan Guth and Alex Vilenkin established more general results to show that this is not the case: In GR the big bang singularity is inevitable. The behavior of space-time geometry as one approaches such space-like singularities has drawn considerable attention. By now there is impressive analytical and numerical evidence for the Belinskii-Khalatnikov-Lifshitz conjecture that 'time derivatives dominate over spatial ones' in this limit.

A second direction that is even more significant in terms of global problems was opened up by the positive energy theorems proved by Richard Schoen, Shing-Tung Yau, Edward Witten and others in the late seventies and early eighties. For physical fields in Minkowski space we know that the total energy momentum is causal and future directed. However, the issue becomes less clear-cut once the gravitational field in included because the gravitational potential energy is negative. Could the negative potential energy not overwhelm the positive matter contributions in the strong field and highly non-linear regime of general relativity? Indeed, such concerns were expressed already at GR3. The positive energy theorems cleared up this issue. They established that, so long as the matter sources have a future directed, causal 4-momentum density, the total (Arnowitt-Deser-Misner) 4-momentum, as well as the 4-momentum at any retarded time instant (the Bondi 4-momentum) are also future directed and time-like. Furthermore, these theorems provided a new invariant for asymptotically flat Riemannian manifolds that is of interest in own right to differential geometry. Through this work, Schoen and Yau introduced non-linear geometric analysis —the area that forms the interface of geometry and partial differential equations— to the general relativity community and general relativity to mathematicians. A whole new generation of mathematicians was thus attracted to global problems in general relativity. Using geometric analysis, non-linear stability of Minkowski space and deSitter could be established by Demitrios Christodoulou, Sergio Klainnerman, Helmut Friedrich and others. Finally, as we heard at GR20, the astute combination of geometric analysis and numerical techniques has now led to the surprising result that anti-deSitter space-time is non-linearly *un*stable.

The third area was not even discussed at GR3 but has seen a true explosion of activity since then: black holes physics. Just after GR3, Roy Kerr generalized the Schwarzschild solution and its physics was understood by a systematic analysis of its global structure by Brandon Carter and Penrose. Hawking's introduction of event horizons enabled a precise formulation of the notion of general black holes. A major surprise came through the black hole uniqueness theorems of Werner Israel, Hawking, David Robinson, Pawel Mazur, G. L. Bunting and others. While there is a very large variety of stationary stars, stationary black holes of astrophysical interest are tightly restricted. Furthermore, space-times describing them are *explicitly known*, provided just by the exact solutions of the Kerr family! This result opened the rich field of perturbations of Kerr space-times. Finally, there was the surprising discovery by James Bardeen, Carter and Hawking that there is a close analogy between the laws of black hole mechanics and the laws governing ordinary thermodynamics, and the simultaneous analysis of this interplay through thought experiments by Jacob

Bekenstein. These results brought out the deep and awe inspiring unity between general relativity, statistical mechanics and quantum physics, and helped attract high energy physicists to gravitation. The conceptual issues that opened up continue to lie at the forefront of fundamental physics.

The last area I want to discuss did not even exist at the time of GR3: Numerical relativity. Introduced by DeWitt and Larry Smarr in the early seventies, it has truly blossomed during the last decade. It has provided brand new insights into the full non-linear regime of general relativity. On the conceptual side, we learned that there is unforeseen critical behavior associated with the gravitational collapse, first discovered by Matthew Choptuik. It came with structures that are standard in statistical mechanics but completely new to gravitational physics: scaling behavior, critical indices and universality. On the mathematical side, through the work of Bernd Bruegmann, Frans Pretorius and others, numerical relativity has finally led to a solution of the famous 2-body problem of general relativity. On the astrophysical side, numerical techniques taught us that the gravitational collapse and inspirals are much tamer than what had been assumed till then: there are no spin flips as black holes merge, no novel signatures of non-linear dynamics of the last plunge. But not everything was foreseen by approximation methods. For example numerical simulations revealed that, because of the gravitational wave emission, the final black hole resulting form a coalescence *can* receive a very substantial kick. Numerical results also pressed upon us to better understand dynamical black holes. This led to a new bridge between analytic and numerical methods through the introduction of quasi-local horizons, definitions of mass, angular momentum and multipole moments associated with them and balance laws for these quantities. While the original motivation for this work was to provide invariant tools to extract physics from numerical simulations in the fully non-liner and dynamical regimes, it has led to interesting results in other areas. In geometric analysis, existence, uniqueness and dynamics of marginally trapped surface were investigated, and new inequalities on angular momentum, reported at GR20, were discovered. Quasi-local horizons have also provided physically interesting generalizations of black hole thermodynamics by dropping the assumption of global stationary, and precise mathematical tools that are needed to track the dynamics of black hole evaporation.

Perhaps the most striking aspect of all these developments is that they have brought out —and continue to bring out— the deep underlying currents that connect areas of physics, astronomy and mathematics that had been seen as disparate for decades. There was no inkling of any of these multifaceted interconnections at the time of GR3.

III. GENERAL RELATIVITY AND THE UNIVERSE

A. Cosmology

It is very difficult for younger researchers to fully appreciate *how much* this field has progressed. Because the observational data were so limited, at the time of GR3 the subject was dominated by philosophical considerations and a majority of European –especially British— physicists preferred the steady state scenario over the big bang paradigm. In fact the term big bang was meant to be pejorative. It was coined by Fred Hoyle, the strongest proponent of the steady state model, to poke fun at the idea of a finite beginning in a big explosion! At GR3 there was not a single main talk, and only two seminars, on cosmology proper. One, by Bondi, was on steady state cosmology while the second by A. L. Zelmanov was on anisotropy and inhomogeneity.

During the last several GR conferences, by contrast, theoretical and observational cosmology have been major focal points. This transformation has come about primarily because of the spectacular advances on the observational front, and also because of the influx of ideas from the theoretical high energy physics community. Now that the theory has been repeatedly confronted with observations via completely independent initiatives, the idea that the universe is in a steady state has been abandoned.

Progress on observational front has been simply spectacular. COBE, the Cosmic Background Explorer was launched in 1989; WMAP, the Wilkinson Microwave Anisotropic Probe in 2001; and Planck in 2009. Most of us were amazed by the accuracy of the COBE measurements when the results were first announced. It has been surpassed by leaps and bounds. WMAP reduced the allowed volume of cosmological parameter space by a factor in excess of $30,000$ by, in particular, improving the angular resolution 33 times with respect to COBE. Planck made a similar jump with respect to WMAP. While WMAP had 5 frequency bands between 23GHz to 94GHz, Planck has nine, between 30GHz to 857GHz; Planck has probed much smaller angular scales where the error bars are even smaller. Therefore, in the Planck analysis, the best fit to the six parameter model is obtained using data at these smaller angular scales. Members of the Planck team often point out that, in terms of overall precision, what Planck could do in a year and a half, WMAP would have needed a thousand years of observations and COBE would have needed a million years of operation.

In addition we have ground based facilities, such as ACT, the Atacama Cosmology Telescope in Chile, and SPT, the South Pole Telescope. In addition to these CMB measurements, Type 1a supernovae (which serve as standard candles) and the Baryonic Acoustic Oscillations (which provide the standard ruler in cosmology) have enriched our understanding of the expansion history of the universe and structure formation. It is a striking fact that all these largely independent measurements provide a consistent picture of the early universe and its evolution, compatible with what has now become the six parameter, standard concordance model in cosmology.

It is interesting to contrast what we know today with what the community knew during the decade of GR3. In the 1960s and 70s, we had no clue as to whether the total energy density in the unverse is close to the critical density, or much less, or much more. Many of the leading physicists, astronomers and cosmologists preferred a closed universe —i.e. critical density grater than 1— on aesthetic grounds, e.g., because it 'neatly avoids the issue of creation'. WMAP taught us that the universe is spatially flat to within 0.6% accuracy! In 1964 the Hubble parameter was estimated to be 125km $\mathrm{s}^{-1}\mathrm{Mpc}^{-1}$. In the 1970s there was a lively debate between de Gérard Vaucouleurs (who argued for larger value) and Allan Sandage and G. A. Tammann (who argued for smaller value) as to whether it was 100km $\mathrm{s}^{-1}\mathrm{Mpc}^{-1}$ or 50km $\mathrm{s}^{-1}\mathrm{Mpc}^{-1}$. The first 15.5 months' data from the Planck satellite has revealed [3] that $H_0 = (67 \pm 1.2)\mathrm{kms}^{-1}\mathrm{Mpc}^{-1}$; the 100% uncertainty is reduced to less than 1.8%! The Planck data also tells us that the Cosmic Microwave Background (CMB) temperature today is $(2.7255 \pm 0.0006)^{\circ}$K, i.e., with a 0.02% accuracy. We are confident, to 10σ level, that the power spectrum is *not* exactly scale invariant. The spectral index n_s for scalar (or density) modes is 0.9608 ± 0.0054 (exact scale invariance would mean $n_s = 1$). At GR3, almost everyone thought that 'normal matter' that constitutes the stars and galaxies, photons and neutrinos accounted for the entire matter content of the universe. Today, we know that it contributes negligibly and the primary drivers of the evolution of the large scale structure of the universe in the ΛCDM model are dark matter and the cosmological constant. The Planck data tells us that the normal matter that makes up stars and galaxies contributes just 4.9% of the mass/energy density of the Universe. Dark matter, which has thus far only been detected indirectly by its gravitational influence, makes up 26.8%, and the rest is 'dark energy' best modeled by a positive cosmological constant. The 'standard' paradigm could hardly have shifted more dramatically!

Progress on the theoretical side has also been impressive. First, one can use the 1 part in 10^{-5} homogeneities observed in the CMB to construct the initial data for perturbations on a Friedmann Lemaitre Robertson Walker (FLRW) background, and evolve them using a mixture of analytical and numerical methods. These simulations use general relativity and well established classical physics, astrophysics and gastrophysics. The result in very good agreement with the large scale structure that has been observed through astronomical surveys such as the Sloan Digital Sky Surveys. This is truly impressive because the evolution starts when the universe was about 370,000 years young and end today, over 13 billion years later. In human terms it is like taking a snapshot of a baby when she is a day old and have a reliable biological and biochemical theory to accurately predict what the person would look like when she is a hundred years old!

These computations trace back the origin of the large scale structure to the minute inhomogeneities observed in the CMB. But one can be an order of magnitude more ambitious and ask for the origin of the *CMB fluctuations themselves*. The surprise here is that there do exist candidate theories —such as the simplest single field inflation with a quadratic potential— that rise up to this challenge.[2] The paradigm does involve some assumptions whose validity is still far from being obvious. But the power of the argument is that: i) these assumptions are few and can be stated precisely; and, ii) if the quantum fields representing perturbations were in one of a natural class of 'vacua' just before the onset of the slow roll inflation, then the initial vacuum fluctuations are naturally magnified during inflation to produce precisely the power spectrum we observe in the CMB. Thus, the issue of the origin of the observed large scale structure of the universe is now pushed back all the way to the onset of inflation, i.e., just a million Planck seconds after the big bang of general relativity. Furthermore, it is very striking that the initial perturbations are now reduced just to the *vacuum fluctuations* of quantum fields. In this scenario, at the 'beginning' the universe was as homogeneous and isotropic as it is possible, subject to the Heisenberg uncertainty relations that *must* be obeyed by the quantum fields representing perturbations. But this paradigm is also incomplete. In particular one of the assumptions is that one can trust general relativity and quantum field theory on FLRW space-times all the way to the big bang singularity. At GR20 we also heard about recent advances in loop quantum gravity where the goal is to obtain self consistent Planck scale completions of the leading paradigms of the very early universe.

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[2] As Penrose and others have emphasized, contrary to what is often claimed, inflation does not 'solve' the flatness and horizon problems. For example, given any inflationary potential, one can just give initial data at a suitably late time with significant spatial curvature and simply evolve them back in time to obtain initial conditions at the onset of inflation. When these are evolved forward, obviously one would find that there is significant spatial curvature at late times! The power of the paradigm lies, rather, in its success in accounting for the CMB anisotropies. (It also solves the 'monopole problem' which, however, is no longer considered as significant by cosmologists.)

To summarize, since GR3, there have been *very* significant advances in cosmology especially on the observational front, but also on the theory side, that have dramatically changed our view of the matter content, dynamics and the large scale structure of the universe. What is even more pleasing is that the pace of progress in this area is likely to remain in a high gear for the foreseeable future.

B. Relativistic Astrophysics

GR 3 did not have a single talk in this area because the field did not even exist. Soon thereafter, there was an explosion of interest, again because of an observational breakthrough, the discovery of quasars, and the field came into official existence with the first Texas Symposium on Relativistic Astrophysics, held at Dallas in 1963. GR3 was the first general relativity conference that Chandrasekhar attended and from conversations with him I know that his decision to devote the next quarter of a century to general relativity was strongly influenced by this conference. This is very fortunate for the development of this area because Chandra firmly believed that the natural home of general relativity is in astronomy. Over the next 2-3 decades, his group at Chicago and Kip Thorne's group at Caltech worked tirelessly to develop this direction.

By now, there are many applications of general relativity to astrophysics. The possibility of gravitational lenses due to general relativistic effects was already discovered by Einstein. But he did not think that they would ever be observed. Not only have they been seen but they have now become a standard and *powerful tool* with a wide range of applications, from the search of exoplanets in astronomy, to the analysis of CMB data in cosmology. General relativity also plays a key role in the study of compact astrophysical objects. In particular, isolated neutron stars as well as those in binaries have been cataloged and their formation and dynamics have been topics of active research in relativistic astrophysics. Stability analysis has helped us put constraints on the equation of state of nuclear matter at these high densities. More generally, relativistic effects are conceptually important in the stability analysis of all rapidly rotating stars.

The most sweeping change in this field has occurred in the area of astrophysical black holes. It may come as a surprise to the younger participants at GR20 that, up until the *late seventies*, black holes were not taken seriously in the physics and astronomy communities. A standard view was: 'just because a theory allows certain solutions does not mean than they are actually realized in Nature'. This may seem odd, given that the work by Robert Oppenheimer and Hartland Snyder on black hole formation through gravitational collapse dates back to 1939. But the Oppenheimer-Snyder calculation was carried out using exact spherical symmetry and the collapsing object was modeled by a sphere of dust with uniform density. This appears to have led to a general perception that the calculation did not apply to realistic stars. Indeed, even within the general relativity community a similar view arose immediately after Israel's discovery that a static black hole had to be spherically symmetric. At first, this result was interpreted as implying that black holes would be very rare: The final result of a gravitational collapse can be a black hole *only* if the collapsing star is spherical and most stars are not; they have higher multipoles. It was Penrose who forcefully argued that the higher multipoles would be radiated away and the generic stellar collapse of sufficiently massive stars would in fact lead to black holes. Subsequently, this view was confirmed by perturbative calculations by Richard Price and others, carried out between early 1970s to early 1990s. Generalization of those results to full non-linear general relativity, and the issue of stability of the Kerr family of black holes singled out by the uniqueness theorems, lie at the forefront of research in geometric analysis I referred to in section II.

As more astrophysical phenomena were discovered, by 1990s the tide turned 180° and black holes became ubiquitous in astronomy. While in the 1974 AAS meeting in Tucson, only Thorne's group was using black holes to model Cyg X1, it became a *norm* to assume that smaller black holes of a few solar masses are the engines behind the spectacular outbursts of energy, e.g., in gamma ray bursts and supermassive black holes in active galactic nuclei and quasars. It is now common to suppose that most galaxies have supermassive black holes at their centers and they are dominant players in determining the dynamics of their host galaxies. This relation and the mechanism of formation and growth supermassive black holes continues to be a frontier topic in astronomy. At GR20 we learned that some of these supermassive black holes were formed *very* early, requiring us to revise the scenarios of birth and dynamics of these huge black holes. We also heard about Sgr A*, the ~ 4 million solar mass black hole at the center of our own galaxy. Interestingly, one has been able to deduce its characteristics, particularly the mass, from a careful observation of stars orbiting it, following the proposal John Mitchell made in 1783, in the very first paper on 'black holes' [4]! In the near future we will have interesting information on the accretion process into Sgr A* because there is a unique opportunity to observe a gas cloud of several earth masses as it is devoured by the central black hole.

In summary, the role of general relativity in astrophysics has magnified very significantly since the Texas symposium. Aspects of relativistic gravity now play an essential role in understanding compact astrophysical objects, the most energetic phenomena in the universe, and the interplay between supsermassive black holes and large scale dynamics.

IV. GRAVITATIONAL EXPERIMENTS

At GR3, there were three talks on experimental gravity that I have already alluded to in section I; one by Ginzburg on tests of general relativity, one by Schiff on the gyroscope experiment and the report by Weber on the Maryland gravitational wave antenna. After reviewing the then status of the three solar system tests of general relativity, Ginzburg emphasized: *"There are no clear-cut experimental results or observations which would negate G.R.T [general relativity theory]. There is not even the smallest cloudlet on the horizon ... (of course this refers to macroscopic physics).. It applies equally well even to cosmology where the necessity of some generalization or the other would not cause any particular surprise (the simplest such generalization is the addition of a Λ term ...)".* But he also emphasized that there were ten other relativistic gravity theories and so it is important to carry out tests. Ginzburg and Schiff were prescient in emphasizing that satellites will play an important role in these tests, particularly that of the Lense-Thirring effect. Apparently, Einstein had said to Thirring that it is unfortunate that the effect is so small for the moon and he wished the moon were closer. Schiff pointed out that we now have many moons (i.e. satellites) that *are* closer! In his conference summary, Bergmann recalled Einstein's sentiment that because the foundation of the theory lies in the equivalence principle, testing it more accurately was more important than the solar system tests. He looked forward to the significant improvement of the Eötvös experiments that were expected at that time from Robert Dicke's experiment at Princeton.

There was a flurry of activity in the mid-eighties when a reanalysis of the Eötvös experiment suggested that here may be violation of the equivalence principle indicating a novel fifth force operating at the scale of 10^4cm. But the numerous new experiments that were performed soon ruled out this possibility. By now the equivalence principle has been tested to one part in 10^{14}! There have been impressive tests of Newton's inverse square law down to 10^{-1}cm. The local Lorentz invariance underlying general relativity has been tested to few parts in 10^{22}. Over half a dozen post-Newtonian parameters that distinguish general relativity from other gravity theories have been measured and agreement with general relativity has been established in the range of one part in 10^3 to one part in 10^{20}. With respect to the important post Newtonian parameters β and γ, there has been an improvement by a factor of 100 over the last 40 years. These tests have brought out the fact that it is *extremely* difficult to non-trivially modify or augment general relativity.

The evidence for validity of general relativity from gravitational phenomena outside the solar system is in many ways even more impressive. The most celebrated among these observations is the continued monitoring of the Hulse-Taylor pulsar PSR1913+16 where general relativity has been confirmed to an accuracy of two parts in 10^4. In 2004, a double pulsar PSR J0737-3039 A/B was discovered. In addition to being a binary in which pulses from both neutron stars are received on earth, is also the most relativistic binary pulsar observed so far. It has already provided accurate measurements of masses —$m_{\rm A} = 1.339\,{\rm M}_\odot$ and $m_B = 1.250\,{\rm M}_\odot$ with a 0.2% accuracy— as well as six post-Keplerian parameters. They have also enabled the first quantitative measurement of general relativistic spin precession. An interesting development is the *Einstein at Home* project in which systematic searches for pulsars are carried out using the immense computational power made available by some 40,000 volunteers who have offered the use of some 200,000 personal computers during periods in which they are idle. This initiative has led to the discovery of six binary pulsars, which has been credited to the *"citizen scientists"* who made it possible through their generosity.

Finally, there is a revolution waiting in the wings that will ultimately prove to be more important than all these tests: Gravitational waves. A global network of detectors is being set up. The LIGO detectors in the US and the Virgo detector in Europe are undergoing transitions to the 'advanced stage' when the sensitivity would be sufficient for observation of several events a year.[3] By the end of this decade they will be joined by the Kagra cryogenic detector in Japan. The LIGO-India observatory will be at a sufficiently different latitude from the first four to significantly enhance the source localization and polarization measurement capability of the global network. Technological advances and the ingenuity of experimentalists have been mind boggling. Whereas at GR3, Weber spoke of measuring root mean square displacements of 10^{-14}cm at ends of an antenna that was 2 meters long, LIGO is capable of measuring displacements of 10^{-16}cm above the noise floor level, between mirrors that are separated by a distance of 4 km! The global computational power devoted to these searches is also impressive. Finally, since gravitational waves interact *so* weakly with everything, they should bring to us faithful signatures of the cataclysmic events all the way from the edge of the observable universe. It is widely expected that this new window

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[3] It is interesting to note that in a discussion session at GR3, Bergmann prefaced a discussion of a theoretical issue by saying *"it is perfectly safe to argue the point on theoretical grounds because Weber is a comfortable number of orders of magnitude away from deciding this question experimentally; and we will probably all be dead by the time the decision is in."* There is every expectation that gravitational waves will finally be seen in this decade.

on the universe will have a transformative effect on astronomy. Indeed even now the upper limits on gravitational waves set by the LIGO-VIRGO collaboration have provided novel insights. For example, the *absence* of any gravitational wave signal at the sensitivity that these detectors had already achieved tells us that there is no mountain on the crab pulsar that is higher than a meter!

While the advances on the experimental front have been significant, so far they have not been as dramatic as those in other areas I have discussed. Nonetheless, it *is* quite astonishing that not only has general relativity withstood such a wide variety of observational tests but, for almost a century now, it has been impossible to modify it and come up with interesting, viable alternatives. Furthermore, the theory predicts that a time changing quadrupole is accompanied by ripples in space-time curvature. It would be surprising if the news they will bring from the far corners of the cosmos does not deepen our understanding of the structure and dynamics of astrophysical systems in unforeseen ways.

V. QUANTUM ASPECTS

At GR3, there were four main talks on quantum aspects of gravity by DeWitt, Feynman, Madelstam, and Lichnerowicz, and well over half a dozen seminars were motivated by these issues. In section I, I mentioned the first three talks. In the fourth, Lichnerowicz discussed his proofs of the existence and uniqueness of various Green's functions on globally hyperbolic, curved space-times, notably the commutator Green's function that provides the point of departure for quantum fields in curved space-times. He covered not only the simplest case of scalar fields, but also spinor fields and the linearized gravitational field. It is impressive to see that the subject was already so advanced. However, he also thought that one could similarly introduce a canonical notion of positive and negative frequency decomposition in a general globally hyperbolic space-time. Work by Parker and others soon proved that this possibility cannot be realized: in a time dependent space-time, there is no canonical vacuum and no objective notion of particles. This understanding subsequently led to the algebraic approach which we heard about at GR20. By now the theory is mature. From a conceptual and mathematical viewpoint, it is at the same level as interacting quantum field theory in flat space-time.

The most significant development in this area is of course Hawking's discovery that, in the external field approximation, black holes radiate quantum mechanically and at late times the radiation is well modeled by a black body. This discovery provided a *physical* basis to black hole thermodynamics discussed in section II. Key researchers in the field had expressed the view that the similarity between the laws of black hole mechanics and thermodynamics investigated by Bekenstein was just a nice coincidence. The discovery of the black hole radiance shifted the entire paradigm, providing a fresh perspective and raising new questions at the interface of general relativity and quantum physics. As we saw in GR20, the ensuing issues are still at the forefront of current research.

The second striking application of quantum field theory in curved space-times is to the early universe. In this epoch, space-time appears to be extremely well approximated by a homogeneous isotropic (FLRW) solution to Einstein's equations, with tiny perturbations. These are best represented by certain quantum fields propagating on the FLRW background. As we saw in section III A, by modeling perturbations as 'vacuum fluctuations' of these quantum fields at the onset of inflation and then evolving them using quantum field theory in curved space-times, one can reproduce the inhomogeneities observed in the CMB. In this sense, cosmology of the early universe has already provided an observational confirmation of quantum effects gravity, at the level of linear perturbations.

At GR3, the emphasis was on full quantum gravity, beyond the external field approximation. On the conceptual side, Léon Rosenfield argued that one could leave the gravitational field classical while Frederic Balinfante pointed out that it would be inconsistent to have a fundamental theory in which a classical metric is coupled to the expectation value of the matter stress energy tensor. DeWitt and Feynman discussed perturbative quantum gravity. There was an interesting exchange between the two. DeWitt expressed the hope that a finite number of counter terms could suffice and quantum gravity would be perturbatively renormalizable *if* one used dressed propagators and not bare ones. Feynman said that he sees that there is a finite number of counter terms at the one graviton loop level, but he did not see what happens when there are two or more loops: *"As far as I can see the gravitational theory is not renormalizable in the usual sense of the term."* Detailed calculations in the mid-eighties by Goroff and Sagnotti showed that Feynman's view was correct: Perturbative general relativity off Minkowski space-time fails to be renormalizable at the two loop level. But at GR3 there was also considerable discussion of non-perturbative quantum gravity, in the canonical framework by Bergmann and others and through path dependent observables by Madelstam. Also, DeWitt pointed out that topology change is possible in non-perturbative quantum theory while David Finkelstein suggested that elementary particles may be regarded as bound states with non-trivial topologies, the geons.

Interestingly, it is possible to trace back most of the main directions of contemporary research in quantum gravity to GR3. The Feynman-DeWitt perturbative approach naturally led to supergravity and culminated in perturbative string theory. There has been a resurgence of interest in supergravity over the last decade because of the discovery that the 4-dimensional, $\mathcal{N} = 8$ maximal supergravity theory is *much* better behaved in perturbation theory than was expected. This realization has even led to the conjecture that the theory may be perturbatively *finite*. In another development, non-renormalizability of perturbative quantum general relativity led Steven Weinberg to suggest that perhaps general relativity may be asymptotically safe, i.e., may admit a *non-Gaussian* fixed point. The asymptotic safety program is now being actively pursued and impressive evidence in its support has steadily accumulated over the past decade. The broad framework of asymptotic safety also underlies the 'causal dynamical triangulation' approach. Finally, Loop quantum gravity can be regarded as the culmination of both sets of non-perturbative ideas discussed at GR3: the Dirac-Bergmann-Wheeler canonical quantization program and Madelstam's approach in which path dependent, Wilson-lines type observables are at the forefront.

Over the past two decades there have been significant developments in this area, particularly in string theory and loop quantum gravity. Even though we are still far from a complete theory in either approach, both avenues have led to concrete advances by removing several of the conceptual and mathematical road-blocs the field faced in 1980s and 1990s. It is quite striking that, in both approaches, the fundamental building blocks of space-time are very different from what a simple minded extrapolation from field theories in Minkowski space-time would suggest. Furthermore these fundamental excitations appear to be 1-dimensional, polymer-like. But beyond these basic similarities, the two paths diverge. In string theory, the original goal was to achieve *unification* of all forces of nature. While the scope of the theory has become more diffuse over the years, the unification theme has had a strong influence on its underlying structures. By contrast, in loop quantum gravity one focuses on the fact that gravity and space-time geometry are intimately intertwined. Consequently, *quantum geometry* effects underlie all the major developments in this approach to quantum gravity. Over the past decade, the paths have diverged even more significantly. In string theory, because of the tremendous success of the ADS/CFT conjecture, the emphasis has been on using parts of gravity theory that we understand well to explore properties of strongly coupled systems in *non-gravitational* areas of physics: properties of the quark gluon plasma, issues in fluid mechanics, problems in condensed matter physics, particularly various aspects of superconductivity, ... On the other hand, in loop quantum gravity, the focus has been on meeting the challenges of quantum gravity proper that have been with us for decades: the resolution of space-time singularities of classical general relativity, extensions of early universe scenarios to the Planck regime, introduction of n point functions and development of a scattering theory in a background independent context, ...

In terms of motivation, quantum gravity programs have a strong similarity with general relativity. Einstein's main goal was to reconcile two fundamental and successful theories; Newtonian gravity and special relativity. He was disturbed by the deep tension between their underlying principles and convinced that the apparent conflict arose because they were special cases of a deeper and grander theory. He was not trying to modify Newtonian gravity to explain any observational discrepancy such as the difference between the calculated and observed values of the perihelion of mercury. Work in non-perturbative quantum gravity is driven by the same spirit: The goal is to find the grander, deeper theory from which general relativity and quantum field theory arise as special cases. But because there is neither observational data to guide us nor, alas!, a second Einstein to leap over this profound limitation, we have not seen definitive paradigm shifts. We do see solutions to some of the important problems but they have come in pieces; a compelling global picture has not emerged. Perhaps the lasting legacy of efforts to date will be that they have provided a host of novel and powerful mathematical tools, and sufficiently sharpened the conceptual issues, to bring out deep tensions between gravity and the quantum that we were blissfully unaware of. The new and incisive questions that have arisen are likely to be our best guides in the coming years.

VI. EPILOGUE

I have presented only an illustrative sample of advances our field has made since GR3. Clearly, there is a lot to be proud of. As we stand at this threshold of the Centennial of general relativity, is there a simple phrase that succinctly encapsulates our collective sentiment? I think there is. Not surprisingly, it comes from Einstein himself.

Einstein presented his calculation of the perihelion advance of mercury to the Prussian Academy on November 18th, 1915. Just ten days later, he wrote to Arnold Sommerfeld in Munich saying: "During the last month, I experienced one of the most exciting and most exacting times of my life and true enough also one of the most successful ...". He then went on to explain " Now the marvelous thing which I experienced was the fact that not only did Newton's theory result as first approximation but also the perihelion of mercury ($43''$ per century) as second approximation ...". Apparently, Sommerfeld was puzzled by this uncharacteristic enthusiasm of Einstein's. So, on February 8th, 1916 Einstein wrote back saying

"Of general theory of relativity, you will be convinced once you have studied it.

> *Therefore I am not going to defend it with a single word."*

A century has passed and yet this assessment continues to capture our core reaction to general relativity.

But it is equally interesting that fundamental issues still remain *even in the classical theory*. For example, we still do not have a satisfactory notion of a *dynamical black-hole*, one that is not teleological, one that can be used to say with confidence that there is no black hole in the room you are now sitting in. The notion of an event horizon allows this possibility because event horizons can form and grow even in regions where the space-time metric is flat![4] Similarly, there is a fundamental conceptual question about gravitational waves. Observationally, the universe seems to have a *non-zero*, positive cosmological constant, Λ. But we do not have a satisfactory theory of gravitational radiation if $\Lambda > 0$. In particular, we do not know the analog of the 'Bondi news'. Even today, 50 years after GR3, there is no reliable framework for us to echo Bondi and say with confidence: *Yes, there is a gauge invariant characterization of gravitational waves in full general relativity with a positive cosmological constant, and they carry positive energy.* We do not even know a precise boundary condition that would correctly capture the idea that there is no incoming radiation in space-times describing isolated systems. Nor do we have the analog of the beautiful positive energy theorems if $\Lambda > 0$.

And of course *many* fundamental issues at the interface of general relativity and quantum physics continue to be hotly debated: the issue of information loss, the fate of classical singularities in the quantum theory, physics of the very early universe in the Planck epoch, the initial conditions and measurement theory in cosmology, ...

These fundamental issues and deep tensions offer great opportunities for the future. So do the challenges of testing general relativity in the truly strong field regime and the tremendous potential of gravitational wave astronomy. Thanks to all these opportunities, the field of gravitational science is becoming ever more fertile. As we approach the centennial of general relativity, a transformation has already been set in motion, one that will take us well beyond Einstein's vision. Research in the type of analytical general relativity that dominated GR3 has been receding. The field has moved into new areas: geometric analysis, cosmology, relativistic astrophysics, computational science, high energy physics, gravitational wave astronomy and particle astrophysics. In future, the emphasis will be on using relativistic gravity to provide us a more holistic view of the cosmos (see e.g. [6]). As this transformation unfolds, there will be numerous fresh insights, unforeseen advances, new puzzles and even paradigm shifts. In another half a century, the GR37 conference will be held (perhaps again in Warsaw!). It will surely be at least as engaging and stimulating a conference as GR3 and GR20 have been. But it will be a *very* different one!

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[4] For further elucidation of limitations of this notion, see. e.g., [5]. Finding a satisfactory characterization of dynamical black holes continues to be an active topic of research. For example, at GR20 we heard of the interesting idea of 'the core of a black hole'.

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