An Excursus on Experimental Gravitation from Space and Relativistic Astrophysics

in Honour of Francis Everitt's 60'th Birthday

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These two volumes of the Proceedings of the Seventh Marcel Grossmann Meeting are dedicated to Francis Everitt in honour of his sixtieth birthday. In many ways Francis's life is intertwined with the maturing of experimental general relativity, both from the ground and from space, and has close ties with major theoretical developments in gravitation physics and astrophysics. Since the beginning his activities have interacted with the scientific achievements of a large and distinguished set of scientists. I take this occasion to recount some of his story and weave it into the narrative of some of my own reminiscences of the times during which the Stanford projects were initiated and developed, and to discuss how the Stanford group interacted with the scientific communities nationally and internationally. A part of this story is the birth of this series of Marcel Grossmann Meetings aimed at bringing the scientists working in gravitational physics together with mathematicians, theoretical physicists, and astrophysicists to stimulate each other's progress. The first section sketches Francis's studies in England and his arrival in the United States as a research associate and the second section covers his first activities in the new Stanford ambiance and the origin of the gyroscope experiment, as well as his first encounter with Irwin Shapiro. In the third section I recall significant moments of the Princeton-Stanford collaboration, including my first visit to Stanford, the ESRO Interlaken meeting, the development of the physics and astrophysics of Black Holes, the birth and flourishing of X-ray astronomy and its impact on relativistic astrophysics, the identification of Cygnus XI as a Black Hole, my spending a quarter at Stanford and finally the XVI Solvay Conference and the LXV Varenna School. In the fourth section I recall the foundation of the Marcel Grossmann Meetings and my return to Italy and in the fifth section my first contacts with China and the Third Marcel Grossmann Meeting (MG3) in China. The last section describes the birth of the International Center for Theoretical Physics (ICRA) and MG4, MG5, and MG6-from China to Stanford via Perth and Kyoto.

I. Francis Everitt: A Life in Science

Francis Everitt was born on March 8, 1934 in Sevenoaks, England, the youngest in a family of four boys and one girl. He was educated at the ancient local school founded in 1418 and then at Imperial College, London, where he obtained his bachelor's degree in 1955 and his doctorate for research in paleomagnetism under P.M.S. Blackett in 1959.

Francis entered the field of space and gravitational physics in 1962 when he moved to Stanford University and he has revealed that his interest in this and in the history and philosophy of science seems to have been prepared for through his family and physics background. His father was a patent attorney trained in engineering under Silvanus Thompson, the electrical engineer and biographer of Kelvin. As a growing boy Francis recalls his-father's reminiscences of Thompson, including Thompson's anecdotes about Kelvin, and Thompson's encounter with Tesla. Francis's father also read many scientific books including Einstein's The Meaning of Relativity, and he took the side of Einstein in the Bohr-Einstein debate. An uncle from the other side of Francis's family had been on the mathematics faculty of Imperial College. Francis's eldest brother Robin is also a mathematician and a former schoolmaster, while another brother is a distinguished historian. In fact, Francis had the unusual experience of having Robin as his mathematics master for two years in school while the two of them were living in the same house, an experience which Francis says was probably more difficult for his brother than for him. It was Robin who in 1949 picked up from a secondhand bookshop the book The Life of James Clerk Maxwell by Lewis Campbell and William Garnett [1] which provided the background for Francis's biographical interest in Maxwell and played a major role in his becoming a physicist.

When Francis entered Imperial College in 1952, the Physics Department was at a low point following the retirement of G.P. Thomson and had still not recovered from the absence of so many of its faculty during World War II. A year later Blackett arrived "like a whirlwind" bringing many people and programs from Manchester, including the very new rock-magnetism group that he had formed with the former radio astronomer J .A. Clegg. This was the group that Francis decided to join at the end of his undergraduate period, although his first publication was neither in rock-magnetism nor in English. In 1955, Francis spent a summer at the Physikalisch Technische Bundesanstalt in Braunschweig doing research on electron optics with K.J. Hanssen, an experience which led to a short paper in German on a new method for measuring the principal planes and focal lengths of electrostatic electron lenses.

Blackett had entered rock magnetism in an unexpected way. During the course of his earlier cosmic ray research he had became interested in the effects of the Earth's and other planetary and stellar magnetic fields, and was led to revive and quantify an earlier conjecture of Schuster's proposing a fundamental connection between magnetism and the rotation of a gravitating body. To test this hypothesis Blackett found it necessary to invent a new magnetometer much more sensitive than any previously existing ones. He did this by effectively reinventing Kelvin's astatic magnetometer. His paper describing this work remains in Francis's eyes one of the great classic works in optimizing instrument design. Blackett's enormous influence on Francis during this period later emerged as an important part of the foundation underlying the design of the Gravity Probe B experiment, although Francis admits that he had to unlearn one strong prejudice he picked up from Blackett, who resisted the world of modern electronics.

Having invented a magnetometer a factor of 1000 more sensitive than any other in existence, and only a factor of 100 less sensitive than modern SQUIDS, Blackett naturally wanted to put it to some use. He was led to recognize that it could be applied to measuring the very weak magnetic moments of sedimentary rocks. The story is told, certainly a believable one, that he asked a geologist's advice on what book to read to learn some geology and on being referred to Arthur Holmes' classic Principles of Physical Geology (1944) he read it in a weekend while making fifteen or twenty pages of detailed notes.

This led to the establishment of two research groups, one with Blackett under Clegg and one at Cambridge and later Newcastle under Blackett's former student S.K. Runcorn, who died so tragically in 1995. These two groups produced many of the early key papers that opened up the new field of plate tectonics. As is not infrequently the case, there was intense rivalry between them: the only joint publication they ever produced was the one that Francis wrote with John Belshe of Cambridge on the paleomagnetism of the British Carboniferous system. It concluded that during the Carboniferous period (~ 300 million years ago) Britain had been roughly 10 degrees south of the equator.

Francis wrote nine more papers on rock-magnetism, including one on an application to geology which resulted in a pleasant correspondence with Arthur Holmes, and three which developed experimentally and theoretically a new understanding of thermoremanent magnetization, namely the magnetism acquired when a body such as an igneous rock cools in a weak magnetic field. In reflecting on Blackett's influence, Francis praises him not just for his brilliance in instrument design but for his extraordinary directness. To someone like Francis who had always been disposed to assume that science is subtle and speculative, this quality was very unexpected. Blackett had been trained as a naval officer and had fought in the Battle of Jutland, and one of his favorite sayings had been that you should "treat your research like a military campaign." Since Francis's maternal grandfather had been a torpedo engineer with the Royal Navy, this message did gradually sink in.

The question Francis had to face after completing his Ph.D. and spending about 18 months as a post-doctoral associate at Imperial College was whether or not to "turn himself into a geologist." Despite much advice not to "throwaway all your experience," he concluded that he preferred to return to physics, and after many discussions with many people of whom the most influential was Philip Morrison, he decided to try low temperature physics. After a seemingly chance encounter that Francis views as miraculous, an opportunity arose to work with Kenneth Atkins at the University of Pennsylvania ("Penn"), and Francis moved to the United States for "two to three" years that have now stretched into thirty-five.

At Penn Francis designed his own elegant apparatus to investigate the possible existence of "third sound", a surface wave in thin films of superfiuid helium, as predicted by Atkins. Within twelve months he, Atkins, and Denenstein had determined all the major properties of third sound through ellipsometry measurement of the thickness of the film. This technique had been invented by L.E. Jackson, and Jackson's student Llewellyn Grimes had sought earlier to apply it to the same problem.

While Francis was at Penn, the physics department there arranged a three-day visit by William Fairbank to deliver their Mary Amanda Wood lectures, and it was through this occasion that Francis met Fairbank and first heard about Leonard Schiff's suggestion of the orbiting gyroscope experiment [2]. Francis likes to tell the story of Atkins' introduction of Fairbank in the lecture series. He said that whenever a low temperature physicist heard of an experiment that was completely impossible, the next news six months later was that Bill Fairbank had just finished it and had done so with even higher precision than the one that had seemed impossible. Privately Atkins also told Francis that finally Bill Fairbank had gone too far. His last suggestion really was impossible. It was to develop a new kind of gyroscope for the Schiff experiment. This encounter with Bill Fairbank signaled the entrance of Francis into the arena of gravitational physics experiments.

II. Development of the Scientific Basis for Gravitational Physics Experiments

II.1 Early Impressions

Like so many other important scientific movements, the new life in gravitational physics that came towards the end of the 1950's seemed to rise almost spontaneously in several places. The group at Princeton led by Robert Dicke was one influential source. Another one was R.V. Pound whose 1959 redshift experiment depended on the Mossbauer effect. Also related were Joe Weber's early reflections on the detection of gravitational waves, which began while he spent a sabbatical at Princeton in 1957. Schiff seems to have entered the picture almost independently from the delayed influence of Robert Oppenheimer, with whom he worked on Mach's principle in 1939. Bill Fairbank's interest was aroused by a remark of Bryce DeWitt's at the Chapel Hill gravitation meeting in 1957 at which DeWitt tossed a piece of chalk in the air and caught it saying (with some hyperbole as well as a parabola) that there was only one experiment in gravitational physics and that we do it over and over again as he had just done. Another factor was the early Gravity Foundation essay by Philip Morrison and Thomas Gold, who sought to explain the absence of significant quantities of antimatter in our universe on the basis of the hypothesis that matter and antimatter repel each other gravitationally.

Francis says that in addition to his meeting with Bill Fairbank, three things drew him to gravity. First there was a proposal made by a young faculty member at Penn to measure gravitational waves using the Mossbauer effect. Second there were Kenneth Atkins's discussions with him about the distinction between active and passive gravitational mass during the time Atkins was writing a general physics textbook. Third there was a remark attributed to T.D. Lee to the effect that the next important field to work in would be gravitation. But of all influences, the decisive one was Fairbank's.

The first thing that struck Francis upon arriving at Stanford, after he had become accustomed to Fairbank's relaxed approach to administrative questions, was the extraordinary range of Bill's interests. The diversity of experiments and students within the group was amazing. Particularly memorable were Bascom Deaver's completed measurement on quantized flux, Morris Bol's search for the London moment, which reached fruition in 1963, Fred Witteborn's experiment to measure the free fall of the electron and (though this was never done) on the positron, and George Hess's experiment to detect quantized vortices in superfluid helium. There were many more. Francis has offered his own appraisal of Bill in the article "The Creative Imagination of an Experimental Physicist" [3) in the volume Near Zero in honour of Bill's 65th birthday.

Three other traits of Bill Fairbank's besides his imagination made a deep impression on Francis. One was the value of memory. Bill had a wonderful ability for recalling numbers for physical quantities during conversations at the blackboard and using them to make rapid and relevant order of magnitude calculations. Another was his ease at revealing his ignorance. In early exchanges Francis recalls feeling almost embarassed on Bill's behalf as he asked absurdly naive questions of some visiting expert. But as discussions advanced it became clear that there was method in this naivete, for often after an hour or so there would emerge out of the chaos a sudden profound question from Bill that no expert had ever thought of. Closely related was Bill's third trait, his willingness to collaborate with people of entirely different backgrounds. Here Stanford offered him an almost unique opportunity.

11.2. The Stanford Ambiance: Physics and Engineering

For historical reasons that are interesting and complex, as discussed by Francis in his article on Gravity Probe B in the book Big Science [4), Stanford University has had a long record of collaboration between physicists and engineers. W.W. Hansen, perhaps Stanford's greatest physicist despite his tragically early death from beryllium poisoning at the age of 43, had begun as an electrical engineer. He is now mainly remembered as the inventor and builder of the first electron linear accelerator, completed in 1949 just after his death, the machine on which Robert Hofstadter performed his classic experiment determining nuclear form factors. Earlier he had been responsible for rescuing the Varian brothers from obscurity and providing them with the space and intellectual guidance they needed for inventing and developing the klystron. No less important was his close work with Felix Bloch in inventing nuclear magnetic resonance. It is not often remembered that the patent for nuclear magnetic resonance was in the names of both Hansen and Bloch (in that order).

Whether consciously or unconsciously Fairbank continued this tradition. From the beginning he had talked about gyroscopes with Robert Cannon, the newly arrived head of the guidance and control group in the Stanford Department of Aeronautics and Astronautics. This began the long and fruitful collaboration between physicists and engineers without which it is inconceivable that Gravity Probe B could have been developed. None of this had been clear to Francis before he arrived, but with the strong engineering background in his family and at Imperial College he found the connection easy to embrace, and was able to establish a close intellectual rapport with Daniel DeBra, Richard Van Patten and other members of the Stanford Department of Aeronautics and Astronautics. Later the facilities of HEPL (the Hansen Experimental Physics Laboratory) growing out of Hansen's earlier initiative were again to prove essential, and have been part of the Stanford ambiance from which Francis and his colleagues have benefited.

II.3. Schiff and the Origin of the Gyroscope Experiment

In addition to Fairbank and this fertile collaborative atmosphere at Stanford, there was Leonard Schiff. Schiff's interest in questions bearing on relativity began in 1939 when, under stimulus from Robert Oppenheimer with whom he was then working, he investigated a question in electromagnetism related to Mach's principle. He then moved into other fields, particularly nuclear physics, but in 1958 quite independently of Bill Fairbank's interest in the question, he examined the Morrison-Gold conjecture about a repulsive gravitational interaction between matter and antimatter. He provided an ingenious counterargument to the effect that in nuclear interactions there are virtual antiparticles present, and that if gravitation acted negatively on these there would be a violation of the equivalence principle already detectable in the Eotvos and Dicke experiments.

This line of reasoning led Schiff to consider the more general question of the experimental foundations of gravitational theories. In October 1959 he wrote a paper for the American Journal of Physics that was simultaneously short, important, pessimistic, partly wrong, and profoundly influential on both Schiff himself and others. Its essential point was that the experimental evidence for general relativity was weaker than was usually assumed. Not only was the gravitational redshift deducible from equivalence principle arguments, but so also (according to Schiff) was the gravitational deflection of light. The famous Einstein factor of two, which had long been thought to be a strict consequence of the full general theory of relativity, seemed to arise simply from Einstein's having forgotten the special relativistic space-contraction in his early calculation of 1911. Schiff's pessimistic conclusion was that it would not be possible to make any interesting test of general relativity with clocks or photons until one attained second order accuracy in the measurement. Such a second order test would have required a clock with a stability of 1 part in 10^18 , which is still beyond our reach nearly four decades later. Remarkably, within two months of submitting this gloomy paper for publication, Schiff had conceived of two entirely new tests of general relativity involving massive bodies, the geodetic and frame-dragging measurements with an orbiting gyroscope.

Even more remarkably, at the same time independently of Schiff, George Pugh had conceived of the same two tests in a document not widely known, and had invented the concept of a drag-free satellite to help perform them. Francis has analyzed this striking example of "simultaneous discovery" in his article on the history of the Gravity Probe B experiment in Near Zero [5]. Pugh had become interested in the subject after hearing a short talk by H. Yilmaz at the 1959 American Physical Society meeting in New York describing his theory of gravitation. This is a clear lesson that sometimes very important experimental ideas can emerge from dubious theories.

Schiff's argument that special relativity combined with the hypothesis that gravitation is to be explained through a metric theory would automatically yield the Einstein deflection of light was incorrect. Scalar-tensor theories are metric theories and give a different answer. The argument was interesting, however, and not so simplistically false as is sometimes thought. In combination with work by H.P. Robertson, it led to a re-examination of-the framework for understanding experimental relativity that Eddington had invented in 1922. This framework for gravitational theories, now called the parametrized post-Newtonian (PPN) theory, emerged in stages. The next step after Schiff was taken by Kenneth Nordtvedt who had been Schiff's student. This was then followed by the work of Clifford Will, Wei Tou Ni, and others at Caltech and elsewhere.

Schiff's American Journal of Physics article was also interesting for the disagreement it caused between Leonard Schiff and Robert Dicke. Dicke's long referee's report on it was transformed into a companion article published alongside Schiff's original article, after which Dicke introduced his own framework for gravitational theories in 1962. In a wonderfully terse footnote to his original text, Schiff introduced what has since become known as "Schiff's conjecture." The dispute with Dicke was related to a proposal by Ramsey and Vessot to perform a clock test of the Einstein gravitational redshift effect in a satellite. Dicke had feared that Schiff's argument would discourage NASA from doing such an experiment. Fortunately it did not and in 1976 Vessot and Levine performed the "Gravity Probe A" clock test in a suborbital rocket, reaching a precision of 1.4 parts in 10^4. Gravity Probe A was one of the most beautiful experiments NASA has ever performed.

11.4. Encounters with Irwin Shapiro

Another very important step forward came in 1964 when Irwin Shapiro conceived of his "fourth test of general relativity," the radar time-delay experiment. The title of Shapiro's paper gave rise to an amusing controversy. Schiff objected to calling the measurement a fourth test, partly because he considered his own twin gyroscope experiments to be the real fourth and fifth tests of Einstein's theory, and partly because he argued on the basis of the PPN formalism that the time delay and light deflection experiments measure the same parameter. Shapiro answered these objections by stating that his test would be the fourth to be performed, not the fourth to be suggested, and that the time delay and light deflecton tests should be treated as distinct, as most relativists now agree. Francis recalls that during a 1966 visit to the Boston area connected with the design of the first experimental dewar vessel for Gravity Probe B, he used the occasion to visit Shapiro. The meeting persuaded him to "abandon any Stanford provincialism." It was also on that occasion that Shapiro made his first (but not his last or most important) suggestion for reducing the error in the Gravity Probe B measurement due to uncertainty in the proper motion of the guide star. He pointed out that one can remove the proper motion error using observations of two gyroscopes at different altitudes. Twenty years later Shapiro suggested to Francis yet another method of subtracting the proper motion error based on finding a guide star that is both a visible and radio source. Radio astronomical observations by Very Long Baseline interferometers (VLBI) then enable one to measure proper motion of the guide star with respect to remote quasars, sharply reducing the error. Actually the idea of using VLBI techniques in order to pin down the proper motion of the guide star had occurred to Shapiro already in the late seventies at the time he was chairing the National Academy Committeee of gravitational physics. There are few better illustrations than this one of the range of technological developments needed to perform Gravity Probe B. Even the method of fixing the position of the star depends on a technique that had not yet been invented when Schiff first proposed the experiment.

The Shapiro time delay measurement is still the most precise test of any positive effect of general relativity. It reached its greatest precision of 1 part in a thousand in the observations on the Viking Lander in 1976, the same year in which Gravity Probe A was flown and a year that may be rightly seen as bringing to a close the first era in testing general relativity from space.

III. On a Princeton-Stanford Collaboration: 1968-1915

III.1 My Arrival in Princeton and the First Stanford Visit

In 1967 after spending a few months in Hamburg with Pasqual Jordan, I was invited to Princeton by John Wheeler and joined his group as a European Space Research Organization (ESRO) postdoctoral fellow. Ulrich Gerlach, Hans Ohanian, and Frank Zerilli were just finishing their Ph.D.'s with him at the time. Among the memorable visitors at Princeton that year was Brandon Carter, who presented his elegant analysis of the geodesics in Kerr-Newman geometries which soon became a basic mathematical tool for the exploration of gravitationally collapsed objects.

The year 1968 was one of the single most important years to date in the history of experimental relativity. This was the year that Jocelyn Bell discovered pulsars while working with a new array conceived by and build under the guidance of Tony Hewis. The following year the Apollo astronauts planted the U.S. retroreflectors on the Moon, which along with the Soviet-landed French reflectors made possible the lunar laser ranging experiment. This experiment, thanks notably to ideas of Kenneth Nordtvedt, would lead to yet another important test of general relativity in the following years.

As a consequence of the pulsar discovery many research centers worldwide jumped into the study of neutron stars. Wheeler himself, who had just published a comprehensive book [6] on this subject, boldly led us ahead of the crowd. On the one hand he recommended reaching a profound understanding of relativity through a deeper geometrical approach. I still remember receiving from him an extended essay on Teichmiiller spaces which left me speechless for several days. On the other hand he also directed us toward the vastly unexplored field of "continued gravitational contraction" first studied by Oppenheimer and Snyder. The fate of a star with mass larger then the critical mass against gravitational collapse became in those days the topic of research of a small number of scientists, no more than fifty worldwide (compared to the many hundreds working today on related topics in some nations alone). In addition to our relativity group in Princeton, there was one in Cambridge, England, one in Moscow, and a newly formed one, also originating from Wheeler, at Caltech.

In June 1968 after an unforgettable car trip across the States, I visited Stanford and began a friendship with Francis that proved more than enduring. During that first visit to Stanford, I wrote a letter to Edoardo Amaldi in Rome describing my encounter with Bill Fairbank, Bill Hamilton, and Francis, and it was in part through later meetings with Fairbank and in part through a fuller appreciation of the astrophysical implications that Amaldi decided to enter the field of gravitational wave research. Hamilton and Fairbank had already begun thinking about a cryogenic bar detector in 1967. The year 1968 saw Joe Weber's first announcement of his apparent detection of gravitational waves through coincidence measurements between room temperature bars at Maryland and Chicago.

Later that same year I attended the international conference GR5 on general relativity in Tbilisi, then in Soviet Georgia, where for the first time I met Andrey Sakharov, Yacov Borisovich Zel'dovich, and many members of their research groups, including Igor Novikov and Rashid Sunyaev. In the years that followed, those acquaintances and the one that was soon to come with Evgenij Lifshitz, became very important for me and they often stimulated my own research and the common interests with the Stanford group. Schiff was also present at the Tiblisi meeting and gave an elegant talk on the gyroscope experiment, still remembered by Russian physicists.

III.2. The First European Physical Society Meeting and the ESRO Interlaken Meeting

April 1969 saw the very impressive first meeting of the European Physical Society in Florence. In those days universities all over the world had been affected by an "intellectual fever" and that "fever" was present as well in Florence. For us this turned out not to be a bad thing after all. During a forced interruption of the meeting while sitting on the steps of "Palazzo della Signoria" watching a demonstration in which thousands of workers and students marched past carrying red flags, Roger Penrose told me about some interesting work he and his student Roger Floyd were doing on energy extraction in the field of a Kerr solution of the field equations of general relativity. Soon after I started a lively correspondence with Floyd on this subject. When I returned to Princeton, Johnny Wheeler and I decided to carefully examine this problem. Wheeler's first suggestion was to systematically analyze all orbits in the

Kerr metric using an "effective potential technique" which turned out to be very powerful in this application. Years later Evgenij Lifshitz graciously dedicated a problem to this work of ours in his last edition of the Landau-Lifshitz treatise The Classical Theory of Fields.

An important occasion to let some of these new ideas and contacts flourish came at the Interlaken Meeting of September 1969 called by Hermann Bondi, then ESRO's Director General. Francis, Leonard Schiff, John Wheeler and I all attended this meeting. This was Francis's first meeting with Wheeler, and the first occasion he had to give a detailed account of the progress on Gravity Probe B in the presence of Leonard Schiff. A large number of challenging questions were asked, especially by Tommy Gold, and Francis seemed to have a detailed quantitative answer to everyone of them. Schiff told us after that he was "very impressed." Meanwhile Joe Weber had just claimed to have some evidence of a sidereal correlation in the coincidence between his detectors in Maryland and Chicago. Wheeler, Francis, and I had already had long discussions about this·on the flight from New York to Paris en route to Interlaken, in those days still a very pleasant daylight trip.

In the joint report that Wheeler and I wrote for the meeting [7], we introduced new concepts which in due course contributed to making the Black Hole not just a mathematical solution of the field equations of general relativity but an object of great relevance to physics and astrophysics. In particular we obtained expressions for the binding energies of particles co-rotating and counter-rotating in the field of a Kerr Black Hole, we introduced the concept of the "ergosphere," giving explicit examples of energy extraction processes, and we estimated the spectrum and the gravitational energy radiated by a particle falling in the background field of a Black Hole. We also presented a theoretical framework for estimating the cross-section and directionality of gravitational radiation detectors and consequently pointed out how difficult it was to interpret Weber's results straightforwardly as gravitational wave signals. The meeting was tremendously helpful in developing a strategy in this new field of relativistic astrophysics we all sensed was on the verge of beginning, both theoretically and experimentally, both from the ground and from space. There was a great deal of enthusiasm among the participant scientists, so well selected by Hermann Bondi, himself a distinguished relativist. Francis and I pondered the many discussions that had been held at Interlaken during long walks in the Maritime Alps near my home town of La Brigue before returning to the United States. This was the first of many happy visits that Francis paid to La Brigue.

III.3. The Mass-Energy Formula for Black Holes

In April 1968 a very special 16 year old high school student, Demetrios Christodoulou, had arrived from Greece and was admitted to Princeton as an undergraduate student. By September 1968 he had already been admitted to graduate school and he passed his general exam in October 1969. In addition Jacob Bekenstein, Bahram Mashoon, Clifford Rhoades, Bill Unruh, and Robert Wald were graduate students at the time. I became the thesis advisor of Demetrios and Cliff. Also working with us at

Princeton was Daniel Wilkins, Francis's graduate student at Stanford who had developed an elegant correction for the effects of the Earth's oblateness on the gyroscope experiment and had joined us in the study of Black Holes. The "process of extraction of gravitational and electromagnetic energy from Black Holes," the "mass-energy formulae of Black Holes" found with Demetrios [8, 9], the "upper limit to the maximum mass of a neutron star" found with Cliff [10], and the "Wilkins effect" found with Daniel [11] all emerged from this very fortunate resonance of ideas, intense work, and personalities.

It was during the writing of my paper with Demetrios on the electrodynamics of Black Holes that we emphasized with Wheeler over and over again the analogy between thermodynamics and Black Hole physics. This topic became the specific thesis topic of Wheeler's student Jacob Bekenstein. Through a profound set of gedanken experiments, Jacob pushed further the analogy between thermodynamics and Black Hole physics. Demetrios and I had formally established the existence of reversible and irreversible transformations in Black Hole physics as well as the monotonic increase, as occurs for entropy in thermodynamics, of the irreducible mass *mirr* of a Black Hole (from which the word irreducible arises) also formally established independently by Hawking in his area theorem [12]. The complete equivalence between the two results immediately follows from the identity relating the surface area S = 16 Pi *mirr*^2 of the Black Hole to *mirr*, as suggested by Bryce DeWitt and confirmed in a quick calculation by Demetrios and myself. Jacob went one step further proposing that the area of the Black Hole S measured in Planck-Wheeler units should indeed be identified with entropy. He did this by formulating a statistical interpretation of Black Hole entropy and introducing the first generalized form of the first law of thermodynamics in physical processes involving Black Holes. These topics even today, more than twenty years later, still inspire lively debate! Jacob's proposal was extremely interesting and very intriguing at the time and remains so for me in some ways even today. The proposal certainly was not contradictory but I could not find a necessity for transforming it into an identity. This entire matter became the subject of even more lively discussion after Stephen Hawking proposed a physical process which if true would transform all these theoretical conjectures into physical reality: the Black Hole quantum evaporation process. This topic also inspires lively debate some two decades later. It is likely that these issues will be clarified once there is a theory encompassing both General Relativity and Relativistic Quantum Field Theories. Like the photoelectric effect at the beginning of the century was of paramount importance for the subsequent development of quantum field theories, so too the existence of this problematic relating quantum fields and thermodynamics to Black Hole physics promises to be a most important motivation and formidable testing ground for looking at the unification of these fundamental physical theories.

Throughout this period I stayed in constant contact with Stanford and followed the developments of Gravity Probe B under Francis and of the gravitational wave program led by Fairbank and Hamilton both at Stanford and at Baton Rouge.

Especially important was Leonard Schiff's rapidly growing interest in strong gravity and relativistic astrophysics. At Stanford a new group of graduate students very committed to this research field was forming. In addition to Daniel, there were Larry Smarr, Mark Peterson, Wick Haxton, and others.

the words of Michael May, director of Lawrence Livermore Laboratories on sabbatical at Princeton in 1970, Princeton had become a "Mecca" of relativistic astrophysics. It was partly through Michael that we established a very important additional collaboration between our group and the San Francisco Bay Area. Jim Leblanc and Jim Wilson had started some impressive work in numerical astrophysics using the Livermore supercomputing facilities. Wilson's work extending it to the relativistic regime soon became of paramount importance: a realistic testing ground for new theoretical ideas and models in relativistic astrophysics. With great enthusiasm Jim was bringing to bear on this new field of research his broad knowledge not only of numerical techniques but particularly of nuclear physics and hydrodynamics, with splendid results for all of us. From his daily work at Livermore, Jim knew the cross-sections and order of magnitude estimates of physics regimes far from my own scientific expertise.

The writing of the Interlaken proceedings took Johnny and me more than one year of very intense work. On January 1971 Physics Today presented an excerpt of our report in our joint article "Introducing the Black Hole." The cover of that issue was also dedicated to our article with a painting by the German born artist Helmut Wimmer working at the New York Hayden Planetarium. I twice discussed with him the "Black Hole" concept. He later told me he could not understand the concept at all. He was almost desperate, and then one morning he woke up at four o'clock and the idea was suddenly clear. He limned in the painting in ten minutes. When I saw the painting I found it to be perfect and beautiful; only the sequence of colours in the iris had to be reversed. He offered me the painting, but I gladly accepted the first proof of the cover and asked him to donate the original to Princeton University where it still hangs in the Physics and Math library. Our complete Interlaken report finally appeared in the book Black Holes, Gravitational Waves and Cosmology [13J which Wheeler and I published with Martin Rees.

Suddenly January 19, 1971 Leonard Schiff died. In the years that followed we increasingly missed him and the uniquely subtle role he played in the development of both experimental and theoretical research in general relativity at Stanford and in the establishment of that very special entente between Princeton and Stanford

III.4. The Uhuru Satellite and the Flourishing of Relativistic Astrophysics

The successful launch, operation, and superb data collection from the UHURU satellite, developed at American Science and Engineering by Riccardo Giacconi and his team and launched by NASA still in 1971, marked an epoch in the development of the new field of relativistic astrophysics. All the theoretical work that preceded it, started by Oppenheimer, Landau, Gamow, and others, could finally be confronted with a wealth of high quality data from continuous observations of galactic Xray sources with good sensitivity and time and angular resolution. Hundreds of neutron stars were observed accreting matter from a normal star, permitting the first measurements of neutron star masses and yielding the first information about their magnetospheres.

Meanwhile, the absolute upper limit on the maximum mass of a neutron star that Rhoades and I had just found offered a decisive step for the identification of a Black Hole in our galaxy. I still remember the excitement of Gloria Lubkin of Physics Today at a Washington meeting of the American Physical Society telling me "Remo, go ... go and listen to the talk of your countryman Giacconi-it seems he has observed the objects Wheeler and you just introduced in Physics Today." I was very impressed by Riccardo but surprised that he had almost completely forgotten his native Italian language! All of this work came to full visibility in July, 1971 at the "Les Houches" conference beautifully organized by Cecile and Bryce de Witt. The lecturers were Jim Bardeen, Brandon Carter, Steven Hawking, Igor Novikov, Kip Thorne, and myself. Herbert Gursky, a close collaborator of Riccardo at American Science and Engineering, came and delivered some beautiful lectures on the observations from the Uhuru satellite. In my lectures I ventured to develop the concept of Black Holes as "alive", not -just as energy sinks but as sources of cosmic energy through their magnetosphere and their rotational energy. This concept originated in my work with Demetrios pointing out that up to 50 percent of the energy of a Black Hole could be stored as rotational and electromagnetic energy and could be extracted in principle [9). More complicated were the difficulties encountered in translating the term "Black Hole" into other languages. The literal Italian translation was considered obscene. A well known journalist had insisted instead on naming them "le buche nere" but I supported the literal masculine version "i buchi neri," which was finally accepted. In the French translation major opposition came from Cecile, who refused the literal translation and proposed the less impudent alternative "les astres occlus." The French title of the proceedings of that impressive school actually used this tamer version. But in due course the French also adopted the literal translation of the name we introduced in the article with Wheeler and they have happily used it ever since: "trou noir"!

III.5. The Identification of Cygnus Xl as a Black Hole

On my return to Princeton from Les Rouches I found Wheeler more and more involved in superspace, spacetime topologies, and Teichmiiller spaces. I chose instead to concentrate on relativistic astrophysics. My strategy was to identify theoretical arguments to firmly distinguish Black Holes from neutron stars, and then to zero in on the observational data that might lead to the first identification of a Black Hole. We needed to sharpen our understanding of the range of masses permitting the existence of neutron stars, and to determine what would make Black Holes, up till then just a mathematical concept, into physically credible and meaningful astrophysical systems. Is a Black Hole stable? Can its horizon be associated with the surface of a meaningful astrophysical object? How does a Black Hole react to an implosion of particles and how does it behave during the emission of gravitational radiation? What characteristic signature can carry signals from processes occurring in the ergosphere and magnetosphere of a Black Hole? With answers to these questions one might hope to discriminate between neutron stars and Black Holes.

In this context, the work with Clifford Rhoades on the critical mass of a neutron star seemed central to us, and I worked out some refinements of the initial proof in order to establish 3.2 solar masses as an absolute upper limit solely on the basis of causality and a fiducial density, independent of the largely unknown interactions of matter at supranuclear densities.

Meanwhile Marc Davis, formerly an MIT undergraduate then a graduate student at Princeton, had found a clever method of numerically integrating the Zerilli equations governing perturbations around a Black Hole. That technical breakthrough allowed us to explore a large variety of physical processes around a Black Hole with unprecedented rigour and accuracy, using the Regge-Wheeler and Zerilli approaches. Almost daily encounters with Tullio Regge, then professor at the Institute for Advanced Study, and the deep friendship with Frank Zerilli made this work even more pleasant. One of the most important results was the identification by Marc, myself, and the Brazilian theoretician Jaime Tiomno of the precise form of the burst of gravitational waves emitted· by a particle falling into a Black Hole. The existence of a precursor, a main burst, and a ringing tail in the overall burst structure of a gravitational wave signal emitted in a process of gravitational collapse that we established then in our specific idealized example has been consistently confirmed. In all subsequent articles, in hundreds of numerical computations, and even in more complex processes like the collision of two Black Holes, results have fit this pattern. These investigations have become part of the wider analysis of the intensities and structures of possible gravitational signals to be seen in detectors like the network of cooled bars at Stanford, Louisiana State University, and Rome and in optical interferometers like the Laser Interferometric Gravitational Observatory (LIGO), the French-Italian project VIRGO, and the German-British observatory GEO 600.

In addition Frank Zerilli and I collaborated in developing the complete nonlinear perturbations of the Einstein-Maxwell fields around a Black Hole. We found two new phenomena: the "gravitationally induced electromagnetic radiation" originating from gravitational radiation impinging on the electromagnetic structure of a Black Hole, and "electromagnetic induced gravitational radiation" originating from electromagnetic radiation impinging on the gravitational structure of a Black Hole.

These investigations on radiation processes and perturbations of Black Holes had in my mind a dual significance. They were significant for the study of gravitational radiation, but even more they were a direct testing ground for the stability of the Black Hole horizons and the so called "uniqueness conjecture" of axially symmetric Black Hole field configurations. In the following years this conjecture was proved mathematically by Brandon Carter. It offered an additional critical test for distinguishing Black Holes from neutron stars-unlike neutron stars, no regular pulsations should be expected from an accreting Black Hole in a binary system.

Finally, in parallel with these activities two refreshing things happened which gave me great serenity. The first was a series of weekly encounters with Kurt Godel at the Institute for Advanced Studies in Princeton. It was really marvelous to have the chance to discuss with Godel himself the motivations for his classic work in cosmology, to be confronted with his profound technical knowledge of mathematics and philosophy and the boldness of some of his views on the universe, and to enjoy his evaluation of the recent progress in general relativity that he had most surprisingly been following in great detail.

The second was that a brilliant group of undergraduate students started to work with me on various topics in general relativity for their junior theses: Robert Jantzen, Mark Johnston, Richard Hanni, and Robert Leach, having been recruited earlier by Jim Isenberg to fill a student initiated seminar on differential geometrical techniques in relativity that I taught. After my experience with Demetrios, I was convinced that the teaching of general relativity should be introduced as soon as possible in the educational career of a physicist in order to exploit the full novelty of the alternative geometrical interpretation of nature. The work with Mark Johnston [14] led to the article in which the diagram of the twisted family of orbits around a Kerr Black Hole was first published, later becoming the logo for the Marcel Grossmann Meetings, reproduced in the article TEST: Traction of Events in Space-Time by Anna Imponente in these proceedings. Rick Hanni initiated with me the study of the electric and magnetic lines of force of a test particle being captured by a Black Hole[15]. This work, inspired in discussions with Wheeler, offered insight in the electrodynamical properties of the Black Hole horizon preceding later studies by many years. Bob Jantzen and I studied and translated from Italian into English Bianchi's original papers on spatial symmetry groups and began a critical re-examination of Bianchi cosmology, inspired by Godel's original papers on rotating cosmologies and discussion with the man himself at the Institute for Advanced Study.

These activities and conceptual considerations with the graduate and undergraduate students supplied a secure theoretical basis for the identification of the Black Hole signatures. Complementary to these theoretical activities were the multiwavelength observational data on binary X-ray sources which in the meantime had became available in X-rays from space observatories and, equally important, in radio and optical wavelengths from the Earth's surface. On these grounds I felt confident in proposing the identification of Cygnus Xl with a Black Hole using the paradigm I proposed in [16]. I also made an oral presentation on this matter in New York as an invited talk at the 1972 Texas Relativistic Astrophysics Symposium in a memorable session chaired by John Wheeler. The New York Academy of Sciences which hosted the Texas symposium had just given me their most prestigious Cressy Morrison Award for my work on neutron stars and Black Holes. Much to their dismay I never wrote the paper for the proceedings since it was contained in [16].

III. 5b. Encounters During a Quarter at Stanford

This work in relativistic astrophysics might seem far removed from the confluence of physics and space engineering occupying Francis's attention. In fact our contacts grew ever closer. In the spring of 1973, I spent a quarter at Stanford, following an earlier delayed invitation from Leonard Schiff, then no longer with us. I enjoyed every minute of my three months at Stanford. The lectures I gave introduced me to a very exciting group of students, including Blas Cabrera, Kyle Baker, Wick Haxton, and Mark Peterson, all of whom have gone on to fine careers, and I had the opportunity to interact more closely than ever in various ways with Bill Fairbank and Francis.

With Bill and his brilliant circle of graduate students and research associates, which included John Madey, Mike McAshan, Steve Boughn, Ho Jung Paik, Blas Cabrera, and then Rick Hanni who had decided to do graduate work at Stanford, the discussions often went far into the night. At the time, Bill was concentrating much of his attention on the cryogenic gravity wave detector in a collaborative effort with Bill Hamilton at Louisiana State University. The first small bar had been cooled down, and Ho Jung was working on the magnetic transducer for it. This gave us the opportunity to have many discussions about sources of gravitational radiation, and also strengthened the ties with Amaldi's group in Rome, leading to the three-way Stanford-LSU-Rome collaboration. The goal we set, on the basis of a better understanding of the cross-sections of the detectors and of the astrophysical setting, was to create an international network of detectors capable of revealing a gravitational wave signal corresponding to an energy of one percent of a solar mass emitted anywhere in our galaxy. Robin Giffard, who contributed so much to the practice and theory of gravitational wave antennas, joined Stanford a year later. Another consuming interest of Bill's at this time was his brilliantly conceived but ultimately ill-fated experimental search for free quarks.

Meanwhile Francis and his colleagues in the gyroscope program, including the three Johns-John Lipa, John Anderson, and John Nikirk-were engaged in the arduous task of developing their first operational cryogenic gyroscopes. Slowly the many new technologies were coming together. What most impressed me was the range of thinking that went into every part of the program involving Francis's physics group, the industrial studies, and the work of Dan DeBra's guidance and control group in the Stanford Department of Aeronautics and Astronautics, which had recently launched the first drag-free satellite. The close cooperation between Bill's group and Francis's group continued through Blas Cabrera's brilliant work on ultralow magnetic field technology and John Anderson's work on SQUID's. It was during this time that I also learned from Francis and his student Paul Worden about their remarkable concept of the orbiting equivalence principle experiment that has now matured into the proposed NASA-ESA (European Space Agency) MiniSTEP program. Francis and I were to have many further discussions about this mission leading to the joint proposal that he, Paul, I and others submitted to the ESA M2 competition in November, 1989.

Two unexpected events also occurred. The first happened during the silence of a night in the Stanford Library. I was quietly approached by a man known to generations of Stanford people, Mr. Chang, who had come to the Physics Department in 1937 as a graduate student from China and had remained there ever since in a strange combination of roles which included acting as a resident guardian of the Department. Then in his seventies Mr. Chang had a very impressive knowledge of the history of physics and he asked me: "Professor, why don't you ever mention Marcel Grossmann in your lectures?" He then showed me the extraordinary two part article published jointly by Einstein and Grossmann in 1913: Part I, a physical section by Einstein in which he attempted the first geometrization of a physical interaction, and Part II, a mathematical section by Grossmann in which he showed that Einstein's ideas could be expressed mathematically using the absolute differential calculus originally developed by the Italian geometers Ricci-Curbastro and Levi-Civita [17]. Together these two parts laid the foundation for the general theory of relativity. This unique interaction between mathematics, geometry, and physics remained fixed in my mind as a point of reference for any future progress in the fundamental understanding of the laws of nature.

The second event was the visit of Werner Heisenberg and his wife. I had met Heisenberg in Munich the previous year, but the occasion to spend three days with him and his wife visiting the Stanford group, discussing with Bill and Francis their experiments and leisurely walking through the campus and through the hills of Portola Valley, gave me the opportunity to learn much more about him, of his relations with Einstein, Fermi, and Chandrasekhar, and of his own role in some of the intricacies of recent European history. I was very pleased to hear Heisenberg recall his encounters with Fermi both in Leipzig and then in Rome after Fermi's appointment to the Chair of Theoretical Physics especially created for him by Orso Mario Corbino. Heisenberg's very lengthy and detailed recollection of the work of Chandrasekhar and of his own encounter with Chandrasekhar and Fermi in Chicago was summarized by Heisenberg in the pregnant phrase "you are right, the work of Chandra has not yet been recognized enough." The meaning of that "yet" became clear to me when Chandra later received the Nobel Prize in 1983.

Also very impressive were the hours of Heisenberg's intense discussions with Bill on the many aspects of the applications of magnetic quantized fluxes to fundamental research ranging from medical physics to space science to the study of the basic constituents of matter. On the other hand I found Heisenberg's considerations on the birth of quantum mechanics versus the birth of general relativity to be very surprising. Even more startling were some of Heisenberg's recollections about his detention at Farm Hill in England at the end of the war, and about the efforts in Germany and the United States to build the first atomic bomb. They were extreme, revealing, and very credible--and at variance with all published statements on the subject. I cross checked them with Johnny Wheeler and Edoardo Amaldi and this was also interesting. I was intrigued by these statements of Heisenberg at the time as I still am now after more than twenty years have passed. Since I consider them somewhat direct and confidential, I will record them in due course at some future occasion when I will feel more at ease about doing so.

On the margins of this time of great excitement, however, I sensed that the Stanford ambience had changed and was almost tense. The very beautiful equilibrium established by Leonard Schiff was gone. Fairbank's heroic and at times ill-fated motto was: "the stronger the wind, the stronger we saiH" Every single achievement seemed to require a sequence of ad hoc actions and sometimes even a confrontation.

III. 6. The XVI Solvay Conference and the LXV Varenna School

Two extremely gifted students from the Paris based Ecole Normale Superieure came to Princeton as ESA fellows to work with me: Nathalie Deruelle in the academic year 1973-1974 and Thibault Damour in 1974-1976. Using their proverbially excellent French mathematical craftmanship, and their exceptional physical insight, we studied a variety of physical processes occurring near the horizon of a Black Hole. Nathalie and I studied some aspects of quantum particle creation near a Black Hole, while Thibault and I developed some elegant general analytical treatments of vacuum polarizations and electro dynamical processes around Black Holes, generalizing the Sauter-Heisenberg and Schwinger formalisms to Black Hole physics. Thibault introduced a new surface-based (or "membrane") viewpoint in Black Hole physics. He came along with me on many of my trips to Stanford and we collaborated with Jim Wilson at Livermore producing a treatment of magnetohydrodynamics around a Black Hole which still serves as a standard today [18]. This work followed another important result Jim and I found together in showing for the first time how to extract energy from a rotating Black Hole with ·the torque created by an appropriate magnetosphere [19].

The year 1974 saw a great event in relativistic astrophysics, the discovery of the binary pulsar by Joe Taylor and Richard Hulse. Nothing could have more beautifully connected our research on relativistic astrophysics with the kind of work Francis and his colleagues were doing with sophisticated new technologies to test Einstein's theory in the weak gravitational field of the Earth. Thibault and I still remember the great excitement on learning of the discovery during an astrophysics lunch at the Institute for Advanced Study in Princeton. It gave us the opportunity to write a paper representing the point of ideal contact between the Stanford work and direct observations in an astrophysical setting. It is well known that Thibault subsequently became a leader in the precise interpretation of relativistic effects in binary pulsars. We argued specifically that one could apply these accurate measurements of gravitomagnetism obtained in Earth orbiting experiments to provide interpretations and estimates of strong gravitational field effects and we illustrated this theme with exact estimates. We considered this discussion so urgent that we asked Andre Lichnerowicz to publish it quickly in the Annals of the French Academy of Sciences [20].

The occasion to present these results systematically came on two successive occasions. The first was the Sixteenth Solvay Conference on Physics [21]. The second, also connected with Stanford, was the annual meeting of the American Association for the Advancement of Science held in San Francisco in February 1974. This meeting led to the book Neutron Stars, Black Holes and Binary X-Ray Sources [22].

I then decided to travel and lecture in the Pacific area, first in Japan at Kyoto where I was warmly received by a newly created relativistic astrophysics group led by Humitaka Sato [23]. From Kyoto I reached Rome via the Trans-Siberian railroad, stopping in Moscow to see Novikov and Zel'dovich. With Riccardo Giacconi I then co directed the LXC International "Enrico Fermi" School on the Physics and Astrophysics of Neutron Stars and Black Holes held in Varenna in July 1975 [24].

I considered this school and its proceedings the occasion to conclude an intense activity on these subjects and look for new problematics. I departed again for the Pacific traveling to Tahiti, Morea, and the Fiji Islands, and then again all the way down to Perth, Australia. I had been invited by Michael Buckingham, a close collaborator of Bill Fairbank, to lecture at the beautiful Perth campus, so reminiscent of Stanford. At that time Michael had promoted an interest in relativistic theories in his department, and a research group was formed led by a young Ph.D.-David Blair. It was during my stop in Rome on my way to Perth that Edoardo Amaldi told me that "the time has come for you to return to Rome" and I answered "I will think about it," and Amaldi replied "No, I think the time has come."

IV. The Foundation of the Marcel Grossmann Meetings

Possibly the most important opportunity provided by my return to Italy was the occasion to meet often and constructively with Abdus Salam. I was very eager to bring the field of relativistic astrophysics to a larger number of scientists as a tool in reaching a deeper understanding of the fundamental laws of nature. In this Salam was an enthusiastic ally. But there was also another aspect of this effort about which both Salam and I felt very strongly: the broadening of fundamental thinking to much wider groups of people internationally. We came up with the motto: "In understanding the laws of nature, no country can afford the luxury of having another country think for it." It is not important if the actual contribution of a country is small or very specialized. What is essential is the exercise of the right to discovery and original thinking which so greatly ennobles the human existence.

We decided to act on two different levels. Knowing my love for the mountains and for adventurous trips, Salam asked me to help create a very special summer school in Pakistan. He selected a beautiful small village called N athiagali in the Indukush mountain range which used to be the summer capital of the North West Frontier province under British rule. The coats of arms of the English battalions carved in the mountains and the millenary forest still populated with jaguars and mountain lions were reminiscent of Kipling's dreamlike descriptions. We were especially concerned about the fate of so many Ph.D. 's coming from third world countries who upon receiving their degrees from leading universities worldwide, after working at the forefront of scientific research, were returning to their home institutions and having difficulty even finding electricity. They were completely isolated scientifically and intellectually. With a grant from the Swedish Academy, we started a school on "Physics and Contemporary Needs" bringing together scientists and university professors from institutions in a broad belt from Indonesia to Morocco. As lecturers we invited scientists who had made the most current scientific discoveries and were especially brilliant communicators.

Our second step was in a totally different direction: in nourishing that most promising discovery process created by a fortunate resonance at the interface between mathematics and physics that we had seen occasionally developing in the field of relativistic astrophysics. Essential to that process was not only the theoretical and geometrical work but also the development of new technologies leading to crucial precise measurements, from the ground and from space, and new observations both at subnuclear and astrophysical scales. As I had discussed with Heisenberg, I considered a most remarkable example of such resonant interaction between mathematics, geometry, and physics the very work of Einstein and Grossmann [17] which paved the way to the birth of general relativity. In celebration of that interaction and as an omen to success Abdus and I decided to initiate the "Marcel Grossmann Meetings", eliciting contributions from general relativity to relativistic field theories, from pure mathematics to astrophysical observations, from numerical and algebraic techniques to new theoretical tools with the only common focus being the progress in determining the basic physical laws of nature.

The First Marcel Grossmann Meeting was held in Trieste in 1976, for which Francis wrote his remarkable paper on "Gravitation, Relativity and Precise Experimentation" [25] which was put in the interesting perspective of the history of science and rooted in the many principles that he and others had been thinking about for Gravity Probe B, the Satellite Test of the Equivalence Principle (STEP), and other tests of general relativity. This article revealed yet another aspect of Francis's activities: as a historian of science. In 1960 he wrote two papers on Maxwell's scientific work, one of which "Maxwell, Osborne, Reynolds and the Radiometer" [26] is remarkable for being the first occasion in which unpublished references and reports about papers found by Francis in the Archives of the Royal Society were used to disentangle a complicated historical question. As a result of this work Francis was asked by the Dictionary of Scientific Biography (DSB) to write an article on Maxwell. Thomas Kuhn called that article the single most important one among the approximately 5000 included in the Dictionary. Francis was asked to turn the article in a short book [27]. Francis also contributed articles for the DSB on Fritz and Heinz London and later on Schiff. In 1976 he was awarded a Guggenheim fellowship which resulted in articles on Maxwell's scientific creativity [28] and on Maxwell's work on Saturn's rings and on molecules and gases [29].

Many people have wondered how Francis managed to combine work at such a high level in the history of science with the development of Gravity Probe B. His reply is that it that having this outlet probably enabled him to pass through some of the difficult early days of the experiment. Maxwell himself set the example by being one of the greatest 19th century physicists who also managed to write his own edition on the work of Cavendish [30]. Combining two careers can benefit both.

By 1976 fourteen years had passed since Francis had come to Stanford, and twelve years since NASA had begun funding the research effort on the program. The first gyro operations had began in 1974 and the first precise measurements of the London moment readout in a spinning superconducting gyroscope were made early in 1975. Blas Cabrera had developed and demonstrated the technology for producing ultralow magnetic fields below 10- 7 gauss. Paul Worden had conceived of the basic idea of the STEP satellite in his doctoral dissertation and had demonstrated some of the technologies in the laboratory. The prototype Gravity Probe B telescope had been built and an artificial star for testing it was under construction. Working with Stanford and the NASA Marshall Center, Ball Aerospace had performed their first Mission Definition Study of a flight program in 1971. Marshall Center developed the ball manufacturing technology and successfully produced gyros which were spherical to better than 1 microinch (7 parts in 107 in diameter). It might have seemed that there was little more to do. In fact all the most difficult technical, management, and political issues of developing a flight program remained. Some of these subtle questions are discussed in Francis's article "Background to History: The Transition from Little Physics to Big Physics in the Gravity Probe B Relativity Gyroscope Program" [4].

V. The First Contact With China

In 1978 I received an award from the Australian government to lecture in universities allover Australia and it was on my return to Rome that I was invited to visit China by Chou Pei Yuan, President of the Chinese Association of Science and Technology and a close friend of Abdus. It was during this long visit to many University campuses which were still recovering from the destruction of the so-called cultural revolution that I met a young astrophysics professor named Fang Li Zhi. It was Fang who accompanied me during the entire trip and translated my lectures. Fang had in common with Demetrios Christodoulou, Evgenij Lifshitz, T.D. Lee and a few others the fact that he obtained his Ph.D. at 19. The many similarities in our thinking and identical scientific interests convinced both of us that we were "twins," in spite of our quite different origin and upbringing! We decided to publish our common scientific interests in a small book which was well received in China and abroad. A "first" collaboration between a Chinese scientist and a Westerner [31]. The realities I saw in that first visit to China had a strong impact on the rest of my life.

In 1979 the second Marcel Grossmann meeting took place in Trieste. Its first International Organizing Committee included Y. Choquet Bruhat, Chou Pei Yuan, J. Ehlers, S. Hawking, A.R. Kadoura, Y. Ne'emann, myself, A. Salam, H. Sato, S. Weinberg, and Ya.B. Zeldovich. We had memorable lectures from P.A.M. Dirac, E.M. Lifshitz, C.N. Yang, J.H. Taylor, and Amaldi and for the first time the enthusiastic participation of a

Chinese delegation. We decided to hold the next Marcel Grossmann Meeting in China. Meanwhile during the same year the Space Sciences Board of the U.S. National Research Council appointed an Ad Hoc Committee on Gravitational Physics Experiments in Space chaired by Irwin Shapiro. In a memorable turn of phrase Shapiro stated that he chose the committee members as a collection of "cancelling biases." In 1981 the Committee issued its report with Space Science Board approval.

Its highest priority recommendation was to perform a precise measurement of the frame-dragging effect through the Gravity Probe B experiment. This recommendation was re-endorsed when the Committee was convened again in 1982 to review the outcome of the NASA-Stanford Phase B study of the program, which had just been completed. Meanwhile still in 1981, NASA had conducted a five-day technology review at Stanford which gave the program the highest technical reading. The year 1981 was also memorable to both Francis and Bill Fairbank for being the first time Gravity Probe B was cancelled and then reinstated with Congressional support. This began Francis's initiation into the ways of the U.S. Congress.

VI. MG meetings From China to Stanford and the Founding of ICRA

In 1982 the Marcel Grossmann Meeting left Italy for China. MG3 was the first international scientific meeting held in China in the early days of its opening to the West. In spite of many revolutions over the ages there has been a profound continuity in the actions of Chinese officials and a marked sense of history. Traditionally China has always been open to international exchanges with beneficial results. The case of Marco Polo is the one most quoted, at least in the West, although the most important for his cultural and scientific impact is probably the case in the early seventeen century of Ri Ma To (the Jesuit Matteo Ricci) who died as a minister of the Celestial Empire. One can find a discussion of his activities in the beautiful book by P. M. D'Elia provocatively titled Galileo in China [32].

It was not surprising therefore that in the halls of the hotels and in the streets we repeatedly read the phrase written in red letters "Welcome friends from allover the world." But getting them to recognize the fact that an ensemble of friends does not make an international meeting, which by its own nature transcends the concept of friendship and must guarantee the right of any interested qualified scientist to participate, independent of race, religion, or political opinion, was one of the most difficult tasks I had to accomplish in my life. After all, this right is a consequence of the Renaissance, the Enlightenment, and Liberalism in Western culture, concepts which are foreign to Chinese society.

Officials had at first refused to allow participants from some countries with no diplomatic relations with China, namely Israel and South Korea. I finally realized that the denial was not due to bureaucratic difficulty but to a different conceptual way of interpreting international relations. After an entire week of fierce bargaining, during which I often made use of wise advice given to me by Mr. Chang and read chapters of the Ri Ma To memoirs [33] every evening, also this difficulty was overcome-all the "problematic" scientists were admitted by the invention of a special passport agreed to by the Chinese ambassador in Rome and myself and signed by Abdus Salam! Many Chinese officials are still grateful today for those efforts, since after the MG3 experience the organization of international meetings in China became almost routine. Needless to say the meeting was a great success due to the great interest of the Chinese people in science and education and to the incomparable beauty of that country and its traditions.

At Stanford in 1984, after many discussions with NASA at the highest level, the decision was made to initiate development in the following fiscal year of flight hardware for Gravity Probe B through the Shuttle Test of the Relativity Experiment (STORE). Critical to Francis and the program development were Brad Parkinson's decision to return to Stanford as a Professor and Program Manager and John Turneaure's decision to accept an appointment as leader of the Gravity Probe B hardware development group. Without this troika of leaders working together, the program would have been simply impossible.

An additional occasion to review the work going on at Stanford in fundamental physics from space, including the beautiful lambda point experiment conducted by John Lipa as well as Gravity Probe B and STEP, came from my being asked to participate in a task group formed by the Space Science Board of the US National Academy of Science from 1985 to 1987. Rene' Pellat from France, Minoru Oda from Japan, and I had the good fortune to be invited from abroad and collaborate in this work with Bill Fairbank, Rainer Weiss, and Bob Schrieffer. The goal was to formulate guidelines for the NASA long term scientific program from 1995 to 2015 in fundamental physics and chemistry [34].

1985 was also the year of MG4, returning' to Italy in Rome, and the meeting in which the Marcel Grossmann Awards were initiated. The institutional award was given to the Vatican Observatory for its continous work on stellar evolution and for its heritage from the Osservatorio del Collegio Romano directed by the Jesuit Angelo Secchi, the founder of the classification of stars according to their spectra. The two individual awards went to William Fairbank and to Abdus Salam for their contributions to the understanding of the fundamental laws of physics.

So many scientists coming to Rome gave us the opportunity to formalize the scientific relations I had developed since my return to the University of Rome "la Sapienza." With the cooperation of George Coyne, the brilliant director of the Vatican Observatory, Fang Li-Zhi, Francis Everitt, Riccardo Giacconi, and Abdus Salam, we founded the International Center for Relativistic Astrophysics (ICRA) in Rome. In addition to "la Sapienza" the member institutions of ICRA are the University of Hofei in China, Stanford University, the University of Washington at Seattle, and the Space Telescope Science Institute in the USA, and the Specola Vaticana (Vatican Observatory), the International Center for Theoretical Physics (ICTP), and the Third World Academy of Science (TWAS) in Europe.

In 1988 MG5 was held in Perth, Australia, at the splendid campus of the University of Western Australia in Nedlands on the banks of the Swan River. The hosting group there led by David Blair had pioneered new approaches and obtained new results in the field of gravitational wave detectors. The institutional award went to the University of Western Australia for its contribution to relativistic astrophysics beginning with the Wallal observations during the solar eclipse of 1919 confirming Einstein's prediction of the deflection of starlight by the sun and continuing with the development of the southern hemisphere link in the worldwide chain of laboratories seeking to observe gravitational radiation, also predicted by Einstein. The individual awards went to Satio Hayakawa for advancing relativistic astrophysics both as an observer of gamma, X- and infrared radiations and of cosmic rays and as a pioneer in the field of binary X-ray sources and to John Archibald Wheeler for leading several generations of scientists to a deeper understanding of spacetime structure and, with his geometrodynamics, expanding Einstein's vision.

During these same years a work of great importance for gravitational physics was Joe Taylor's progressive refinement of his experimental work on the binary pulsar leading to the beautiful proof of gravitational wave damping. It is interesting that Taylor had already established this result at the fifteen percent level at the time of the Shapiro report. Thibault Damour, first in collaboration with Nathalie Deruelle, and then in direct association with Joe Taylor has been particularly involved in developing theoretical aspects of binary pulsar experiments. One result not obvious at first sight is that the large self-energies of the two bodies allow not only tests of weak field effects but also of some strong field effects. These take the form of null tests that can set limits on alternative theories of gravitation which could not be obtained from solar-system experiments.

Meanwhile, there has been progess in several other directions, including the development of gravitational wave antennas of both the cryogenic bar and laser types. Progress has also continued on lunar laser ranging with observations being taken up in both Europe and the U.S. The principal center for laser ranging is now the CERGA center in France.

Francis has recalled receiving a memorable phone call from me about STEP on November 2, 1989. At that time STEP was still a very small laboratory research effort at Stanford by Paul Worden and his colleague Matthew Bye. For several years, Francis and I had been having discussions about whether a flight program might somehow be developed as a U.S.-Italy collaboration. My phone call was to explain that there was an opportunity for a wider European collaboration through ESA's M2 announcement. I had first learned about this from Dr. Roger Bonnet, the Director of Science for ESA, who was present in my office at the time of the call. Casually Francis had asked when proposals were due. The answer was November 30, less than a month away. Fortunately, Francis was already committed to a trip to Europe on November 16, so with great speed he and Paul Worden prepared a draft proposal that he was able to take to colleagues in Rome, Pisa, Paris, and London, and send to others including those in Norway and Germany. The theory team most happily included Thibault. The numerous European improvements and suggestions were safely incorporated and the proposal reached ESA Headquarters just in time. To our delight STEP was selected for an assessment and then a Phase A study.

In 1991 MG6 was held in Kyoto, Japan. The institutional award went to the Research Institute of Theoretical Physics (RITP) for keeping alive research in relativity, cosmology, and relativistic field theory and the development of a school of international acclaim. The individual awards went to Minoru Oda for participating in the pioneering work of the early sixties in X-ray astronomy and for his subsequent molding of an agile and diversified Japanese scientific space program investigating the deepest aspects of relativistic astrophysics and to Stephen Hawking for his contributions to the understanding of spacetime singularities and of the large scale structure of the Universe and of its quantum origins.

This brief sketch of past Marcel Grossmann Meetings interwoven with Francis's story and that of GP-B finally brings us back to Stanford with MG7, held there in 1994 and organized by the GP-B group. The Marcel Grossmann Awards went to the Space Telescope Science Institute and to Subrahmanyan Chandrasekhar and Jim Wilson, the citations for which appear in these proceedings.

Francis could have hardly imagined when he entered the field of gravitational experiments in space in 1962 how much there was to do or how wide the interest would become. Much work still remains to be done both in the theoretical and experimental fields.

While writing these remarks, I read the article "Earthly Politics Boosts Space Probe" in the magazine Science [35], and looking at the diagram in it captioned Time Travel: A History of Gravity Probe B, I could not refrain from asking Francis: how did you manage to go through all this successfully? He answered by quoting from a speech Winston Churchill had made in 1942 at his old school Harrow. Churchill said that in looking back on his own career he could see that he had done some things successfully and others not so well, and in pondering its lessons he concluded that they could be summarized in nine simple words. They were: "Never give up, never give up, never, never, never." On behalf of the organizing committees of the Marcel Grossmann Meetings I am happy to dedicate these proceedings to Francis Everitt: ad majora!

Remo Ruffini

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