Chapter 6
The Fifth Force: A Personal History, by Ephraim Fischbach

6.1 Introduction

At approximately 11 AM on Monday, January 6, 1986 I received a call from John Noble Wilford of the *New York Times* inquiring about a paper of mine which had just been published in *Physical Review Letters* (PRL). As a subscriber to the *Times* I knew who John was, and so it was exciting to find myself speaking to him in person. My excitement was tempered by the fact that I had returned the day before to Seattle with a major cold which made it difficult for me to talk to him or anybody else. Two days later a front page story appeared in the *Times* by John under the headline "Hints of 5th Force in Universe Challenge Galileo's Findings," accompanied by a sketch of Galileo's supposed experiment on the Leaning Tower of Pisa. Thus was born the concept of a "fifth force". As used now, this generically refers to a gravity-like long-range force (i.e., one whose effects extend over macroscopic distances) co-existing with gravity, presumably arising from the exchange of any of the ultra-light quanta whose existence is predicted by various unification theories such as supersymmetry. Depending on the specific characteristics of this hypothesized force, it could manifest itself in various experiments as an apparent deviation from the predictions of Newtonian gravity.

Our paper in *Physical Review Letters* entitled "Reanalysis of the Eötvös Experiment" (Fischbach et al. 1986a), was co-authored by my three graduate students Carrick Talmadge, Daniel Sudarsky, and Aaron Szafer, along with my long-time friend and collaborator Sam Aronson. As the title suggests, our paper re-analyzed the data obtained from what is now known as the "Eötvös Experiment", one of the most well-known experiments in the field of gravity (Eötvös et al. 1922; Szabó 1998). The authors of that 1922 paper, Baron Loránd Eötvös, Desiderius

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Pekár, and Eugen Fekete (EPF), had carried out what was then the most precise test of whether the behavior of objects in a gravitational field was the same independent of their different chemical compositions. Their conclusion, that it was the same to approximately one part in 10^9, provided experimental support for what is now known as the Weak Equivalence Principle (WEP), which is one of the key assumptions underlying Einstein's General Theory of Relativity (Will 1993). However, the result of our reanalysis of the EPF paper (Eötvös et al. 1922; Szabó 1998) was that the EPF data were in fact "sensitive to the composition of the materials used," in contrast to what EPF themselves had claimed. If the EPF data and our reanalysis of them were both correct, then one implication of our paper would be that EPF had discovered a new "fifth force" in nature.

Approximately 30 years have elapsed since the publication of our PRL, and we now know with a great deal of confidence that a "fifth force" with the attributes we assumed does not exist. We can also exclude a large number of generalizations of the original fifth force hypothesis by noting that, at present, there is no evidence for any new force beyond the established strong, electromagnetic, weak (or electroweak) and gravitational forces. Among the many things we do not know is what EPF could have done in their classic experiment to have delivered to us (some six decades later) evidence at the $\sim$8 standard deviation (8σ) level for a new force with attributes that could not have even been conceptualized at that time.

As discussed in the epilogue of Sect. 6.8, it is, of course, possible that EPF did everything correctly, in which case our apparent failure to understand, and thereby reproduce, their results may be our fault not theirs. The fifth force story is thus a continuing one, in which its past will certainly inform its future. This story is also of interest in that it provides yet another example of how the scientific community gives birth to an idea, tests it, and then accepts or rejects it based on the results of experiment.

My objective here is to present the fifth force story as I experienced it personally, from its inception to the present. My task has been greatly simplified by the existence of Allan Franklin's history, *The Rise and Fall of the Fifth Force* (Franklin 1993), which gives a detailed annotated history of the fifth force effort along with extensive references. Several other sources will also be helpful. In 1999 Carrick Talmadge and I published a detailed technical description of fifth force searches under the title *The Search for Non-Newtonian Gravity* (Fischbach and Talmadge 1999). In preparation for this book we felt it appropriate to compile a formal bibliography of more than 800 experimental and theoretical papers related to the fifth force searches prior to 1992 which was published in the journal *Metrologia* (Fischbach et al. 1992). Since the central focus of this review will be on our reanalysis of the EPF paper, I will also make reference to the much expanded version of our original paper which appeared in 1988 in *Annals of Physics* (Fischbach et al. 1988), which is briefly outlined in Appendix 1.

In order to streamline the fifth force narrative, I have provided additional technical background in the appendices when needed. As noted above, Appendix 1 contains a brief summary of the fifth force formalism, and Appendix 2 presents the phenomenology of the K^0–$\overline{K}^0$ system. Appendices 3, 4, and 5 present, respectively, historically interesting correspondence from Robert Dicke, *Physical Review Letters*, and Richard Feynman. Appendix 6 relates to one of the lighter moments in the fifth force saga.

Let me conclude by apologizing in advance to my many friends and colleagues whose contributions, for reasons of space, I have not been able to discuss here. The history covered here focuses on small parts of the story which were significant to me personally at the time for various reasons. It is my hope that in the references cited here, especially in Allan's book (Franklin 1993), our book (Fischbach and Talmadge 1999) and the accompanying Metrologia bibliography (Fischbach et al. 1992), they will receive the full credit they genuinely deserve.

6.1.1 Brief History

In tracing back the body of work now known by the generic rubric "fifth force", it is natural for historians to ask "where and how did it all begin?" The answer to "where" is relatively straightforward: it began at my home institution Purdue, motivated in large measure by the beautiful, Colella, Werner, Overhauser (COW) experiment in 1975 (Colella et al. 1975) to be discussed below, followed by sabbaticals at the Institute for Theoretical Physics (ITP, now C.N. Yang ITP) at Stony Brook (1978–1979), and at the Institute for Nuclear Theory at the University of Washington (1985–1986).

The "how" is less obvious, and consequently much more interesting. In broad outlines, to be fleshed out below, the COW experiment which tested the validity of Newtonian gravity at the quantum level, led me to pursue the question of whether we could test Einstein's theory of General Relativity (GR) at the quantum level. In considering the possibility of alternatives to GR at the quantum level, I was implicitly considering the possibility that new forces existed in nature whose presence had not yet been detected. This was the focus of much of my work at ITP-Stony Brook during my (1978–1979) sabbatical, and led to several publications (Fischbach 1980; Fischbach and Freeman 1980; Fischbach et al. 1981), including an award for an essay submitted to the Gravity Research Foundation (Fischbach and Freeman 1979).

However, my research at Stony Brook produced a surprise as a result of a collaboration with Sam Aronson related to an anomalous energy dependence he was detecting in Fermilab data on neutral kaons. When produced in strong interactions, the neutral kaon K^0 and its antiparticle $\overline{K}^0$ are distinguished by the strangeness quantum number, $S = +1$ and $S = -1$, respectively. However, when they decay via the weak interaction strangeness is not conserved, and this results in a mixing of K^0 and $\overline{K}^0$ to form two new neutral states K^0_L and K^0_S. These are eigenstates of the full Hamiltonian, and their decays follow the usual exponential decay law with K^0_L (K^0_S) being the longer- (shorter-) lived state. The K^0_L–K^0_S system is thus described by the mean lifetimes τ_L and τ_S of the two states, and their (slightly different) masses m_L and m_S. Additionally, the observation of CP-violation in the K^0_L–K^0_S system introduces the parameters η_{+-} and η_{00} which characterize, respectively, the amplitudes for the CP-violating decays $K^0_L \rightarrow \pi^+\pi^-$ and $K^0_L \rightarrow \pi^0\pi^0$. As explained below, these data hinted at the possible presence of a new force, and hence my research during the period 1979–1985 focused heavily on analyzing these data, as well as on my ongoing interest in tests of GR at the quantum level.[1]

In August 1985 I traveled with my family to the University of Washington (UW) in Seattle to spend a year-long sabbatical at the Institute for Nuclear Theory in the Department of Physics. I was accompanied by one of my three graduate students, Carrick Talmadge, for whom our eventual reanalysis of the Eötvös experiment would become the subject of his Ph.D. dissertation. I had been working up to that point with Norio Nakagawa at Purdue on possible modifications of the electron anomalous magnetic moment ($g-2$) arising from the suppression of some electromagnetic vacuum fluctuations due to the ($g-2$) apparatus (Fischbach and Nakagawa 1984a,b). (This is vaguely similar to the well-known Casimir effect.) We had submitted our latest paper for publication, but the reviewer wanted us to carry out some additional calculations, which neither of us was interested in doing. So I turned my attention instead to studying neutral kaon experiments as probes for new long-range forces.

There was no compelling evidence then (nor is there any now) for new long-range forces. Hence the best that kaon experiments (or any other experiment) can do is to constrain the magnitudes of the various parameters that would characterize such a force in a particular theory. As we discuss below, a very useful compilation of such constraints was published in 1981 by Gibbons and Whiting (GW) (1981), based on an elegant formalism developed by Fujii (1971, 1972, 1974). However, the implications of the classic 1922 paper by Eötvös, Pekár, and Fekete (EPF) were not included, and neither were the similar experiments of Roll, Krotkov, and Dicke (RKD) (1964), or Braginskii and Panov (BP) (1972), for reasons to be discussed below. The ABCF series of papers (Aronson et al. 1982, 1983a,b; Fischbach et al. 1982) written by Sam Aronson, Greg Bock, Hai-Yang Cheng, and me had yet to appear at the time of the GW paper, and hence there was additional information on possible long-range forces yet to be incorporated into an overall set of constraints on new forces. As will become clear shortly, these constraints taken together would become central in our analysis of the EPF experiment.

My sabbatical at the University of Washington had been arranged by Wick Haxton whom I knew from the time when he was an Assistant Professor at Purdue. Wick was also the colleague who brought to my attention the work of Frank Stacey and Gary Tuck (Stacey 1978, 1983; Stacey et al. 1981; Stacey and Tuck 1981, 1984) in Australia. Frank and Gary had determined the Newtonian gravitational constant G as measured in a deep mine and found that it was larger than the standard laboratory value G_0 by approximately 0.5 %–1.5 %. One possible explanation of this

[1]For further discussion of the K^0_L–K^0_S system, see Appendix 2.

difference would be a new long-range force whose influence would extend over a limited distance scale of a few kilometers. As noted in our paper (Fischbach et al. 1986a) (see also Appendix 1), such a force could be described by introducing a non-Newtonian interaction of the form

$$V(r) = -G_\infty \frac{m_1 m_2}{r}\left(1 + \alpha \mathrm{e}^{-r/\lambda}\right) \equiv V_\mathrm{N}(r) + \Delta V(r)\,, \tag{6.1}$$

where $V_\mathrm{N}(r)$ is the usual Newtonian potential energy for two masses m_1 and m_2 separated by a distance r. In a private communication from Frank Stacey he noted that the discrepancy that he and Tuck had found could then be explained if α and λ had the values

$$\alpha = -(7.2 \pm 3.6) \times 10^{-3}\,, \qquad \lambda = 200 \pm 50\,\mathrm{m}\,. \tag{6.2}$$

Upon examining the paper by GW (Gibbons and Whiting 1981) in more detail, I recognized that an interaction characterized by (6.1) and (6.2) with the indicated values of α and λ was in fact reasonably compatible with then-existing data. Moreover, the RKD and BP results, which did not appear in the GW paper, were also compatible with (6.1) and (6.2), and hence the only remaining experiment which could rule out a new force characterized by (6.1) and (6.2) was the original EPF experiment. This realization then became the proximate motivation for our reanalysis of the EPF experiment, and our discovery in the EPF paper of evidence for what shortly became known as the "fifth force".

From the preceding discussion it may seem at first surprising that the earlier (and less sensitive) EPF experiment became the focus of my attention, rather than the similar (but much more sensitive) RKD and BP experiments. The reason for this is that the later experiments achieved their increased sensitivity in part by measuring the acceleration differences of two samples to the Sun, whereas EPF compared the accelerations of their samples under the influence of the Earth's gravitational field. Using the Sun as a source allowed the daily rotation of the Earth to modulate any potential signal in a way that suppressed possible systematic errors. In contrast, EPF resorted to physically rotating their apparatus in the laboratory to suppress effects such as intrinsic twists in their torsion fibre. However, this also had the unwanted effect of disturbing the fibre itself, which RKD and BP sought to avoid.

Since the Sun was the presumed source of any possible acceleration difference of the test masses used in either the RKD or BP experiments, a force emanating from the Sun whose range λ was only of order 200 m, would have no influence on any terrestrial experiment. This follows from (6.1) by noting that $\mathrm{e}^{-r/\lambda}$ is immeasurably small when $r = 1.5 \times 10^8$ km is the Earth–Sun distance and $\lambda \approx 200$ m. Hence, the EPF experiment remained as the only potential obstacle to formulating a theory based on (6.1) and (6.2) which could potentially account for both the anomaly detected by Stacey and Tuck, and the anomalous energy dependence the kaon regeneration data that Sam, Greg, Hai-Yang, and I had published.

However, one last question remained before I was willing to commit myself and Carrick Talmadge to the time-consuming effort of re-examining the EPF experiment in detail. That was making absolutely certain that the presumed source of any effect in the EPF experiment was in fact the Earth and not the Sun. I was much more familiar with both the RKD and BP experiments because I had used their data just a year earlier in a paper co-authored with Hai-Yang Cheng, along with Mark Haugan and Dubravko Tadić (Fischbach et al. 1985). This paper, which established an interesting connection between Lorentz-Noninvariance and the Eötvös experiments, did not actually use the EPF data, but only the more sensitive RKD and BP results.

Because I do not read German I enlisted the help of Peter Buck who was a postdoc at INT from Germany. I tasked him initially with answering the question of whether EPF were comparing the accelerations of objects falling to the Earth, which he did in the affirmative. Eventually Peter's effort extended to a full-translation of the EPF paper as we describe below.

Having convinced myself that the EPF experiment was the only remaining impediment to postulating the existence of a new force capable of explaining both the anomalous energy dependence of the neutral kaon parameters, and the anomalies found by Stacey and Tuck, I set about the task of re-analyzing the EPF paper. Not surprisingly, the trajectory that began in 1975 with my focus on the COW experiment and quantum gravity, and which ultimately led through kaon physics to the EPF experiment, was more complicated than suggested by this brief outline. The remainder of this Introduction will thus be devoted to filling in these missing details, some of which were crucial in leading to our reanalysis of the EPF experiment and the fifth force hypothesis.

6.1.2 The COW Experiment and Its Impact

As noted above, in 1975 my colleagues Roberto Colella and Al Overhauser published a remarkable paper which provided much of the original motivation for my subsequent work leading to our group's reanalysis of the EPF experiment. In this paper the authors showed that one could carry out an experiment which tested the quantum behavior of neutrons in a gravitational field. Not long thereafter they were joined by Sam Werner in actually carrying out this experiment (Colella et al. 1975), now known as the COW experiment, in which they verified experimentally that the quantum-mechanical behavior of nonrelativistic neutrons in a weak gravitational field agreed with theoretical expectations based on Newtonian gravity and the Schrödinger equation. (The original apparatus is now on display in the Physics and Astronomy library at Purdue.)

This pioneering experiment had only one shortcoming from my point of view, and it is best illustrated by an anecdote that Al told relating to the time he gave a lecture on this experiment at Brookhaven National Laboratory. When he got to the conclusion that the COW results were in agreement with predictions (assuming Newtonian gravity and the Schrödinger equation), Maurice Goldhaber commented to the effect that "… of course they do, if they didn't we would never have allowed you to publish them!" The content of Goldhaber's comment was clear: since both Newtonian gravity and the Schrödinger equation have been so well tested, and that is all that is needed to derive the theoretical prediction for the COW effect, there is no way COW could have obtained any other result. Thus, although the COW experiment is a genuine test of gravity at the quantum level, it did not test gravity in a way that would provide much insight into how to formulate a truly quantum theory of gravity, a problem which remains unsolved to this day.

Al's office was just a few doors down from my own, and we talked very often about subjects of mutual interest, especially about the COW experiment and its implications. Al was convinced that the observed CP-violation in the K^0–$\overline{\mathrm{K}}^0$ system was due to some external gravity-like field, and in one conversation we had early in the "COW era" he made a comment which eventually led me to the following observation. In the Earth's gravitational field, consider the energy difference between a K^0_L and K^0_S (whose mass difference is $\Delta m = m_\mathrm{L} - m_\mathrm{S}$) over a vertical height $\hbar/c\Delta m$. This energy difference is given by $m_\mathrm{K} g(\hbar/c\Delta m)$, where $m_\mathrm{K} = (m_\mathrm{L} + m_\mathrm{S})/2$, and $g = 980\,\mathrm{cm/s^2}$. (This vertical distance is that which a virtual relativistic kaon would travel in a time $t = \hbar/c^2\Delta m$.) If we compare this energy difference to the mass-energy difference of $\mathrm{K_L}$ and $\mathrm{K_S}$, we find (Fischbach 1980)

$$\frac{m_\mathrm{K} g(\hbar/c\Delta m)}{c^2\Delta m} \approx 0.84 \times 10^{-3} \,. \tag{6.3}$$

This is tantalizingly close to the magnitude of the CP violating parameter $\mathrm{Re}\,\varepsilon/2 = (0.80 \pm 0.01) \times 10^{-3}$ (PDG 2014). Although this may be no more than a surprising coincidence, it certainly provided part of our subsequent motivation to somehow connect anomalies in the K^0–$\overline{\mathrm{K}}^0$ system with gravity via the EPF experiment.

Since kaon experiments are inherently relativistic, the suggestion of (6.3) that there could be a connection between gravity and CP-violation in the K^0–$\overline{\mathrm{K}}^0$ system led me to ask whether we could design a relativistic analog of the COW experiment. In contrast to the COW experiment itself, which only tested Newtonian gravity, such a relativistic experiment could in principle test some aspects of Einstein's General Theory of Relativity (GR) and various alternatives to GR. Stated another way, a relativistic experiment could test whether the parametrized post-Newtonian (PPN) parameters α_PPN, β_PPN, $\gamma_\mathrm{PPN}, \ldots$, which characterized the metric tensor in the weak-field limit at the macroscopic level, were the same as would describe the metric tensor at the quantum level. At the macroscopic level these parameters are defined in the terms of the components of metric tensor $g_{\mu\nu}(x)$ for a spherically symmetric geometry expressed in isotropic coordinates. To lowest order in $\Phi = GM_\odot/cr^2$,

$$\mathrm{d}s^2 = f(r)\left(\mathrm{d}x^2 + \mathrm{d}y^2 + \mathrm{d}z^2\right) + g_{00}(r)\left(\mathrm{d}x^0\right)^2 \,, \tag{6.4}$$

where

$$r = \left(x^2 + y^2 + z^2\right)^{1/2} . \tag{6.5}$$

The metric components $f(r)$ and $g_{00}(r)$ are then given by

$$f(r) = 1 + 2\gamma_{\mathrm{PPN}}\Phi + \frac{3}{2}\delta_{\mathrm{PPN}}\Phi^2 + \mathcal{O}(\Phi^3) , \tag{6.6}$$

$$-g_{00}(r) = 1 - 2\Phi + 2\beta_{\mathrm{PPN}}\Phi^2 + \mathcal{O}(\Phi^3) . \tag{6.7}$$

The utility of the PPN formalism is that it allows the predictions of various theories of gravity to be readily inter-compared in terms of a common set of PPN parameters (Will 1993). Going further, we can reproduce some classic predictions of GR at the macroscopic level without even knowing much about GR at all (Fischbach and Freeman 1980). For example, the gravitational deflection of light by the Sun can be calculated as a classical geometric optics problem by noting that a photon can be viewed as propagating in a Minkowskian space-time but with a local index of refraction

$$n(r) = [-f(r)/g_{00}(r)]^{1/2} . \tag{6.8}$$

It seemed to me that, absent such basic information, it would be difficult to make rapid progress in formulating a truly quantum theory of gravity. As but one example, this would address to some extent the question of whether gravity at the macroscopic level was merely an effective theory, where the PPN parameters were appropriate averages over some other parameters which would characterize space-time at the quantum level.

From many points of view the K^0–$\overline{\mathrm{K}}^0$ system would be an ideal choice to pursue this question because relativistic kaons exhibit interference phenomena which are clear indications of quantum behavior (Aronson et al. 1982, 1983a,b; Fischbach et al. 1982). Studying the behavior of kaons in a weak gravitational field would thus be a quantum analog of the deflection of light passing the Sun. This is the famous Eddington experiment which brought world-wide fame to Einstein by demonstrating (in modern terminology) that γ_{PPN} was indeed close to 1 as predicted by GR.

There is, however, a fundamental problem with the K^0–$\overline{\mathrm{K}}^0$ system, and it is the very feature which makes it interesting. In order to carry out an analog of the COW experiment one would have to coherently split a kaon beam in a gravitational field and then recombine the split beams after they had traveled along different paths in the field. For the low-energy neutrons which were used in the COW experiment, their de Broglie wavelengths were comparable to the silicon lattice spacing in the crystal used. Hence the lattice could coherently split the neutrons, just as it would an X-ray beam of comparable wavelength. This splitting of the neutron beam with wavelength λ then produces a phase shift $\Delta\phi$ of the two components given by

$$|\Delta\phi| = \frac{2\pi m_n^2 g \ell_1 \ell_2 \lambda}{h^2}\,, \tag{6.9}$$

where m_n is the neutron mass, $g = 980\,\mathrm{cm/s^2}$, ℓ_1 is the linear distance they travel, and ℓ_2 is the vertical separation. In the original COW experiment $A = \ell_1\ell_2 \approx 10\,\mathrm{cm}^2$ was the macroscopic area enclosed by the split beams, and this leads to a macroscopically observable signal. However, the de Broglie wavelength of a relativistic kaon is so small that splitting it via any atomic lattice is not feasible. For example, the de Broglie wavelength of a kaon with momentum $10\,\mathrm{GeV}/c$ is approximately 10^{-6} Å, which is much smaller than any atomic lattice spacing. However, the preceding discussion does not entirely preclude tests of GR at the quantum level, and an example of such an experiment is given in Fischbach (1984). Consider the process

$$\mathrm{e}^+ + \mathrm{e}^- \longrightarrow \phi(1020) \longrightarrow \mathrm{K}_\mathrm{L}^0 + \mathrm{K}_\mathrm{S}^0\,, \tag{6.10}$$

where both K_S^0 and K_L^0 can decay into $\pi^+\pi^-$, the latter by virtue of CP-violation. In the absence of gravity various symmetry arguments constrain the form of the $2(\pi^+\pi^-)$ final state. However, in the presence of gravity these final states are perturbed in a manner that could allow for a test of GR at the quantum level. The difficulty with carrying out such an experiment in practice is that for $\phi(1020) \to \mathrm{K}_\mathrm{L}^0, \mathrm{K}_\mathrm{S}^0$, the outgoing K_L^0, K_S^0 are nonrelativistic and hence this particular decay mode is not particularly useful for our purposes. By way of contrast, the K_L^0 and K_S^0 produced in the decay of $\mathrm{J}/\Psi(1\mathrm{S})$ would be sufficiently relativistic to provide a meaningful GR test in principle. However, although the final K^0–$\overline{\mathrm{K}}^0$ state is one of the dominant decay modes of $\phi(1020)$, it is only a minor decay mode of $\mathrm{J}/\Psi(1\mathrm{S})$ decay. Thus the small branching ratio for this mode (2×10^{-4}) precludes at present any meaningful test of GR using the K^0–$\overline{\mathrm{K}}^0$ (or K_L^0–K_S^0) system.

6.1.3 Stony Brook Sabbatical (1978–1979)

I had been a research associate at ITP-Stony Brook during the years 1967–1969, and I had been invited to return for my sabbatical. The decision to go on sabbatical was not an easy one for my wife Janie and me: our second son Jeremy was born prematurely in April of 1978, and the thought of moving from Indiana to Stony Brook with the very young children was not appealing. Janie and I had even talked about simply canceling our sabbatical plans entirely. But in the end Janie felt that this sabbatical was important to me, although neither of us could foresee at that time what would eventuate. We were accompanied on my sabbatical by my two graduate students, Hai-Yang Cheng and Belvin Freeman.

The previously discussed difficulty of testing GR at the quantum level, by developing an analog of the COW experiment in the K^0–$\overline{K}^0$ system, eventually led me to consider tests in atomic systems, specifically in hydrogen and positronium. Eventually this became the subject of Belvin's Ph.D. thesis. As is well known, in classical Bohr theory the velocity of an electron in the ground state of hydrogen is $\beta = v/c \approx \alpha = e^2/\hbar c \approx 1/137$. This is sufficiently large to motivate consideration of the possibility of testing GR in hydrogenic systems. My problem was that the requisite calculations involved understanding, and dealing with, the Dirac equation in GR with which I was not familiar. Although I had taught GR, relativistic quantum mechanics, and introductory field theory a number of times, I had never discussed the effects of gravity in relativistic quantum systems. Fortunately for me Fred Belinfante of our department, a noted GR expert, decided to teach GR during the Fall of 1976 prior to my sabbatical, and this included studying the Dirac equation in GR.

Much of the 1978–1979 sabbatical at Stony Brook was devoted to exploring with Belvin possible experimental tests of GR in hydrogen and positronium, using the formalism I had learned from Fred Belinfante. We showed in a series of papers (Fischbach and Freeman 1979; Fischbach 1980; Fischbach et al. 1981) that for a hydrogen atom at rest the Earth's gravitational field produced an analog of the electromagnetic Stark effect, in the sense of mixing unperturbed states of opposite parity. The energy scale for these effects is determined by a constant $\eta = g\hbar/c$, where $g = 980\,\text{cm/s}^2$ is the familiar acceleration of gravity at the surface of the Earth. Not surprisingly, $\eta \to 0$ when either $\hbar \to 0$ or $c \to \infty$, which supports our intuition that we are in fact studying a genuine GR effect at the quantum level. Since $\eta = 2.2 \times 10^{-23}$ eV at the surface of the Earth, and would only be 3.5×10^{-12} eV at the surface of a typical neutron star, prospects for directly observing GR effects in hydrogen or positronium are bleak at present. Our summary paper (Fischbach et al. 1981), written in collaboration with Wen-Kwei Cheng at the University of Delaware, made it clear how difficult it is likely to be to detect the presence of GR effects in even the most sensitive atomic systems.

Although my intention at the outset of my Stony Brook sabbatical was to devote myself primarily to testing GR in atomic systems, my research took an unexpected turn after a visit from my friend Sam Aronson, who was then in the Physics Department at Brookhaven National Laboratory, and subsequently rose to be its Chairman. Sam eventually became the Director at Brookhaven, and is the 2015 President of the American Physical Society. Sam and I had known each other from our undergraduate days at Columbia when we were both in the same philosophy of science class at Barnard taught by Daniel Greenberger. The purpose of Sam's visit was to enlist my help in a problem he was having understanding the results of an experiment at Fermilab with which he was involved, along with Val Telegi, Bruce Winstein, Greg Bock, and others. This experiment was aimed at studying the process of K_S^0 regeneration in which K_S^0 mesons could be regenerated from a K_L^0 beam by passing that beam through a target such as hydrogen, carbon, or lead. The experimental results were of interest because there was well-developed formalism

(Regge pole theory) which predicted what this energy dependence should be. (See Appendix 2 for a discussion of kaon regeneration.)

Neutral kaon regeneration is an extremely interesting phenomenon in part because it is an elegant example of quantum mechanical interference. This interference arises from the fact that both K_L^0 and the regenerated K_S^0 can decay into $\pi^+\pi^-$ (and also $\pi^0\pi^0$). The former decay is CP-violating and is hence suppressed, while the latter decay is CP-allowed but is suppressed by virtue of the fact that the regeneration amplitude is itself small. The net effect is that the decay amplitude of a neutral kaon beam into $\pi^+\pi^-$ arises from the interference between two decay processes with amplitudes which can be roughly comparable. This leads to an oscillatory behavior of the detected $\pi^+\pi^-$ amplitude which is described by a function $\cos[\Delta mt + \phi_\rho(E) - \phi_{+-}]$ where (in units where $\hbar = c = 1$) E is the laboratory energy, and ϕ_{+-} is the phase characterizing the CP-violating $K_L^0 \to \pi^+\pi^-$ decay. Knowing E and ϕ_{+-} one can then extract the desired strong interaction phase $\phi_\rho(E)$. Sam's problem was that the energy dependence he and his group were finding at Fermilab was far greater than that expected from theory (Fig. 6.1). (See Appendix 2 for more details.)

Sam and I arranged for us to meet with C.N. Yang, and during this meeting Yang agreed that Sam's data were not compatible with any model that he knew. Sam was analyzing the Fermilab data with his student Greg Bock at the University of Wisconsin, and I was accompanied on my sabbatical by my students Hai-Yang Cheng and Belvin Freeman. Since Hai-Yang had essentially finished his Ph.D. research by that time, I suggested that he and I join forces with Sam and Greg to try to understand the apparently anomalous energy dependence of the Fermilab data.

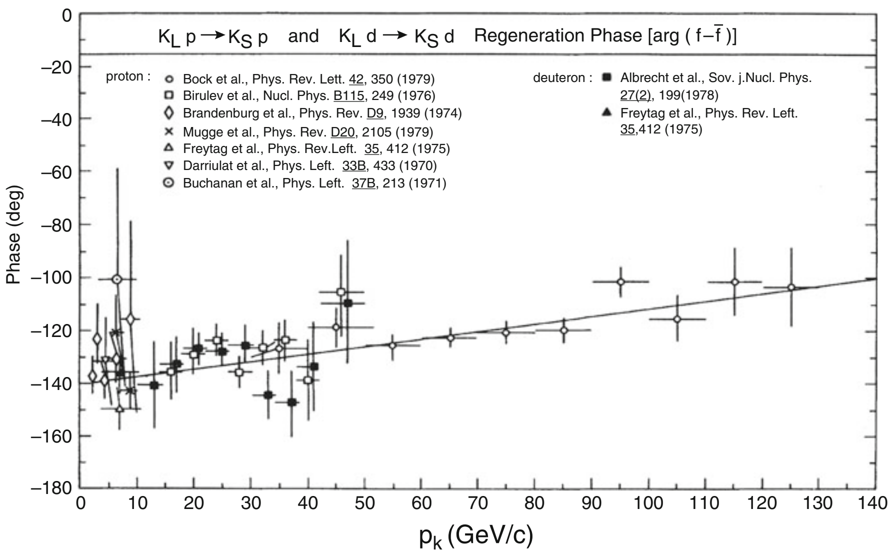

Fig. 6.1 Plot of ϕ_{21} vs. kaon momentum taken from Aronson et al. (1983a)

As it turns out the strong-interaction formalism being used to predict the regeneration phase was Regge pole theory, a subject which I had previously promised myself never to get involved with. Having no choice at this point, I immersed myself in this formalism, and eventually wrote a long appendix to one of our papers (Aronson et al. 1983a) in which we verified that Regge pole theory did in fact predict too small an energy dependence to account for the observed Fermilab data. (This discussion was sufficiently detailed that one of the reviewers of this paper commented that this appendix should have been published as a separate paper.)

Although kaon regeneration would seem to have nothing to do with the COW experiment, gravity, or the eventual search for a fifth force, a pivot point came during a meeting one day among Sam, Greg, Hai-Yang, and me. As noted above, the regeneration phase $\phi_\rho = \phi_\rho(E)$ appeared in the relevant formulas via a factor $\cos[\Delta m t + \phi_\rho(E) - \phi_{+-}]$, where $\Delta m = m_{\mathrm{L}} - m_{\mathrm{S}}$ is the $\mathrm{K}^0_{\mathrm{L}}$–$\mathrm{K}^0_{\mathrm{S}}$ mass difference, and ϕ_{+-} is the phase of the CP-violating parameter η_{+-}. The energy dependence of ϕ_ρ thus depended on assuming (as we all then did) that Δm, η_{+-} , and ϕ_{+-} were fundamental constants of nature, and hence independent of the laboratory energy of the kaon beam that we were studying. (It should be noted that measurements of these parameters are traditionally referred back to the kaon rest frame.) Hence any energy dependence of the combination $(\phi_\rho - \phi_{+-}) \equiv \Phi$ must be due to ϕ_ρ, and this energy dependence was the problem we were facing in light of our Regge pole analysis, along with the work of others.

The pivotal moment came when we started to consider the possibility that ϕ_{+-} itself was energy-dependent, and hence that the energy dependence of Φ was actually due mostly to that of ϕ_{+-}. We recognized that, as unconventional this suggestion was, such an energy dependence could arise from the interaction of the K^0–$\overline{\mathrm{K}}^0$ system with some new external field. This was not a new idea, since such an interaction had been proposed independently by Bell and Perring (1964) and independently by Bernstein et al. (1964) to explain CP-violation. However, their formalisms implied that the energy variation of the CP violating parameter $|\eta_{+-}|$ would be quite large (see below), and hence this proposal was quickly ruled out.

Nonetheless, through a study of the energy dependence of $\phi_\rho(E)$, Sam, Greg, Hai-Yang, and I had raised the idea of some sort of new long-range force. This thread would ultimately connect to the work of Stacey and Tuck, whose geophysical determination of the Newtonian constant of gravity G found an anomaly, which could also be attributed to the presence of a new force.

Eventually Sam, Greg, Hai-Yang, and I felt sufficiently confident in our analysis that we submitted a paper giving our results to *Physical Review Letters* (PRL). Our original version met with stiff resistance from PRL. Just as it looked as though we would never succeed in publishing these data, not to mention the accompanying theoretical analysis, I had an idea motivated by a Bruegel painting I had studied as an undergraduate at Columbia. In this painting, "Landscape with the Fall of Icarus", Bruegel takes the central purpose of the picture, namely depicting the story of the fall of Icarus escaping from Crete because he flew too close to the Sun, and makes it an incidental detail in an otherwise pastoral scene (Hughes and Bianconi 1967).

So incidental is Icarus' plunge into the sea, that it could easily be missed by someone not familiar with the painting. In fact, on a trip out West many years ago with my family we ended up in a motel room with this painting on the wall. Except that the painting had been cropped to allow it to fit into one of their standard size frames, with the result that Icarus was now completely missing![2]

As applied to our situation at that time, my suggestion to the group was to write a theoretical/phenomenological paper focusing on our formalism in which our actual experimental results appeared to be almost incidental. This stratagem worked, and a phenomenological paper containing our data was accepted relatively quickly by *Physics Letters*, and was published on 30 September 1982 (Fischbach et al. 1982). In the meantime, a rewritten version of our original data and analysis was submitted to PRL and accepted, and was published on 10 May 1982 (Aronson et al. 1982). The acceptance of these papers appeared to break the log jam we were confronting, and full length papers presenting our data and our phenomenological formalism appeared in back-to-back papers in *Physical Review D* (Aronson et al. 1983a,b).

There was, however, a problem remaining in trying to attribute the apparent energy dependence of the K^0–$\overline{\mathrm{K}}^0$ parameters to a new external field, namely the experimental evidence that this could not explain CP-violation. A critical turning point came on the evening of December 6, 1983. I had been asked to sit on an NSF panel charged with awarding NATO postdoctoral fellowships, and I was leaving the next morning to San Francisco to join that panel. After dinner I decided to tidy up the notes I was working on during the day as a form of relaxation. Sometime around 10 PM I made what to me was at that time a startling observation in an equation I had just written down. As noted above, it had been shown by Bell and Perring (BP) (Bell and Perring 1964), and simultaneously by Bernstein, Cabibbo, and Lee (BCL) (Bernstein et al. 1964), that if the observed CP violation was due to the interaction of the K^0–$\overline{\mathrm{K}}^0$ system with an external source mediated by a quantum ("hyperphoton") that had a spin J (in units of Planck's constant), then the magnitude of the CP-violating parameter η_{+-} should vary with the laboratory energy E (or velocity $\beta = v/c$) of the kaons as γ^{2J}, where $\gamma = E_\mathrm{K}/mc^2 = \sqrt{1-\beta^2}$ is the usual relativistic factor. Since the hyperphoton was presumed to be a vector field ($J = 1$), which was required in such a picture to produce an energy difference between K^0 and its antiparticle $\overline{\mathrm{K}}^0$, the expected energy dependence was thus γ^2. Shortly after their proposal experiments searched for a γ-dependence, but found none (De Bouard et al. 1965; Galbraith et al. 1965; Lee and Wu 1966). This was a compelling argument at the time against the hyperphoton mechanism as an explanation of the observed CP-violation. However, what I had observed in the equation I had just written was a cancellation among terms which, for the system I was analyzing, eliminated the term proportional to γ^{2J} leaving a residual term with a much smaller energy dependence. If my algebra was correct, the hypercharge mechanism as an explanation of CP-violation was now again viable.

[2]For a literary reference, see W.H. Auden "Musée des Beaux Arts".

The implications of this result were immediately obvious to me, so much so that I could not even write down the next equation, in which the canceling terms would have no longer been present. As a teenager I had played a lot of chess, and so I pictured what had just happened as if I had "checkmated" the problems associated with the hypercharge mechanism. I went to sleep and arranged to awaken at 4 AM the next morning to check my algebra in an effort to make sure that I had not committed some sign error. I proceeded to verify that my results the previous evening were in fact correct, although I had no physical understanding of why the cancellations had occurred.

Aided by many more calculations en route to San Francisco and in subsequent days, I finally realized what was going on. The hypercharge model of BP and BCL had assumed that the field was spatially constant over the size of the experiment, which would be the case if the field was of cosmological origin. However, I had been calculating the effects of a field which could vary spatially over the dimensions of the experimental system. As seen in the rest frame of the K^0–$\overline{K}^0$ system, which is the frame in which the data are typically analyzed, the kaons would see a spatially (and temporally) varying field, and this variation produced an additional γ-dependence which offset the γ^2 dependence arising from the vectorial nature in the field. The shorter the range of this field the greater the γ-dependence, and in the limit of a very short-range field described by a delta function, these two γ-dependences exactly canceled, thus eliminating the criticism of the hypercharge mechanism as an explanation of the observed CP-violation. This observation eventually made it into the invited talk I gave at the 1986 High Energy Conference at Berkeley (Fischbach et al. 1987). For a vector field A_μ with components $[\boldsymbol{A} = 0, A_0 = \sigma\delta(z)]$, which crudely simulates the effects of a short-range potential ΔV, then if the lab (x) and kaon (x') coordinate systems coincide at $t = t' = 0$, then for a boost in the z-direction the potential fA_0' seen by the kaons in their frame is given by Fischbach et al. (1987)

$$fA_0' = \gamma f\sigma\delta(z) = \gamma f\sigma\delta(\gamma\beta t') \approx f\sigma\delta(t') \; , \tag{6.11}$$

where we assume that $\beta = v/c \approx 1$ in the last step, as is appropriate for high-energy kaons. We see from (6.11) that for a potential of zero range the two sources of γ-dependence exactly offset each other, so that the potential experienced by a high-energy kaon in its rest frame is actually independent of γ.

This result had a significant influence on my thinking, since it revived the possibility that an external hypercharge field could explain both CP violation and the anomalous energy dependence we had found in the high-energy kaon data at Fermilab. As we noted in the published write up of the Berkeley talk, as the range of a putative hypercharge interaction decreases, the γ-dependence of the kaon parameters, such as η_{+-}, ϕ_{+-}, $\Delta m(K_{L,S})$ and τ_S, become "softer", possibly more in line with the gentler γ-dependence that we already reported. When we later became aware of the anomalous geophysical results from Stacey and Tuck, it thus became more plausible that a common mechanism could explain both anomalies.

6.2 Reanalysis of the EPF Experiment

As noted above, shortly after arriving in Seattle, I returned to the question of studying the implications of existing data on possible new long-range forces.

6.2.1 The Review of Gibbons and Whiting

Among the papers that had the most direct influence on our original PRL were those by Stacey and Tuck on the geophysical determination of the Newtonian gravitational constant (Stacey 1978, 1983, 1984, 1990; Stacey et al. 1981, 1986, 1987a,b,c, 1988; Stacey and Tuck 1981, 1984, 1988), and by Lee and Yang on the implications of a long-range coupling to baryon number (Lee and Yang 1955). Additionally, the review by Gibbons and Whiting (GW) in *Nature* (Gibbons and Whiting 1981) played an important role by organizing the then-existing constraints on the strength α and range λ of a putative new long-range force into the now familiar α–λ plot. Among the other experimental results, the GW α–λ plot included both those of Dan Long (1976, 1980) which claimed a deviation from Newtonian gravity, and the results of Riley Newman's group (Spero et al. 1980) which found no discrepancy. A subsequent experiment by Newman's group (Hoskins et al. 1985) further strengthened the limits on non-Newtonian gravity over laboratory distance scales, and these generate the limit labeled "Laboratory" in Fig. 6.9 below.

However, what is of interest from a historical point of view is that the GW review did not include any constraints on α and λ arising from the EPF experiment, or from the subsequent RKD (Roll et al. 1964) or BP (Braginskii and Panov 1972) versions, as we have already noted. Although not explicitly stated by GW, this omission was presumably due to the recognition that for these experiments α would depend explicitly on the composition of the samples. Specifically, for a long-range force arising from a coupling to baryon number B, α would be given by

$$\alpha = -\left(\frac{B_1}{\mu_1}\right)\left(\frac{B_2}{\mu_2}\right)\xi_{\mathrm{B}} \tag{6.12}$$

where $B_{1,2}$ are the baryon numbers of the interacting objects, and $\mu_{1,2}$ the corresponding masses in units of the $^1H_1$ mass (see Appendix 1). In this picture ξ_{B} is the universal constant which, for composition-dependent experiments, plays the same role as α for composition-independent experiments. Evidently, an analogous equation would apply if the putative long-range force coupled to lepton number (L) or isospin (I), and hence each of these possibilities would generate different constraints on the corresponding constants ξ_{L} and ξ_{I}.

As is clear from the above discussion, the phenomenology of composition-independent experiments is qualitatively different from that of composition-dependent experiments, as we explore in more detail in Appendix 1. Had the

GW review been extended to include composition-dependent experiments, the implications of the EPF experiment might have been considered earlier.

6.2.2 Description of the EPF Experiment

The EPF experiment can be thought of as a descendent of the Guyòt experiment, which is in turn a descendent of the Newton pendulum experiment as described in (Fischbach and Talmadge 1999, p. 124). The purpose of Newton's experiment was to search for a possible difference between the inertial mass m_{I} of an object and its gravitational mass m_{G}, when the object is suspended from a fiber of length ℓ in the Earth's gravitational field. If θ denotes the angular displacement of the fiber from the vertical, the differential equation describing its motion is

$$m_{\mathrm{I}}\ell\frac{\mathrm{d}^2\theta}{\mathrm{d}t^2} + m_{\mathrm{G}}g\sin\theta = 0\,. \tag{6.13}$$

For small displacements the oscillation period T is then given by

$$T \approx 2\pi\sqrt{\frac{\ell}{(1+\kappa)g}}\,, \tag{6.14}$$

where $m_{\mathrm{G}}/m_{\mathrm{I}} \equiv 1+\kappa$. By comparing the periods T_1 and T_2 of two masses of different composition Newton was able to set a limit on $\Delta\kappa_{1-2} \equiv \kappa_1 - \kappa_2$ from

$$\Delta\kappa_{1-2} \approx -\frac{2(T_1 - T_2)}{T}\,. \tag{6.15}$$

Newton found $|\Delta\kappa| \lesssim 1/1000$, a result which was later improved upon by Bessel who obtained $|\Delta\kappa| \lesssim 1/60{,}000$. In the Guyòt experiment the normal to the surface of a pool of mercury was compared to the normal of masses of different composition suspended over the mercury. Note that all of these experiments utilize objects suspended from fibers, and variants of this technology continue to the present as the source of the most sensitive limits on $\Delta\kappa$.

In the EPF experiment several balances were used, one of which is depicted in Fig. 6.2. What will be particularly relevant in the ensuing discussion are these features: the triple-layer walls for thermal protection, and the thermometers riveted to the apparatus, which attest to the concern of EPF about thermal influences. Additionally, the sample to be tested and the Pt standard are located at different elevations in the Earth's gravitational field, making this apparatus particularly sensitive to gravity gradients. EPF corrected for gravity gradients by taking various differences and ratios of their measured quantities.

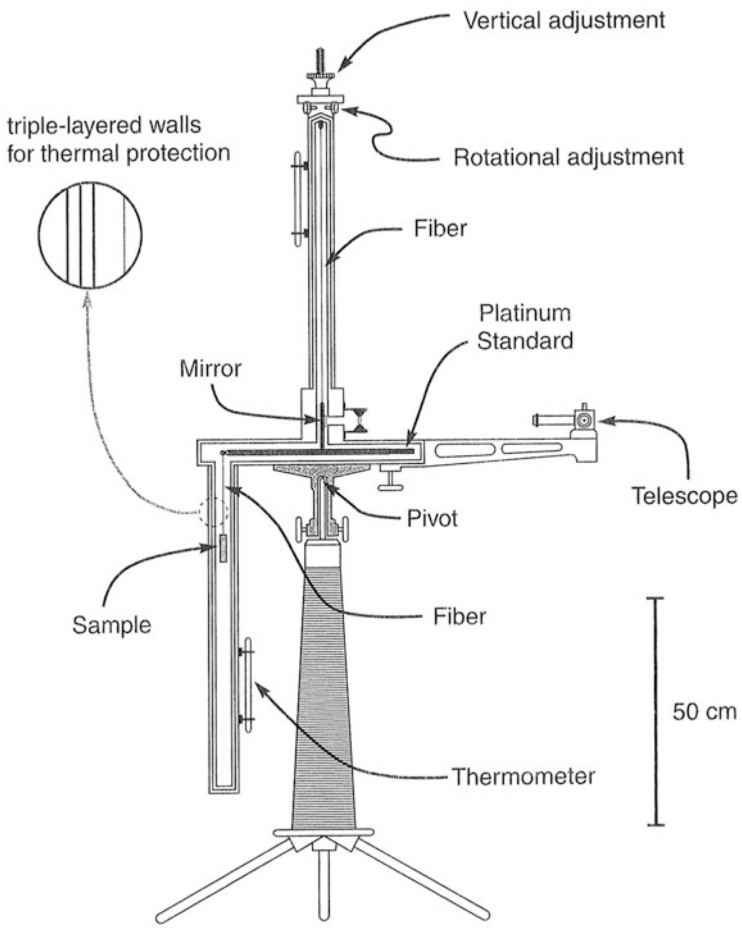

Fig. 6.2 EPF experiment apparatus (Fischbach and Talmadge 1999, p. 133)

6.2.3 Evaluation of B/μ for the EPF Samples

Late in September of 1985 Carrick and I sat down to evaluate the baryon number-to-mass values B/μ for the EPF samples. At this point we were using the data EPF compiled in the table on p. 65 of their paper, in which the accelerations of various test masses were compared to those of a Pt standard. With my limited knowledge of German I knew enough to discern what the samples were, but not enough to recognize at that time that these were not the actual raw data that EPF had measured (see below). For copper, water, and magnalium (a magnesium–aluminum alloy) the compositions were well known, and hence it was straightforward to calculate the corresponding B/μ. Since I had done such calculations in connection with my previously discussed paper connecting Lorentz invariance and the EPF experiment

(Fischbach et al. 1985), Carrick had no problem understanding my explanation of what to do. At that point, I left the calculations to Carrick, and took off with my family for a weekend of hiking in the mountains.

6.2.3.1 The Copper Sulfate Datum

By Monday, Carrick had analyzed three of the EPF data points. Surprisingly, when the results for the acceleration difference in each pair of samples ($\Delta\kappa$ in the EPF notation) were plotted against the difference in the baryon number-to-mass ratio [$\Delta(B/\mu)$ in our notation], the three points fell along a common sloping line, as would be expected if there did in fact exist a new long-range force whose source was baryon number or hypercharge. Of course, this was hardly compelling evidence for a new force, particularly since the data (and associated errors) that we were using were those presented by EPF in their table on p. 65 of their paper, and had large uncertainties. As I shall discuss below, the error bars on their data were artificially large, which made it rather more likely that a satisfactory fit could be obtained with three points.

We next agreed to analyze the copper sulfate datum. Carrick returned to his office, but when he reappeared in mine he was clearly dejected. The copper sulfate datum did not fall along the line determined by the previous three points, and the best fit to what were now four points was no longer even minimally suggestive of anything interesting. Even though we had no "right" to be despondent, we both clearly felt a sense of loss. (I remember thinking at the time of the biblical story of Jonah and the shade tree.) Although Carrick was always extremely careful, and rarely made even small mistakes, I felt obliged to go over his calculation just to make sure he had not slipped up. We began with me asking him what the chemical formula was for copper sulfate, and he told me (correctly) $CuSO_4$. As a high school student I had become fascinated with chemistry, and entered Columbia in 1959 as a chemistry major. No sooner had Carrick told me the formula he used for copper sulfate, I recalled that the familiar blue crystals that we associate with copper sulfate contain water of hydration. As would be both poetic and prophetic for what would become known as the fifth force, I guessed that the blue crystals existed in the pentahydrate form, $CuSO_4 \cdot 5H_2O$.

My interest in chemistry had been sparked in part by my uncle William Spindel, who had been at various times a professor at Rutgers University and Yeshivah University. For my 15th birthday he rewarded my interest in chemistry with a gift of the 38th (1956–1957) edition of the CRC Handbook of Chemistry and Physics, and it was with me during my sabbatical at the University of Washington (UW). I reached for it and turned to page 516, and there it was: the blue triclinic crystals were indeed $CuSO_4 \cdot 5H_2O$. I asked Carrick to go back and recalculate the copper sulfate datum assuming that the sample was in fact the blue crystals. He returned about an hour later beaming: using the correct formula, $CuSO_4 \cdot 5H_2O$ now fit beautifully on the same straight line determined by the previous three points. As I looked at his graph I felt an adrenaline rush which was my body's way of telling

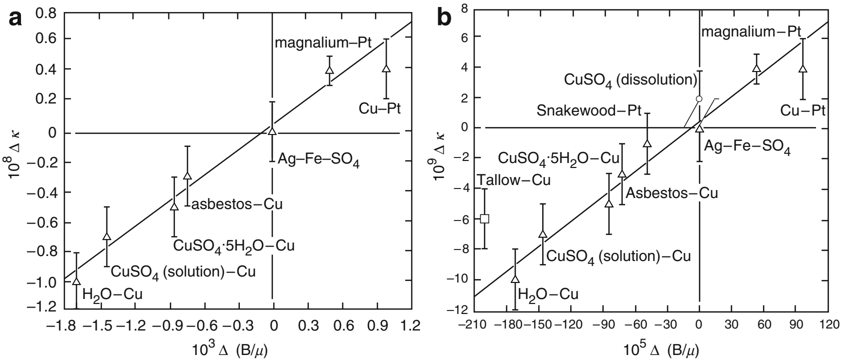

Fig. 6.3 Dependence of Eötvös parameter on baryon number: (**a**) is from Fischbach et al. (1986a) and (**b**) is Fig. 2 of Fischbach et al. (1988)

me that we were seeing an interesting effect. From that point on I felt convinced that the remaining EPF data would fall along the same line, and they did (see Fig. 6.3).

In hindsight Carrick and I were lucky that the copper sulfate datum was the 4th to be analyzed, and not among the first or last three. Had it been among the first three there would have been at the outset no obvious pattern, and we might have quit the analysis of the EPF paper at that point. Had it been among the last three, by which time a pattern would have been evident, we might simply have viewed the (incorrect) result obtained as an outlier, and not bothered to establish its correct formula. But having the correct formula for copper sulfate was important because it led to the recognition that, surprisingly, platinum and copper sulfate had very nearly identical B/μ values, although they differ in every other known physical attribute. Interestingly, the EPF data show that they have very nearly the same acceleration in the Earth's gravitational field. Is this an extraordinary coincidence, or perhaps another hint of a new interaction? The significance of this observation will be discussed in Sect. 6.2.6.

Although EPF explicitly state that they used "crystallized copper sulfate" (Szabó 1998, p. 2), we did not have the translation available to us at that time, and hence the form of copper sulfate remained an issue for us until we resolved it to our satisfaction as described below.

With some help from colleagues at UW we decided to show that even if EPF had started with the anhydrous form of $CuSO_4$, which is a whitish powder, that in the course of their experiment they would have ended up with $CuSO_4 \cdot 5H_2O$ due to absorption of water from the atmosphere. We began by heating a sample of blue crystals for several hours to drive out the water, and then literally ran to another room to weigh the sample. Running was necessary since this was a rainy period in Seattle, and the ambient humidity was sufficiently high that the sample started to turn blue immediately while we were en route to weighing it. We repeatedly weighed the sample over the next few weeks, and found that the sample—initially $CuSO_4$—rapidly absorbed water, and asymptotically approached a composition $CuSO_4 \cdot 4.7H_2O$. Had EPF actually started with $CuSO_4$ rather than with $CuSO_4 \cdot 5H_2O$, they would have found their sample mass increasing in time, which would have thwarted their attempt to accurately measure the acceleration of this sample.

6.2.3.2 Other EPF Samples

We next turned our attention to snakewood, which is an exotic dense wood whose uses include violin bows and other musical instruments (Fischbach et al. 1988).[3] We succeeded in obtaining samples of snakewood from a local instrument maker, Alex Eppler, and confirmed that they were in fact snakewood through the U.S. Forest Products Laboratory. My hosts, the Institute for Nuclear Theory at UW generously agreed to underwrite the cost of a chemical analysis of snakewood, and when the results of this analysis were used to compute B/μ for snakewood we found a surprise: notwithstanding the obvious physical difference between snakewood and more familiar woods, the resulting value of B/μ was virtually identical to that of its main component, cellulose $[(C_6H_{10}O_5)_x]$. Moreover, this would be true for all of the woods we analyzed (Fischbach et al. 1988, Table IX). Carrick thought that it would be amusing to connect the disciplines of forestry (trees) and quantum physics (B/μ) by compiling B/μ for 20 types of wood. This table made it into his Ph.D. thesis, and (to my great surprise and his delight) got into our summary paper in *Annals of Physics* published in 1988 (Fischbach et al. 1988).

The last sample we addressed was *talg* (tallow, fat, suet, ...) whose composition could vary widely depending on (among other issues) its water content. (When I visited Stanford on November 13, 1986 to give a talk about our paper, Bill Fairbank noted that Dicke had erroneously translated *talg* as talc, which is actually *talk* in German.) The best we could do was to estimate B/μ for typical animal fat, and not surprisingly, this datum appears as somewhat of an outlier on the line determined by the other samples.

6.2.3.3 The Ag–Fe–SO_4 Datum

Among the pairs of materials whose accelerations were compared by EPF were the reactants before and after the chemical reaction

$$Ag_2SO_4 + 2FeSO_4 \longrightarrow 2\ Ag + Fe_2\ (SO_4)_3. \qquad (6.16)$$

[3]The 2003 Summer catalog from Fahrney's in Washington, D.C., featured the Faber-Castell 2003 Pen-of-the-Year crafted in snakewood, which it characterized as "a beautiful and costly wood often used for violin bows and works of art." The pen was priced at \$ 790.

EPF noted that their interest in this process was motivated by an earlier paper in which Landolt suggested the presence of some anomaly. At first glance this datum would seem uninteresting in the present context, since the chemical constituents before and after the reaction are evidently identical. Thus it would seem unsurprising that EPF found $\Delta\kappa = (0.0 \pm 0.2) \times 10^{-9}$ for this pair, i.e., the expected null result.

However, there is much that can be learned from this datum as was pointed out to me in a personal communication from Clive Speake. To begin with the Landolt reaction produces Ag which precipitates out of the original solution. Clive estimated that had there been no correction for differences in the centers-of-mass of the reactants, then EPF should have found $\Delta\kappa = +19 \times 10^{-9}$ instead of their published null result quoted above (Fischbach et al. 1988, p. 34). We can infer from Clive's astute observation that EPF clearly understood this problem and must have taken the proper steps to deal with it. This is, after all, not surprising given that Eötvös was arguably the world's leading expert at that time on gravity gradients, and that his torsion balances were specifically designed to measure gravity gradients. Further analysis of this datum can be found in Fischbach et al. (1988), which also discusses the implications of the null result for a possible magnetic influence on the EPF apparatus.

Unfortunately the details of how EPF corrected for either gravity gradients or magnetic effects do not appear in their published paper. As we have noted above, the introduction to the EPF paper states that the current version represents a "considerable abridgement" of the original size of this work. It is reasonable to presume that the original draft, which Eötvös himself prepared, might have included a more detailed discussion of this datum.

The practical impact of this datum in the earliest days following publication of our original work was significant—at least to me. It indicated that EPF must have paid careful attention to a variety of potential problems which could have produced spurious non-null signals, along the lines first suggested by Dicke. My confidence in the validity of the EPF data further increased following my visit to Hungary in 1988, which included a visit to ELGI (the Geophysical Observatory in Tihany) where I had the opportunity to examine some of the Eötvös balances in detail. The sketch on p. 133 of Fischbach and Talmadge (1999) shows the presence of thermometers which were attached to the balance, presumably to mitigate the effects of temperature fluctuations, but were not discussed in the EPF paper. A more detailed discussion of my visit to Hungary is given in Sect. 6.3.7.

6.2.4 Translation of the EPF Paper

The EPF paper was written in German. However, since I know very little German it would have been difficult for me to embark on an analysis of that paper but for the fact that their results were summarized in a convenient table on p. 65 of the original paper Eötvös et al. (1922) and Szabó (1998, p. 295) (see Fig. 6.4). In that table the data are presented in the form of the acceleration differences of the various

	$\kappa - \kappa_{pt}$
Magnálium	$+0{,}004 \cdot 10^{-6} \pm 0{,}001 \cdot 10^{-6}$
Schlangenholz	$-0{,}001 \cdot 10^{-6} \pm 0{,}002 \cdot 10^{-6}$
Kupfer	$-0{,}004 \cdot 10^{-6} \pm 0{,}002 \cdot 10^{-6}$
Wasser	$-0{,}006 \cdot 10^{-6} \pm 0{,}003 \cdot 10^{-6}$
Kristall. Kupfersulfat	$-0{,}001 \cdot 10^{-6} \pm 0{,}003 \cdot 10^{-6}$
Kupfersulfatlösung	$-0{,}003 \cdot 10^{-6} \pm 0{,}003 \cdot 10^{-6}$
Asbest	$+0{,}001 \cdot 10^{-6} \pm 0{,}003 \cdot 10^{-6}$
Talg	$-0{,}002 \cdot 10^{-6} \pm 0{,}003 \cdot 10^{-6}$

Fig. 6.4 Table of results of the EPF experiment taken from p. 65 of Eötvös et al. (1922)

test samples compared to a platinum standard (this is denoted as $\kappa - \kappa_{\mathrm{Pt}}$ in their notation). Following our analysis of the $CuSO_4 \cdot 5H_2O$ datum discussed above, the remaining samples did indeed fall along a common straight line. This was obviously an exciting and surprising result, and so I set out to write this up for PRL.

As noted above, it was critical to confirm that EPF were measuring the acceleration differences to the Earth in each pair of materials. This would ensure that the non-null EPF effect would not conflict with the null results from the more sensitive experiments of Roll, Krotkov, and Dicke (RKD) (1964), and that of Bragniskii and Panov (BP) (1972), which compared the accelerations of test samples to the Sun. To this end I enlisted the help of Peter Buck who was a postdoc from Germany at the Institute for Nuclear Theory, where I was. I asked Peter to initially read just enough of the EPF paper to confirm that they were measuring accelerations to the Earth, which he did. This point is noted explicitly on the first page of our PRL (Fischbach et al. 1986a).

As the PRL draft was proceeding I decided one day to page through the EPF paper to see what I could glean from it. Notwithstanding the fact that I could not read German, I was able to discern that there were results in the body of the paper that did not appear in the summary I had been using. Working with Peter Buck, I eventually came to the understanding that the results tabulated on p. 65 of the EPF paper, were not the raw results from their experiment. Interestingly, the results that appeared in the body of the paper were more statistically significant than those appearing in the table, in the sense that the deviations from the expected null results were systematically larger than for the tabulated results. As I discuss below, $(\kappa - \kappa_{\mathrm{Pt}})$ for water was $-(6 \pm 3) \times 10^{-9}$, which is a 2 standard deviation (2σ) effect, whereas the original $(\kappa_{\mathrm{water}} - \kappa_{\mathrm{Cu}})$ datum given on p. 42 of the EPF paper is $-(10 \pm 2) \times 10^{-9}$ which is a 5σ effect.

My "discovery" of the results in the body of the EPF paper made it clear that we had to understand what EPF had actually done in greater detail, and this necessitated translating the entire paper from German into English. Fortunately I was able to assemble a team at the Institute for Nuclear Theory to carry out this task. In addition to Peter Buck, the team consisted of J. Achtzehnter, M. Bickeböller, K. Bräuer and G. Lübeck, aided by Carrick who knew some German. From the translation it became clear that the entries in the table were obtained by combining the actual raw results in the body of the paper in such a way as to infer a comparison of the various samples to Pt (Fischbach et al. 1988, p. 14). Using water as an example the water datum was inferred by writing

$$\kappa_{\rm water} - \kappa_{\rm Pt} = (\kappa_{\rm water} - \kappa_{\rm Cu}) + (\kappa_{\rm Cu} - \kappa_{\rm Pt}) \ , \tag{6.17}$$

which, when numerical values are inserted, gives

$$\begin{aligned}(-10 \pm 2) \times 10^{-9} + (4 \pm 2) \times 10^{-9} &= \left(-6 \pm \sqrt{2^2 + 2^2}\right) \times 10^{-9} \\ &= (-6 \pm 3) \times 10^{-9} \ . \end{aligned} \tag{6.18}$$

As can be seen from this example, the effect of combining their raw data in such a way as to infer a comparison of each sample to Pt reduced the statistical significance of the quoted result. Since this was systematically true for the remaining data points as well, my initial response was to wonder whether the correlation between $\Delta\kappa$ and $\Delta(B/\mu)$ that had emerged from the table was to a large extent an artifact of the inflated uncertainties in the tabulated $(\kappa - \kappa_{\rm Pt})$ values (Fig. 6.5).

The content of (6.17) and (6.18) was noted in footnote 13 of our original PRL. Although not discussed further at the time, we privately considered the possibility that Pekár and Fekete had presented the data as they did, referenced to Pt, in order to minimize any suggestion of a conflict with the Weak Equivalence Principle (WEP). The WEP was at the heart of Einstein's General Theory of Relativity published in 1915 (Will 1993), and confirmed following Eötvös' death on April 8, 1919 during the solar eclipse of May 29, 1919. It was thus plausible to assume that Pekár and Fekete were responsible for presenting their data as they did on p. 65 of their paper. However, following the publication of our PRL I received a letter from Wilfred Krause in which he attached a letter written by Eötvös around 1908 (since published in Krause 1988). This letter contains essentially the same summary

Materials compared	Page quoted	$10^8\Delta\kappa$	$10^3\Delta(B/\mu)$
Cu-Pt	37	$+0.4 \pm 0.2$	$+0.94$
Magnalium-Pt	34	$+0.4 \pm 0.1$	$+0.50$
Ag-Fe-SO_4	39	0.0 ± 0.2	0.00
Asbestos-Cu	47	-0.3 ± 0.2	-0.74
$CuSo_4 \cdot 5H_2O$-Cu	44	-0.5 ± 0.2	-0.86
$CuSO_4$(solution)-Cu	45	-0.7 ± 0.2	-1.42
Water-Cu	42	-1.0 ± 0.2	-1.71
Snakewood-Pt	35	-0.1 ± 0.2	?
Tallow-Cu	48	-0.6 ± 0.2	?

Fig. 6.5 Table from Fischbach et al. (1986a)

of the EPF data as would later appear in the published EPF paper. As Krause notes "...the idea of referencing all data to platinum was familiar to Eötvös, and not introduced after his death by Pekár and Fekete." Krause speculates that "...Eötvös planned new measurements under conditions of reduced man-made mechanical noise, an undertaking which eventually had been hampered by World War I." These planned new investigations are in fact referred to at the beginning of the EPF paper. However, as we discuss below, to the best of our knowledge the correlation between their measured values of $\Delta\kappa$ and the non-classical quantities $\Delta(B/\mu)$, cannot be accounted for by any classical effect such as "mechanical noise".

Armed with our translation Carrick and I went through the EPF paper and replotted their results using the data presented in the body of the paper. Happily, the effect of using the original data to plot $\Delta\kappa$ versus $\Delta(B/\mu)$ was to increase the statistical significance of the slope in this plot to 8σ, which was a dramatic non-null result. To ensure that readers of our paper who were interested in reproducing our plot used the correct data, we decided to cite in Table I of our paper the page in the original EPF paper where each datum was listed.

In 1998, which was the 150th anniversary of the birth of Eötvös (July 27, 1848), the Eötvös Roland Geophysical Institute (ELGI) of Hungary published a volume entitled "Three Fundamental Papers of Roland Eötvös", one of which was the EPF paper, and we were invited to contribute our translation to this volume, which was published along with the original German paper (Szabó 1998). Carrick and I revisited our original translation, with the goal of making it more readable to modern researchers while at the same time adhering as closely as possible to the original text. Significantly, this translation corrects a number of typographical errors in the original EPF paper. These were uncovered by Carrick who carefully checked their final results against the raw torsion balance data presented by EPF. These corrections are identified in various footnotes in the text of the translation, and are distinguished from the footnotes present in the original EPF paper.

6.2.5 The Refereeing Process

Our paper was received by PRL on November 7, 1985. At that time the leading experts in the world on the Eötvös experiment were Robert Dicke at Princeton and V.B. Braginskii at Moscow State University. It was thus natural to assume that Dicke would be one of the referees, and he was. Normally the referees at PRL (and at most other physics journals) are anonymous, but Dicke chose to identify himself through a message he sent directly to me on November 20 (see Appendix 3). In that message he raised the possibility that the EPF data could be explained in terms of conventional physics, and asked us to reanalyze the EPF data to test his suggestion. Specifically, Dicke began by noting that the brass containers in which the EPF samples were contained were of different lengths, and hence had different cross-sectional areas. Thus if there were a thermal gradient present in the vicinity of the EPF apparatus there could arise an air current, and this could lead to a differential force on the two samples being compared in each pair. Given that the various samples used by EPF had very nearly the same masses, it follows that samples of higher density were contained in cylinders of smaller volume and hence of smaller surface area, owing to the fact that they had similar diameters (Fischbach et al. 1988, p. 48)). The Dicke model, later elaborated upon by Chu and Dicke (1986), provided a nice pedagogical example of how a purely conventional mechanism could have produced a differential signal in the EPF experiment which depended on a property of the samples, specifically $1/\rho$, where ρ is the sample density (Fischbach et al. 1988, p. 49).

Dicke's message to us was gracious and indicated that he was inclined to accept our paper once we addressed his question. Carrick and I set about immediately to analyze Dicke's model. Leaving aside the details of exactly how such a mechanism might work, which are discussed in detail in Fischbach et al. (1988), the simple question at that time was whether any such correlation actually existed. Carrick plotted the data, which are exhibited in Fig. 7 of Fischbach et al. (1988) (see Fig. 6.6). It was immediately clear that the fit was quite poor, with the snakewood–Pt datum falling far off the best-fit line. We conveyed this result to Dicke on November 27 (Appendix 3), and eventually suggested that a note be added to our paper presenting this result. He agreed, and recommended to the PRL editors to allow us to include such a note. The editors agreed even though its inclusion would

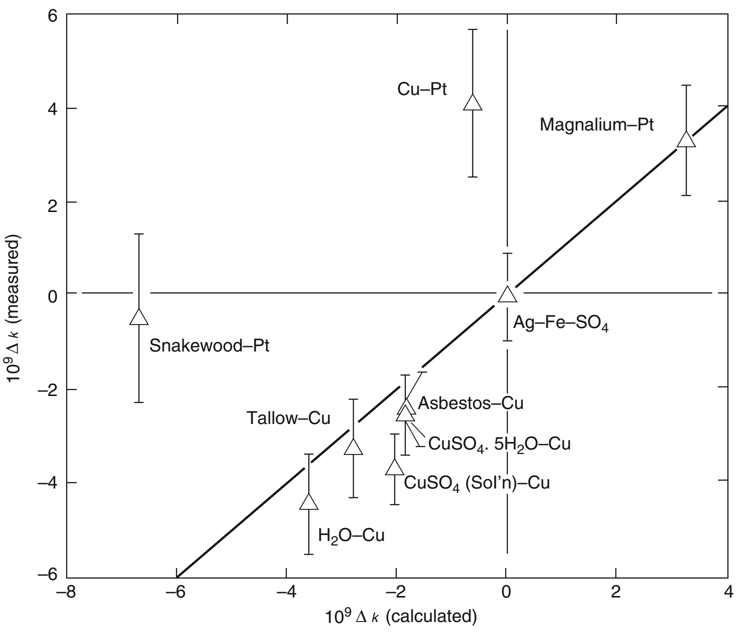

Fig. 6.6 Figure 7 from Fischbach et al. (1988)

lengthen our paper beyond the maximum allowed by PRL at that time.[4] In that note we observed that the failure of this model, in contrast to one based on B/μ as the charge, was

> [...] a consequence of two special properties of B/μ: (6.1) it has an anomalously low value for hydrogen, and (6.2) it has a maximum near Fe and is lower toward either end of the Periodic Table.

As noted above, the question raised by Dicke was later elaborated upon in a Comment published in PRL (Chu and Dicke 1986), to which we responded in Fischbach et al. (1986b). Surprisingly, this exchange of short comments was picked up by the *New York Times* in a story "Physicists Challenge Theory of a 'Fifth Force' beyond Gravity," by John Noble Wilford that appeared on October 18, 1986.

Considering the fact that our PRL was suggesting the presence of a new force in nature it may seem surprising that the refereeing process went as smoothly as it did.[5] I would identify three likely reasons for this. Most significantly, our reanalysis of the EPF experiment did not challenge the work of anyone who was still alive. In fact the only earlier work which our PRL may have called in question was that of Renner (1935), which had been previously criticized by Dicke (1961) and Roll et al. (1964). Furthermore, we took pains to note in our paper that the experiment of Roll, Krotkov, and Dicke (RKD) (1964), and that of Braginskii and Panov (BP) (1972), would not have been sensitive to a new force whose range was of order 1 km, since both of these experiments measured the accelerations of pairs of materials to the Sun. Hence any evidence arising from our reanalysis of the EPF experiment suggesting a new intermediate range force would not contradict the more precise RKD and BP experiments.

The second feature of our original PRL paper, which may have aided its rapid acceptance, was the recognition that various theories predicted the existence of new long- or intermediate-range forces. As we have noted previously, our original PRL paper was motivated in part by the elegant 1955 paper by Lee and Yang (1955), who used the EPF paper to set limits on a long-range force coupling to baryon number. Additionally, one of our primary motivations was the geophysical determination of the Newtonian gravitational constant G by Stacey and Tuck (1981) and Holding and Tuck (1984) which had been motivated in turn by an elegant and prescient paper by Fujii dealing with modifications of Newtonian gravity (Fujii 1971). In recent years theories based on supergravity, supersymmetry, and string theory have produced many candidates for new macroscopic fields, which explains in part the continuing

[4] In contrast, when a similar situation arose with respect to a story about our work in *National Geographic*, the editors insisted that their word count limit be strictly enforced, as discussed in Sect. 6.4.3.

[5] One measure of this surprise is a published comment from Lawrence Krauss, then a young assistant professor at Yale (Krauss 2008): "I reacted with surprise that the paper [our PRL] had survived the refereeing process, which at the time had very strict self-imposed requirements of general interest, importance, and validity." See also Sect. 6.4.2.

interest in fifth force tests, specifically, tests of both the weak equivalence principle and the gravitational inverse square law.

The third factor which contributed to the relatively smooth referee process was the fortunate choice of reviewers. As noted above, Robert Dicke, the towering figure in the field, was both insightful and gracious, and his recommendation to publish our paper no doubt carried great weight with the editors. At that time I did not know who the second referee was. Only later did I learn from Vern Sandberg (who had been at Los Alamos at the time) that he was the second referee. Vern and I have had several conversations about our paper, which he clearly read quite carefully. He is by all accounts a very conscientious reviewer, and he also shares my view of the refereeing process. In my case it is derived in part from a conversation I overheard as a young faculty member in which Francis Low of MIT said something to the following effect to a colleague: when reviewing papers he gives authors the benefit doubt, because publication is cheap, but not on grant proposals because the available pot of money is limited. The actual reports from Dicke and Sandberg are given in Appendix 4, along with the correspondence with PRL.

6.2.6 An Alternative Explanation

As noted above, one of the arguments against an explanation of the EPF results as an "environmental" effect, as had been proposed by Dicke (see Sect. 6.2.5), was the fact that the EPF correlation depended on the value of B/μ for each sample and this was a non-classical parameter. One way of expressing the implication of this fact is the observation that two of the materials employed by EPF were Pt ($B/\mu = 1.00801$), and $CuSO_4{\cdot}5H_2O$ ($B/\mu = 1.00809$) which were very nearly equal. There is no conventional physical quantity (e.g. density, electrical conductivity, etc.) which is the same for these two materials. By combining the EPF data for Pt–Cu and $CuSO_4{\cdot}5H_2O$–Cu, we can find (Fischbach et al. 1988)

$$\frac{\Delta(B/\mu)_{\text{Cu-Pt}}}{\Delta(B/\mu)_{\text{CuSO}_4\cdot 5\text{H}_2\text{O-Cu}}} = \frac{+94.2 \times 10^{-5}}{-85.7 \times 10^{-5}} = -1.10\,, \tag{6.19}$$

$$\frac{\Delta\kappa_{\text{Cu-Pt}}}{\Delta\kappa_{\text{CuSO}_4\cdot 5\text{H}_2\text{O-Cu}}} = \frac{(+4.08 \pm 1.58) \times 10^{-9}}{(-4.03 \pm 1.33) \times 10^{-9}} = -1.01 \pm 0.51\,. \tag{6.20}$$

The close agreement between the measured $\Delta\kappa$ ratios, and the theoretically expected values based on the $\Delta(B/\mu)$ ratios, appears to provide strong support to the view that EPF were seeing an unconventional effect uniquely tied to a non-classical quantity such as baryon number or hypercharge. (We recall that baryon number and hypercharge were only introduced into the physics literature many years following publication of the EPF paper.)

To our great surprise this conclusion would be challenged by a 1991 paper that Carrick received from PRL to review. The authors were Andrew Hall and Horst

Armbruster who were then, respectively, a graduate student and faculty member at Virginia Commonwealth University in Richmond, Virginia. The primary driving force (and first author) was Hall, who was claiming in this paper that he had constructed a phenomenological "charge" which could explain the EPF data just as well as our hypercharge hypothesis. This "charge" Q depended on the intrinsic nuclear spins of the EPF samples and was defined by

$$Q = M\delta \,, \qquad \delta = \begin{cases} 1 & \text{if } J > 0 \,, \\ 0 & \text{if } J = 0 \,, \end{cases} \tag{6.21}$$

where M is the mass of the nucleus, and J is its nuclear spin (in units of $\hbar$).

Carrick and I greeted the Hall/Armbruster (H/A) paper with a great deal of skepticism. We were no doubt biased in our view that B/μ was not only the correct "charge" to explain the EPF data, but that it was also unique by virtue of the preceding discussion. Additionally, we could not understand how a "charge" which depended on nuclear spin could be relevant in an experiment utilizing samples which were unpolarized, as was presumably the case for the EPF samples. Nonetheless we were determined to take this paper seriously, and so we decided to verify Hall's claim that Q given by (6.21) could in fact explain the EPF data.

As it turned out I had a dinner engagement the day Carrick received the paper, but I arranged with him to return to his office around 10 PM, at which time we would then work on the H/A paper as long as needed. When I returned we divided the work as follows: Carrick would modify his existing code to allow us to compute Q for the EPF samples. While he was doing that I busied myself with the task of determining the nuclear spins of the elements in the EPF samples from various tables. By midnight we were able to compute the analog of our plot of $\Delta\kappa$ versus $\Delta(B/\mu)$, where $\Delta(B/\mu)$ was now replaced by ΔQ for each pair of samples. Carrick hit the `ENTER` key on his NeXT computer, and instantly a figure appeared on his screen which looked almost indistinguishable from our published figure (Fig. 6.7). Although the relative positions of the various data points were different, the overall quality of the fit was as good as ours using $\Delta(B/\mu)$.

It would be difficult to overestimate the significance of the H/A paper, had it turned out to be correct. The design of any experiment can depend critically on the specific theory being tested. For example, to test the B/μ theory we had advanced in our original paper, it was advantageous to compare samples widely separated in the periodic table, such as Al–Au, Al–Pt, Be–Cu, and so on. For the purpose of repeating the EPF experiment, the nuclear spins of the sample would be irrelevant in a B/μ picture, whereas they would evidently have been relevant in the actual EPF experiment in the H/A framework. The fact that experiments were framed in terms of specific theories is a recurring theme in the history of the fifth force, as we shall see.

Carrick (and I) accepted the H/A paper for publication in PRL. However, their paper never appeared in PRL, presumably because it must have been rejected by another referee. (Under the policy followed by PRL—at least at that time—a split

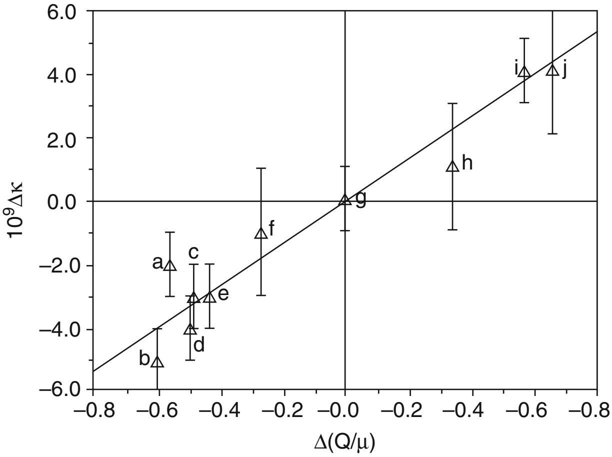

Fig. 6.7 Plot of $\Delta\kappa$ versus $\Delta(Q/\mu)$, where Q is the "charge" defined in (6.21). This plot is Fig. 8.1 from Fischbach and Talmadge (1999), and the labels on the samples are defined in Hall (1991)

decision was typically resolved against the authors.) Eventually I contacted Andrew Hall and informed him that Carrick and I had reviewed his paper (positively) for PRL. He then confirmed that another referee had rejected his paper. Since Carrick and I felt that the H/A results should be publicized, we arranged to include a revised version of this paper as my contribution to a conference in Taiwan (Hall 1991), which was co-authored by Horst Armbruster and Carrick.

Some years later I learned who the other reviewer of the H/A paper was. Not surprisingly, the shortcomings of the original H/A paper which necessitated the revisions that Carrick and I felt should be incorporated into (Hall 1991), also concerned this reviewer, and formed the basis for rejecting the H/A paper.

The story of the Hall "spin-charge" raises the broader and deeper question of the reproducibility of experiments, a subject which has been much in the news recently.[6] As we have noted above, the design of any experiment to search for the presence of a fifth force depends to a great extent on having some model of how the sought-after effect depends on whatever aspects of the experiment are under the control of the experimentalist. This might include the choice and preparation of samples, design of apparatus, data analysis, etc. In fact the very notion of repeating an experiment carries with it some notion that the effect being studied should not depend in a significant way on when the original and subsequent experiments were carried out, which may not always be the case.

[6]See *New York Times*, Sunday Review, February 2, 2014, p. 12. See also Centerforopenscience.org.

6.3 Immediate Aftermath of Publication

As noted previously, our paper was published in PRL on January 6, 1986. By coincidence it was the first paper in PRL published in 1986, although I doubt that this had much to do with the attention it was about to receive. Our Christmas vacation had been delayed due to an unusually heavy smog that settled over the Seattle area during the Christmas period, which affected air travel among other inconveniences.

As a consequence of the smog, and the unpleasant weather we encountered in California, I was suffering from a massive head cold by the time we left California for home on Sunday, January 5, 1986. By the time we landed in Seattle I was experiencing a significant hearing loss resulting from the congestion associated with the cold, along with a persistent cough. When I arrived in work the next day both the hearing problem and cough had improved, but only slightly. And so when the phone rang in my office at around 11 AM on Monday, I wasn't quite sure that I was hearing properly when John Noble Wilford from the New York Times called to talk about our paper—which I had yet to see in print.

My conversation with John was very pleasant, although he was a little vague when I asked the obvious question, how he even knew about our work. I gathered from what he did say that he had a number of contacts who would suggest stories to him. By Tuesday, January 7, I had been sent a sketch of the alleged Galileo experiment on the leaning tower of Pisa, which would appear the next day with the full story. By Tuesday evening there was a brief mention of our work on the CBS-TV evening news, anchored by Bob Schieffer, and somewhat longer story on NBC radio.

The headline on John Noble Wilford's story on Wednesday, January 8, "Hints of 5th Force in Universe Challenge Galileo's Findings", introduced the notion of a "fifth force". In this reckoning the other four forces, in order of decreasing strengths, are the strong, electromagnetic, weak, and gravitational. Although some might quibble with drawing a distinction between the electromagnetic and weak manifestations of what we now consider to be the unified electroweak interaction, the notion of a generic "fifth" force has made its way into the published literature usually without attribution. As used, this refers to a long-range non-gravitational force presumably arising from the exchange of any of the ultra-light quanta whose existence is predicted by various unification theories such as supersymmetry. Although I cannot be sure of historical precedents, this is likely to be a rare (and possibly unique) instance in which a widely used physics concept owes its name to a journalist.

Wilford's story appeared Wednesday January 8, surprisingly on the front page, along with the aforementioned picture. My day began, unfortunately, at approximately 4 AM with a call from an Australian reporter who was unaware of what time zone Seattle was in. He was interested in the connection between our paper and the work of his fellow Australians Frank Stacey and Gary Tuck, which we had cited as part of the motivation for our work. After I politely indicated to him what time it was for me, we agreed to have a longer talk later in the day, which we did. After breakfast I drove to my office, stopping along the way at the UW bookstore to pick up a half-dozen copies of the *Times*. By the time I reached my office I found a stack of phone messages from reporters on my desk, and for the remainder of the day I did nothing but try to respond to these, while at the same time answering calls as they came in. Additionally reporters from local Seattle media showed up at my door, and I was eventually forced to unplug my phone in order to make time available for them.

Some time after 6 PM I decided that it was time for dinner, given that I had nothing for lunch, and so I left for our rental home in Bellevue. Ordinarily the traffic on the 520 Floating Bridge across Lake Washington, which connects Seattle and Bellevue, was bothersome. However, given the stressful day that I was now escaping from, the traffic was a blessing of sorts. Absent cell phones, which were still many years in the future, I was able to enjoy 45 min of peace and quiet during which nobody could reach me.

As it turned out, my day was not yet over. Shortly after sitting down to dinner the phone rang, and Janie picked it up. "It's *The National Enquirer*," she said, "and they want to talk to you about your work." During the earlier part of the day I had made a special effort to explain to each reporter what our work was about in terms that I felt were appropriate to his/her level of interest and understanding. So how was I now to explain what we had done to a tabloid such as the *Enquirer*? To my relief the caller was actually Bruce Winstein, who is a high-energy experimentalist then at Stanford, and he was interested in the arrangements for my talk the following Monday at Stanford, which had been arranged long before the *N.Y. Times* story. In an odd twist of events, Bruce's seemingly innocuous phone prank led to an unfortunate interaction with Richard Feynman, as I describe in Sect. 6.3.1.

The first public lecture on our paper was at TRIUMF in Vancouver, Canada which had been arranged for the next day Thursday, also long before the publicity generated by the *N.Y. Times* story. Janie and I had just purchased a new Honda Civic, and I was looking forward to breaking it in on the roughly 300 mile round trip to Vancouver. Carrick and I left early in the morning, and after arriving at TRIUMF I was quickly requested to do a radio interview with the Canadian Broadcasting Corporation (CBC). The only problem was that I still had a lingering cough, which the CBC interviewer indicated was causing them problems. Somehow I managed to suppress my cough long enough to get through the short interview. The talk itself went very well, which was gratifying, since this was the same talk I was going to give the following Monday at Stanford.

6.3.1 Interaction with Richard Feynman

By Friday January 10 a degree of calm had been restored to me and my family. At around 8 PM the phone rang. Janie was busy cleaning up from dinner, I was busy giving Michael a bath, and so it fell to Jeremy to answer the phone. "Dad, a Mister Fineman is on the phone ..." I picked up the phone, and without even formally saying "hello" I said something like "Bruce, stop trying to pull my leg, I've had a very long week ..." From the other end of the phone came, "...this is Richard Feynman, I am a theoretical physicist at Caltech..." The fact that the caller had to identify himself made it certain to me that this was in fact Bruce Winstein calling again from Stanford (recall, no caller ID in those days!) "Bruce, enough is enough ..." "This is Richard Feynman, I have a few questions about your recent paper in PRL." By this point I had become convinced that either this was the best impersonation of Feynman that I had ever heard, or that "Fineman" was actually "Feynman".

After obliquely complimenting me for actually reading and analyzing the EPF paper, he launched into his main criticism. In (9) of our paper we used the EPF data to determine the quantity $f^2\epsilon(R/\lambda)$, where f is the unit of hypercharge (analogous to the electric charge e), assuming that an intermediate-range hypercharge force was responsible for the non-zero slope seen in the EPF data. Since hypercharge $Y = B + S$, where B is baryon number and S is strangeness, the hypercharge of any sample of ordinary matter is simply its baryon number B, the sum of its protons and neutrons.[7] The function $\epsilon(x)$ is given by

$$\epsilon(x) = \frac{3(1+x)}{x^3}\mathrm{e}^{-x}(x\cosh x - \sinh x) \; , \tag{6.22}$$

and is a "form factor" arising from the integration of an intermediate-range hypercharge distribution over the Earth, assumed to be a uniform sphere of radius $R = \lambda \cdot x$. In (9) of our paper we found

$$\left[f^2\epsilon\left(\frac{R}{\lambda}\right)\right]_{\mathrm{EPF}} = (4.6 \pm 0.6) \times 10^{-42} e^2 \; , \tag{6.23}$$

where e is the electric charge in Gaussian units. By way of comparison, the value determined from the geophysical data of Stacey et al. which constituted part of the original motivation for our paper, was

$$\left[f^2\epsilon\left(\frac{R}{\lambda}\right)\right]_{\mathrm{geophysical}} = (2.8 \pm 1.5) \times 10^{-43} e^2 \; , \tag{6.24}$$

I had regarded it as miraculous that two experiments as disparate as EPF and Stacey et al. agreed within an order of magnitude. However, Feynman viewed the factor 16

[7]Ordinary matter is composed exclusively of baryons (and not anti-baryons). It follows that a fifth force arising from a vector coupling whose source is baryon number or hypercharge would give rise to a repulsive force between ordinary objects. Since gravity is, in contrast, an attractive force, a number of stories described our original PRL as providing evidence for "anti-gravity". This in turn has the consequence that in the falling "coin and feather" comparison, the feather falls faster. See also Sect. 6.4.2.

discrepancy between these two results as a strong argument against our hypercharge hypothesis as an explanation of the EPF results.

Our conversation ended somewhat better than it had started when I apologized for the manner in which I had answered the phone. However, Feynman remained unconvinced by our analysis, and said so publicly in a letter published on January 25 in the *Los Angeles Times*, which had previously carried a story on our work on January 8 (see Appendix 5). It appears from the letter Feynman sent to the *L.A. Times* that he was motivated to respond to the op-ed piece about our paper entitled "The Wonder of It All," which they had published on January 15. Feynman had been asked what he thought of our theory, and he had responded "Not much." In his follow-up letter, which the *L. A. Times* published on January 25 (and which refers to our phone conversation), he felt the need to elaborate on his quoted remark (see Appendix 5). More interestingly, he apparently also felt the need to explain to me in technical terms the basis for his view. The content of this letter represents a tour de force on Feynman's part, especially considering the fact that he was evidently working from the original EPF paper in German. He begins by focusing on the factor of 16 difference between the results in (6.23) and (6.24), with respect to which he and we had different views. He then considers possible scenarios in which various combinations of α and λ in (6.1) could reconcile the available data, but suggests that this is unlikely.

Feynman's tour de force then follows in which he examines in minute detail the various measurements that EPF carried out. This is a very impressive discussion, which concludes with his comment, "Well, that is the best I can do." I know of no other paper which has analyzed the EPF data in this level of detail, and hence to me Feynman's analysis is all the more remarkable. Given the fact that the fifth force implied by the EPF experiment has not been seen in other experiments, it may be that Feynman's general criticisms were correct, although not necessarily for the specific issues he raised. This question is discussed in greater detail in the epilogue (Sect. 6.8).

Given Feynman's well-deserved reputation in the world of physics and beyond, one might have expected his criticism of our paper to have dealt a fatal blow to our work. However, this proved not to be the case: by the time his letter appeared in print on January 25, a number of groups had recognized that the simplistic model of a uniform spherical Earth acting as a source for a putative hypercharge force was inappropriate for a force whose range was hypothesized to be $\sim$200 m. In fact we had already noted this explicitly following (10) in our original paper (Sect. 6.3.5). For a force of so short a range, local inhomogeneities such as buildings and basements would play an important role in determining the correct functional form for the expression to be used in place of $\epsilon(x)$ in (6.23) and (6.24). As we discuss in Sect. 6.3.5, the recognition of the importance of local inhomogeneities served to clarify both the magnitude and sign of the putative hypercharge force.

The significance of local inhomogeneities led to several papers which were submitted at nearly the same time to PRL, including one by our group (Bizzeti 1986; Milgrom 1986; Thieberger 1986). The submission of our paper was slightly delayed owing to our desire to obtain the approximate dimensions of the building in which it was presumed that EPF carried out their experiment, which we received from Judit Németh (Talmadge et al. 1986, p. 237). In the end we demonstrated that (Talmadge et al. 1986, p. 236) "neither the magnitude nor the sign of the effective hypercharge coupling can be extracted unambiguously from the EPF data without a more detailed knowledge of the local matter distribution." Although our paper was accepted by the reviewers for publication in PRL, in an unusual move the editors of PRL declined to publish any but the first paper to have been received, which was an elegant paper by Peter Thieberger from Brookhaven National Laboratory (Thieberger 1986).

The appearance of the papers on the influence of the local matter distributions, even in preprint form, served to mute Feynman's criticism which in the end appears to have had little lasting impact. What impact it did have was further muted by the Challenger disaster three days later on January 28, 1986, in whose subsequent investigation Feynman played so crucial a role. I do not know whether Feynman was aware of the above papers. However, following the conclusion of the Challenger investigation, in which Feynman famously pointed to the problem with the O-ring seals (by dipping one in ice water), I re-engaged with him on the question of local inhomogeneities through a letter I sent on April 14 (see Appendix 5).

6.3.2 The Talk at Stanford

This was the second public presentation of our paper and, as I anticipated, was more probing. Although Stanford was happy to pay for me to fly from Seattle to San Francisco, I opted to drive instead with Carrick in my new Honda. I had arranged to stay with my close friends Jim and Marilyn Brittingham in Livermore, California where Jim (since deceased) was on the staff of Lawrence Livermore National Laboratory. Carrick and I left Seattle around 7 AM and arrived in Livermore some time between 9 and 10 PM.

The next morning we drove to Stanford, and joined some faculty for lunch. There I met Bill Fairbank for what would prove to be the first of a number of subsequent pleasant encounters. As I noted above, Bill began by complimenting Carrick and me for correctly identifying *talg* as fat or suet. (Credit for this goes directly to Carrick!) At the talk itself the questions were polite, as illustrated by the following from Bruce Winstein. He noted that if we had plotted the EPF result for (Pt–magnalium) rather than for (magnalium–Pt) as we did, that datum would have ended up in the 3rd quadrant of our PRL Fig. 1, rather than in the first, and the figure would have looked less dramatic. I responded by first acknowledging that this would be so, but then noting that this (arbitrary) shift would merely change the "optics" of the figure but not the slope of the resulting line nor its $\sim 8\sigma$ significance, which were the physically important results. I then added that in writing this paper we had included the following sentence specifically to address questions of the sort that Bruce had raised: "Table I gives $\Delta\kappa$ for each of the nine pairs of materials measured by EPF, *exactly as their result is quoted on the indicated page of Ref. 6*" (emphasis added).

By the end of the talk I felt that it had gone sufficiently well that the inevitable calls from members of the audience to their colleagues elsewhere would have converged an overall positive tone.

On the return trip to Seattle Carrick and I were joined by Idella Marx, who flew up from Los Angeles to attend my talk at Stanford and then decided to drive home with us. Idella was a science enthusiast who had hired me in 1963 to expose her children to "fun" science. Idella's husband Louis had founded the Marx Toy Company, and she used her resources to indulge her interest and that of her family in science, physics in particular. What neither of us knew as we started out was that she was about to experience one of the great thrills in her life, a surprise meeting with T.D. Lee (see Sect. 6.3.3).

Our otherwise routine trip back to Seattle revealed another surprise for Carrick and me: somehow we got on the subject of the Pentagon papers dealing with the Vietnam war. They were publicly disclosed in 1971 by Daniel Ellsberg who is married to Idella's stepdaughter Patricia Marx. The resulting story of how various missteps by the prosecution which allowed Ellsberg to go free would have been worthy of a Hollywood movie.

We arrived in Seattle late in the evening of January 14, and dropped Carrick off at this apartment. Idella and I then drove to our place in Bellevue, stopping along the way to pick up the latest issues of *Newsweek* and *Time*. Idella had guessed correctly that both would carry stories on the fifth force, and the *Newsweek* version by Sharon Begley (p. 64) was particularly good. Her story began with a bit of word play which I missed, but which other readers caught: "**F**ew **i**mages **f**rom **t**he **h**istory of science..."

The talks at TRIUMF and Stanford were the first of more than 75 talks that I gave in many countries on the EPF experiment/fifth force between 1986 and 1992 (when I stopped keeping track). In the early days, before the results of new experiments became available, the EPF experiment and our analysis of their data were on occasion the subject of some pointed exchanges during these talks. I dealt with the associated stress by noting to myself that some day when new experimental results became available, I could sit at the back of the room and watch the authors of these experiments focus on one another, and no longer on me and my co-authors. That day came for me on July 6, 1989 when I was attending the GR-12 conference in Boulder, Colorado, the home of the University of Colorado. Just prior to the session on the fifth force I purchased a bag of popcorn and brought it to the conference. There, sitting in the back row, I enjoyed both the popcorn and the excitement of the experimentalists challenging one another and not me.

6.3.3 Meeting with T.D. Lee

As noted earlier, the recognition that the presence of a new long-range (i.e., $r/\lambda \ll 1$) force could be detected by a violation of the Equivalence Principle originated in a beautiful one-page paper by T.D. Lee and C.N. Yang published in *Physical Review* in 1955 (Lee and Yang 1955). Our 1986 paper had extended the work of Lee and Yang in two ways: First, we modified their formalism to allow for this force to have a finite range, unlike gravitational and electromagnetic forces which are believed to extend over an infinite range. Our second, and more important, contribution was to actually plot the EPF data against our theory.

By an extraordinary coincidence, T.D. Lee had been invited to give a series of three public Danz lectures, one of which he delivered on January 15, 1986, just nine days after the publication of our paper in PRL. This had been arranged before I arrived at UW for my sabbatical, and had nothing whatever to do with the publicity surrounding our EPF paper. Notwithstanding the reference to the Lee–Yang paper in our EPF paper in PRL, I suspect that few of my colleagues at UW fully appreciated the deep connections between these two papers. Lee's visit to UW extended over several days, and I arranged to speak with him personally. He obviously knew of our reanalysis the EPF paper and began by congratulating me for it. After some brief discussion of the paper itself, I got around to asking the obvious question: why hadn't he and Yang actually plotted the EPF data, as we had done, instead of assuming as they did that EPF had obtained a null result? I remember Lee chuckling a bit, and then explaining that their one page paper was written at a time when they were deeply involved in other questions, which they regarded as more pressing, such as parity non-conservation in the weak interactions. (Their EPF paper appeared in March 1955, and their Nobel prize-winning paper on parity non-conservation appeared in October 1956.) We can only speculate on how elementary particle physics might have changed had they taken out the time to actually plot the EPF data as we had done. Would this have riveted their attention on the gravitational interaction rather than the weak interaction? And how long would it have taken for them or somebody else to return to parity non-conservation?

During Lee's lecture on January 15, he exhibited some posters he had hand-drawn to accompany his talk. Following his talk I introduced him to Idella Marx who was thrilled to meet Lee. She gently asked whether she could have the posters, and he graciously agreed. This was clearly the highlight of Idella's stay with us.

6.3.4 Some Wrong Papers

The publicity following publication of our paper in PRL led to a flood of comments and criticisms, many of which we received to review. (See PRL editorial comment: *Physical Review Letters* **56**: 2423 (1986).)

Among the papers that arrived in the white-and-green PRL envelopes were several from colleagues whom I personally knew, or at least knew of, which were flawed. Carrick and I carried out a rough triage on all the incoming papers, which some days were arriving at a rate of one or two a day, in contrast to my expected frequency of one every few weeks. Irrespective of what our decision was, Carrick and I worked closely to clearly explain to the authors, editors, and other potential reviewers the basis for our decision. In the end we found that virtually all of our recommendations were followed, so that relatively few incorrect papers made it into the published literature.

With the notable exception of the Thodberg paper, discussed in Sect. 6.3.5 below, which correctly pointed out a sign error in our paper, many of the papers that we received to review contained conceptual errors of one sort or another. A good example is provided by a criticism of our calculation of the B/μ values for our samples that was raised by two senior physicists, one of whom I knew personally. As we note in Fischbach et al. (1988), given the fact that B/μ is close to unity for all substances, it follows that determining $\Delta(B/\mu)$ requires that the values of B/μ for individual elements be calculated to at least six decimal places. For example, $B/\mu\,(\mathrm{Mg}) = 1.008453$ and $B/\mu\,(\mathrm{Al}) = 1.008515$. To do this the values of B/μ for each isotope of an element, which are known with great precision, must be properly weighted by the relative abundances of these isotopes in the naturally occurring element. These authors then (correctly) note that these abundances are much less well known. (This is due in part to the fact that the abundances can vary from one location to another due to fractionation.) They then argue (incorrectly!) that the uncertainties in these relative abundances would introduce sufficiently large errors in calculating $\Delta(B/\mu)$ as to preclude drawing the conclusions we did in our paper.

This argument, although superficially convincing, is in fact wrong, and led me to reject this paper. What the authors failed to consider is that the values of B/μ for the individual isotopes of an element are so close to one another that it hardly makes a difference what the relative abundances of a given element are. On p. 26 of Fischbach et al. (1988), we illustrate this point quantitatively using as an example the isotopes of Mg, which is a constituent of the magnalium alloy sample used by EPF. There we show explicitly that the actual fractional uncertainty in the calculation of B/μ is approximately 8×10^{-9}, which is completely negligible.

6.3.5 Shortcomings of Our PRL Paper

It is not uncommon in the world of physics for the same idea or observation to occur independently to more than one individual or group at approximately the same time (see, for example, Sect. 6.3.6). Since I myself had experienced this more than once, it was not surprising that when we found the correlation between the EPF data for $\Delta\kappa$ and our calculated values of $\Delta(B/\mu)$, that I started to worry that some individual/group could stumble upon the same observation. In fact my concern was not unreasonable, since the content of the paper was sufficiently straightforward that, following publication of our paper, I learned that it had been assigned as a graduate or undergraduate homework problem by a number of colleagues at various institutions. This self-imposed time pressure resulted in some oversights which, luckily, did not detract from the basic message of the paper.

The most obvious shortcoming was an error we made in the sign of the putative fifth force as inferred from the EPF data. If the force between a source and a test mass is proportional to the product of their respective baryon numbers (or hypercharges), which is what the EPF correlation indicated, then that force had to be intrinsically repulsive since all stable matter has positive baryon number. This leads to clear predictions for the signs of the acceleration differences $\Delta\kappa$ for the various EPF sample pairs. Shortly after our PRL appeared Thodberg (1986) correctly pointed out that in the simple model we were assuming, where a spherical Earth was the source of the observed acceleration differences, the sign of $\Delta\kappa$ between Cu and water as measured by EPF could correspond to an attractive (not repulsive) force.

In the course of writing our paper Carrick had drawn attention to the sign problem, and its connection to both the model of the Earth and the influence of the local matter distribution (see discussion below). My view was that since the sign problem would take some time to sort out, particularly the effects of the local matter distribution, we should not risk the possible consequences of delaying submission of our paper. This view was bolstered by my conviction that the reviewers of our paper would surely require major revisions, which would then allow us the time needed to deal with the sign question. To our surprise our paper was quickly accepted by PRL, with only the minor addition suggested by Dicke, as discussed in Sect. 6.2.5. However, since we clearly appreciated the importance of the local matter distribution, specifically as it would bear on the comparison of (9) and (10) of our paper, we added a note to this effect following (10). What Thodberg's observation pointed out was that understanding the local matter distribution was also necessary to account for the sign of $\Delta\kappa$ for Cu–H_2O as measured by EPF.

To understand how an apparently attractive force can emerge from an interaction which is intrinsically repulsive, imagine that the Earth is a completely uniform sphere, except for a huge hole located somewhere in the vicinity of the EPF experiment. It is then easy to see that the absence of the repulsive force that would have arisen if the hole were not there, would effectively look like the presence of an attractive force in the presence of the hole.[8] To quantify this effect we set out to find the dimensions of the buildings where EPF were presumed to have carried out their experiment. As noted in Sect. 6.3.1, we obtained this information from Judit Németh, and an analysis of the implications of what we learned formed the basis of the writeup of the talk that Sam Aronson gave about our work at the 1986 Moriond meeting (Talmadge et al. 1986).

An oversight which had the potential to cause problems was an initial lack of awareness of the work of both Renner (1935), Bod et al. (1991) and later Kreuzer (1968). As discussed in Fischbach and Talmadge (1999), Renner was a student of Eötvös who repeated the EPF experiment in 1935. Because he claimed higher sensitivity than the EPF experiment, yet saw no effect, this could have doomed our paper at the outset. Fortunately, we eventually became aware of the careful analysis of the Renner paper by Dicke (1961) and Roll et al. (1964), in preparation for their own experiment (Roll et al. 1964). These authors pointed out various inconsistencies in Renner's results which rendered them unreliable, a conclusion which Renner

[8] See footnote on "anti-gravity" in Sect. 6.3.1.

himself confirmed to Dicke (Fischbach and Talmadge 1999, p. 138). A brief note to this effect is contained in Ref. 7 of our original PRL.

Given the potential significance of Renner's results, had they been correct, it was not surprising that we re-engaged with Dicke on this question, in the course of learning more about the locations of the EPF and Renner experiments (see Appendix 3). As can be seen from Dicke's letter of June 27, 1986, he had shown that Renner's errors were too small because Renner failed to account for the fact that his measured values were not independent, since each datum was used more than once. Dicke then goes on to note that although Renner claimed that this procedure was the same as that used by Eötvös, the EPF data seem to be statistically consistent. This agrees with the conclusion we arrived at in our PRL, and in our subsequent more detailed analysis (Fischbach et al. 1988).

The 1968 experiment of Kreuzer (1968), of which we were unaware at the time of our original PRL, was originally conceived as a test of the equality of active and passive gravitational mass. However, it can also be interpreted as a test for an intermediate range force, as was pointed out by Neufeld (1986). Fortunately, the resulting upper limit inferred from the Kreuzer experiment was compatible with the EPF result.

An oversight which was both more significant and more personal was our failure to refer to the seminal papers by Yasunori Fujii (1971, 1972, 1974, 1975, 1981) and Fujii and Nishino (1979). These formed part of the motivation for the geophysical determination of the Newtonian constant G_0 by Stacey and Tuck which in turn motivated our own work. Shortly after our PRL appeared I received a polite note from Fujii pointing out this connection, which I subsequently confirmed in a conversation with Frank Stacey. What Fujii had shown was that in the dilaton theory he was proposing the effective gravitational constant G_0 at laboratory distances could differ by a factor of 4/3 from the constant G_∞ that would describe planetary motion (see Appendix 1). The Fujii papers strongly motivated the work of Stacey and Tuck, which at the time of our PRL was in fact indicating a difference between G_0 and G_∞, and this in turn stimulated our work as we have noted above. Given the clear link between Fujii's work and ours, his paper clearly should have been cited.

Interestingly, in the years prior to our EPF analysis I had compiled a bibliography of relevant interesting papers (Fischbach et al. 1992), and I later found that Fujii's paper in *Nature* (Fujii 1971) was in that bibliography. The same self-imposed time pressure described above ensured that I never consulted this bibliography while drafting our paper, which accounts for our neglect of his paper. I immediately responded to Fujii and apologized. Subsequently I went to some lengths to correct my oversight by detailing the significance of his work in both our review in *Annals of Physics* (Fischbach et al. 1988) and in our book (Fischbach and Talmadge 1999). Eventually we met and became colleagues and friends. We collaborated on a paper (Faller et al. 1989), and during the subsequent years I had the pleasure of being his guest on several visits to Japan.

If it seems surprising that I was upset at missing a single reference in a single paper, my reaction reflects what has always been a firm commitment of mine to fairly credit the work of others, as I would hope they credit my own.

In the category of shortcomings that were not our fault, Ref. 7 of our PRL contains two very unfortunate typographical errors, which were not present in our original manuscript. In order to speed up the publication process, *Physical Review Letters* did not send galley proofs of accepted papers before publication, and hence we had no opportunity to correct these errors. For the record the correct references, as they should have appeared in our paper, are R.H. Dicke: Sci. Am. **205**, 84 (1961) and P.G. Roll, R. Krotkov, and R.H. Dicke: Ann. Phys. (N.Y.) **26**, 442 (1964). The error in the first of these references was particularly embarrassing, especially given the gracious response of Professor Dicke to our paper. Although I apologized to him, he indicated that this was unnecessary since, as one of the referees, he had seen the original manuscript and knew that we had cited him correctly.

Finally, a point which we failed to comment upon, but which arose in subsequent questions, was the role of the brass vials themselves. Specifically, what would the EPF data look like if the samples were taken to be the combination of the brass vials and their contents. Intuitively we had assumed that since the vials were presumably all of the same composition, their contributions would cancel when measuring acceleration differences. Nonetheless this was a question which needed to be addressed in detail, and we did so in our review (Fischbach et al. 1988) by introducing the distinction between "reduced" and "composite" samples, where composite referred to samples when the brass vials were included. As we anticipated, the statistical significance of the EPF results remained unchanged, thus reflecting our original intuition that the contribution from the vials essentially canceled.

6.3.6 Experimental Signals for Hyperphotons

One of the questions that I had been concerned with in the weeks following the submission of our paper to PRL was the possibility of directly detecting the hyperphotons γ_Y, the presumed quanta mediating the field which we had postulated as the source of the EPF result. It had been noted earlier by Weinberg (1964) that branching ratios for decays into hyperphotons can become quite large for reasons discussed below. The EPF results thus motivated us to revisit this question with the aim of relating the hyperphoton coupling constant f in (6.32) (see Appendix 1) implied by the EPF data to existing limits on kaon decays.

Much of the work to be described below was completed before the publication of our PRL on January 6. However, as a consequence of the (previously unexpected) attention following January 6, work on the decays into hyperphotons was interrupted for approximately two weeks. At that point we came to realize that the public attention being devoted to our PRL could stimulate others to raise the same question about constraints implied by decays into hyperphotons. I decided to stay home for part of each day in order to complete the work which Sam Aronson, Hai-Yang Cheng, Wick Haxton, and I had already started. As it turns out our concerns were completely justified: We submitted our paper to *Physical Review Letters* and it was

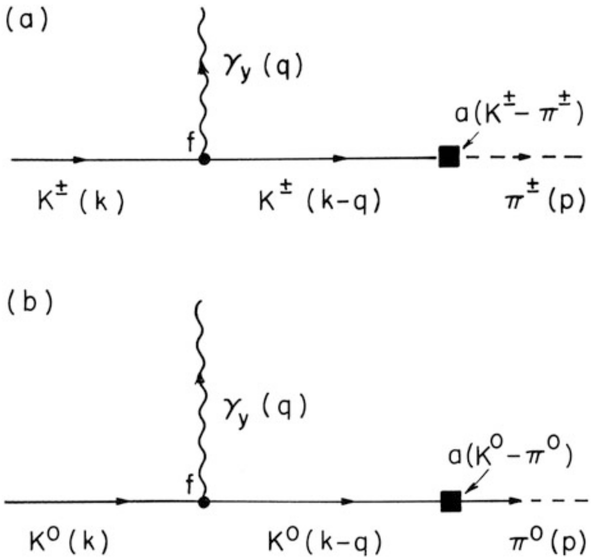

Fig. 6.8 Decays of $K^{\pm}$ (**a**) and K_S^0 (**b**) into hyperphotons (Aronson et al. 1986)

received on January 27. Similar papers, arriving at roughly similar conclusions, were received by *Physical Review Letters* from Suzuki (1986) on January 20, and by *Physics Letters B* from Lusignoli and Pugliese (1986) on January 28, and from Bouchiat and Iliopoulos (1986) on January 29.

Our idea, presented in Aronson et al. (1986), was to examine the decays $K^{\pm} \to \pi^{\pm} + \gamma_Y$ and $K_S^0 \to \pi^0 + \gamma_Y$ shown in Fig. 6.8. As seen in the rest frame of the decaying kaons, conservation of linear and angular momentum strictly forbids decays into massless photons, but allows decays into massive hyperphotons. Since the coupling constant f in Fig. 6.8 is small, the probability of a detector actually responding to γ_Y is also small. Hence the signal for $K^{\pm} \to \pi^{\pm} + \gamma_Y$ or $K_S^0 \to \pi^0 + \gamma_Y$ would be the appearance of a $\pi^{\pm}$ or π^0 of energy $m_K/2$, corresponding to $|\boldsymbol{p}_k| = 227\,\text{MeV}$, not accompanied by any other detected particles. The results of a detailed calculation gives the branching ratio (Aronson et al. 1986)

$$\frac{\Gamma(K^{\pm} \to \pi^{\pm} + \gamma_Y)}{\Gamma(K^{\pm} \to \text{all})} = (4.7 \times 10^{14}\ \text{eV}^2)\frac{f^2/e^2}{m_Y^2}\,, \tag{6.25}$$

where e is the electric charge. We see from (6.25) that for $m_Y = 1 \times 10^{-9}\,\text{eV}$, corresponding to $\lambda = 200\,\text{m}$, the branching ratio can be large enough to imply interesting constraints on f^2 or α in (6.1). (The relationship between f^2 and α is discussed in more detail in Appendix 1.) Specifically, using the then-existing limits from Asano et al. (1981, 1982), we found

$$\left|\frac{\alpha}{1+\alpha}\right|\left(\frac{\lambda}{1\ \text{m}}\right)^2 \le 4.7\,. \tag{6.26}$$

A more detailed discussion of decays into hyperphotons can be found in Fischbach and Talmadge (1999), which includes later calculations of the branching ratios $K^{\pm} \rightarrow \pi^{\pm} + \gamma_Y$. Notwithstanding the various theoretical uncertainties that arise in calculating $a(K^{\pm} - \pi^{\pm})$ in Fig. 6.8, the overall conclusion that emerged from the original analysis was that it would have been difficult to simultaneously account for the ABCF data on the energy dependence of the K^0–$\overline{K}^0$ parameters and the EPF data, while at the same time incorporating the constraints from $K^{\pm} \rightarrow \pi^{\pm} + \gamma_Y$. Of course this assumes that all the claimed effects arise from a single new vector field, and so models with additional new fields are not necessarily excluded.

6.3.7 Visit to Hungary

In the period following publication of our PRL, I received a large number of invitations to speak both in the United States and abroad. Several of these stand out in my mind, particularly my visit to the Eötvös University[9] in Budapest, Hungary May 12–14, 1987. This was arranged by George Marx and included an award to me by the University recognizing my contributions to promoting the importance of the work of Baron Roland von Eötvös. I had several goals in mind, apart from presenting a public lecture on our reanalysis of the EPF experiment and its implications. To begin with, I wanted to determine where in the university EPF had actually carried out their experiment as this would help us to assess the impact of the local mass distribution on the EPF results (see Sect. 6.3.1). Second, I wanted to examine the actual EPF balances which were located in the Geophysical Museum in Tihany, Hungary near Lake Balaton.

As I recall, George and I spent the better part of two days exploring various possible sites. These were signaled by the presence of "Cleopatra's needles", stone piers approximately 1 m on a side, sunk into the ground, presumably to reduce the effects of vibrations. Not surprisingly some of these piers were totally or partially obscured by subsequent construction. Nonetheless we were able to identify likely sites, and this led to an eventual publication (Bod et al. 1991). This reference contains much useful historical material relating to the site of the EPF experiment, as well as additional details on the experiment itself. In the end, we were able to reach a consensus on the likely locations of the EPF experiment, aided by additional input from Jeno Barnothy (see below), Peter Király, Adam Kiss, L. Korecz, A. Körmendi, Judit Németh (see Sect. 6.3.1), and Gábor Palló.

The correspondence in Appendix 3 includes an exchange with Dr. Jeno Barnothy who was a professor at the Eötvös Institute at the University of Budapest from 1935 to 1948, and a colleague of Pekár. Dr. Barnothy, and his wife Dr. Madeleine Barnothy, had retired to Evanston, Illinois the location of Northwestern University,

[9] Loránd Eötvös University was founded in 1635, and took the name of its famous one-time teacher in 1950.

and he had contacted me shortly after the publication of our paper. Since Evanston was only a 2.5 h drive from Purdue, I arranged to visit Jeno and Madeleine, and as a result he was able to confirm the locations of the experiments of both EPF and Renner.

Our visit to the Geophysical Museum was even more informative and led to a deeper appreciation of the design of the Eötvös balances. I took a number of pictures and made several drawings of the balances. These led to the diagram shown on p. 133 and the cover of our book (Fischbach and Talmadge 1999). Most notably, the balances contained thermometers which were riveted to the balances, a detail which was not evident in the drawing of the balance contained in Dicke's article in *Scientific American* (Dicke 1961). The significance of the thermometers to us was that Eötvös evidently paid close attention to temperature as a possible systematic influencing their results. From a historical point of view this is of interest in connection with Dicke's proposal that air currents produced by a temperature differential could have accounted for the EPF results. As we have already noted, the Dicke model is not supported by the EPF data, as discussed in Fischbach et al. (1988), and in Sect. 6.2.5. Along with Clive Speake's observations on the significance of the Ag–Fe–SO_4 datum, it is clear that EPF did indeed pay close attention to possible systematic influences on their results. In my view this makes their published non-null results even more compelling, and possibly explains why their original results were not published in Eötvös' lifetime.

The trip to Hungary was exciting for an additional reason. Shortly before I left for Budapest I was contacted by *National Geographic* (see Sect. 6.4.3) in connection with the story which John Boslough was working on, and which eventually appeared in the May 1989 issue (Boslough 1989). *National Geographic* is well known for its photography, and they were interested in some photos of me to accompany the story. Given the fact that I was enroute to Budapest it was arranged that a photographer, Adam Woolfitt, would meet up with us in Budapest, which he did. George, Adam, and I drove together to the museum at Tihany. Adam took a large number of photos, and one of them did in fact make it into the story. Adam graciously sent me some of the others, which were quite useful to Carrick and me in writing our book (Fischbach and Talmadge 1999).

6.3.8 The Air Force Geophysics Laboratory Tower Experiments

At the time our PRL appeared the United States Air Force maintained two laboratories dedicated to geophysical research, one located at Hanscom AFB in Bedford, Massachusetts and the other at Kirtland AFB in New Mexico. (At present there is a single site at Kirtland.) The Hanscom site was then headed by Don Eckhardt who, along with Andrew Lazarewicz, Anestis Romaides, and Roger Sands, organized an experiment to measure the local acceleration of gravity g up a tall tower. In some sense this was the mirror image of the original experiment of Stacey and Tuck, and was in principle sensitive to deviations from the inverse-square law over the same 1 km range. Based on conversations I have had with Don, it seems that the initial motivation for these experiments was to improve the upward continuation of gravity measurements taken at the surface to altitudes where they would be relevant for missile inertial guidance systems. In fact Don had been planning a balloon experiment to measure gravity at altitudes up to $\sim$100,000 ft. It is not hard to imagine that then-existing inertial guidance systems might be sensitive to deviations from Newtonian gravity at a level suggested by the data of Stacey and Tuck, and/or our EPF analysis. (However, rumors at the time that Air Force missiles were missing their targets in test firings by more than had been expected, have not been confirmed to me by Don.)

In any case, the conceptual framework was clear: By measuring Newtonian gravity over a sufficiently large area surrounding a tall tower, one could use Newtonian gravity to extrapolate these data and predict what g should be going up the tower. These predictions would then be compared to the actual measurements on the tower carried out by a sensitive Lacoste-Romberg gravimeter which Anestis and Roger carried up the tower. Any discrepancies between these measurements and predictions could then be a signal for deviations from the inverse-square law.

Eckhardt and his collaborators at the Air Force Geophysical Laboratory (AFGL) carried out their first experiment using the 600 m WTVD television tower in Garner, North Carolina, and initially found what they characterized as a "significant departure" from the predictions of Newton's inverse-square law. Their quoted departure, "approaching $(500 \pm 35) \times 10^{-8}\,\mathrm{m/s^2}$ at the top of the tower," was published in *Physical Review Letters* on June 20, 1988, a few weeks before the Fifth Marcel Grossmann meeting in Perth Australia (Eckhardt et al. 1988) (see also Sect. 6.4.3). Since the sign of their effect corresponded to a new "attractive" force, in contrast to the repulsive fifth force implied by the EPF data, Eckhardt and collaborators characterized their result as the discovery of a new "sixth force", and this was one of the exciting stories at the Marcel Grossmann meeting.

However, the results of the tower experiment, along with those of the original Stacey experiments, were soon called into question by Bartlett and Tew (BT) (1989a, 1989b, 1990). In brief, BT noted that the evidence for non-Newtonian gravity reported in each case could have arisen from "terrain bias", wherein the gravity measurements in the vicinity of each site did not accurately reflect the actual terrain at the site. The AFGL collaboration refined their analysis, and eventually withdrew their claim of evidence for non-Newtonian gravity (Jekeli et al. 1990).

In 1990 the AFGL (renamed the Phillips Laboratory) began another tower experiment, this time at the 610 m WABG tower in Inverness, Mississippi. By this time Carrick was being supported as a postdoc by AFGL/Phillips, thanks to the efforts of Don Eckhardt, so he and I were invited to join this new effort.

GPS location required that at least four satellites be in view, but at the time of our experiment in the early 1990s this was not always the case. However, Anestis had a program which told us when at least four satellites could be seen, and this sometimes required us being up late at night or getting up early in the morning. To avail ourselves of GPS, a circular grid was defined by Anestis and Roger extending out to approximately 10 km from the WAGB tower.

One of the tasks assigned to Carrick and me during our first visit in November 1991 was to install platforms at the 128 designated sites at which ground-level gravity measurements were made. Given the “terrain bias” effects that had been problematic at the previous WTVD site, we were absolutely committed to installing these platforms exactly where they were supposed to be as specified on a map, irrespective of how unwelcoming these sites might be for one reason or another. Some of these were in wetland areas, and others were near catfish ponds whose owners were not always thrilled at having strangers on their property. Since Mississippi has a strong military tradition, our encounters with local residents on whose property we were carrying out our work were generally pleasant, once they learned we were on an Air Force project. In our subsequent visits in December 1991 and the Spring of 1992, when GPS and gravity measurements were actually performed at these sites, we were faced with the problem of carrying relatively expensive equipment to these sites, hoping that we would not drop any of this equipment into some body of water.

However, things did not always go well. The WABG tower was located in the Mississippi delta region, whose soil formed a fine wet clay that locals called “gumbo”. On more than one occasion our military “humvee” got stuck in the “gumbo”, as did one of our rental vehicles. On another occasion a prison work gang ran over one of our sites, located in plain view in front of a church, and destroyed the car battery running the GPS equipment.

In addition to problems with the ground survey, we also experienced problems with the tower gravity measurements to which the ground measurements were to be compared. Given the extreme sensitivity of the Lacoste–Romberg gravimeters that we were using, vibrations of the tower due to wind precluded obtaining useful measurements unless the wind speeds were very low, typically less than 5 km/h. Although this meant that days went by when gravity measurements up the tower could not be made, eventually measurements were made at the lower levels on days that were sufficiently calm. Additionally we experienced radio-frequency interference with our measurements, which was not surprising given that we were on a television tower. This problem was eventually resolved by moving our equipment to a slightly different position on the tower. Finally there was always the problem of lightning strikes while somebody was on the tower. These were potentially problematic given the very slow speed of the elevator used to move up or down the tower. The group managed to see storms moving in our direction in sufficient time to get down, and nobody was hurt.

In the end, the choice of the WABG tower was a good one. The flatness of the terrain, combined with the stability of the WABG tower (and the cooperation of its owners), allowed us to significantly improve on the earlier results from the WTVD tower. Our results led to agreement with Newtonian gravity, represented by the largest difference being

$$(\text{observed} - \text{discrepancy}) = (32 \pm 32)\ \mu\text{Gal @ } 56\,\text{m}\,, \tag{6.27}$$

where $1\ \mu\text{Gal} = 10^{-8}\ \text{m/s}^2$ (Romaides et al. 1994, 1997).

The null result from the WABG tower experiment is supported by two other tower experiments, which were carried out at approximately the same time: Speake et al. using the 300 m NOAA meteorological tower in Erie, Colorado (Speake et al. 1990), and Thomas et al. using the 465 m tower at Jackass Flats, Nevada (Thomas et al. 1989; Kammeraad et al. 1990). Although all three tower experiments arrived at a null result with respect to possible deviations from Newtonian gravity, they demonstrated for the first time that such experiments could in fact be carried out with sufficient sensitivity to provide useful α–λ constraints over the 1 km distance scale (Fig. 6.9), as was first suggested by Don Eckhardt. Further discussion of these experiments can be found in Fischbach and Talmadge (1999).

Examination of Fig. 6.9 reveals an interesting fact that I incorporated into all of my early fifth force talks. As indicated in the figure caption, the only values of α and λ that were allowed by the existing data in 1981 or 1991 are those falling below the corresponding shaded regions in the figure. We then see that as late as 1981 (almost 300 years after Newton), α could be as large as 0.1 (corresponding to a 10 % discrepancy with Newtonian gravity) over a distance scale of approximately 10 m, and still be consistent with experiment. I called this at the time the "10–10" mnemonic (10 % at 10 m), and it came as a big surprise to my audiences, particularly since 10 m seems to be a distance scale that is readily accessible to laboratory

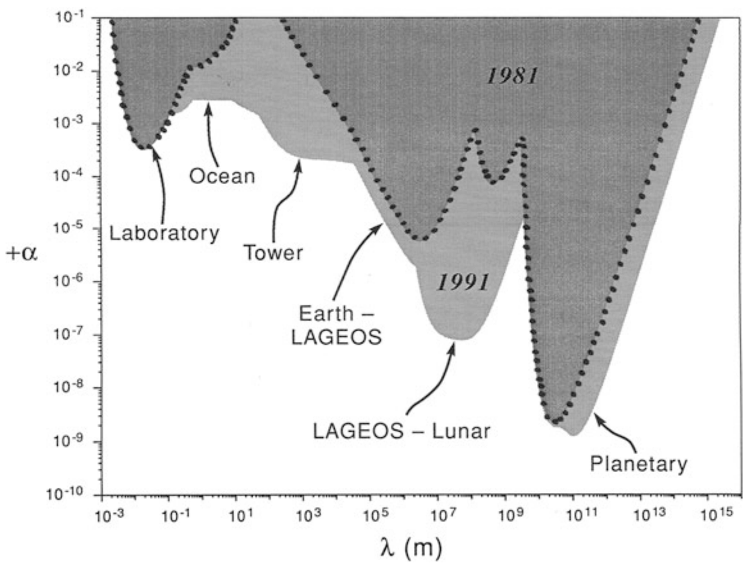

Fig. 6.9 Long-range constraints on α as a function of λ as of 1991 (Fischbach and Talmadge 1992b). For each of the two regions labeled 1981 and 1991 and above them, the *shading* denotes values of α and λ which are excluded by the indicated experiments or analyses. The *dotted curve* denotes the envelope of allowed values as of 1981 and, as indicated in the text, the 1981 data allowed for a discrepancy with Newtonian gravity of 10 % for distances scales of order 10 m. See also Talmadge et al. (1988)

experiments. This in turn relates to another question, which is directly related to the tower experiments: why can't we carry out a precise measurement at one particular distance scale and have it apply to all scales?

To answer this question consider a possible fifth force contribution to the precession of the perihelion of an elliptical orbit about the Sun of planet P with semi-major axis $a_{\rm P}$. It is straightforward to express the precession angle $\delta\phi_a$ in terms of α, λ, and $a_{\rm P}$ (Fischbach and Talmadge 1999, p. 114):

$$\delta\phi_a \approx \pi\alpha\left(\frac{a_{\rm P}}{\lambda}\right)^2 {\rm e}^{-a_{\rm P}/\lambda}\,. \tag{6.28}$$

One can show that for a given value of α, $\delta\phi_a$ reaches a maximum when $\alpha_{\rm P}/\lambda = 2$, and vanishes when either $a_{\rm P}/\lambda \to \infty$ or $a_{\rm P}/\lambda \to 0$. In the former case the range of the fifth force is too short for the Sun to influence the planet. In the latter case the range of the fifth force is so long, that an observer at $a_{\rm P}$ would experience a predominantly $1/r^2$ force, which causes no precession of the perihelion. Similar arguments apply to other inverse-square tests of Newtonian gravity, such as the limits labeled "Laboratory" in Fig. 6.9, which are from Spero et al. (1980) and Hoskins et al. (1985). The preceding discussion explains why the most sensitive limits on α at a given λ are obtained when the size of the system being studied (the analog of $a_{\rm P}$) is close to the magnitude of λ being studied. This also helps to explain why the constraints arising from planetary data in Fig. 6.9 are so much more restrictive than those at other scales: there are simply many more data available at solar system length scales than elsewhere. For further discussion, see Talmadge et al. (1988).

It should be noted that the situation regarding composition-dependent fifth force searches is quite different since a very long-range (i.e., $1/r^2$) force which was composition-dependent would still show up as a deviation from the predictions of Newtonian gravity. This is, of course, precisely the theory that Lee and Yang were testing in their classic paper (Lee and Yang 1955). The fact that a composition-dependent deviation from Newtonian gravity can be present and detected, even for a long-range ($1/r^2$) force, explains why the resulting constraint curves look qualitatively different from the α–λ curves describing composition-independent searches.

6.3.9 An Electromagnetic Fifth Force?

Just as a fifth force coupling to baryon number could produce deviations from the predictions of Newtonian gravity, so one could imagine another type of fifth force coupling to electric charge, whose presence could be detected via deviations from the predictions of Maxwell's equations or quantum electrodynamics. This possibility was raised by Bartlett and Lögl (BL) (1988) who considered the implications of a potential $V(r)$ between two electric charges e having the form [in analogy to the gravitational fifth force potential energy (6.1)]

$$V(r) = \frac{e^2}{r}\left(1 + \beta \mathrm{e}^{-r/\lambda}\right) , \tag{6.29}$$

where β is the dimensionless strength relative to the Coulomb force, and λ is the range. Although it is natural to assume that electromagnetism has been sufficiently well tested over all distance scales as to allow only very small values of β, BL pointed out that there was in fact a region $\lambda \approx 1\,\mu\mathrm{m}$, where limits on β were relatively poor. As in the case of gravity, this "gap" arises because there are fewer systems of this size which are readily accessible to experiments.

The paper by Bartlett and Lögl led to a series of papers by our group Krause et al. (1994), Fischbach et al. (1994), and Kloor et al. (1994), which was part of my student Harry Kloor's physics Ph.D. thesis (Sect. 6.7). In the process of deriving new geomagnetic limits on the photon mass, using data on the Earth's magnetic field supplied by Bob Langel (who was then on sabbatical at Purdue) (Fischbach et al. 1994), Harry became interested in other limits which appeared to be more restrictive. He eventually found that the then-existing best limit quoted by the Particle Data Group (PDG) could not be justified. We subsequently informed the PDG, and this eventually led to me becoming for a time the consultant to the PDG for the photon mass (PDG 1998).

6.4 Reflections

6.4.1 The Moriond Conferences

No organizational effort contributed more to searches for non-Newtonian gravity (and other related exotic phenomena) than the Rencontres de Moriond under the leadership of J. Trân Thanh Vân. Following the publication of our original paper in January 1986, Sam Aronson was invited to give a talk on our work at the Twenty-First Rencontre de Moriond, which took place from March 9–16, 1986. The meeting was held at Les Arcs, which is a ski resort conveniently located approximately 3 h by bus from Geneva and CERN, where Sam was working at that time, on leave from Brookhaven. Sam gave a general presentation of our work, and the write-up which appeared in the Proceedings (Talmadge et al. 1986) focused on the issue of local mass anomalies, which we have discussed above.

The Moriond organization had a workshop scheduled for January 24–31, 1987 entitled "New and Exotic Phenomena", which would also take place at Les Arcs. In addition to sessions on such (then) exotic topics as CP-violation, dark matter, neutrino mass and oscillations, they had decided to include a session devoted to the fifth force. By that time there were already a number of experiments underway, and representatives of some of these efforts were present. These included Frank Stacey, Fred Raab (Eöt-Wash experiment), Peter Thieberger, Pier Giorgio Bizzeti, Riley

Newman, and Kazuaki Kuroda. Additionally there were related talks by Mike Nieto and Bill Fairbank, on tests of the gravitational acceleration of antimatter, and theory talks by John Moffat, Bob Holdom, and Alvaro de Rújula.

The schedule of the Les Arcs meeting, and other Moriond meetings, was typically as follows: talks began at 8:00 and lasted to 12:00. There followed a break until 16:00 which included lunch and time for skiing. Talks then resumed until 20:00, followed by dinner. Since skiing is not one of my better sports, I welcomed the opportunity to improve my skills, with the aid of some instruction arranged by the Moriond organization. The break from 12:00 to 16:00 encouraged informal physics conversations both on and off the slopes.

Dinners provided an opportunity for more detailed discussions among participants with common interests. One evening, while several of us from the fifth force session were having dinner together, the conversation drifted to criticisms of the Eöt-Wash experiment as described by Fred Raab, led by Frank Stacey. Fred was reporting a null result whereas Frank's anomalous result was part of the motivation for our original PRL paper. Fred stuck to his guns despite intense questioning by Frank and others, myself included. In the end it turned out that Fred was correct, whereas Frank withdrew his published anomalous results, as noted in Sect. 6.3.8.

Towards the end of that week there was an organizational meeting called to plan for the next Moriond Workshop in January 1988. I was invited to that meeting which I interpreted as a sign that the quality of the fifth force talks had met with general approval from the group. This view was not unanimous, with Felix Boehm expressing some concern that this work was still highly speculative. Nonetheless the decision was made to go ahead with a larger fifth force session in 1988: the workshop title was to be "5th Force Neutrino Physics".

Measured in terms of the experimental effort devoted to fifth force experiments, the 1988 workshop was the high-water mark, and gave this nascent field a major boost. As the organizer primarily responsible for arranging the fifth force session, I worked hard to cover as many of the ongoing experiments, or proposed experiments, as possible. In the end there were 26 talks in the fifth force session, which I opened with an overall introduction to current research. The written version of my talk, which appeared in the Proceedings (Fischbach and Talmadge 1988), contained an additional feature which we included in subsequent talks: this was a list of all the experiments known to us as of April 1, 1988, broken down by category. The 1988 tabulation listed 45 experiments, which was quite remarkable considering that only two years had passed since the publication of our original paper in PRL.

Support by Rencontres de Moriond for research related to the fifth force continued in subsequent years. The 1989 January workshop also included a session on the fifth force with 16 talks, and the 1990 January workshop featured 13 talks which were fifth force related. By 1993 it had become clear that virtually all modern experiments were finding null results, the lone exception being Peter Thieberger's floating ball experiment (Thieberger 1987). The January 1993 Workshop included a session on gravitation, with 11 talks on tests of the Equivalence Principle, the rubric which to some extent has superseded the fifth force in searches for composition-dependent deviations from Newtonian gravity.

In 1996, the tenth anniversary of the publication of our original paper in PRL, I was invited to give one of two "special lectures", which are meant to be somewhat broader in scope so as to be understandable to all of the participants at the workshop. I chose as the title of my talk "Ten Years of the Fifth Force", and in that talk Carrick and I reviewed what we had learned in the previous 10 years:

> One can summarize the current experimental situation as follows: There is at present no compelling experimental evidence for any deviation from the predictions of Newtonian gravity in either composition-independent or composition-dependent experiments. Although there are some anomalous results which remain to be understood, most notably in the original Eötvös experiment, the preponderance of the existing experimental data is incompatible with the presence of any new intermediate-range or long-range forces.

Notwithstanding that somewhat disappointing conclusion, there was much that had been learned in the preceding decade. To start with many novel and clever experiments had been carried out and refined during that period.[10] Additionally, a phenomenological framework had been established which characterized most experiments in terms of the parameters α and λ (or ξ and λ) as summarized in Appendix 1. The constraints on α or ξ as a function of λ implied by different experiments could thus be combined on a single common plot as shown in Figs. 1 and 2 in my 1996 Moriond talk (Fischbach and Talmadge 1996). Examination of these plots showed that even by 1996 significant regions of the α–λ and or ξ–λ planes had been excluded by various experiments, and this trend has continued to the present. The α–λ and ξ–λ plots have by now become useful tools for theorists in constraining possible new scenarios for physics beyond the standard model. For example, theories involving extra spatial dimensions typically predict deviations from Newtonian gravity over short distances. As discussed in Sect. 6.6, the number of extra dimensions allowed can be constrained by appropriate inverse square law tests carried out over small separations, whose results can be expressed in α–λ plots. It is gratifying that such constraints have been included in the Particle Data Group reviews (PDG 2014).

My guess is that searches for deviations from Newtonian gravity would have had a much more difficult time becoming part of mainstream physics, had it not been for the Rencontres de Moriond and the credibility they lent to such efforts. In addition to the meetings themselves, and the opportunities they provided for interactions among the participants, the Proceedings from each meeting played an important role by collecting together many of the early experimental results and theoretical ideas. In the early years these were usually edited by Orrin Fackler and Vân himself. We all owe this group, under the leadership of J. Trân Thanh Vân, and more recently Jacques Dumarchez, a deep sense of gratitude.

In March of 2015, on the occasion of the 50th anniversary of the Rencontres de Moriond, I was asked by Jacques Dumarchez to give another general interest

[10] We have learned a great deal from these experiments, for example, that great care must be exercised in continuing gravity measurements taken at the surface of the Earth upward to towers or downward to mines.

"special lecture" to a joint session of the two workshops that were meeting at the same time. I chose as the title of this talk, "Rencontres de Moriond and the 5th Force". Aided by my long-time collaborator Dennis Krause, we assembled a review of the entire history of the fifth force as recorded by the proceedings of the Rencontres de Moriond over the years since 1986. Following my talk, Jacques made the interesting observation that not only had Moriond given a boost to the fifth force but, reciprocally, the fifth force had helped Moriond by motivating the Rencontres to expand into new areas beyond particle physics. These included gravitation and atomic physics, which have become increasingly exciting areas, but which had not been regular topics prior to 1986.

6.4.2 Some Amusing Moments

The *New York Times* story, and the associated depiction of the falling coin and feather, spawned a number of amusing moments, some intentional and others not. In the former class was a cartoon published in the *Seattle Post-Intelligencer* shortly after the *Times* story drawn by Steve Greenberg (see Fig. 6.10). This was clearly based on the depiction of the fifth force in the *Times* drawn as opposing gravity, and hence acting as a new "anti-gravity" force (see Sect. 6.3.1). Idella Marx, whose husband Louis Marx had been on the cover of *Time* magazine, had much more experience with the media than I did, and so took it upon herself to obtain the

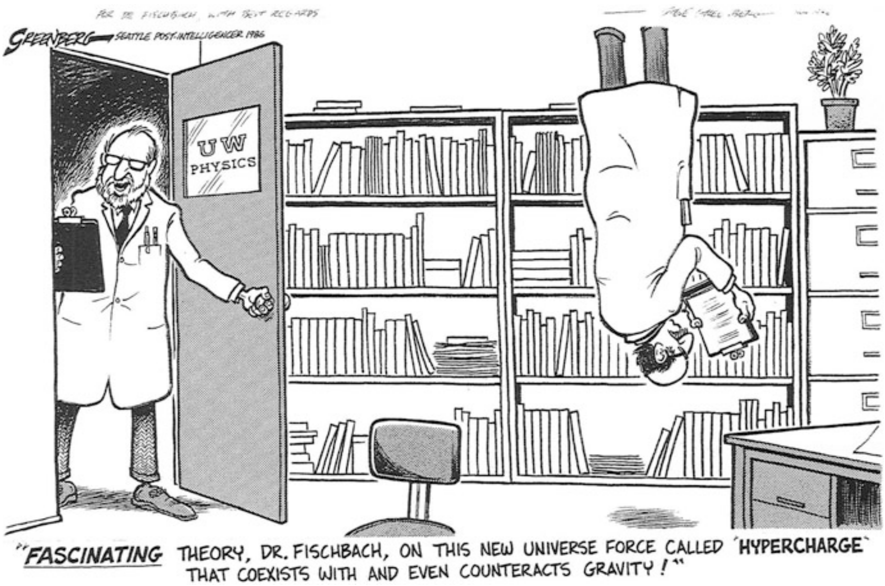

Fig. 6.10 Cartoon by Steve Greenberg published in the *Seattle Post-Intelligencer* on the fifth force

original of that cartoon for me. I learned from her that the authors of cartoons often sell the originals as an additional source of income. She asked Greenberg to donate the original to me, which he graciously did along with his autograph, and it now hangs in my study.

In the category of unintentional amusing moments spawned by the fifth force is another "coin and feather" story, and its consequences. In 2000 I was nominated by my department head to interview for an assignment with the Thinkwell company of Austin, Texas. This involved filming a series of laboratory demonstrations to accompany an online undergraduate text that they were developing. For each candidate the "interview" consisted of filming a demonstration of the applicant's choosing in which he/she explained the physics behind the demonstration. Naturally I chose the "coin and feather" demonstration, which began with me demonstrating that with air present in the glass tube apparatus, the coin fell faster, as we expected. I then rotated the stopcock on the glass tube and started the vacuum pump to remove the air. Finally I turned the glass tube upside down to demonstrate that in a vacuum the coin and feather fell at the same rate. Except that they didn't! At that instant I responded by blurting out "…because this demonstration *didn't* work this proves that it is a genuine physics demo!" What had happened was that the glass tube had a somewhat unusual stopcock which required another 1/4 revolution to connect to the vacuum pump. I quickly repeated the demonstration and explanation which now worked. Since I was pressed for time, I decided not to edit the film and sent it as is to Thinkwell. To my surprise I was hired for the assignment and, as it turns out, my humorous response to the original failure turned out to have been a net plus in my interview.

As time went on, and it became clear that a fifth force with the characteristics we assumed did not exist, I became known in the family as "…the discoverer of the non-existent fifth force." Naturally, I took this in good spirits, particularly since it fostered a collective sense of humor in our family which we all appreciated. An incident which (almost) happened occurred on October 23, 1993 when my son Jono took the SAT college entrance exam, while many other students across the country took the alternative ACT test. The latter included a reading comprehension section on the fifth force taken from a piece written by Michael Lemonick entitled "Working Against Gravity".[11] I received a number of calls that day from friends and former grad students whose children took the ACT and recognized my name. We can only speculate what Jono's reaction would have been had he taken the ACT rather than the SAT. Would he have answered the question correctly? Would the surprise of being confronted with that question have distracted him and impacted his overall performance? Fortunately we will never know—he did quite well on the SAT and was accepted to Princeton.

An amusing incident which did happen took place during the summer of 1987. We had arranged to meet Jerry and Sharon Lloyd along with their children Brendan and Heidi whom we had met during my sabbatical at UW. Brendan and our son

[11] Although the ACT declined my request for a copy of the question, they indicated that the same passage was administered to approximately 308,000 test takers between 1990 and 1999.

Jeremy had become close friends, and so we decided to meet in Durango, Colorado for a week together. One day we decided to take the famous train ride from Durango to Silverton, and we sat in our open gondola car as the collection of five children from the two families scampered from side to side to better view the spectacular scenery. A very staid passenger looked upon the scene with silent—but obvious—disapproval. On the return trip from Silverton to Durango I ended up chatting with him and learned that he was a high school physics teacher from Quebec. One thing led to another, and when he eventually learned that I was a physics professor at Purdue he asked whether I knew the individual who was working on the fifth force. When I acknowledged that I did, he kept asking questions, not quite realizing who I was. Gradually, like those old Polaroid pictures which slowly came into focus, he realized that I was the individual he was asking about. At that point we broke the ice, and we both enjoyed a big laugh.

No discussion of the humor associated with the fifth force could be complete without reference to the spoof written by Lawrence Krauss, which was actually submitted for publication to *Physical Review Letters* shortly following the appearance of our paper. Krauss, who was then at Yale and is now at Arizona State University, distributed a preprint (which I received) entitled "On Evidence for a Third Force in the Two New Sciences: A Reanalysis of Experiments by Galilei and Salviati." This "paper" is quite funny, but at the time I had no idea that this was actually submitted for publication in PRL. George Basbas, who was the PRL editor at the time, obviously realized this was a spoof, and returned six reports on it, "one [report] for each force." Although the Krauss paper was not accepted by PRL, it was published in 2008 in *Physics Today* (Krauss 2008), along with the six "reports", and is well worth reading.

6.4.3 Fifth Force Stories: Journals vs. Magazines

The publication of the *New York Times* story about our work on Wednesday, January 8, 1986 was preceded by short items on Tuesday evening on NBC radio, and on CBS TV evening news with Bob Schieffer. Following the full story in the *New York Times*, stories also appeared in newspapers all over the world. Given the overwhelming world-wide impact of the *New York Times* story, there can be little doubt that—at least in those days—the *New York Times* exerted an enormous influence in determining which stories were newsworthy. I recall somebody with expertise in such matters opining that virtually every major newspaper in the world must have mentioned this story in the subsequent weeks, including one of my favorites, a newspaper in Iceland. Subsequently, "fifth force" made it into an Icelandic–English dictionary that was being compiled by my friend Christopher Sanders and others. (For the record the translation is "ofurhl∂slu kraft/ur" (Hólmansson et al. 1989).)

For many of these stories the journalists/science writers contacted me directly, and I could tell immediately that some were much more eager than others to spend the time to understand the details of what we had done, and what the implications would have been if there really were a fifth force. Among the many newspapers that ran stories in the subsequent weeks and months, I was particularly impressed with both the *New York Times* and the *Los Angeles Times*.

One of the persistent problems in dealing with the popular press was ensuring that they appropriately credited my co-authors: my students Daniel Sudarsky, Aaron Szafer, and Carrick Talmadge, as well as Sam Aronson whose early collaboration with me was the motivation for the EPF analysis. My co-authors on the original (and subsequent) papers were exceptionally talented as individuals and as a group, and their contributions to this paper were as significant as my own. I was particularly interested in seeing to it that Carrick be recognized since this work became the central part of his Ph.D. thesis. Although I had little influence over major publications such as the *New York Times* and the *Los Angeles Times*, they mentioned all of the co-authors of our paper, for which I was deeply grateful. For our local newspaper, the *Lafayette Journal and Courier*, I felt that I could exert greater influence, and I did whenever possible. I recall receiving a call at home on the eve of Rosh Hashanah just as I was leaving for services at our synagogue. I explained to the reporter why I couldn't talk, but he was eager for an interview anyway. So I agreed to meet with him after services at the newspaper, in exchange for a commitment on his part to feature my students in his story. So following services I drove to the newspaper and rang a bell at the particular entrance where we had agreed to meet. By now it was nighttime, and we stood huddled at the entrance to the paper talking in the dark, in a scene that evoked in me images of "Deep Throat" speaking to Carl Bernstein and Bob Woodward.

In dealing with the popular press, whether in the form of newspapers or magazines, I often felt the tension between me as a scientist trained to appropriately cite other researchers whose work motivated my own, and story writers who almost always labor under stringent word limits for their stories. This became more of a problem as other researchers entered the field and made significant contributions of their own, which deserved to be recognized in print.

For me, the most dramatic example of this tension presented itself in the story by John Boslough in the May 1989 issue of *National Geographic* (Boslough 1989). This story was based in part on a dinner in Perth, Australia to which John had invited Eric Adelberger and me. Eric and I were attending the Fifth Marcel Grossmann meeting in Perth, August 8–13, 1988, and John was interested in learning more about both the underlying theoretical ideas (from me), and the experimental situation (from Eric). Following my review of the motivation for our reanalysis of the EPF experiment, Eric gave a nice description of his experiment, emphasizing (appropriately) the many improvements his Eöt-Wash collaboration had made over both the previous RKD and BP experiments. In fact Eric's presentation was so compelling, that I entertained the humorous thought that perhaps the reason why he was not reproducing the EPF results was that his experiment was *too* perfect!

Eventually John's story was completed, and led to my first encounter with "fact checkers", members of the *National Geographic* staff whose job it was to literally check and verify every fact and statement in the story. I was sent a pre-publication copy of the story and asked to verify a number of items directly related to parts of the story relating to me. There were indeed a few minor mis-statements which I pointed out, but my task did not end there: I was asked to replace the existing text with a corrected version that would not take up additional space. In most journals, such a request made by a referee would not be a problem, since space is not usually an issue. However, the changes that were required were mostly ones which would have benefitted from greater elaboration, and hence more space—which I was not allowed. Nonetheless, I worked closely with the two fact checkers to arrive at a compromise, and they were appreciative for my efforts on their behalf.

By this time I had developed a close relationship with the fact checkers over the course of several phone conversations, and so I decided to press them to correct what I felt was an unfortunate omission in John Boslough's otherwise superb story: there had been no mention of the elegant experiment by Peter Thieberger from Brookhaven National Laboratory, which was the very first experimental test of the fifth force idea, and which had in fact found a positive result which could have been interpreted as supporting our EPF analysis (Thieberger 1987). Although subsequent experiments have not found evidence supporting the idea of a fifth force, it is not clear what—if anything—was wrong with Thiebeger's experiment. It became clear immediately that there were two problems that I was facing in trying to include mention of Thieberger's work: Although *National Geographic* and its authors were presumably happy to have me correct aspects of the story as written, they were not inclined to allow me to modify the story by including new material. Additionally, whatever new material I wanted to add would have to come up against the stringent space requirements discussed above.[12] I decided to tackle the second problem first, by compressing a description of Thieberger's experiment down to 26 words. I then found a comparable savings elsewhere in the story, so my suggestion was "word neutral". Although I do not know exactly what happened thereafter, I presume that the fact checkers must have contacted John Boslough and received his approval, which thus solved the first problem. In the end my proposed text appeared in the final published version on p. 570, much to my delight.

6.4.4 John Maddox and Nature

The publication of our PRL occurred during the period when John Maddox was the editor of *Nature*. Although our original paper was not published in *Nature*, John took a keen interest in our work, and wrote several favorable editorials on the subject (Maddox 1986a,b, 1987, 1988a,b,c, 1991). Additionally, he invited Carrick and me to write a review of the field to be published in 1991, which would have allowed us to adopt the mellifluous title "Five Years of the Fifth Force". Unfortunately, various delays ensued, so that by the time the review appeared in 1992 (Fischbach and

[12]Recall that, in contrast, PRL allowed us to exceed their nominal length allowance in order to address a question raised by Dicke, as we note in Sect. 6.2.5.

Talmadge 1992a), we were forced to change "Five" to "Six". Based on conversations I had at the time it is clear that the prestige of *Nature* was such that our review, along with John's editorials, gave our work and the field in general a significant boost at a critical time.

6.5 My 1985–1986 Sabbatical at the University of Washington

As noted above I had been invited to spend the 1985–1986 academic year at the Institute for Nuclear Theory (INT) at UW, mostly due to the efforts of Wick Haxton who had been an Assistant Professor at Purdue before joining the UW faculty. I was warmly welcomed by the INT faculty, including Ernest Henley, Larry Wilets, and Jerry Miller among others. The INT faculty went well beyond what would have ordinarily been expected of them. For example, INT agreed to pay to have the snakewood sample chemically analyzed, and to pay the INT secretary JoAnn LaRock overtime to come in on a weekend to help me answer the dozens of letters I received following the publication of our paper. More importantly, the UW faculty viewed our paper seriously to the extent that several faculty undertook experiments to test the implications of our PRL paper. Most notably, Eric Adelberger, a well-known and highly respected nuclear physics experimentalist, and now member of the National Academy of Sciences, established the "Eöt-Wash" collaboration (a pun on the Hungarian pronunciation of "Eötvös"). He has by now become the world's leading experimentalist in searching for deviations from the predictions of Newtonian gravity. Eric was joined over the years by Jens Gundlach, Blayne Heckel, Fred Raab, and Chris Stubbs among others, and the work of this group continues to date. In addition, Paul Boynton entered the field, and over time joined forces with Riley Newman and Sam Aronson. The work of this group also continues to date. Among the efforts at the time, Dick Davisson (a son of Nobel Laureate Clinton Davisson) designed an extremely clever test for a composition-dependent fifth force using a MACOR sphere suspended in water by means of an "inverse Cartesian diver". Unfortunately this experiment was never completed.

During my stay at the UW I enjoyed the many conversations I had with Eric Adelberger and other members of the Eöt-Wash collaboration as well as with Paul Boynton and his group, and with Dick Davisson. Although I was never an actual participant in any of the UW experiments, I kept in reasonably close contact with the various experimental efforts. So it came as no surprise to me when I received a call one evening from Paul Boynton urging me to return to the Physics Department, because he was seeing evidence for a fifth force. I was almost ready to leave home, a 45 min drive to UW, when I sensed that he was just trying to test me, so that in the end his call was just an attempted prank.

However, some time later the Boynton group did in fact claim to see evidence for a fifth force (since withdrawn), and their paper was accepted for publication in

PRL (Boynton et al. 1987). Having learned from Paul that the American Institute of Physics (AIP) was preparing a press release on this experiment, I quickly prepared my own spoof press release by modifying AIP letters I had received, along with a covering letter (Appendix 6). I arranged to have it sent to Paul from New York, the home of AIP, so that it would look authentic. Having been myself the subject of a number of stories in the press, I began with the usual stiff formal language, but then gradually introduced a "humor gradient", where each successive sentence was increasingly implausible. Paul was apparently taken in until the very end, and was on the verge of contacting the AIP and complaining when he realized that this was a spoof, and we all had a big laugh.

6.6 Short-Distance Searches for a Fifth Force

Just as our book (Fischbach and Talmadge 1999) was being completed, a new set of ideas was emerging leading to the prediction of new macroscopic forces manifesting themselves over very short distances (Antoniadis et al. 1998; Arkani-Hamed et al. 1999; Randall and Sundrum 1999). Broadly speaking these forces are a reflection of the hypothesis that we live in a world with n-additional compact spatial dimensions, which could manifest themselves over scales from submillimeter to angstrom distances, or even smaller. As can be seen from Figs. 6.11 and 6.12, the limits on the strength α as a function of the range λ of a new force become increasingly less stringent as λ gets smaller. As a result current limits on new forces at the sub-micron level allow for the existence of new macroscopic forces significantly stronger than gravity.

If we denote the scale of the n additional spatial dimensions as r_n, then in typical theories the effective gravitational potential $V(r)$ between two point sources is given

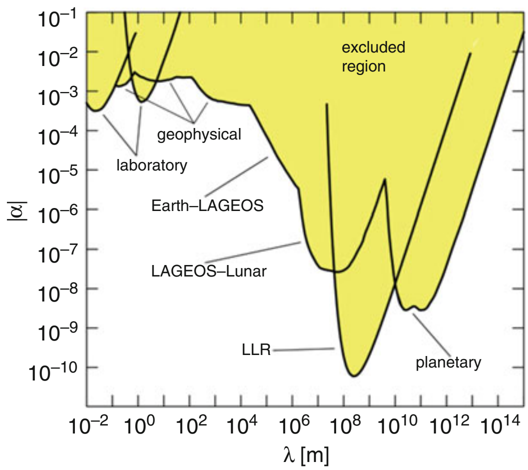

Fig. 6.11 Limits on the fifth force strength $|\alpha|$ for $\lambda \geq 1$ cm from laboratory, geophysical, and astronomical measurements (Adelberger et al. 2009)

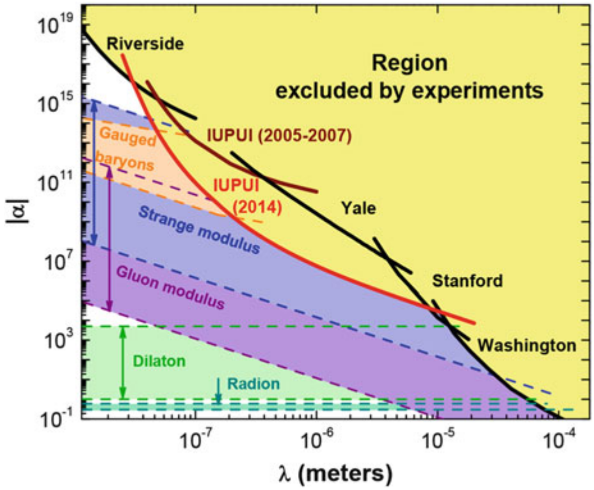

Fig. 6.12 Limits on the fifth force strength $|\alpha|$ for $\lambda \leq 0.1$ mm from short-distance force experiments along with predicted strengths from various theories (Chen et al. 2014). "IUPUI" labels constraints coming from experiments with Ricardo Decca and Daniel López utilizing "iso-electronic" effect experiments

by (Floratos and Leontaris 1999; Kehagias and Sfetsos 2000)

$$V(r) = \begin{cases} -\dfrac{G_\infty m_i m_j}{r}\left(1 + \alpha_n e^{-r/\lambda}\right) , & r \gg r_n , \\ -\dfrac{G_{4+n} m_i m_j}{r^{n+1}} , & r \ll r_n . \end{cases} \tag{6.30}$$

Here α_n is a dimensionless constant, which would depend on the number of additional spatial dimensions and their compactification, $\lambda \sim r_n$, and G_{4+n} is the more fundamental Newtonian constant in the $(4 + n)$-dimensional space-time. In a theory where all the additional dimensions are of the same size, and have toroidal compactification, then $\alpha_n = 2n$.

It follows from the preceding discussion that the signal for new physics implied by the presence of additional spatial dimensions would be a violation of the Newtonian inverse-square law, as discussed in Appendix 1. Given the facilities then available at Purdue, Dennis Krause and I joined with my colleague Ron Reifenberger and his graduate student Steve Howell to carry out an experiment using atomic force microscopy (AFM) at the nanoscale (Fischbach et al. 2001). This experiment, and all subsequent experiments that we carried out at this scale (see below), was complicated by the Casimir force, the attractive force between two bodies due to vacuum fluctuations (Bordag et al. 2015; Simpson and Leonhardt 2015). Although this force is negligible for macroscopic experiments, it is the dominant known force between electrically-neutral non-magnetic bodies at the sub-micron separations which were of interest to us, and we eventually chose to deal with it in two complementary ways. The conceptually simplest was to calculate the Casimir force, and subtract it from the experimentally measured force. This was the heroic task carried out by our colleagues Vladimir Mostepanenko and Galina Klimchitskaya (Decca et al. 2005a). The second approach utilized what we called the "iso-electronic" effect in which one searches for force differences between dissimilar materials with similar electronic properties (and hence the same Casimir force), for example, two isotopes of the same element (Krause and Fischbach 2002; Fischbach et al. 2003). When we were eventually joined by our colleagues Ricardo Decca and Daniel López, a better technique emerged: simply measure the force difference between a probe and *any* two dissimilar samples coated with a common ∼150-nm thick layer of gold (Decca et al. 2005b). Since the Casimir force is primarily a *surface* effect, this layer is sufficiently thick to make the Casimir force between the samples and probe the same, but thin enough to permit force differences due to new gravity-like interactions which are *bulk* effects.

The experimental and theoretical collaboration among Dennis Krause, Ricardo Decca, Daniel López, Vladimir Mostepanenko, Galina Klimchitskaya, and me has now led to a long series of papers, resulting in the most stringent limits on a Yukawa-type fifth force in the 40–8000 nm range (Fig. 6.12) (Chen et al. 2014). Not surprisingly, these limits still allow new forces many times stronger than Newtonian gravity over short distances, and hence the community is not yet near the point of excluding new forces weaker than gravity, over these distances.

6.7 Our Book: "The Search for Non-Newtonian Gravity"

On April 18, 1986 I received a letter from Robert Ubell at the American Institute of Physics (AIP), copied to Rita Lerner, discussing the possibility of writing a book on the fifth force. These were very early days in the fifth force effort, but as the Consulting Editor in the AIP Books Division he was interested in such a project irrespective of what the eventual outcome would be. Following an exchange of letters in the ensuing months I received a letter from Rita on May 4, 1988 enclosing a contract. As originally envisioned, this book would be co-authored by Sam Aronson, Carrick Talmadge, and me. I drafted an outline of the proposed book on June 30, 1988, and by August 31, 1988 all three of us had signed and returned the contracts. In a subsequent letter dated November 15, 1989 from Tim Taylor, then the manager for AIP of the division in charge of our book, the target date for completing this book was set at August 1990.

The aforementioned dates are of interest for historical reasons, but primarily because they reveal how much longer it took for us to complete the book than we had anticipated. To start with, Sam was the Deputy Chairman of the Physics Department at Brookhaven at the time we signed the contract, and would eventually become Chairman as we have previously noted. Given his administrative responsibilities, Sam decided that it would be best if he were not a co-author. Carrick and I carried on, but each of us had other research and/or teaching responsibilities which had higher priority. We divided the topics in my earlier outline according to our respective interests and wrote as rapidly as our schedules allowed.

As has been my practice for many years, I broke up my assigned work into individual segments, and began with the segment that was easiest to write. This was on the significance of the shape of B/μ across the periodic table, and I handed my draft to my secretary Nancy Schnepp on January 8, 1991. It began with the words (Fischbach and Talmadge 1999, p. 23): “It is instructive to plot B/μ as a function of atomic number Z for the elements in the periodic table.” As indicated by the date on my first segment, we had obviously missed the proposed August 1990 deadline even before we started, and the situation only got worse. Fortunately AIP kept in touch with us, and were extremely understanding.

As time went on my embarrassment continued to increase, and in 1994 an opportunity arose in which I was able to reflect on this in a more public manner. On August 7, 1994 my graduate student Harry Kloor became the first person anywhere to receive two Ph.D. degrees for two completely different projects, in two different areas (physics and chemistry), *on the same day!* Given the novelty of this accomplishment, the *New York Times* sent a photographer to the graduation ceremony, and the *Times* did a story on him on August 8 (p. A6). As the chair of his physics Ph.D. committee, and also a member of his chemistry Ph.D. committee, I was asked to reflect on his achievement, which included defending both theses on the same day. My response was instructive: “What is intimidating is that in four months he wrote these two theses totally more than 700 pages, and I’m struggling to write a book with a co-author and we’ve barely done 200 pages in several years.”

Eventually, however, the book was completed and we sent it off on April 9, 1997 to Maria Taylor who was the editor then in charge of our book. Totaling more than 300 pages, it is an attempt to give a beginning graduate student an introduction to all of the relevant facets of research into the fifth force from both the experimental and theoretical viewpoints, as they were understood by us. By the time our book was actually published in 1999, AIP had joined forces with Springer-Verlag, so that our book appeared as a Springer title.

6.8 Epilogue

As noted in the Introduction, approximately 30 years have elapsed since the publication in PRL of our original paper on the EPF experiment, and so it is appropriate to reflect on what we have learned during this time about a possible fifth force. With the exception of the EPF experiment itself, and possibly the Thieberger floating ball experiment (Thieberger 1987), there is at present no evidence for any deviations from the predictions of Newtonian gravity on any length scale from the solar system down to sub-atomic scales. This conclusion, which applies to both composition-dependent and composition-independent tests, as well as to data on the behavior of the K^0–$\overline{K}^0$ system, is supported by dozens of experiments and hundreds of phenomenological papers.

However, questions remain about the EPF experiment, and to a lesser extent about the Thieberger experiment, and so we cannot close the book on the fifth force story quite yet. Broadly speaking, the EPF correlations could arise from a broad class of interactions characterized by a potential of the form

$$V_{ij} = B_i B_j F(\boldsymbol{r}_i, \boldsymbol{r}_j, \boldsymbol{v}_i, \boldsymbol{v}_j, \boldsymbol{s}_i, \boldsymbol{s}_j, ?) \tag{6.31}$$

where B_i and B_j are the baryon numbers of the samples, and where $F(\ldots)$ is a function of the other variables (such as position $\boldsymbol{r}$, velocity $\boldsymbol{v}$, spin $\boldsymbol{s}$, etc.) upon which V_{ij} could depend. The critical point in (6.31) is that since B_i and B_j are non-classical quantities, it has not yet been proven possible to account for the EPF correlation in terms of classical systematic effects such as temperature or gravity gradients. Although there may be other systematic effects to be reckoned with, it is clear from what we know that Eötvös, perhaps the greatest "classical" physicist of his time, worried about these in great detail.

It might then be argued that this correlation is just a statistical fluke. However, as noted in Fischbach et al. (1988), the likelihood that EPF obtained $\Delta\kappa \neq 0$ by a statistical accident is extremely small, approximately 5×10^{-12}. Moreover, in a comment at a Moriond conference, de Rújula noted that for the eight "good" points in Figs. 2–5 of Fischbach et al. (1988) the probability of simply getting the *sequence* correct is $2/8! \approx 5\times10^{-5}$. Finally, the likelihood of accidentally obtaining approximately the same accelerations for Pt and $CuSO_4 \cdot 5H_2O$, as discussed in Sect. 6.2.3.1, adds to the burden carried by any argument that the EPF data are merely a statistical anomaly.

There is clearly some "tension" between the many careful experiments, most notably from the Eöt-Wash group, which see no evidence for a fifth force, and the EPF experiment. What we can say, however, is that the simple model for a fifth force proportional to baryon number, as presented in our original PRL, is clearly not supported by the totality of existing data. However, we cannot at this stage dismiss the possibility that the function $F(\ldots)$ in (6.31) above could be quite different from what we originally proposed, in such a manner as to admit the possibility of a different kind of fifth force.

Although the final chapters in the fifth story are yet to be written, it is clear that the EPF data have already had a significant impact on gravitational physics by motivating a large number of new (and sometimes novel) experiments and theories. On the experimental side, the torsion balance experiments of the Eöt-Wash group (Adelberger et al. 2009), of Nelson et al. (1988), Boynton (1988), Fitch et al. (1988), and others can be viewed as direct descendants of the EPF experiment, just as that experiment is the descendant of the Guyòt experiment (Fischbach and Talmadge 1999). However, the EPF experiment also stimulated a large number of novel gravitational experiments. These include the floating ball experiments of Thieberger (1987) and Bizzeti et al. (1989); the dropping experiments of Niebauer et al. (1987), Cavasinni et al. (1986), and Kuroda and Mio (1989); the pumped lake experiments of Hipkin and Steinberger (1990) and Cornaz (1994); the Laplacian detector of Moody and Paik (1993); and of course the various tower experiments discussed earlier (Sect. 6.3.8). Finally, the EPF experiment has no doubt played a role in motivating the upcoming MICROSCOPE experiment, which will be the first space-based test of the Weak Equivalence Principle (Touboul and Rodrigues 2001).

On the theoretical side, the early work by Fujii (1971, 1972, 1974, 1975, 1981), Fujii and Nishino (1979), Gibbons and Whiting (1981), and others discussed above, along with the many theories motivated by the EPF data, have drawn attention to the connection between low-energy gravity experiments and high-energy elementary particle physics. This connection, which is explored in Fischbach and Talmadge (1999), can be summarized as follows. Two natural mass scales arise in elementary particle physics, the nucleon mass $m_{\mathrm{N}} \sim 1\,\mathrm{GeV}/c^2$, and the Planck mass $M_{\mathrm{P}} \equiv \sqrt{\hbar c/G_{\mathrm{N}}} \sim 10^{19}\,\mathrm{GeV}/c^2$, where G_{N} is the Newtonian gravitational constant. Their ratio $m_{\mathrm{N}}/M_{\mathrm{P}} \equiv \sqrt{f^2/\hbar c} \sim 10^{-19}$ defines a new dimensionless constant f which is the analog for some putative new force of the electromagnetic charge e. In many theories the product $\mu \equiv m_{\mathrm{N}}\sqrt{f^2/\hbar c} \sim 10^{-10}\,\mathrm{eV}/c^2$ defines yet another mass scale whose Compton wavelength $\lambda = \hbar/\mu c \approx 2000\,\mathrm{m}$. If μ is the mass of a light bosonic field, then the combination of the parameters f and μ could characterize a new field of gravitational strength whose influence would extend over macroscopic distances. It follows that a search for new macroscopic fields of gravitational strength is yet another means of studying high-energy particle physics. As noted in Sect. 6.6, theories which introduce additional compact spatial dimensions provide yet another link between gravitation and high-energy physics.

In our original PRL we attempted to bring together three anomalies that presented themselves in the 1986 time frame (Fischbach et al. 1986c, Fig. 1; Schwarzschild 1986). These were the EPF data, the discrepancy between the geophysical determinations of G_{N} and the laboratory value, and the anomalous energy dependence of the K^0–$\overline{\mathrm{K}}^0$ parameters, as discussed in Appendix 2. We have already considered the EPF data, and also noted that the original results of Stacey et al. (1987b) were likely due to "terrain bias", as discussed by Bartlett and Tew (1989a). This leaves the puzzling energy dependence of the K^0–$\overline{\mathrm{K}}^0$ parameters as the remaining anomaly to be explored.

At the time I arrived at the University of Washington in August 1985, an experiment was underway at Fermilab measuring the mean life τ_{S} of $\mathrm{K}^0_{\mathrm{S}}$ over the momentum range 100–350 GeV/c. Sam Aronson, Carrick Talmadge, and I were very interested in this experiment for obvious reasons, and through Sam we maintained contact with this group as they analyzed their data. When the results of this experiment were published (Grossman et al. 1987), they revealed no dependence of τ_{S} on the $\mathrm{K}^0_{\mathrm{S}}$ momentum. Understandably, this had the effect of eliminating the second "leg" of our putative 3-way coincidence among the above anomalies depicted in Fig. 1 of Fischbach et al. (1986c). The experiment of Grossman et al. was done quite carefully, especially given that they were fully aware of the ABCF results, and made repeated references to them.

However, in contrast to the result of Stacey and Tuck, no explanation for the apparently anomalous results obtained by ABCF from Fermilab E621 has emerged. In this way the situation with respect to the ABCF results is somewhat similar to that for the EPF data. With respect to ABCF, Grossman et al. carefully note the difference between their experiment and E621, including the fact that their experiment studied decays from K_S^0 made in proton–tungsten collisions, rather than via K_S^0 regeneration as in E621. Additionally they chose a proper time range where "the contribution of CP non-conservation is insignificant." However, one difference which was not noted is that the E621 beam line was not horizontal (i.e., parallel to the Earth's surface), but rather entered the ground at approximately 8.25×10^{-3} rad to a detector below ground. The possibility that this difference could be relevant has been raised privately with me by Gabriel Chardin, who has independently explored the possibility that CP-violation could be due to some external field (Chardin 1990, 1992). Given that there is no fundamental theory of CP-violation at present, such a mechanism—although unlikely—cannot be excluded at present.

The situation with respect to the ABCF analysis of the E621 data reminds me of a conversation I had some years ago with Melvin Schwartz, who shared the Nobel Prize with Leon Lederman and Jack Steinberger for the discovery of the muon neutrino. I had been invited to talk at Brookhaven on the fifth force, following which several of us went to dinner. In reflecting on the EPF experiment, Schwartz told me of an experiment he tried to carry out some years earlier where he kept getting the "wrong" result. I do not recall why he thought the result was wrong, whether because it disagreed with another experiment or with theory. In any case he kept trying to look for something amiss in his experiment, but to no avail. Finally he decided to disassemble the experiment completely, lead brick by lead brick, and then rebuild it from scratch. For whatever reason, the rebuilt experiment (using exactly the same equipment) now obtained the "right" answer. Schwartz was not able to figure out why these two seemingly identical versions of the same experiment gave different results, and this obviously continued to trouble him.

Although we may never figure out why E621 gave the results obtained by ABCF, I suspect that in time we will eventually understand the EPF data, whatever they reveal in the end. Perhaps there is some subtle detail in E621 or EPF to which we are not paying attention, which is the secret. I am reminded of an appropriate line from the novel *A Taste for Honey* by H.F. Heard (1980):

> This situation is in some way what we all confront in life: those people and events which we treat most contemptuously and thoughtlessly are just those which, watching us through their mask of insignificance, plead with us to understand and feel, and failing to impress and win us, have no choice but to condemn us, for we have already condemned ourselves.

It might thus be an amusing resolution of the fifth force story if the understanding of the EPF experiment was hiding in plain sight all along.

Finally, let me conclude with an update of my co-authors on our PRL. As noted previously, Sam Aronson became chairman of the Physics Department at Brookhaven National Laboratory, and eventually the Director of Brookhaven. He is now (2015) President of the American Physical Society. Carrick Talmadge received his Ph.D. under my supervision in 1987, and eventually switched his interest to acoustics and the human ear. He is now a senior scientist and research associate professor with the National Center for Physical Acoustics at the University of Mississippi. Daniel Sudarsky received his Ph.D. under my supervision in 1989, and is currently a professor at UNAM in Mexico City. Aaron Szafer left Purdue in 1986 with a Master's degree, and received his Ph.D. at Yale in 1990. He is now a technical program manager at the Allen Institute for Brain Science in Seattle.

Acknowledgements I wish to express my deep indebtedness to Dennis Krause whose close collaboration with me on this project has allowed it to be completed in a timely manner. I also wish to thank Allan Franklin for his many helpful editorial suggestions on the early drafts of this manuscript, and Nancy Schnepp for her assistance in organizing this paper. Finally I wish to express my thanks to my co-authors Sam Aronson, Daniel Sudarsky, Aaron Szafer, and Carrick Talmadge who, along with Riley Newman, Peter Thieberger, Mike Mueterthies, and my colleagues at the University of Washington who have helped to clarify various aspects of the historical narrative.

Appendix 1 Fifth Force Phenomenology

In this appendix, I present a summary of the fifth force phenomenology adapted from Fischbach and Talmadge (1999). In the formalism assumed in the original PRL (Fischbach et al. 1986a), the total potential energy $V(r)$ between two interacting samples i and j is the sum of the Newtonian potential $V_{\mathrm{N}}(r)$ and a new fifth force potential $V_5(r)$, viz.,

$$\begin{aligned} V(r) = V_{\mathrm{N}}(r) + V_5(r) &= -\frac{G_\infty m_i m_j}{r} + \frac{f^2 B_i B_j}{r} \mathrm{e}^{-r/\lambda} \\ &= -\frac{G_\infty m_i m_j}{r} \left(1 - \frac{f^2 B_i B_j}{G_\infty m_i m_j} \mathrm{e}^{-r/\lambda}\right) \\ &\equiv -\frac{G_\infty m_i m_j}{r} \left(1 + \alpha_{ij} \mathrm{e}^{-r/\lambda}\right) , \end{aligned} \tag{6.32}$$

where G_∞ is the Newtonian gravitational constant in the limit $r \to \infty$, in which case the contribution from $V_5(r) \to 0$. The functional form of $V_5(r)$ is suggested by models in which this contribution arises from the exchange of an appropriate boson of mass m, and hence $\lambda = \hbar/mc$. B_i and B_j are the respective baryon numbers of i and j, and f is the analog for the putative baryonic force of the electromagnetic charge e. It is conventional to express all masses in terms of the mass of hydrogen $m_{\mathrm{H}} = m({}_1\mathrm{H}^1) = 1.00782519(8)u$, in which case we write $m_i = \mu_i m_{\mathrm{H}}$, and

$$\alpha_{ij} = -\frac{B_i}{\mu_i} \frac{B_j}{\mu_j} \xi , \tag{6.33}$$

where

$$\xi = \frac{f^2}{G_\infty m_{\mathrm{H}}^2} \,. \tag{6.34}$$

We note from (6.32), (6.33), and (6.34) that in the presence of $V_5(r)$ the potential energy $V(r)$ depends not only on the masses m_i and m_j, but also on the compositions of the samples via their respective values of B_i/μ_i and B_j/μ_j. As we now show, the accelerations of the two test masses j and k in the presence of a common source i (e.g., the Earth) will depend on the compositions of j and k through the difference $(B_j/\mu_j - B_k/\mu_k)$.

Returning to (6.32) we can calculate the force $\boldsymbol{F}(r) = -\nabla V(r)$:

$$\begin{aligned}\boldsymbol{F}(r) &= -\frac{G_\infty m_i m_j}{r^2}\hat{\boldsymbol{r}}\left[1 + \alpha_{ij}\left(1 + \frac{r}{\lambda}\right)\mathrm{e}^{-r/\lambda}\right] \\ &\equiv -\frac{G(r) m_i m_j}{r^2}\hat{\boldsymbol{r}} \,. \end{aligned} \tag{6.35}$$

In the form of (6.35) the force exerted by m_i on m_j is governed by a "variable Newtonian constant" $G(r)$ which depends not only on r, but also on the compositions of i and j. For experiments carried out over distance scales where $r/\lambda \ll 1$ holds, we can write approximately

$$G(r) \approx G(0) \equiv G_0 = G_\infty(1 + \alpha_{ij}) \,, \tag{6.36}$$

so that G_0 can be identified with the normal laboratory value. At the other extreme for planetary motion, or for some space-based experiments, where $r/\lambda \gg 1$, $G(r) \approx G(\infty) \equiv G_\infty$. The geophysical experiments of Stacey and Tuck (Stacey 1978; Stacey et al. 1981; Stacey and Tuck 1981), which provided part of the motivation for our reanalysis of the EPF experiment, can be viewed as a determination of the difference between G_0 and $G(r)$ for $\lambda \approx 200\,\mathrm{m}$.

Returning to (6.35) we see that the presence of the term proportional to α_{ij} leads to two general classes of experiments directed towards searching for a possible fifth force through deviations from the predictions of Newtonian gravity. Broadly speaking these are (a) searches for a *composition dependence* of α_{ij} (also called WEP-violation searches), and (b) searches for an *r-dependence* of $G(r)$. The latter are also referred to as tests of the gravitational inverse-square law, or *composition-independent* tests. Although in principle the term proportional to α_{ij} in (6.35) will generally give rise to both composition-dependent effects and to deviations from the inverse-square law, in practice most experiments have been designed to optimize the search for one or the other effect.

In the preceding discussion we have viewed deviations from the predictions of Newtonian gravity as arising from the presence of a new intermediate-range interaction, as in (6.32). However, similar deviations could also arise from the gravitational interaction itself if gravity did not couple to all contributions to the mass-energy of a test mass with a common universal strength. To this end it is useful to view an atom, and particularly the nucleus, as a "universal soup" of particles in which almost any particle and any interaction (real or virtual) can be present, if only fleetingly. Thus, although we may naively think of an atom as being composed of protons, neutrons, and electrons, in reality part of the mass-energy of an atom arises from virtual e^+e^- pairs, $\pi^\pm$ and π^0 mesons, etc. Hence, if any of the real (p, n, e) or virtual (e^+, e^-, $\pi^\pm$, ...) contributions to the mass-energy of an atom behaved anomalously in a gravitational field, this could produce a non-zero result in a WEP or fifth force experiment. Since these are differential experiments, which compare the forces on two samples, detecting these anomalous behaviors depends on choosing samples for which the anomalous contribution(s) comprise different fractions of the total mass-energy of each sample. Thus by an appropriate choice of pairs of samples one can in principle determine whether the anomalous behavior is due to an external fifth force field coupling to baryon number, isospin, etc., or to a fundamental violation of Lorentz invariance (Fischbach 1965; Fischbach et al. 1985), or to some entirely different mechanism. The observation that a Lorentz non-invariant interaction (LNI) can also show up in WEP experiments is of renewed interest at present in connection with more general searches for LNI effects (Mattingly 2005). Typically an anomalous coupling of gravity to a particular form of energy (e.g., the weak interaction contribution E_W to a nucleus) would give rise to a WEP-violating acceleration difference Δa_{1-2} of two test samples of masses M_1 and M_2 having the form

$$\frac{\Delta a_{1-2}}{a} = \eta_W \left(\frac{E_{W1}}{M_1} - \frac{E_{W2}}{M_2} \right) , \tag{6.37}$$

where η_W is the WEP-violating parameter we are seeking to determine (Fischbach et al. 1985).

In addition to their "universality", another feature of WEP experiments which makes them so interesting is their great sensitivity. Existing laboratory experiments can measure fractional acceleration differences $\Delta a/a$ between samples at roughly the 10^{-13} level, and anticipated space-based experiments such as MICROSCOPE (Touboul and Rodrigues 2001), may push the sensitivity down to 10^{-15}–10^{-16}. At these levels the combination of the universality and sensitivity of WEP experiments makes it interesting to search for various higher-order processes which may be conceptually important, but make relatively small contributions to the mass-energy of a nucleus.

This was the motivation we had in mind in 1995 when I joined with my colleagues Dennis Krause, Carrick Talmadge, and Dubravko Tadić to consider the possibility that an anomalous coupling of neutrinos (ν) and/or antineutrinos ($\overline{\nu}$), to gravity. Neutrinos have been a continuing source of surprises in elementary particle physics starting with their very existence, their role in parity non-conservation, and more recently in flavor oscillations and the solar neutrino problem. As virtual particles the exchange of ν–$\overline{\nu}$ pairs of any flavor gives rise to a 2-body interaction among pairs of nucleons which was first calculated by Feinberg and Sucher in the current–current model (Feinberg and Sucher 1968), and later by Feinberg, Sucher, and Au in the Standard Model (Feinberg et al. 1989). This interaction makes a small contribution to the nuclear binding energy, and hence the question is whether an anomaly in this small contribution could nonetheless be large enough to be detectable in a present or future WEP experiment.

In principle the nuclear binding energy contribution from the exchange of ν–$\overline{\nu}$ pairs, which gives rise to a nucleon–nucleon potential energy $V_{\nu\overline{\nu}}(r)$ proportional to $1/r^5$, could be evaluated for a given nucleus in analogy to the evaluation of the Coulomb contribution $V_C(r)$ which is proportional to $1/r$. However, the contribution from a $1/r^5$ potential would diverge as $r \to 0$ were it not for the nucleon–nucleon hard-core separation, $r_c \approx 0.5\,\text{fm}$, which sets a lower limit on r. As shown in Fischbach et al. (1995), evaluation of $\langle 1/r^5 \rangle$ over a spherical nucleus for $r_c \neq 0$ can be facilitated by use of techniques from the field of geometric probability. This led to the suggestion that an anomalous coupling of gravity to ν or $\overline{\nu}$ could lead to a WEP violation $\Delta a/a \approx 10^{-17}$. Although this is below the nominal sensitivity of current terrestrial experiments, or of the forthcoming space-based MICROSCOPE experiment, it is possible that a ν–$\overline{\nu}$ anomaly could be larger than the predicted nominal value and hence be detected. Should the MICROSCOPE experiment, or any other experiments, detect a WEP-violating anomaly ($\Delta a_{1-2}/a \neq 0$), then in principle future experiments could determine the underlying mechanism for this violation by studying the dependence of the anomaly on the compositions of various pairs of test samples.

The possibilities of searching for an anomalous coupling of gravity to neutrinos via the 2-body potential $V_{\nu\overline{\nu}}(r)$ eventually led to an analysis of many-body contributions arising from neutrino exchange (Fischbach 1996). Although higher-order long-range forces arising from many-body neutrino exchanges are greatly suppressed, they can also be significantly enhanced in some circumstances due to various combinatoric factors. This had led to the suggestion of a lower bound on neutrino masses, $m_\nu \gtrsim 0.4\,\text{eV}/c^2$ (Fischbach 1996).

Appendix 2 Phenomenology of the Neutral Kaon System

We briefly review the phenomenology of the neutral kaon system which played an important role in motivating our re-analysis of the EPF experiment. As noted in Sect. 6.1.1, when K^0 and $\overline{K}^0$ are produced by strong interactions they are eigenstates of strangeness S, with eigenvalues $S = +1$ (K^0) or $S = -1$ ($\overline{K}^0$). However, because strangeness is not conserved by the weak interactions which govern kaon decays, the eigenstates of the total Hamiltonian are K_L^0 and K_S^0, which are linear combinations of K^0 and $\overline{K}^0$ given by Aronson et al. (1983a)

$$|K_L^0\rangle = \frac{1}{\sqrt{|p|^2 + |q|^2}} \left(p|K^0\rangle + q|\overline{K}^0\rangle \right) , \tag{6.38}$$

$$|K_S^0\rangle = \frac{1}{\sqrt{|p|^2 + |q|^2}} \left(p|K^0\rangle - q|\overline{K}^0\rangle \right) . \tag{6.39}$$

CP conservation implies that $p = q$, and hence the parameter $\epsilon = 1 - q/p$ is a measure of CP-violation, as are the parameters η_{+-} and η_{00} defined by

$$\eta_{+-} = |\eta_{+-}|e^{i\phi_{+-}} = \frac{A(K_L^0 \to \pi^+\pi^-)}{A(K_S^0 \to \pi^+\pi^-)} , \tag{6.40}$$

$$\eta_{00} = |\eta_{00}|e^{i\phi_{00}} = \frac{A(K_L^0 \to \pi^0\pi^0)}{A(K_S^0 \to \pi^0\pi^0)} . \tag{6.41}$$

Numerically (PDG 2014, p. 944),

$$|\eta_{+-}| = 2.232(11) \times 10^{-3} , \qquad |\eta_{00}| = 2.220(11) \times 10^{-3} ,$$
$$\phi_{+-} = 43.51(5)^\circ , \qquad \phi_{00} = 43.52(5)^\circ , \qquad |\epsilon| = 2.228(11) \times 10^{-3} .$$

As discussed in Sect. 6.1.3, the thread connecting kaon decays and our analysis of the EPF experiment emerged from our analysis of Fermilab data on K_S^0 regeneration. This is the phenomenon in which K_S^0 particles can be regenerated from a pure K_L^0 beam by passing that beam through a target such as hydrogen, carbon, or lead. This phenomenon is interesting since it is a probe of strong interaction models such as Regge pole theory. If we temporarily neglect the effects of CP-violation, then from (6.38) and (6.39) we can write approximately

$$|K_L^0\rangle \approx \frac{1}{\sqrt{2}} \left(|K^0\rangle + |\overline{K}^0\rangle \right) , \tag{6.42}$$

$$|K_S^0\rangle \approx \frac{1}{\sqrt{2}} \left(|K^0\rangle - |\overline{K}^0\rangle \right) , \tag{6.43}$$

and inverting (6.42) and (6.43),

$$|K^0\rangle \approx \frac{1}{\sqrt{2}} \left(|K_L^0\rangle + |K_S^0\rangle \right) , \tag{6.44}$$

$$|\overline{K}^0\rangle \approx \frac{1}{\sqrt{2}} \left(|K_L^0\rangle - |K_S^0\rangle \right) . \tag{6.45}$$

We see that a beam of K^0 produced by a strong interaction process such as $\pi^- + \mathrm{p} \rightarrow \Lambda^0 + \mathrm{K}^0$, would initially consist of approximately equal amplitudes of K_L^0 and K_S^0. Since K_S^0 decays rapidly ($\tau_\mathrm{S} \sim 10^{-10}$ s) compared to K_L^0 ($\tau_\mathrm{L} \sim 600\tau_\mathrm{S}$), a beam of K^0 produced via the strong interaction will eventually become a pure K_L^0 beam after the initial K_S^0 component decays away. However, a K_S^0 component can be regenerated from a pure K_L^0 beam if that beam is passed through matter, as we now discuss.

Consider the possible strong interactions that can occur when a K_L^0 beam passes through matter. As an example, the $\overline{\mathrm{K}}^0$ component of K_L^0 can scatter via $\overline{\mathrm{K}}^0 + \mathrm{n} \rightarrow \Lambda^0 + \pi^0$, whereas strangeness conservation forbids the analogous process where $\overline{\mathrm{K}}^0$ is replaced by K^0. Since similar differences arise as well for virtual processes, it follows that the amplitudes f_K $(\bar{f}_\mathrm{K})$ for the elastic scattering of K^0 $(\overline{\mathrm{K}}^0)$ on matter are in general unequal. It then follows that if $f_\mathrm{K} \neq \bar{f}_\mathrm{K}$ the relative admixtures of K^0 and $\overline{\mathrm{K}}^0$ in a beam which is initially all K_L^0 will be altered when this beam passes through matter. Specifically,

$$|\psi_\mathrm{in}\rangle = |\mathrm{K}_\mathrm{L}^0\rangle \approx \frac{1}{\sqrt{2}}\left(|\mathrm{K}^0\rangle + \overline{\mathrm{K}}^0\rangle\right) \longrightarrow |\psi_\mathrm{out}\rangle \approx \frac{1}{\sqrt{2}}\left(f_\mathrm{K}|\mathrm{K}^0\rangle + \bar{f}_\mathrm{K}|\overline{\mathrm{K}}^0\rangle\right) \; . \tag{6.46}$$

Combining (6.44), (6.45), and (6.46), we can then write

$$\begin{aligned} |\psi_\mathrm{out}\rangle &\approx \frac{1}{\sqrt{2}}\left[f_\mathrm{K}\left(\frac{|\mathrm{K}_\mathrm{L}^0\rangle + |\mathrm{K}_\mathrm{S}^0\rangle}{\sqrt{2}}\right) + \bar{f}_\mathrm{K}\left(\frac{|\mathrm{K}_\mathrm{L}^0\rangle - |\mathrm{K}_\mathrm{S}^0\rangle}{\sqrt{2}}\right)\right] \\ &\approx \frac{1}{2}\left[\left(f_\mathrm{K} + \bar{f}_\mathrm{K}\right)|\mathrm{K}_\mathrm{L}^0\rangle + \left(f_\mathrm{K} - \bar{f}_\mathrm{K}\right)|\mathrm{K}_\mathrm{S}^0\rangle\right] . \end{aligned} \tag{6.47}$$

The second term in (6.47) thus represents the regenerated K_S^0 component resulting from the incident K_L^0 beam scattering on a target. In the special case of scattering in the forward direction ($\theta = 0$), if $f_\mathrm{K}(0) \neq \bar{f}_\mathrm{K}(0)$ then the regenerated K_S^0 component will be coherent with the unscattered K_L^0 beam, and interesting interference phenomena can be observed. It is useful to relate the regenerated K_S^0 amplitude to the incident K_L^0 amplitude via a complex parameter ρ defined by Aronson et al. (1983a)

$$|\mathrm{K}_\mathrm{S}^0\rangle \equiv \rho|\mathrm{K}_\mathrm{L}^0\rangle \; . \tag{6.48}$$

It can be shown that for a target of length L having N nuclei per unit volume ρ is given by Aronson et al. (1983a)

$$\rho = \mathrm{i}\pi N \Lambda_\mathrm{S} \alpha(L/\Lambda_\mathrm{S})\left[f_\mathrm{K}(0) - \bar{f}_\mathrm{K}(0)\right]/k \; , \tag{6.49}$$

where $\Lambda_S = \beta\gamma\tau_S$ is the mean decay length of K_S^0, k is the wave number of K^0, and

$$\alpha(L/\Lambda_S) = \frac{1 - \exp\left(-\frac{1}{2} + i\Delta m\,\tau_S\right) L/\Lambda_S}{\frac{1}{2} - i\Delta m\,\tau_S}\,, \tag{6.50}$$

with $\Delta m = m_L - m_S$. The function $\alpha(L/\Lambda_S)$ accounts for the fact that the regenerated K_S^0 is decaying in the target with a characteristic length Λ_S, while also producing a phase change relative to K_L^0 due to the K_L^0–K_S^0 mass difference Δm.

Consider now the time evolution of the coherent K^0 state emerging from a target at $t = 0$. Recalling that this state is a superposition of the initial K_L^0 and the regenerated K_S^0, we can express the initial K^0 state $|\Psi(0)\rangle$ as

$$|\Psi(0)\rangle = |K_L^0\rangle + \rho|K_S^0\rangle\,, \tag{6.51}$$

where we have temporarily suppressed an overall normalization coefficient. Since both K_S^0 and K_L^0 can decay into $\pi^+\pi^-$ (the latter by virtue of CP-violation), then the net $\pi^+\pi^-$ decay amplitude $\langle\pi^+\pi^-|\Psi(0)\rangle$ is given by the coherent superposition of the two terms in (6.51):

$$\langle\pi^+\pi^-|\Psi(0)\rangle = \langle\pi^+\pi^-|K_L^0\rangle + \rho\langle\pi^+\pi^-|K_S^0\rangle\,. \tag{6.52}$$

Interestingly, the two amplitudes in (6.52) can be roughly comparable: the suppression of $K_L^0 \to \pi^+\pi^-$ measured by the CP-violating parameter $|\eta_{+-}|$, can be comparable to the suppression of the CP-allowed $K_S^0 \to \pi^+\pi^-$ decay due to the smallness of ρ. It follows from (6.52) that the resulting $\pi^+\pi^-$ decay rate arising from N_L incident K_L^0 particles is

$$\begin{aligned}\frac{dI^{+-}}{dt} = \Gamma\left(K_S^0 \to \pi^+\pi^-\right) N_L \Bigg\{ &|\rho|^2 e^{-t/\tau_S} + |\eta_{+-}|^2 e^{-t/\tau_L} \\ &+2|\rho||\eta_{+-}|\exp\left[-\frac{t}{2}\left(\frac{1}{\tau_S}+\frac{1}{\tau_L}\right)\right]\cos(\Delta m\,t + \phi_\rho - \phi_{+-})\Bigg\}\,.\end{aligned} \tag{6.53}$$

As noted in Sect. 6.1.3, it follows from (6.53) that the energy dependence of the strong interaction phase $\phi_\rho = \phi_\rho(E)$ can be determined in principle from the time-dependence of the oscillatory factor $\cos[\Delta m\,t + \phi_\rho(E) - \phi_{+-}]$ under the assumption that Δm and ϕ_{+-} are energy-independent fundamental constants.

We conclude this appendix and its relevance to the discussion in Sect. 6.1.3 by elaborating on the anomalous energy dependence of $\phi_\rho(E)$ which eventually led to the suggestion that ϕ_{+-} itself may have been energy-dependent. Returning to (6.48), (6.49), (6.50), and (6.51), we see that ϕ_ρ can be expressed as a sum of three contributions (Aronson et al. 1983a):

$$\phi_\rho = \frac{\pi}{2} + \phi_{\text{geo}} + \phi_{21} \equiv \phi_\rho(E) , \tag{6.54}$$

where the geometric phase ϕ_{geo} and ϕ_{21} are given by

$$\phi_{\text{geo}} = \arg\left[\alpha(L/\Lambda_{\text{S}})\right] , \tag{6.55}$$

$$\phi_{21} = \arg\left[f_{\text{K}}(0) - \bar{f}_{\text{K}}(0)\right]/k . \tag{6.56}$$

Among the three contributions to $\phi_\rho(E)$ the only quantity whose energy dependence is unknown is ϕ_{21}. Thus a measurement of the energy dependence of the phase

$$\Phi \equiv \phi_\rho - \phi_{+-} = \frac{\pi}{2} + \phi_{\text{geo}} + \phi_{21} - \phi_{+-} \tag{6.57}$$

gives a single constraint on the energy dependence of $(\phi_{21} - \phi_{+-})$.

An extensive discussion of models predicting the energy dependence of $\phi_{21}(E)$ is given in Appendix B of Aronson et al. (1983a), along with a comparison to Fermilab data then available from experiment E621. For regeneration in hydrogen the experimentally determined phase $\phi_{21}^{\text{exp}}(\text{H})$ for kaon momenta in the range $35 \leq p_{\text{K}} \leq 105\,\text{GeV}/c$ was found to be

$$\phi_{21}^{\text{exp}}(\text{H}) = \left[-(139.5 \pm 6.6) + (0.28 \pm 0.09)p_{\text{K}}\right] \text{deg} . \tag{6.58}$$

Over the indicated momentum range this momentum (or energy) dependence would give rise to a phase change in $\phi_{21}^{\text{exp}}(\text{H})$ of (19.3 ± 6.3) deg. By way of comparison, typical theoretical models studied in Aronson et al. (1983a) give $\phi_{21}^{\text{exp}}(\text{H}) \lesssim 2°$ over the indicated momentum range (see Fig. 6.1).

As discussed in Sect. 6.1.3, the fact that the combination $(\phi_{21} - \phi_{+-})$ exhibited an energy dependence incompatible with any known model for ϕ_{21}, eventually led us to consider the possibility that ϕ_{+-} itself was energy-dependent. Since such an energy dependence could arise from the coupling of the K^0–$\overline{\text{K}}^0$ system to an external hypercharge field, the Fermilab data provided a compelling argument to search for possible new long-range forces, and eventually led to our reanalysis of the EPF experiment.

Appendix 3 Dicke Correspondence

Princeton University

Department of Physics: Joseph Henry Laboratories
Jadwin Hall
Post Office Box 708
Princeton, New Jersey 08544

November 20, 1985

Professor Ephraim Fischbach
Department of Physics, FM-15
Institute for Nuclear Theory
University of Washington
Seattle, WA 98195

Dear Professor Fischbach:

I read your letter of November 6th and your preprint with great interest. Table 1 and Figure 1 apparently show a convincing correlation between ΔK and $\Delta(B/\mu)$. The obvious question concerns the origin of this correlation. Could it be due to some experimental difficulty?

One possibility is a temperature gradient effect giving a torque approximately proportional to the length of a sample. Owing to the absence of a 2-fold symmetry axis, this effect does not disappear when the two sample lengths are equal. One might expect such an effect to vary as $c_1L_1 - c_2L_2$ where L_1 and L_2 refer to the lengths of upper and lower weights respective and c_1 and c_2 are regression coefficients to be determined by least squares. I do not recall if the sample lengths are given but they could be assumed to be inversely proportional to the density. In this case the torque would be of the form $c_1/\rho_1 - c_2/\rho_2$.

If the above fit (with two adjustable parameters) should be worse than the one you show, this could strengthen your argument.

I regret that my reprints for the two papers are gone.

Sincerely,

R H Dicke
R. H. Dicke

RHD:mrf

Fig. 6.13 First letter from R.H. Dicke, one of the reviewers of our original PRL (See Fig. 6.19 for his actual report)

The University of Washington

Dept. of Physics, FM-15
Seattle, Washington 98195

Institute for Nuclear Theory
Ephraim Fischbach
Visiting Professor of Physics
(206) 543-2898
Bitnet: ephraim@uwaphast

November 27, 1985

Professor Robert H. Dicke
Department of Physics
P.O. Box 708
Princeton University
Princeton, NJ 08544

Dear Professor Dicke:

Thank you for your letter of November 20. We have taken your suggestion and have fitted the EPF data to the form

$$\kappa_1 - \kappa_2 = a + \frac{b}{\rho_1} - \frac{c}{\rho_2}$$

where a, b, and c are constants to be fitted for. The input data for the fit are given in the accompanying Table, and are plotted in the enclosed graph. Here the contours are obtained by fixing ρ_2 to be that of Cu or Pt, depending on the sample. As you can see, the fit is quite poor ($\chi^2 \approx 28$ for 6 degrees of freedom), especially when compared to the fit presented in our paper. As part of our longer paper we are also checking for other correlations as well.

We very much appreciate your suggestion, and would welcome any additional thoughts that you may have.

Sincerely,

Ephraim Fischbach

Ephraim Fischbach

EF/jl

Fig. 6.14 Response to the first letter from R.H. Dicke (Fig. 6.13)

Princeton University

Department of Physics: Joseph Henry Laboratories
Jadwin Hall
Post Office Box 708
Princeton, New Jersey 08544

June 16, 1986

Professor Ephraim Fischbach
Department of Physics, FM-15
University of Washington
Seattle, WA 98195

Dear Professor Fischbach:

Thank you for your letter of June 6, 1986.

You raise an interesting question concerning the constancy of the thermal effects. But have you been able to find anything definite concerning the dates the Eötvös experiments were performed; I haven't. The measurements reported appear to be single sets of data not averages of many days data. The whole series of measurements might have been taken in a few weeks. It is normal for an experiment of this type to require much more time for design, construction and debugging than for observations. (For our experiment I would guess a ratio of 10:1.) Another question concerns the position of the apparatus, e.g. where was the outside wall, etc.?

Best wishes.

Sincerely,

R. H. Dicke

RHD:mrf
R. H. Dicke

Fig. 6.15 Dicke's second letter following up the letter of Fig. 6.14

The University of Washington

Dept. of Physics, FM-15
Seattle, Washington 98195

Institute for Nuclear Theory
Ephraim Fischbach
Visiting Professor of Physics
(206) 543-2898
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June 19, 1986

Professor R. H. Dicke
Department of Physics
Princeton University
Princeton, NJ 08544

Dear Professor Dicke:

Thank you very much for your letter of June 16. We have been considering in detail the very questions you asked, aided by the limited information we have. This comes in part from the EPF paper itself (as we discuss below), and in part from correspondence with Professor Jeno Barnothy which we enclose. Dr. Barnothy was a Professor at the Eötvös Institute at the the University of Budapest from roughly 1935 until 1948, as well as being a colleague of Pekár. Also enclosed is a translation of the paper by Eötvös, Pekár, and Fekete (which we may already have sent you, but we're trying to make sure that you have received a copy).

Unfortunately we have no definite record of the exact time frame over which EPF performed their experiments. However, in the first paragraph on page 25 of the translation, they mention that the time between individual measurements of the equilibrium balance position was approximately one hour. Our understanding of the measurement procedure (in particular from the first two paragraphs on page 38 of the translation) is that EPF measured in succession the equilibrium position of the torsion balance with the apparatus aligned respectively in the North, East, South, and West directions. If we call the equilibrium positions for these four alignments respectively, n, e, s, and w, then, as we understand it, EPF obtained their measurements of the equilibrium positions in the following temporal order:

$$n_1,\ e_1,\ s_1,\ w_1,\ n_2,\ e_2,\ s_2,\ w_2,\ \ldots$$

In other words, our understanding is that EPF did not simply make 114 measurements (for example) of n, rotate the apparatus, make 64 measurements of e, etc., in obtaining their values for v and m for the magnalium-Pt observations. If our understanding is correct, then for just the set of measurements of v and m for the magnalium-Pt datum would have taken at least

$$(114 \times 2 + 64 \times 2 \text{ measurements}) \times 1 \text{ hour/measurement} = 356 \text{ hours} \approx 14+ \text{ days}.$$

Fig. 6.16 Our response to Dicke's second letter (Fig. 6.15)

To Professor R. H. Dicke June 19, 1986 Page 2

If we assume the Method I comparisons for magnalium-Pt and snakewood-Pt were done in parallel (since, as you correctly point out, they used different instruments for these two comparisons), and similarly for Method II, then we estimate that the total run-time required to make the entirety of their observations was roughly 3500 hours, or 140+ days run-time. If we assume that they were able to achieve 40 hours run-time per week, this would imply that the experiment took 88+ weeks (if we don't make the above assumption then the observation time would have been over 4000 hours).

Our present view is that your model is sufficiently promising to warrant more detailed study, which is what we have been doing. For example, we have considered in detail the torque exerted on the pendants by a gentle breeze (arising presumably from horizontal thermal gradients), and have shown both qualitatively and quantitatively that your model could work. We would very much like your model to succeed, in the sense of providing a credible explanation of the EPF data, in the (realistic) chance that no effects are seen in the current experiments. The two questions which we have with this model are regarding the constancy of horizontal thermal effects, which we have already discussed, and the fact that the fits for the Pt data do not work that well. One explanation of this could be that the data are pointing to a double-valued function which B/μ is, but $1/\rho$ is not.

We very much welcome your comments.

Sincerely,

Ephraim Fischbach

Carrick Talmadge

Fig. 6.16 (continued)

JENO M. BARNOTHY
833 LINCOLN STREET
EVANSTON, ILLINOIS 60201

312 - 328-5729

Dr.Ephraim Fischbach
Department of Physics
Purdue University
West Lafayette, IN 47907

Dear Ephraim:

Thank you for your letter of June 16. I am glad that you had a good time in Hungary.

Your drawing is correctly showing where Eotvos and Renner have made their measurements with respect to the room. I do not know of course the exact place in the rooms. In Eotvos's case it was certainly on one of the "Cleopatra's needles", as we called these pillars. But I do not know how deep these pillars extended down in the earth,since this was the only building in the Institute which had no cellar. Renner's room had a cellar about 10 feet high.I do not know about the new building toward west. At Eotvos's time the entire institute as you have sketched it was separated from other buildings by at least 200 feet.

In 1947 I have published a paper on elementary particles, in which the proton and the neutron had in addition to their real mass an imaginary mass of 636 electron masses, which had the properties of a small electric charge. It could numerically correctly explain the magnetic moment of the earth and the sun, and the deviations from the equivalence principle in Eotvos's experiments, but was in contradiction with Dicke's observations.

With best wiches

Jeno

Jeno Barnothy

Fig. 6.17 Letter from Barnothy relating to the location of the EPF experiment

Appendix 4 Referee's Reports on Our PRL

THE PHYSICAL REVIEW
AND
PHYSICAL REVIEW LETTERS
EDITORIAL OFFICES · 1 RESEARCH ROAD
BOX 1000 · RIDGE, NEW YORK 11961
Telephone (516) 924-5533
Telex Number: 971599
Cable Address: PHYSREV RIDGENY

11 December 1985

Dr. Ephraim Fischbach
Physics Department, FM-15
University of Washington
Seattle, WA 98195

Re: Reanalysis of the Eotvos experiment

By: Ephraim Fischbach et al. LL3052

Dear Dr. Ephraim Fischbach:

The above manuscript has been reviewed by our referee(s).

On the basis of the resulting report(s), we judge that the paper is not suitable for publication in Physical Review Letters in its present form, but might be made so by appropriate revision. Pertinent criticism extracted from the report(s) is enclosed. While we cannot make a definite commitment, the probable course of action if you choose to resubmit is indicated below.

(X) Acceptance, if the editors can judge that all or most of the criticism has been met.
( ) Return to the original referee(s) for judgement.
( ) Submittal to new referee(s) for judgement.

We are returning your manuscript for revision. Please accompany your resubmittal by a summary of the changes made, and a brief response to any criticisms you have not attempted to meet.

Yours sincerely,

Stanley

Stanley G. Brown
Editor
Physical Review Letters

Fig. 6.18 PRL's editor report of our paper

May be disclosed to the authors

The results of the statistical fit given in eq. (8), and Fig. 1 are quite convincing. If the authors' interpretation of the fit is correct, the conclusion is of great importance and should be published without question. However, this conclusion is revolutionary and, in my opinion, the authors should briefly discuss, other possible less revolutionary interpretations.

I have already suggested to Professor Fischbach the possibility of a torque induced by a temperature gradient at the Eötvös Balance. He has recently examined this possibility with interesting results. I suggest that his results be very briefly summarized in the article.

Fig. 6.19 Dicke's referee report of our paper

Referee's Report
Physical Review Letters
Manuscript # LL3052
Title: Reanalysis of the Eotvos Experiment
Authors: E. Fischbach *et. al.*

I found the result presented in this paper to be very exciting. I highly recommend the paper for publication in Physical Review Letters.

I have a few comments the authors may wish to consider:

The first comments are regarding the references. Frank Stacey presented a summary of terresterial measurements of G and discussed the form of V(r) (Eq.(1)) in a talk presented at the Workshop on Science Underground, which was published as Science Underground, AIP Conference Proceedings #96, Edited by M. M. Nieto *et. al.* , American Institute of Physics (New York 1983), pp. 285. The authors may wish to augment their references 1 and 2 with Prof. Stacey's talk. A minor typographical detail is that the reference to S. Weinberg on page 6 should be reference #16, not reference #15.

My final comment is regarding the effect of a repulsive force on glactic cluster dynamics. On page 6 the authors raise the question of the effect of the mass of the "hyperphoton" on the "missing mass problem" in cosmology. A related question is what is the effect of the repulsive term, however small it might be, on the morphology of galaxies and the dynamics of clusters of galaxies.

It was a pleasure to review this paper.

Fig. 6.20 Sandberg's referee report of our paper

Appendix 5 Feynman Correspondence

5.1 Los Angeles Times Editorial

The Wonder of It All

L.A Times Jan 15/86

Knowledge, it has been said, is like a circle. What is known is inside, and what is unknown is outside. The larger the diameter of this circle of knowledge, the greater its circumference. And the greater its circumference, the more the circle borders on the unknown. Every time a question is answered, new questions are raised that people didn't even know were questions before. There is no end. Knowledge is infinite and unbounded.

So it should not be surprising that recent reports challenge some basic assumptions of modern physics. Mind you, 20th-Century physics has hardly been a stable body of knowledge in the first place. Physicists have been much better able to gather data than to put it all together in a consistent, coherent theory that can both explain and predict. But progress has been made. Two books were published last year that asserted that physics was on the verge of a complete explanation of the universe.

One of the tenets of physics has been that there are four basic forces in the universe—gravity, electromagnetism and the so-called strong and weak forces of nuclear structure. Prodigious efforts have been made to find a Grand Unified Theory that would demonstrate that all four forces are the same.

Now comes word from a team of physicists led by Ephraim Fischbach of Purdue University that there may be a fifth force in the universe that acts against gravity and causes objects to fall at slightly different rates. This force, which they call hypercharge, would contradict the findings of one of the most famous stories in the history of science: Galileo's dropping cannon balls from the Leaning Tower of Pisa to show that (air resistance aside) all objects fall with the same acceleration regardless of weight or material.

Hypercharge is supposed to be very small and to work only on objects that are fairly close to each other (up to about 600 feet), which would explain why it has not been observed before. We called our friend Richard Feynman, the great theoretical physicist at Caltech, and asked what he thought of this theory. Not much, he said. The new paper by Fischbach and his colleagues is based on experimental data collected in 1909 by Roland von Eotvos. It is not clear, Feynman said, that variations in Eotvos' measurements of gravity result from an unknown fifth force. They could just as easily have been caused by variations in the conditions of Eotvos' experiments. Many more experiments need to be done, he said.

But Feynman had no fear that the existence of a fifth force would damage the structure of physics. Science is a *process* of finding out the truth, he said, and the process is as important as the results. Far from being a stumbling block to a Grand Unified Theory, a fifth force could help scientists refine their ideas and choose among competing models.

In the meantime, it is reasonable to insist on more evidence before rewriting the physics texts.

5.2 Feynman's Letter to the Los Angeles Times

CALIFORNIA INSTITUTE OF TECHNOLOGY

CHARLES C. LAURITSEN LABORATORY OF HIGH ENERGY PHYSICS

January 15, 1986

Mr. L. Dembart
Science Writer
Los Angeles Times
Los Angeles, CA 90012

Dear Mr. Dembart:

Thank you for mentioning my name in your editorial. If you have no intention of writing a longer article, would you please consider the following letter for the "Letters to the Editor" page?

"You reported in an editorial 'The Wonder of It All' about a proposal to explain some small irregularities in an old (1909) experiment (by Eötvös) as being due to a new "fifth force." You correctly said I didn't believe it - but brevity didn't give you a chance to tell why. Lest your readers get to think that science is decided simply by opinion of authorities, let me expand here.

If the effects seen in the old Eötvös experiment were due to the "fifth force" proposed by Prof. Fischbach and his colleagues, with a range of 600 feet it would have to be so strong that it would have had effects in other experiments already done. For example, measurements of gravity force in deep mines agree with expectations to about 1% (whether this remaining deviation indicates a need for a modification of Newton's Law of gravitation is a tantalizing question). But the "fifth force" proposed in the new paper would mean we should have found a deviation of at least 15%. This calculation is made in their paper by the authors themselves, (a more careful analysis gives 30%). Although the authors are aware of this (as confirmed by a telephone conversation) they call this "surprisingly good agreement," while it, in fact, shows they cannot be right.

Such new ideas are always fascinating, because physicists wish to find out how Nature works. Any experiment which deviates from expectations according to known laws commands immediate attention because we may find something new.

But it is unfortunate that a paper containing within itself its own disproof should have gotten so much publicity. Probably it is a result of the authors' over-enthusiasm."

Sincerely,

R. P. Feynman

Richard P. Feynman

5.3 Exchange with Feynman

Jan. 24, 1986

(My secretary says to be sure to tell you I wrote this, not she. She wouldn't make so many errors!)

Dear Professor Fischbach;

Thank you for sending me the papers on the k meson and the latest by Prof. Stacey et al about possible gravity anomalies.

I have been asked by the local paper what I thought of your most recent (Jan. 6) letter to the phys. Rev. Letters. I have had to say that as it stands it is clearly wrong, and that the figures you yourself give prove it wrong. An anamoulous force proportional to hyperon number having a range of 200 meters would have to be 16 times bigger than anything seen in mines to account for the Eotvos results; as you yourself say. There is an additional factor of the ratio of the density of the rock 200 meters below Eotvos' lab relative to the average density of the earth that should go into the formulas. The average of Earth's density is 5.5, and I would guess an average surface density near Eotvos' lab of about half that (granite is 2.7). This makes the discrepency worse, about 35 times what is observed in mines.

Thus I must object to such statements in your paper as "We will demonstrate explicity that the published data of EPF..strongly support the specific values of the parameters (in Eq 2 where lambda is given as 200-+50 meters)..". Or, " if lambda is in fact on the order of 200 meters details of the local matter distribution ..could lead to improved agreement " when in fact it almost certainly leads to worse agreement, and in no conceivable way could it lead to any reasonable possible agreement (within say a factor of two, the upper limit of tolerance for the size of the mine discrepencies if 200 meters is the range).

But upon studying the data of Stacey I agree that it is possible that agreement could be reached. If there is a layer of density d1 overlying one of density d2, calling s the thickness of d1 in units of lambda (this damn textwriter doesn't handle greek letters or equations, easily) I find, easily enough, that if someone tries to measure the gravity constant relative to the laboratory one, by comparing g, the accelaration of gravity at the surface of d1 to the gravity at the bottom of d1 (ie. at the top of d2) he will get an anamalous value if there is this extra force given by the alpha term. The ratio to the lab constant differs from 1 by alpha times -1 + f*(1 - exp(-s))/s where f is 1 - d2/(2*d1). We apply it first to data in the Australian mine, where d1 = d2 (so f = .5) nearly, and several depths from 200m to 1000m were measured. They all give about the same gravity anomally of about + .8 % roughly. This could result if lambda is 200m or less and alpha is - .008, the solution quoted in your paper.

On the other hand if lambda exeeds the greatest depth measued, so s is always small we have another solution. But now alpha must be twice as large, as Stacey notes, .016. At first one might think that this would disagree with the Exxon data in the Gulf of Mexico where perhaps d2 = 2*d1. This would mean the discrepency in the Gulf would be twice that in the mine, 1.6 %. But it is, pretty much!

If alpha is .016 how big must lambda be if th Eotvos data in your paper is to agree? It comes out 3000m. Is there any data that does not permit this? There is only the comparison of the average gravity on the Earth's surface to the satelite accelarations. This ratio falls below one by directly the a = 5.65 * 10^-6 that you want for Eeotvos multiplied by the ratio of the average density of the first

3000m of the earths surface to the density under the laboratory of Eotvos. Most of the earth is water, so maybe the average density to 3000m is 1.7 or thereabouts, and guessing 2.7 again for the stuff below the Eotvos lab we expect a deviation of 3.6 * 10^-6. It is claimed in a paper that I have not studied yet (Rupp) that only one part per million is allowed here. But I don't believe it, for most of my friends around here doubt that enough good sampling has been done over the Earth, (and the seas, particularly) to claim that a theoretical deviation of 4 parts per million is definitely excluded. But I should say nothing until I study the evidence more carefully. Stacey does take it more seriously and will not let himself get much above 1000m on account of it. If we do that we can't get enough juice to account for the Eotvos results.

Well could the Eotvos results be just a coincidence? How big are the errors, in fact. Now we are getting (you seem to like this sort of thing) into the horrible uncertainties of possibly misreading reality in the configuration of chance errors. It is very easy to fool yourself. Of course the statistical errors that Eotvos gives are only that. There are always systematic uncertainties and how are we to determine those? Unfortunately Eotvos never (at first sight) appears to repeat an experiment, so we can see how well he does. For example first he measures magnalium and then compares it to platinum. How much we wish he would come back to magnalium again to see how consistent he is! In fact, his original plan on how to make these comparisons had to be changed when he realized that the torsion constant of his fibre had unexpectedly changed by 10%. The result is then calculated supposing all the shift occured at once, when he changed the metal samples, but there is no check on that. So this first comparison on page 34 is quite suspect. He does one more the same way with wood and platinum (which you didn't use) but changes his method presumably because the method is suspect. If he doesn't like it, and changes his method of measurement, should we not also be cautious?

He continues now by taking a succession of measurements in different directions to try to average out a possibly slowly drifting fibre. But all the measuremente are taken on one sample before the other is measured (and he never goes back again to check). And now the question resolves as to whether ambient gradients of gravitational force in the lab are constant. (By the way, we have not enough data on the questionsd of small electrical forces and the like..). He makes two more comparison this way on a single balance, (Cu-Pt and the Ag reaction before and after) and then improves his technique again. This time he has two balances parallel and nearly in the same place. To compare A to B he does it on two balances. First, on balance 1 he has A, and on balance 2 he has B, and measures both balances. He then reverses putting B on balance 1 and A on balance 2 and measures again. Combining all four measurements he proposes to get something least sensitive to the possibly changing ambiance.

But look, we have what we want, a repeated experiment! Think of just one of the balances, say balance 1. What we have done is change a sample from A to B on that balance - just the kind of thing we had been doing before (in the Cu - Pt comparison, for example). It is possible to calcculate with expression (20) just what we would get. But the same pair was also compared on balance 2, in the opposite order, and we can make a separate determination of the difference as measured in balance 2. His final result is the average of these two numbers, but we can use the separate ones to get an idea of the systematic errors (at least of the previous measurements which only used one balance). I have done this. For example, taking Cu-H2O, v1/m1-v1'/m1' (with a small correction for the difference in delta alpha) is -.005 +- .002, while v2/m2-v2'/m2' is -.006 +- .0015, very nearly the same. Systematic errors might be only .001 or less if this were the only sample we had.

The next, CuSO4-Cu gives -.006 one way and +.001 the other. This indicates a systematic uncertainty of plus-minus .003. And here (page 43) we notice something unusual happened in this case. The number n1 changed by 23 scale units whereas in no other of theses cases did n1 or n2 change by more than 2 units.. in fact as time went on the experimental technique gets better and better, these n's remain more perfectly equal. But the systematic differences remain, differences in the final experimental numbers in the third column of your paper being twice the following numbers (ie the two experimental determinations from balance 1 and from balance 2 considered separately are the average in the table plus and minus the following numbers): Asbestos-Cu 0.5 : Salt-Cu 0.5 : Solution-Cu 0.2 : Water - Cu 0.1 : Tallow- Cu 0.7. I take it from this that reasonable estimates of the systematic uncertainties in Eotvos experiment is about 0.4 (parts per 10^8), twice the statistical error.

Well, that is the best I can do. It is not sure. But that does make it a very nerve-wracking thing to believe in the various deviations when they are just about the size of the errors. On the other hand, they are all in the right direction. Still it is my best guess after studying things like this for years that you have had the bad luck of an apparent fit. It isn't as wonderful as we now believe looking back. For looking forward, it was too large and we have had to strain Stacey, and further we have added a new idea to the mine data, that the effect is proportional to hypercharge. It could have been just a gravity effect proportional to mass. Maybe we latched on to hypercharge because the accidents of Eotvos were accidently that direction. It is hard to say, but I"ll bet you ten to one against it. I'll bet even more than that, when I remember the Stacey effects are far from certain, as there are systematic effects comparable to the "effects" there too.

I look forward to seeing the new kaon analysis. The papers you sent me (which I have not studied carefully, but there again it looks like someone is taking statistical errors to be expected errors, disregarding systematic errors indicated by the lack of consistency of the whole thing)--the papers you sent me say clearly the effect is not something like hypercharge that changes sign under charge conjugation. Yet the negative sign of the alpha, the force between like particles and the fact that the force between unlike (particle and antiparticle) is always attraction in quantum field theory, means that the field must be one that changes sign in charge conjugation.

Well thank you for interesting me in these things. I hope you will keep me posted. I enclose some remarks attributed to me in an editorial in the L.A. Times, and a letter I wrote to the opinion page which they haven't printed. I hope, for your sake that I am wrong.. but, of course, I don't think so. All this is just a personal letter to you, to show you how much your work interested me. I don't intend to publish any of this, as far as I know. Good luck.

R. P. Feynman

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April 14, 1986

Professor Richard P. Feynman
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Dear Professor Feynman:

I heard from Steve Koonin, who is visiting here, that you are back at Caltech, so I am taking this occasion to reply to your letter of January 24.

1) Concerning the comparison of the Eötvös slope $\Delta\kappa/\Delta(B/\mu)$ and that implied by the geophysical data [Eqs. (9) and (10) of our paper] you have already noted that with the revised Stacey data that better agreement could be achieved. However, since we spoke there has been further work on understanding the Eötvös experiment itself, by us and other workers. The result has been to note that local matter anomalies [buildings, *etc.*] play a far larger role in these experiments than had herefore been appreciated. When their effects are taken into account, they appear to give the dominant contribution to the Eötvös anomaly $\Delta\kappa$. A simple model of the matter distribution, which we discuss in the enclosed paper, can bring the results of Eqs. (9) and (10) into agreement (to within a factor of 2-4), even if we use the original Stacey data. Thus there no longer appears to be any compelling discrepancy between the Eötvös and geophysical data, and at the same time there is no conflict with the satellite results of Rapp. Unfortunately we did not completely understand this at the time that you and I spoke, and so I did not raise this point then.

2) As to whether the Eötvös results are a coincidence this is, of course, always a possibility. However, in the meantime we have included in our analysis the two data which we previously excluded. One of these (snakewood-Pt) is very well established: We obtained several snakewood samples and had two of them chemically analyzed. These yielded virtually identical results, which in turn were quite similar to those one would obtain from other more common woods. The remaining material, tallow, is somewhat more uncertain, and the quoted data point give our best guess (with some horizontal error bar understood). The line has been fitted to all the triangular points. In this connection, the data shown are somewhat different than those we quoted, since we have now gone back to their raw data and recalculated the values of $\Delta\kappa$, without rounding off as EPF did. This slightly changes the quoted results.

To Professor Richard P. Feynman April 14, 1986 Page 2

I may also add that the resulting line is substantially unchanged if we calculate $\Delta(B/\mu)$ for the combination of the brass vial + contents (which occurs since $(B/\mu)_{\text{brass}} \cong (B/\mu)_{\text{Cu}}$). This point has also been noted independently by a number of other authors.

Given the fact that 9 points now seem to fall along the same line, my feeling is that this probably represents some sort of systematic effect, rather than a statistical fluctuation. We are looking in detail at various systematic effects, including variations of Dicke's "thermal gradient" model which we refer to in the paper. However, at the moment, no model we have looked at thus far seems capable of explaining the indicated correlation. In this connection we would especially welcome suggestions from you.

3) With reference to the kaon data, you have noted correctly that we make of point of saying that a C-odd hypercharge field cannot explain those results. However, that entire analysis explicitly assumes that the γ-dependence comes from a new force which is *long-ranged*, which then naturally leads to a characteristic γ-dependence such as

$$\Delta m = \Delta m_0[1 + b_\Delta \gamma^2] \tag{1}$$

When the force is of intermediate range, then there arises another dimensional parameter, which is the range λ, and these parameters can enter in combinations such as $(\gamma/\Delta m - \lambda)$. For appropriate λ and γ the contributions from these terms in simple models can be comparable, and in any case give a γ-dependence which is more complicated than that given in Eq. (1) above. For this reason the results of the published analysis cannot be taken over to exclude a hypercharge field of intermediate range. Indeed, in some toy models we have examined, a hypercharge field with the indicated properties does indeed give consistent results. Nonetheless it is much to early to be more definitive on this point at present, but we are continuing our work along these lines.

I hope that I have clarified some of the questions in your letter, and I would very much enjoy hearing from you any additional comments and suggestions that you may have.

Sincerely,

Ephraim Fischbach

Appendix 6 Boynton Spoof

AIP

news release

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Public Information Division
David A. Kalson, Manager

FOR IMMEDIATE RELEASE

The putative "fifth force" took a major step towards reality today with the publication of a paper by a group at the University of Washington. The paper, entitled "Search for an Intermediate-Range Composition-Dependent Force" by P. Boynton, D. Crosby, D. Ekstrom, and A. Szumilo, in the September 28 issue of Physical Review Letters present decisive evidence that objects of different chemical composition accelerate differently in the gravitational field of the Earth.

The suggestion that objects of different compositions accelerate differently in the field of the Earth, emerged from reanalysis of the classic Eötvös experiment by E. Fischbach and collaborators, who was then at the University of Washington. "When I read Fischbach's analysis, I thought it was complete nonsense", said Boynton, and it was to establish this, that Boynton undertook his experiment. "To my surprise, Fischbach turns out to be correct", said Boynton and he adds "---there is absolutely no doubt that our experiment establishes unequivocally the presence of the fifth force. It appears that Fischbach isn't as crazy as I thought," says Boynton.

The experiment of Boynton and collaborators was carried out at a site near Mt. Index in the Northern Cascade Mountains in Washington. "We convinced the Robbins Manufacturing Company thatour experiment was the most important experiment in the world of physics", Boynton explains, "---and on this basis Robbins agreed to drill a hole for us in the side of this mountain." The Robbins Co. went further and arranged for the experimental site to have running water and a comfortably cool ambient temperature. Says Anthony Szumilo, a graduate student who worked with Boynton "...this was the most comfortable experimental environment that I have ever worked in."

Future plans for the Boynton group include repeating their experiment with a new pair of materials, to confirm his earlier results. "We know Newton is wrong" says Boynton, and he adds "----we hope that our next experiment will show that Einstein, Bohr, and Heisenberg were also wrong."

Fig. 6.21 Spoof AIP Press Release sent to Paul Boynton. In reality the hole was drilled by the Robbins Company long before Boynton proposed this experiment, and the “running water” at the site was the result of unwanted drainage from Mount Index which complicated the experiment. The experimental site, rather than having a “comfortably cool ambient temperature”, was actually unpleasantly dark, cold, and wet

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September 18, 1987

Professor Paul Boynton
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Dear Professor Boynton:

Enclosed please find the news release that we are distributing to the media in conjunction with the publication of your paper. As is our practice, we have paraphrased your remarks to make your work more understandable to the average reader, and we hope this meets with your approval. I have also sent a copy to Professor Fischbach whom the article mentions. If you have any questions please feel free to get in touch.

Sincerely,

Judy Chamesh

Judy Chamesh
Publicity

JC:cf

cc/E. Fischbach

Fig. 6.22 Spoof cover letter accompanying the "press release" to Boynton (Fig. 6.21). The letter was signed by my secretary Nancy Schnepp to give it a feminine touch, and the name is completely fictitious

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