MATTERS OF GRAVITY

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Editorial

The newsletter strides on. I had to perform some pushing around and arm-twisting to get articles for this number. I wish to remind everyone that suggestions and ideas for contributions are **especially** welcome. The newsletter is growing rather weak on the theoretical side. Keep those suggestions coming!

I put together this newsletter mostly on a palmtop computer while travelling, with some contributions arriving the very day of publication (on which, to complicate matters, I was giving a talk at a conference, my email reader crashed and our network went down). One of the contributions is a bit longer than the usual format. Again it is my fault for failing to warn the author in due time. I apologize for this and other errors.

The next newsletter is due February 1st. Many thanks to the authors and the correspondents who made this issue possible.

If everything goes well this newsletter should be available in the gr-qc Los Alamos bulletin board under number gr-qc/yymmnnn. To retrieve it send email to gr-qc@xxx.lanl.gov (or gr-qc@babbage.sissa.it in Europe) with Subject: get yymmnnn (issue 2 is available as 9309003 and issue 3 as 9402002). All issues are available as postscript files in the WWW http://vishnu.nirvana.phys.psu.edu/ Or email me. Have fun.

Jorge Pullin

Correspondents

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Report on the APS Topical Group in Gravitation

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As most of you already know, a petition to form a Topical Group in Gravitation (TGG) in the Americal Physical Society (APS) has been circulating since early February. The objective of the TGG is to provide a distinct focus within the APS for research in gravitational physics. The existence of major projects such as LIGO makes the formation of such an entity especially important now. At this moment, we have 165 of the 200 signatures required to form the TGG. The final effort to reach the goal is now underway. I urge any of you who are APS members and have not yet signed the petition to please do so now. (A copy accompanies this note.)

The next stage in the process will be the formation of an *ad hoc* organizing committee (AHOC) to draw up bylaws and to serve as a nominating committee for officers of the TGG (as required by APS). Several of you have already volunteered for the AHOC. I welcome any others who are interested.

Please stay tuned for future developments.

PETITION TO THE COUNCIL OF THE AMERICAN PHYSICAL SOCIETY

We, the undersigned members of the American Physical Society, petition the Council of the American Physical Society to establish a Topical Group in Gravitation. Areas of interest to the proposed Topical Group include, but are not limited to, experiments and observations related to the detection and interpretation of gravitational waves, experimental tests of gravitational theories, computational general relativity, relativistic astrophysics, solutions to Einstein's equations and their properties, alternative theories of gravity, classical and quantum cosmology, and quantum gravity. The purpose of the Topical Group is to provide a unified forum for these areas of current research which now span several Divisions of the Society.

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Gravitational Microlensing and the Search for Dark Matter
A Personal View

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Gravitational microlensing is a phenomenon based on the General Theory of Relativity: any mass concentration, a black hole, a star, a brown dwarf or a planet, distorts space time and changes the direction of the light rays, acting like a lens. The prediction that stars observed near the eclipsed solar disk should be displaced by almost 2 arc-seconds was verified observationally be the 1919 expedition headed by Sir A. Eddington.

In the following decades the same theoretical discovery was made independently every few years, by prominent physicists and astrophysicists as well as by little known waiter from Brooklyn: if the observer and any two stars in our own galaxy are well aligned then two images of the more distant star will be formed by the gravitational field of the closer star acting as a lens. One can make an estimate that the alignement should be better than one part in 10^8, i.e. the angular separation between the two stars should be less than a milli arc second. And the problem is that there are no telescopes capable of resolving such small angles. The other problem: such a precise alignement is very improbable.

Eight years ago I made pretty much the same discovery for the tenth time (Paczyński 1986). The two independent referees pointed out correctly that there was nothing original in my paper, it was just a compilation of fragments of various ideas. Somehow I managed to persuade the Editor to accept the paper. As it turned out we were all wrong. There was one very new and important ingredient in my paper, the magic words: "dark matter". Without realizing it I bridged the inter-disciplinary gap. A physicist from the nearby Jadwin Hall at Princeton, Dave Bennet, became a frequent visitor to my office at Peyton Hall. He was very seriously interested in the problem of dark matter.

The problem existed for about half a century. There was evidence from the dynamics of galaxies and their clusters that there is plenty of matter out there that cannot be accounted for with the stars that shine (Hammond 1994). Was the dark matter made of elementary particles with a mass of 10^{-5} eV, or supermassive black holes of 10^6 $M_\odot$ (one million solar masses) each, was anybody's guess. Dave Bennett was one of the many who were eager to solve the mystery. My paper proposed a specific way of conducting the search: all it takes is to monitor the brightness of a few million stars for a few years. Whenever any object more massive than planet Earth moves in front of one of those stars a double image forms. Even though the two images would be seen as one, their total brightness would change in a predictable way. For the lens as massive as the sun the characteristic time scale for the so called microlensing event is a month or two. And other things being equal the time scale is proportional to the square root of mass of the lensing object. And millions of stars are needed because the expected rate of events is very small.

The principle was fine, but it was considered a science fiction by me as well as all other astronomers, with a possible exception of Ken Freeman from Australia. Dave Bennet was not an astronomer and he did not know that there are all those variable stars that would form a background of "noise" for the vary rare microlensing events. He did not know that the atmospheric seeing is blurring stellar images in a different way every night. He did not know many other things. The bottom line was: he did not know the project could not be done, and so he tried to persuade me, a seasoned astronomer, that the project was feasible. Well, I knew better, and so he failed.

But Dave Bennet was a stubborn fellow. He went off to California and tried to persuade Charles Alcock, who was a physicist as well as an astronomer. And the interdisciplinary gap was bridged for the second time. Charles Alcock gave a colloquium in Berkeley, outlining the project: the search for dark matter by means of monitoring the brightness of a few million stars in the nearby galaxy, the Large Magellanic Cloud. The response was enthusiastic. So much so that Jim Rich, visiting from Sacley, France, immediately called his collaborators and adviced them to start their own experiment. And so the first two microlens searches were founded: the American-Australian collaboration of 18 participants, called MACHO (MAssive Compact Halo Objects), and the French collaboration of 28 participants, called EROS (Expérience de Recherche d'Objects Sombres). Both teams had some astronomers among them but they were really dominated by the particle physicists.

I did not know about all these developments at the time. I was dreaming of having a personal 1 meter telescope. First I thought that funds were the limiting factor. Pretty soon I realized that even though I had no funds there was another, much more limiting factor: I had no instrumental skills. But I was lucky. A friend pointed out to me that there was a truly outstanding instrumentalist at the Warsaw University Observatory, Andrzej Udalski. Being well connected to Warsaw (I lived there till 1981) I contacted Andrzej Udalski, and pretty soon we were dreaming together. Now all we needed were the funds, and naturally a good observing site. I can no longer recall how the Las Campanas Observatory (operated by the Carnegie Institution of Washington) came about. Perhaps it was the presence of our old friend from Warsaw there: Wojtek Krzeminski worked for Carnegie since 1982. He lived in Chile, and he knew everything about Chile. One way or another, one day I got a phone call from George Preston from Pasadena, California, the headquarters of the Carnegie Observatories. George Preston proposed some form of collaboration between Carnegie, Warsaw and Princeton. I think it was rather vague initially, but pretty soon two somewhat independent, somewhat related projects took off.

One project was a long term and fairly ambitious. The idea was that the Warsaw University Observatory was to build their own 1-meter class telescope at the Las Campanas site. The telescope was to be dedicated to long term massive photometric searches of variable stars of all kinds, among them the microlensing events. Carnegie would contribute the site, Princeton - the operating expenses. The telescope would be managed from Warsaw. Marcin Kubiak, the director of the Warsaw University Observatory begun the fund raising effort. It did no harm to have a friend astronomer Robert Glebocki as the Minister of

Higher Education in the first post-communist government in Poland. But the telescope could be operating no sooner than 1995.

In the mean time another, more modest project took shape. A bunch of 8 astronomers from Warsaw, Pasadena and Princeton applied for about 60 nights in the 1992 season on the Swope 1-meter telescope, the oldest one at Las Campanas. We were to monitor about 1 million stars in the galactic bulge, close to the galactic center. The idea was to check if we could detect microlensing events where they were guaranteed to be present, as there are many ordinary stars in the galactic disk, between us and the million stars located at the galactic bulge. As a by product we were going to find thousands of variable stars, a real treasure for us, as our team was all made of professional variable stars observers. It is worth pointing out that the same variable stars were mostly noise from the point of view of particle physicists in their quest for dark matter. We were lucky: we got between 70 and 80 nights on the telescope for each of the three observing seasons: 1992, 93, 94, and we have a good chance to be awarded a similar amount in 1995, at which time the transition will take place to the new instrument, which grew up to become a 1.3 meter telescope.

What we needed now was the name - so we could be recognizable as a team. Our first clumsy attempts in a search for a good acronym made George Preston go non-linear in outrage. He was an old friend of mine - I worked for him in 1962/63 as an observer at Lick Observatory in California, and he is a man of many talents. In just one afternoon he created a list of about 10 acronyms, all great, and we e-mailed the list to Warsaw - let them decide which should be our name. And so the OGLE (Optical Gravitational Lensing Experiment) came to be.

The next major event was the early morning phone call from Charles Alcock. It was September 1993, and I was in Warsaw for a few days. I was told the MACHO had their first microlensing event, or the candidate event as it was cautiously named. That was fascinating! I was very excited. It was good, as my 1986 idea was sound. It was not so good as OGLE had no event yet. It turned out that MACHO had their event for some weeks, but they were waiting for more data before going public. However, they were notified by Jim Rich that EROS had two events to be announced at something that sounded to me like a meeting of underground physics in Italy. One way or another MACHO decided to make their announcement at the same meeting. Unfortunately, OGLE had nothing to announce, and even if we had we were in no way connected to the underground physics.

Within a week of my return to Princeton I got a phone call from Marcin Kubiak in Warsaw: the first OGLE event has been found! By that time the media was full of excitement about the discovery of dark matter. Unfortunately we were one week too late, and we missed the boat. We also knew that MACHO and EROS submitted their announcement papers to Nature which is well known for a very short publication cycle. There was no way we could make it for the same issue. On top of that I had very bad personal relations with Nature, and this would not help with a speedy publication. Fortunately, Marcin Kubiak happened to be the editor of Acta Astronomica, a quarterly journal. Normally, to wait 3 months for a publication would be of no use in this high speed month of October 1993. By some incredible twist of luck the last issue had been just sent to the publisher. Kubiak called and asked: "can you hold printing for two days?". The publisher agreed, and the discovery paper was written and recycled a few times between all the authors scattered on two or three continents, all in 48 hours - the marvels of computer networking. As it turned out the issue of Acta Astronomica with the OGLE paper arrived to some libraries a few days ahead of Nature with the papers of the two competing groups.

This irritated some. I got angry e-mails from a few EROS participants: why did we not quote their work presented in Italy at a conference? My answer was simple. We have never received any information from EROS directly, no preprint, no e-mail. The only tangible evidence of their discovery we had from the article in the Time magazine. I wanted to quote that article in our Acta Astronomica paper, but I was outvoted by all other OGLE members - they felt quoting Time would be too frivolous. I am still sorry we did not do it. When the issue of Nature finally arrived I could read the two papers. I discovered that the very existence of OGLE was not even mentioned in the EROS paper, That made me feel better about not quoting Time about EROS. In any case, some good came from the incident. Our purely astronomical team has been noticed by the mostly elementary particle team.

There were more surprises as time went by. Many people, including myself, took it for granted that while the OGLE event was most likely caused by an ordinary star somewhere in the galactic disk, the MACHO and the EROS events were caused by brown dwarfs, which presumably made up the dark matter. Not everybody agreed. Some, including Jeremiah Ostriker at Princeton, were pointing out that the time scales of all events were about the same, a few weeks, hence they may all be caused by the same type of objects, perhaps just ordinary stars located somewhere between us and the Magellanic Clouds. The confusion grew. At the same time OGLE kept finding new events towards the galactic bulge. We had a feeling there were too many of them. When I was again in Warsaw in January 1994 a student, Marcin Kiraga, pointed out to me, that even though the geometric location of the galactic disk stars makes them more efficient for gravitational lensing, the shear number of the galactic bulge stars more than outweights their geometrical disadvantage. We quickly wrote a joint paper together presenting quantitative evidence that the majority of the bulge events are most likely due to other galactic bulge stars acting as lenses (Kiraga and Paczyński 1994).

No new official reports were available from the MACHO and EROS teams, but there was a general feeling that the rate of events towards the Magellanic Clouds was much too low to account for all the dark matter. Perhaps those few events could really be accounted for by the ordinary stars in our galaxy? In the spring of 1994, back in Princeton, I received two papers on the same day. One was published in the Astrophysical Journal, and its title was: "Microlensing Events: Thin Disk, Thick Disk, or Halo?" (Gould et al. 1994). The second was a paper from Nature to referee. An unknown post-doc Kailash Sahu (1994) (from Instituto de Astrofisica de Canarias at Tenerife, Canary Islands) pointed out that the lenses towards the Magellanic Cloud might be the ordinary stars in the Magellanic Cloud itself. I was struck with the simplicity of the idea: it was basically the same as Kiraga's, just applied to the Magellanic Cloud rather than the galactic bulge. Of course the two men came up with their insights independently. They also shared their status - both were unknown outsiders. All the prominent people working in the field missed their point. I missed it twice.

By June 1994 I was at the Institut d'Astrophysique in Paris, and OGLE has come up with their first estimate of the so called optical depth to gravitational microlensing towards the galactic bulge stars based on 9 events found in a systematic search in the 1992 and 1993 data (Udalski et al. 1994a,b). The optical depth is a number proportional to the probability of finding the lensing events. Theoretically it was supposed to be between 0.5 and 1.0 parts in a million, it turned out to be 3.3 ± 1.2. MACHO also came up with a similar estimate based on their 4 events (Alcock et al. 1994). This was a major surprise: what was going on? My tentative speculation is that the excess may be accounted for if the galactic bulge is in fact a galactic bar, with its long axis pointing more or less towards us: the same number of stars can act as lenses much more efficiently (Paczyński et al. 1994). About 50% of all spiral galaxies are known to have bars at their centers, and de Vaucoulers (1964) presented compelling evidence for the bar in our own galaxy. Now this is a common knowledge among the few specialists, but it is practically unknown to general astronomers or physicists. So, in 1986 I rediscovered gravitational microlensing, and in 1994 the OGLE search has rediscovered the galactic bar. I seem to be valways a few decades late.

As for the dark matter - it is still a mystery. There is no doubt that the recent experiments have demonstrated that the technology works: the monitoring of millions of stars for a few years, and the detection of the few microlening events has been done. Even the detection of the first double lens has been reported (Udalski et al. 1994c). The results are already useful for the studies of galactic structure. Perhaps in the future the same technique will lead to the discovery of dark matter, but we have no clear evidence that this has already happened.

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From the beginning of 1994 season the OGLE project is capable of near real time data processing (Paczyński 1994). The new computer system automatically signals the events while they are on the rise, making it possible to carry out photometric and/or spectroscopic follow-up observations. The observers who would like to be notified about the on-going events should send their request to A. Udalski (udalski@sirius.astrouw.edu.pl).

The photometry of the OGLE microlensing events, their finding charts, the updated OGLE status report, including more information about the "early warning system", can be found over Internet at "sirius.astrouw.edu.pl" host (148.81.8.1), using the "anonymous ftp" service (directory "ogle", files "README", "ogle.status", "early.warning"). The file "ogle.status" contains the latest news and references to all OGLE related papers. PostScript files of some papers, including Udalski et al. (1994b), are also available. The

OGLE results are also available over "World Wide Web": "http://www.astrouw.edu.pl/".

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Laboratory Gravity: The G Mystery, and Intrinsic Spin Experiments.

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• **Disconcerting G News:** The accepted "CODATA" value for G, based on the 1982 measurement of Luther and Towler [1], carries an uncertainty of 128 ppm. In the last newsletter, I reported a startling result from the PTB (German NIST equivalent) group: G a whopping **0.6% higher** than CODATA. This was officially announced at the NIST-sponsored "CPEM" conference in Boulder this summer [2]. At the same conference the Wuppertal group [3] reported a G consistent with CODATA, but the Measurement Standards Laboratory of New Zealand [4] presented a G **0.1% lower** than CODATA!?!!

Not at CPEM, but just published in English translation [5], are the results of a Russian group that finds inexplicable temporal variations at a level of about 0.7% in their measured G (for mass separations about 18 cm; for mass separations about 6 cm the variations appear smaller). The Russian group is thoroughly puzzled, and seriously considers the possibility of a real time variation in an effective G.

Meanwhile, Luther with C. Bagley are developing an instrument that will use and compare both of the two customary approaches to the use of a torsion balance for determining G: measurement of equilibrium angle, and of torsional period. This will address speculation that the period measurement approach might have undiscovered systematic effects linked to torsion fiber properties.

The following table summarizes the disconcerting recent results:

Source	Method	$G \cdot 10^{11}$	Assigned uncertainty(ppm)	$\mathbf{G - G}_{CODATA}$ ppm	standard deviations
CODATA value[1]	fiber torsion balance, period	6.6726	128	$\equiv 0$	$\equiv 0$
PTB[2]	mercury supported torsion balance, static	6.7154	83	+ 6400	+ 42.0
Wuppertal[3]	dual pendulums, static	6.6719	117	- 105	- 0.6
New Zealand[4]	fiber torsion balance, static	6.6656	95	- 1050	- 6.6
Russia[5]					
@18 cm	fiber torsion balance, period	6.6655	(0.7% temporal variation)	- 1060	
@ 6 cm		6.6771		+ 670	

• **Intrinsic Spin Experiments** The possibility of a special dependence of the gravitational interaction on intrinsic spin has provoked experimentalists for many years. Several new results put greatly improved limits on such behavior.

Spin-spin interactions An elegant new experiment by Wei-Tou Ni and collaborators [6] puts a convincing limit $(1.2 \pm 2.0) \times 10^{-14}$ on the ratio of the strength of an anomalous electron spin-spin interaction (of the same form as the magnetic interaction) to that of the magnetic interaction. Some background is required here:

Early tests for such interactions used torsion balances [7,8]. Two Russian experiments [9,10] applied an ingenious and potentially far more sensitive technique, based on the idea that an anomalous interaction from a spin-polarized source should penetrate a superconducting magnetic shield and induce a spin polarization in a shielded ferromagnetic sample. The induced polarization would produce a magnetic flux, to be sensed by a SQUID. But there is a serious concern here, raised by Ritter [7]: might the ferromagnetic domains be too "sticky" to respond to the extremely small aligning torques associated with the sought-for anomalous interaction? This concern is avoided in Ni's experiment, which uses the basic technique pioneered by the Russian groups, but substitutes a high susceptibility **paramagnetic** sample for a ferromagnetic one.

$\sigma \cdot \hat{r}$ **(monopole-dipole) interactions** Does the gravitational interaction of a polarized particle depend on whether its spin points away from or towards the source of the force? For ranges greater than about one meter, the best limits on this (for nucleons) come from recent NMR experiments [11,12]. Venema et al. conclude (invoking a minimal bit of nuclear modeling) that the energy difference between a spin-up and spin-down neutron in the Earth's gravitational field is less than 2.1×10^{-20} eV (compared to its total gravitational energy of about 0.7 eV). Limits on the fractional contribution of spin to total energy get rapidly weaker as shorter interaction ranges are assumed; below 1 meter the best limits (for electrons) have come from torsion balance experiments of Ni et al. [13] and Ritter et al. [14], giving comparable limits which may be expressed: the spin-dependent part of the force on an electron about 4 cm from an attracting mass is less than about 5000 times the total gravitational force due to that mass.

But the same clever trick that the Russian groups and Ni have applied to spin-spin interaction searches has now been applied by Ni and collaborators to search for an electron $\sigma \cdot \hat{r}$ interaction: a paramagnetic sample within a superconducting shield is examined for induced magnetism as an unpolarized mass is rotated around the outside of the shield. Results, announced by Ni at MG7 this summer, improve limits on such an interaction for ranges between a few centimeters and one meter by nearly two orders of magnitude.

Does intrinsic spin recognize an anisotropy in space? A recent torsion balance experiment by Wang, Ni, and Pang [15] puts a limit of 3.5×10^{-18} eV on the energy splitting of the spin states of an electron relative to the spacial directions explored as the earth turns. A similar experiment, aimed especially at possible effects associated with galactic dark matter, is being conducted by Ritter [16].

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[2] W. Michaelis, H. Haars, and R. Augustin*, Physikalisch- Technische Bundesanstalt (PTB), Postfach 3345, 38023, Braunschweig, Germany. FAX 49531 5924015

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[4] Mark Fitzgerald and Tim Armstrong*, Measurement Standards Laboratory of New Zealand. E-mail: t.armstrong@irl.cri.nz

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* Results 2,3,4 were presented at the Conference on Precision Electromagnetic Measurements (CPEM), Boulder Colorado, 27 June - 1 July, 1994. Result 3 was presented also at MG7 at Stanford; the value for G quoted here was that given at MG7, where I have added formal and systematic error estimates in quadrature.

LIGO Project update

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A major step for LIGO took place this spring when construction of the first of the two 4-km LIGO facilities began at Hanford, Washington. The official ground-breaking ceremony was held on July 6, against a backdrop of earthmoving equipment already at work leveling the site and preparing the soil for the foundation. This inital earthwork is expected to be completed later this fall.

At the other LIGO site in Livingston, louisiana, the purchase of the land by Louisiana State University (LSU) and a lease transferring the site to NSF are in the final signature stage. With the cooperation of the original landowner and LSU, we were able to arrange for the clearing of the site, begin the on-site geotechnical investigations, and apply for the necessary wetlands permit from the Army Corps of Engineers, in advance of the final land transfer. Progress on these items should help speed the start of construction at the Louisiana site.

The engineering design of the beam tubes which span the full 4km arms of the two facilities has been completed. A qualification test of the design using full diameter tube segments and the field assembly procedures is being carried out by our design contractor, Chicago Bridge and Iron, Inc. The successful completion of this test, expected near the end of this year, will clear the way to begin fabrication of the required 16 km of beam tube. The other major engineering design contracts are for the building and site design and for the remainder of the vacuum system. The Request for Proposals (RFP) for the building and site design has been released to qualified Architect-Engineering firms, and the RFP for the remainder vacuum system is in preparation.

As exciting as the beginning of construction is, some of the best news has come from the R&D work. The reconstructed 40 m interferometer at Caltech, called "Mark II" to distinguish it from its predecessor, has come into full operation now. This new version of the interferometer is housed in a new vacuum system, one which gives us much more room for testing concepts and hardware for the full-scale LIGO interferometers. The most significant change to the interferometer itself, namely, the replacement of the seismic isolation stacks, has resulted in an improvement in the low frequency performance of the interferometer by as much as a factor of 100. The next major change to the 40 m interferometer (currently underway) is the replacement of the old test masses with new ones which have the required supermirror coating deposited on on a polished face of the test mass itself, eliminating the need to have a mechanical attachment of a separate mirror to the test mass. The preliminary indications are that this change will reduce the amount of thermal noise in the interferometer and give yet another improvement in performance.

A major effort to demonstrate the optical phase sensitivity required for the LIGO interferometers is underway at MIT. A 5 m interferometer is being built with seismically-isolated, suspended mirrors. This interferometer is designed to operate with the same laser power incident on its beamsplitter as the full-scale LIGO interferometer. Thus, it should have the same shot noise (measured as an uncertainty in optical phase) as the LIGO interferometers. In this way, it will test for any other optical noise sources and verify that they can be controlled with adequate precision. Together, the results from the 5 m and 40 m interferometer should provide a firm foundation on which to base the LIGO interferometer design.

Many of the other R&D activities have less readily explained results (for the nonexpert), though the work is not necessarily less important or less difficult! We have made steady progress in understanding the complexity of the alignment systems required, in establishing the capability of industry to fabricate the high precision LIGO optics, in developing the input optics which control and stabilize the laser beam, and in modeling the various aspects of the interferometer. All of these efforts are essential to a success project.

The past six months have also seen a reorganization of the project to enhance the project management. Barry barish has been appointed as Principal Investigator, and Gary Sanders has been recruited from Los Alamos to become Project Manager. The new management plans will be reviewed by NSF in September along with an updated cost estimate. Success at this review (which we expect) will give us the go-ahead for the increasing level of effort required to complete LIGO on schedule.

PASCOS '94

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PASCOS '94, the 4th meeting in the series called Particles, Strings, and Cosmology, was held at Syracuse University from May 19 through May 24. Remarkably fine weather surprised the organizers and delighted the participants when the latter weren't glued to their chairs for a stimulating set of invited talks. Although at times the diverse program seemed about to demonstrate that physicists form a set of communities separated by a common language, great efforts were made to bridge our cultural differences. An idiosyncratic listing of highlights of the meeting follows.

Two excellent talks on the solar neutrino opportunity ("problem" is too negative a word) were presented by Ettore Fiorini, representing the experimental community, and by John Bahcall, who tried to make his talk look like an experimenter's by displaying histograms of the results of one thousand different solar models. Both agreed that the glass of neutrinos was half full, the detection of solar neutrinos at roughly the right rate demonstrating that the Sun shines by fusion of hydrogen into helium. The persistent deficit in neutrino fluxes (consistent results from SAGE and GALLEX joining the list with Kamiokande and the pioneering Homestake Mine Experiment) is called an opportunity to learn new physics. The MSW effect is the odds-on favorite.

An upbeat presentation on the status of the next hard experiment, LIGO, was given by Deputy Director Stan Whitcomb. Site work has begun at Hanford, and a contract for engineering design of the beam tubes has been let. The schedule calls for the first site to be available for interferometer installation in 1998, and for the first coincidence run between the two sites around 2000.

Astrophysical cosmology was the subject of a set of excellent talks. Ned Wright gave a very clear summary of the results from COBE, and of the limits set by those observations on standard and not-so-standard cosmologies. The inflationary paradigm gets a clean bill of health. The topic was picked up by David Spergel in his talk entitled "Examining the Wreckage", in which he acknowledged that "many of our theories are stretched to the breaking point." Understanding the rich data base now accumulating on large scale structure, especially the comparison between the COBE map's power spectrum and the correlations between galaxies revealed by the ever-improving surveys, seems to be difficult in the context of any model simple enough to have been considered beautiful heretofore. Multi-component dark matter, non-zero cosmological constant, "tilted" fluctuation power spectra, or a Hubble constant even lower than Sandage's are among the bizarre alternatives taken seriously now. Spergel and Joshua Frieman gave summaries of the large number of new observational efforts that will, with luck, make theorists' work even harder in the coming years.

A beautiful description of the status of searches for Massive Compact Halo Objects (MACHOs) was presented by David Bennett. He showed convincing evidence for several microlensing events, but perhaps the highlight of his talk was the evidence for the link between experiment acronyms and national character: MACHO (American/Austrialian), EROS (French), and OGLE (Polish). The international effort seems to have established microlensing as a real phenomenon, but at a disappointingly low rate. The dark matter problem is not solved yet.

J.V. Narlikar gave a classic talk on the Quasi Steady State Cosmology, which can be adjusted to look arbitrarily similar to the Hot Big Bang Cosmology. Perhaps the steadiest thing about the Steady State theory is its adherents' determination to make some version of it work.

A session that included talks by Andy Strominger, Lenny Susskind, and Abhay Ashtekar was a remarkable example of cultural convergence. The first two devoted their talks to black holes. Strominger spent a portion of his talk explaining Penrose diagrams to the uninitiated, while Susskind (who prefers Kruskal diagrams) was effusive in his praise of the membrane paradigm. Each was selling a different "stringy" solution to the information problem in black holes. (In response to a question, Strominger admitted to having swept under the rug a naked singularity, but said he didn't think it was important, since it was "only at one point.") Ashtekar, in the meantime, described progress in relativity-inspired quantum gravity research with a distinct emphasis on the analogies to string theory, albeit formal ones for the most part.

The prize for the melding of cultures must be awarded, though, to Mark Bowick, whose outstanding talk on studies of the evolution of defects in liquid crystals managed to be simultaneously about early universe cosmology, high energy theory, and experimental condensed matter physics.

Among the highlights from the strictly particle physics side of the meeting was the detailed presentation by Henry Frisch of the evidence for (don't say "discovery of") the existence of the top quark. Perhaps in Stockholm they should give out evidence for the Nobel Prize at the 2 to 3 sigma level. Riccardo Barbieri gave a pep talk for the prospects of supersymmetry, an idea that almost rivals gravitational waves for its longevity without an experimental discovery.

The conference was characterized by many lively exhanges between speakers and questioners. Some nearly-verbatim quotes: "It is remarkable that you got money when you didn't know what you were doing," "That isn't physics, it's meaningless," "I proved your idea was incorrect in the referree's report I wrote on your last paper," "We won't settle this here, we'll settle it later [with gesture toward the door]", and "Thank you for giving my talk."

After the banquet on May 23, science journalist Gary Taubes regaled diners with the dirty details surrounding the cancellation of the SSC. But perhaps the clearest summary of the intellectual enterprise represented at the meeting came in entirely non-verbal form on the previous evening. Two gifted women, Roxanne Kamayani Gupta and Amita Dutta, gave a recital of classical Indian dance. They performed on a stage graced with a statue of Shiva Nataraja, the Lord of the Dance, whose connection to physics, we were reminded, was made clear by Fritjof Capra. Dancing in two highly elaborate and remarkably distinct styles, without speaking a (non-Sanskrit) word, the two managed to embody the quest for beauty that unites us all.

The Vienna meeting

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From July 25 to July 29, 1994, a conference on Mathematical Relativity was held at the Erwin Schrödinger Institute for Mathematical Physics (ESI) in Vienna, organized by P. C. Aichelburg and R. Beig. The conference was attended by 90 people, roughly one third of whom were also participants in the ESI workshop on the same topic running from 1st July through 'til 16th September 1994. The talks at the conference focussed on one hand on soliton type solutions of the Einstein equations coupled to nonlinear matter fields, on the other hand on global issues in classical General Relativity in general.

The one hour talks were: R. Bartnik - The Non-Abelian Einstein-Kaluza-Klein System, P. Bizoń - On the No-Hair Conjecture, D. Brill - Testing Cosmic Censorship with Black Hole Collisions, M. Choptuik - Critical Phenomena in Gravitational Collapse, Y. Choquet-Bruhat - Non Abelian Relativistic Fluids, P. Chruściel - Strong Cosmic Censorship in Locally Homogeneous Spacetimes, H. Friedrich - Boundary Conditions for Anti-de-Sitter Type Spacetimes, G. Gibbons - Gravitating Solitons, W. Israel - Effect on Radiative Wave Tails on Black Hole Interiors, D. Maison - Analytical and Numerical Methods for Einstein-Yang-Mills and Related Systems, V. Moncrief - The Reduction of Einstein's Equations in 3+1 Dimensions and the Analogue of Teichmüller Space, K. Newman - The Structure of Conformal Singularities, N. Ó Murchadha - Spherical Gravitational Collapse, G. Rein - On the Spherically Symmetric Vlasov-Einstein System, A. Rendall - Crushing Singularities in Spherically Symmetric Cosmological Models, B. Schmidt - The Newtonian limit of Einstein's Theory of Gravitation, N. Straumann - On the Einstein-Yang-Mills System for Arbitrary Gauge, R. Wald - Classical Thermodynamics of Black Holes in Arbitrary, Lagrangian Theories of Gravity Coupled to Matter.

The half-hour talks were: P. Brady - Critical Behaviour in Self-Similar Scalar Field Collapse, M. Iriondo - Existence and Regularity of CMC Hypersurfaces in Asymptotically Flat Sapcetimes, H. Pfister - Dirichlet Problem for the Stationary Einstein Equations with Applications to Stability Limits of Rotating Stars, B. Temple - An Astrophysical Shock-Wave Solution of the Einstein Equations Modelling an Explosion, G. Weinstein - N-black Hole Stationary Axially Symmetric Solutions of the Einstein-Maxwell Equations.

An ESI preprint containing the abstracts of these lectures should be available by September and can be obtained via 'anonymous ftp', 'gopher' or 'WWW' : FTP.ESI.AC.AT

The Pitt Binary Black Hole Grand Challenge Meeting

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A meeting of the BINARY BLACK HOLE GRAND CHALLENGE ALLIANCE was held at the University of Pittsburgh on May 5-6, 1994. This was the first meeting planned since the project was funded so that the major emphasis was placed on organizational matters and background presentations for the benefit of the Advisory Committee, which Kip Thorne had just agreed to head. Two meetings are anticipated each year, which will rotate between the sites of the eight participating universities: Cornell, Illinois, North Carolina, Northwestern, Penn State, Pitt, Syracuse and Texas. One of these meetings will be in the format of an open numerical relativity conference, as held at Penn State last Fall, and the other will be focussed on keeping the Alliance on track. The Pitt meeting gave the co-PI's, research scientists, graduate students and other collaborators on the Grand Challenge a chance to meet, show their progress and what they had to offer, develop further collaborations, identify problems and methods of attack and give feedback to overall and individual plans. The Proceedings, comprising approximately 200 pages of handouts and xeroxes of transparencies, may be ordered by sending a check for $20, made out to the University of Pittsburgh, to Dr. Roberto Gomez, Department of Physics and Astronomy, University of Pittsburgh, Pittsburgh, PA 15260.

The main goal of the project is to supply a catalog of gravitational waveforms for the inspiral and coalescence of two black holes. Richard Matzner, the project leader, gave an overview of the necessary subtasks, their schedules for completion, how they fit together and the collaborations that had been formed among the groups. The programs, timelines and progress of the relativity groups were presented by co-PI's S. Shapiro and S. Teukolsky (Cornell), L. Smarr and E. Seidel (Illinos), C. Evans (North Carolina), S. Finn (Northwestern), P. Laguna (Penn State), J. Winicour (Pitt), and R. Matzner (Texas). The issues bearing on the computer science groups were detailed by co-PI's P. Saylor and F. Saied (Illinois), G. Fox (Syracuse) and J. Browne (Texas, unable to attend due to illness but in a written version). W. Rheinboldt (Pitt) gave a survey of DAE's (constrained systems of differential algebraic equations), which are used in the computational approach of the Illinois group. Also, E. Newman (Pitt) surveyed the eth formalism, now being developed in finite difference form by the Pitt group to allow use of spherical coordinates in 3D tensorial problems. The trends in supercomputing at the national centers and the current transition from vector to parallel and heterogeneous processing were outlined by L. Smarr (NCSA), G. Fox (NPAC) and R. Roskies (PSC).

The main relativity topics discussed were the development of 3D codes; finding and tracking the apparent and real horizons, which enter into the interior boundary conditions for a Cauchy evolution; matching the exterior Cauchy boundary to a characteristic evolution in order to supply an outgoing radiation boundary condition and propagate the radiation to scri; the physics of the initial data set; and numerical issues such as elliptic solvers, coordinate systems and adaptive mesh refinement. The horizon problem, involving the trouser shaped merger of the two black holes, is potentially the most difficult of these but remarkable progress, in the case of head-on collisions, was presented by the Cornell and the Illinois groups (in collaboration with W. Suen of Washington U.). They are able to track the jump in the apparent horizon and follow it for sufficient time for it to settle down close to the event horizon. A late time slice of the apparent horizon is then used to reconstruct the actual horizon by tracing back a null hypersurface.

The computer science aspect of the project is important for developing a backbone structure that will allow the various groups to interface codes and data and to spearhead the adoption of High Performance Fortran standards which will enhance use of the emerging generation of teraflop machines. Roughly half the meeting was devoted to these issues. This has resulted in a proposal for a uniform Fortran 90 data structure, which can be examined using mosaic/www with URL

http://www.npac.syr.edu/NPAC1/PUB/haupt/bbh.html

(in the 'data structures' section). This standard, which all groups will adopt by Fall 1994, is the first step toward developing a software toolkit for the project.

The meeting concluded with an administrative session with the freshly appointed Chairman of the Advisory Committee. Kip reported estimates of signal to noise ratio in the tens (for advanced LIGO) from inspiraling black holes of tens of solar masses at distances of 500Mpc. This supplies practical feedback for the minimal quality of Black-Hole waveforms that should constitute the Alliance catalog. Kip also related the impression: It seems that every group wants to do everything in order to solve the waveform problem. This reflected the energy and skill of the groups as well as the individualistic tradition of general relativity. The meeting had made progress in setting the Alliance on a cooperative course in crucial areas where collaboration would speed progress and avoid duplication of effort.

International Symposium on Experimental Gravitation, Nathiagali, Pakistan, 1993

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The main purpose of the symposium was to provide a forum for physicists engaged in designing/performing/thinking about experiments in gravitational physics. In order to allow maximum participation apart from invited talks there were few contributed papers which could be read to all participants without any parallel sessions. There were 75 participants from 17 countries from five contintents. The Proceedings have been published by the Institute of Physics Publishing (UK) and are available from them. Their address is: Techno House, Redcliffe Way, Bristol BS1 6NX, UK. The invited papers are reprinted in a supplementary issue of the June 1994 issue of the journal Classical and Quantum Gravity. The subjects covered in the Symposium described experiments to test the predictions of the general theory of relativity over a range of phenomena from the classical to the quantum, over distances from millimeters to the astronomical, on time scales from the present to the early universe and from the weak to the strong-field approximation. An overall review of experimental gravitation was provided by Braginsky, progress reports on resonant and laser gravitational wave detectors were given by Bassan and Kawashima, respectively. The status of tests of the universality of free fall was presented by Adelberger and Boynton, some new results as well as a review of previous experiments on gravitational effect on superconductors and the quantum mechanical phase shift due to gravity was given by Anandan and Werner. Paik gave a comprehensive description of superconducting accelerometers and recent results from a null test of the inverse square law of gravity. Results from binary pulsars, cosmic background radiation and gravitational lensing was reported by Wolszczan, Partridge and Swings. There were several interesting contributed papers also. Details of the contents of this report can be found in the Proceedings referred to earlier.

10th Pacific Coast Gravity Meeting

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The pre-eminent centers for General Relativity on the West Coast of the US are at Caltech and at UC Santa Barbara. About ten years ago, some of us from out near scri decided that it would be nice to hear about the research that the people at those two places were doing; and it would especially nice if this could be done on one trip. Fortunately, the people at Caltech and UCSB also thought that this was a good idea, and so with the support of Kip Thorne, Doug Eardley, Jim Hartle, and Gary Horowitz, the first Pacific Coast Gravity Meeting was organized and held at Caltech in March of 1985.

There was no time to get any grant money, so we decided that this would be a near-zero budget conference, modeled after the "Steven's Meetings" held years earlier on the East Coast. There were no invited plenary speakers, and anyone who wanted to come could come; while anyone who wanted to speak could speak. Talks by grad students were especially encouraged. We also aimed to get people representing the full breadth of GR and gravitational research–from astrophysics to experimental measurements, and from numerical relativity to mathematical relativity.

At the first PCGM, there were 33 talks going from Friday morning until Saturday evening. A very wide range of research was discussed– there were talks on quasilocal mass, on black hole uniqueness theorems, on the newly popular superstring theory, on rapidly rotating neutron stars, on gradiometer measurements, and on competing schemes for detecting gravitational radiation. People learned a lot, had a good time, and decided to do it again the next year.

Pacific Coast Gravity meetings have been held (in February or March) every year since then, with essentially the same format and even some of the same faces, but with talks reflecting the changing interests in GR. This year's PCGM was held in late March at Oregon State University in Corvallis, Oregon. Tevian Dray and Corinne Manogue did the bulk of the organization, and again we had an interesting and enjoyable meeting, with about 80 participants listening to 36 talks spread out from Friday morning until late Saturday afternoon.

One of the themes of this year's meeting was the need to prepare theoretically for the coming of LIGO and its European counterpart VIRGO. Just 10 days before the meeting, and just a couple of hundred miles away from Corvallis, work had begun on site preparation for the western branch of the laser interferometry gravity wave observatory, in Hanford, Washington. Kip Thorne noted that LIGO should be on the air by 1998, and so he emphasized that if the theoreticians were going to be ready to interpret the data it collects, they needed to be calculating detailed profiles of the gravitational radiation which one expects LIGO to see. Thorne indicated that the primary sources of the radiation which LIGO and VIRGO are expected to see are various coalescing binary objects. Millions of templates of these sources, both for searching for radiation and for interpreting it, are needed, and they must be calculated now. A number of researchers are already working on this, and talks by Flanagan, Apostolatos, Laurance, Poisson, and Kennefick discussed some of these results to date. The talk by Price, on a new simple "hand calculator" study of colliding black holes in the close limit, relates to this issue as well.

Another theme of this year's meeting was the success of the so-called "EotWash" experimental group in determining the accuracy of the Equivalence Principle. Ever since the flurry of interest during the mid 80's in the so called "5th Force", experimentalists have been very carefully looking for any deviations from the Equivalence Principle which might occur. Various distance ranges have been explored, and various disparate materials have been examined. Adelberger's "EotWash" group in Seattle has done much of this careful work, and he together with his students Smith, Gundlach, and Harris reported on their results. So far, the Equivalence Principle appears to be accurate to all orders, in all circumstances.

Quantum gravity has been a part of every one of the ten PCGM's. At this year's meeting, the emphasis was on semiclassical studies, especially near black holes. Hiscock talked about extreme black holes, and how their special properties could be used to produce a possible solar-sized laboratory for quantum gravity studies. Hiscock's students–Bruekner, Whitesell, Herman, and Loranz–discussed semiclassical analyses in Reissner-Nordstom interiors, in black holes with scalar fields, and in 2-dimensional black holes. Other talks related to quantum gravity included the one by Brooks which discussed the problem of finding observables, with topological field theories used as a laboratory for studying this problem; and the one by Cosgrove on 2+1 quantum gravity and the possibility of topology change.

Many of the astrophysics talks at the Corvallis meeting focussed on the problem of radiation production by coalescing binaries, as noted above. There were other astrophysics-related talks as well. Ipser discussed low frequency modes of rapidly rotating (periods of about 1 to 10 seconds) systems, and showed that neutron stars with such periods are consistent. Nordtvedt described some celestial mechanics studies of the lunar orbit, noting that in principle the lunar orbit could be one of the best laboratories for studying nonlinear GR effects. Das presented some exact solutions which describe spherical collapse of an anisotropic body into a black hole. A more formal mathematical treatment of spherical GR systems was discussed by Romano.

Topics ebb and flow. At the PCGM four years ago in Eugene, there was a lot of discussion of whether an arbitrarily advanced civilization could build a time machine. At last year's meeting in Santa Barbara there were no talks on this topic, but at this year's meeting, there were two: Isenberg discussed a result which disproves the "Fountain Conjecture" of Hawking and Thorne (The Fountain Conjecture plays a role in Hawking's argument that time machines can't be built). And Tanaka did a semiclassical study of scalar fields which he says indicates that quantum divergence may not work as a device to prevent the construction of these machines. Superstring theory, too, has provided lots of talks at some PCGM's and not so many at others. This year, there was just one superstring talk: that by Horne, on S-duality and chaos. Cosmology, which has been well-represented at many meetings, was really only discussed in one talk at Corvallis: Agnew discussed certain exact cosmological solutions of Einstein's equations with a stiff (nonperfect) fluid, and with planar symmetry.

One mainstay of all the PCGM's is Joe Weber. He reminded us that the cylindrical resonant bar detectors at Maryland, Rome, and elsewhere have been on the air for years, searching for gravitational radiation.

A number of the talks at the meeting this year involved spinors and Clifford algebras. Both Dray and Pezzaglia used Clifford algebra ideas to get double sets of spinors, and each described how they might be used in physics. Differ proposed a scheme for representing all physical fields in terms of certain Clifford algebra-valued fields. Taub described some features of spinors related to O(3,C) as compared to those related to SL(2,C). And Schray discussed some possible uses of octonionic spinors.

There were three other talks at the 10th PCGM. Boersma discussed some of the mathematics which one encounters in studying spacetimes as evolving local spatial geometries seen by a congruence of observers. Pinto described how one might experimentally probe time-dependent gravitational fields by examining atomic transitions in Rydberg atoms near massive stars stars or neutron stars. And Isaacson gave an Economics 101 seminar on the changing economic realities at the NSF. There is money at the NSF. But it may go increasingly to areas of research which are deemed "strategic" to the future of the nation. (Congress gets to decide what is "strategic" and what isn't.) Isaacson noted the need for us to let Congress know why we think the sort of gravitaional research we do is useful and deserving of support.

This coming year, on its 10th anniversary, the Pacific Coast Gravity Meeting will return to Caltech, its place of birth. We warmly invite you to come; if not this year, then some February or March in the future.