# PRECISION MEASUREMENT WITH GRAVITY

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In this paper two questions are addressed: (1) what is precision measurement? and (2) where do ideas come from? To answer these questions, research examples are presented from the area of gravitational physics.

This year marks the 100th anniversary of the founding of the American Physical Society. To celebrate this occasion, the APS has organized a number of activities—one of which was the creation of a list of some 200 lecturers to act as APS Centennial Speakers who are available to present physics lectures of a general nature at colleges and universities throughout the United States. My talk today is based on a talk entitled "Precision Measurement with Gravity and Other Things" that I developed as one of those lecturers. Here, given the topical focus of this Moriond meeting, I will use examples from the gravity measurement portion of that talk and omit portions concerning the determination of the exponent in Coulomb's law or, equivalently, the determination of a laboratory upper limit on the photon rest mass.

The first thing I discovered on receiving the APS-provided viewgraph (Fig. 1) listing "ALL Areas of Physics" was that somehow in making up their "comprehensive" list they had overlooked both gravitational physics and precision measurement—the two areas I find most exciting. My sense is that talking about physics without recognizing the role of measurement science is like trying to write a poem without meter or an organ piece without a pedal line. [On inquiring, I was told they "simply didn't have enough space." I note, however, that the builders of most edifices do find space for a foundation.]

So what are the goals of this talk? They are (1) to discuss some of the issues I see facing physics and physicists, (2) to describe some of the character and excitement of this (non)field of precision measurement (or should it be measurements?) with particular application to gravity, and (3) using examples from this field, to answer or at least try to answer the question "where do ideas come from" or, equivalently, "how do ideas come about."

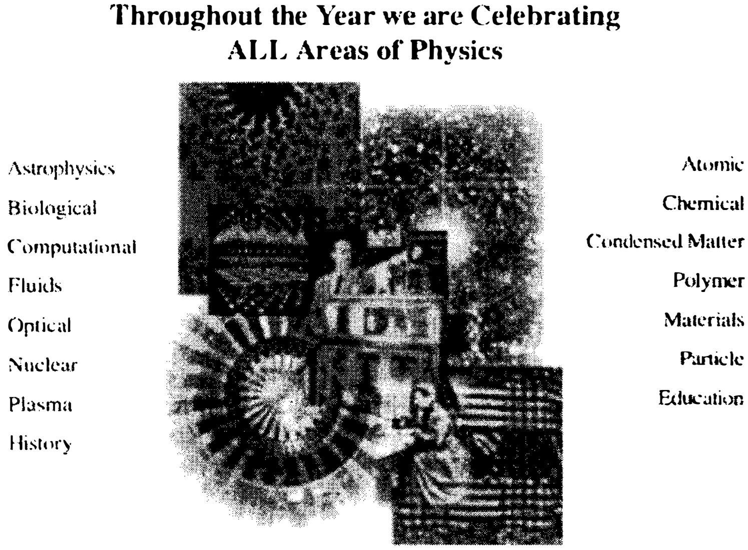


Figure 1: APS-provided viewgraph.

One problem facing physics and physicists today is image. This is, however, not a new problem. Science and scientists have long been tagged with images such as that described by Dickens (in The Lamplighter, 1841), to wit: "He was dressed all slovenly and untidy, in a great gown..., with a cap..., and a long old flapped waistcoat; with no braces, no strings, very few buttons... Tom knew by these signs, and by his not being shaved, and by his not being over-clean, and by a sort of wisdom not quite awake... that he was a scientific gentleman." In more recent times, in the book by Richard Rhodes entitled The Making of the Atomic Bomb, the following appears: "The bomb was a weapon created not by the devilish inspiration of some warped genius but by the arduous labor of thousands of normal men and women working for the safety of their country. *Yet they were not normal men and women. They were scientists.*"

I would suggest that much of the problem is self-inflicted. There's a Peanut's cartoon in which Linus asks "Lucy, why is the sky blue?" And to her answer, "Because it isn't green," he says, "That just shows how stupid I am. I thought there would be a more complicated reason." Physicists are not always as effective as they should be in communicating their research to the public. In fact, David Pratt's "jargon allows us to camouflage intellectual poverty with verbal extravaganzas" might be thought (at least sometimes) to apply. On the other hand, if we are to "communicate physics"—be it in the classroom or in the popular press—then the communication needs to be done by real (practicing and understanding) physicists. A cartoon that appeared during the time when I was a doctoral student showed Christ speaking to the multitude on top of a hill with two Arabs at the bottom, one saying to the other, "So he's a great teacher... what has he published?" This cartoon could be taken as arguing that these two aspects are separable, OR, as I believe to be the case, arguing that they are in fact inseparable. Someone more recently has said that research is to teaching as sin is to confession... if you haven't been involved with the first, then you shouldn't be involved in the second. In 1940, James Bryant-Conant, then President of Harvard University, said that "The stumbling way in which even the ablest of scientists in every generation have had to fight through thickets of erroneous observations, misleading generalizations, inadequate formulations and unconscious prejudice, is rarely appreciated by those who obtain their scientific knowledge from textbooks." The researcher part of the researcher-teacher paradigm is required to reveal and communicate a true picture of science and discovery.

One of the greatest mysteries of science is *how* does it happen. Where *do* ideas come from? For example, one might ask, *what* is the origin of the tonal sequence in the Big Ben Chimes? The answer is that it was "taken" from Handel's Messiah. The tonal sequence appearing in the fifth bar of the violin line in the opening section of part three of the Messiah was especially chosen to honor Handel—who was, incidentally, born in Germany— as England's greatest musician. Does having a "great" person involved result in a great idea every time? If that were the case, then the answer to getting great ideas would simply be to gather together great people and let them do their thing. Unfortunately, life is oftentimes not quite that simple. Consider a full-page advertisement in the January 16th 1998 issue of *Science* magazine which carried the headline: "Albert Einstein Worked For Us. Will You?" It went on to explain that his job was to submit ideas for attaching mines to enemy ships. However, the next sentence read: "None of his submissions were feasible." This particular failure of a great person to have a great idea may also reflect the considerable difference between theory and experiment. Some years ago a cartoon appeared in the *New Yorker* showing Leonardo's figure the "schema delle proporzioni del corpo umano" as plan A and a considerably more scruffy figure of a short and heavy-set man in a baggy suit labeled as Plan B. The same two figures could equally been labeled Theory and Experiment.

A question you might ask is: should I be talking about precision measurement or precision measurements? Some years back, an article in *Science* written by Pipkin and Ritter [1] carried the title "Precision Measurements and Fundamental Constants." This field was described as comprising "an important but largely unrecognized area of the physical sciences consisting of metrology, the science of measurement, and the determination of the fundamental constants required for relating measurements to theory." Unrecognized, yes, but I think the title should have been "Precision Measurement and Fundamental Constants." Why have the word *measurement* in the singular? Consider two different ways of picking apples. One approach would be to take a ladder and a basket, disappear into an orchard, find a reasonable tree, and then proceed to pick every apple on that tree. (Some areas of physics seem to proceed quite successfully this way—with scientists carrying out experiments that are very similar or even functionally identical.) Another approach is to pick from a given tree a particularly interesting apple and then move on to another tree before picking some other particularly interesting apple (perhaps a green one rather than a red one or one of considerable difference in both size and shape). This process can then continue throughout the orchard. This latter approach is similar to what happens in the business of carrying out individual precision measurements. Each picking (measurement), however different, still shares some degree of commonality—the basket and the ladder and the fact that having picked a red apple one is better prepared to pick a green one, and so on. The essential point is that the learning gain that results from picking (carrying out one experiment) can be applied to the next picking (next experiment). They may be "different trees" but they are situated in the same "field." And it is for this reason that I feel measurement science should be thought of as involving precision measurement, not measurements. Even though the experiments are different, the techniques (fortunately) are common.

So what then are some of the characteristics of precision-measurement physics? First, like the pedal line in an organ piece, precision measurement underpins, sustains, and supports all of the activity in the rest of physics (the melodies played using the upper manuals). Second, the experimentalist's tongue-in-cheek motto that "a month or two in the laboratory will save you an hour in the library" simply does not apply to this field. The subtleties of the precision-measurement trade rarely, if ever, find themselves included in journal articles with the necessary detail. As a result, the subleties almost always need to be discovered (alas oftentimes rediscovered) in the lab.

At this point, I would like to give some specific examples of precision measurement. Given the subject matter of this conference, I will focus on some measurements in gravity with which I have had laboratory involvement and accordingly have considerable familiarity. I will also focus, when possible, on experiments discussed in other papers given at this meeting or on experiments that were discussed at prior Moriond meetings. I will begin this discussion as I began my career, with the measurement of g, the acceleration of gravity. Figure 2 indicates the progress that has been made in the measurement accuracy of g over the past 35 years. Two things are striking: (1) there has been nearly a 3-orders-of-magnitude improvement in accuracy during this time period and (2) the rate of improvement is—of late—slowing down. The indicated improvements in accuracy have resulted from the many technological advances that have occurred during this time span (for example, improvements in lasers, clocks, and electronics). The more recent, slower rate of improvement, is a result of today's measurement accuracy having reached the point where a number of difficult-to-measure geophysical effects now appear as "noise sources" contributing significantly to the error budget.

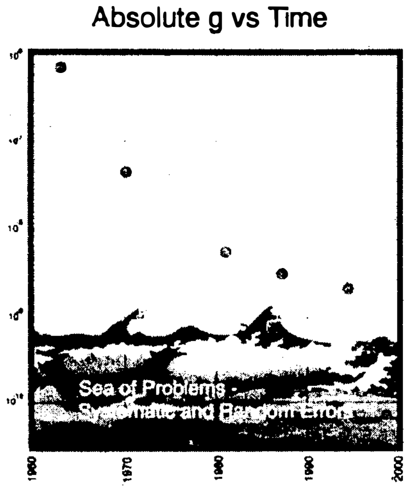


Figure 2: Measurement accuracy for g over time.

So how has little g been measured during this 35-year time frame? The answer is, simply, by dropping things and timing their rate of fall (Fig. 3). Shortly after World War II it was recognized that the electronic technology that had been developed for measuring short time intervals would now yield the necessary precision to permit the measurement of g by the method of free fall. In addition, it was realized that the methods of optical interferometry could be applied to defining the position of the freely falling object. The optical system in all of the instruments developed during this period consisted of a Michelson interferometer utilizing for the mirrors corner cubes—because of their rotational insensitivity. The first of these instruments (each point in Fig. 2 represents a new instrument), which was developed before lasers, had to use multiple white-light fringes in order to have a reasonable number of photons in a fringe. The second instrument in the figure utilized the first commercially available (5 parts in $10^8$) Lamb-dip stabilized laser as its light source. The third utilized a co-falling chamber (as in a drag-free satellite) in which the dropped object was kept centered as it fell (by servo-ing the chamber's position to track the dropped object) with the purpose of both easing the vacuum requirement and facilitating the handling of the free-fall conditions (creating a lift-off and soft catch). Also a "super spring," a mass on a simple spring electronically fed back to achieve a long (> 30 seconds) period was used to support and isolate the reference mirror. The next-to-last instrument in this figure was in fact a series of six instruments called the JILAg series.

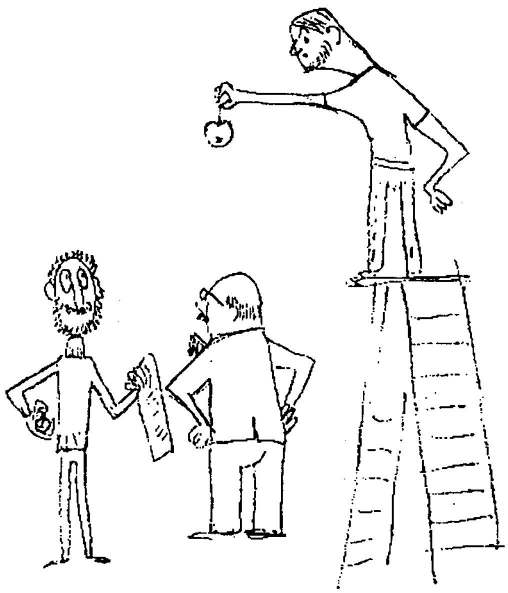

Figure 3: Free-fall method of measuring g.

These instruments incorporated all of the features discussed above and were made in response to a number of external requests for this technology. Figures 4 and 5 show this particular instrument—an instrument that has been and continues to be used successfully by research groups throughout the world. I will eventually return to absolute gravity; but before that, in order to better explain how things evolved, I need to interpose several other topics.

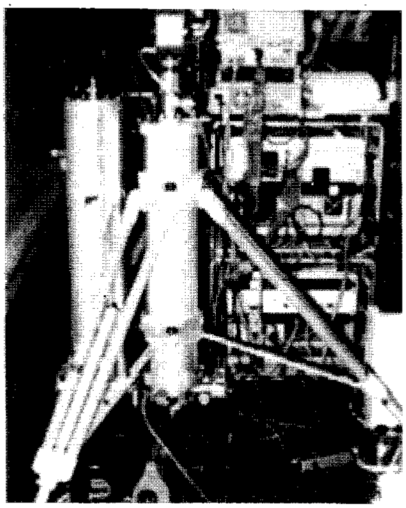

Figure 4: JILAg instrument.

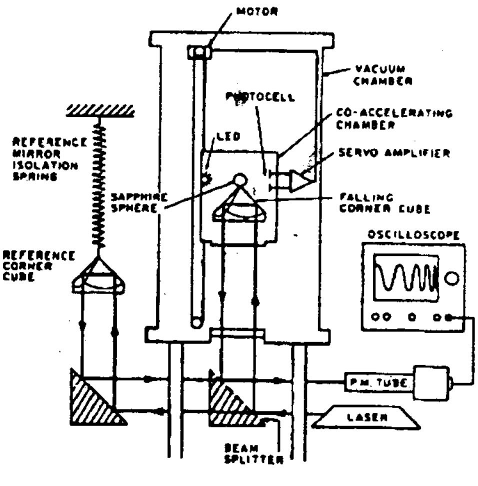


Figure 5: Schematic of JILAg instrument.

During this meeting, Ken Nordvedt in his talk "The Empirical Foundations for the Gravitational Interaction" described lunar laser ranging (LLR) as, "the near complete relativistic gravity experiment." What I will now discuss is how the idea for this experiment came about. As you will see, the intellectual trigger was the fact that LLR uses, as its implementing optical element,

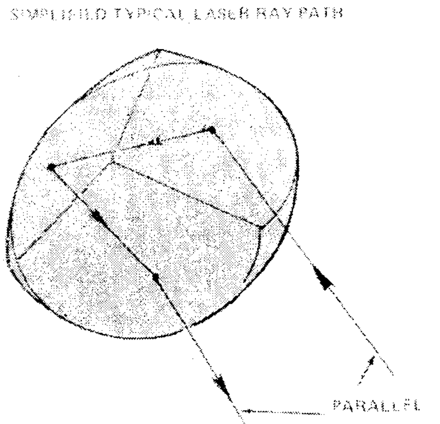


Figure 6: Optical retroreflector.

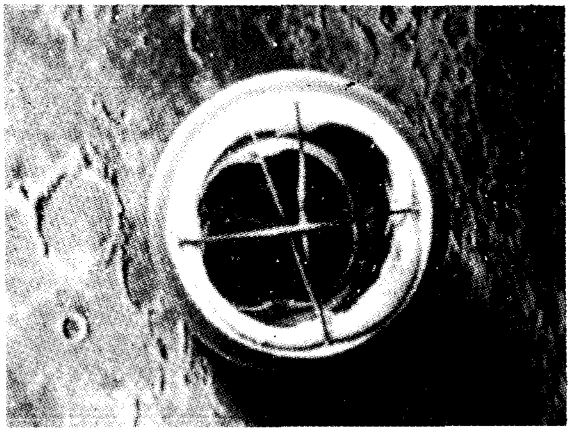

Figure 7: Proposed lunar package.

optical retroreflectors (Fig. 6) that are identical to those used in the heretofore-described free-fall gravimeters. The lunar laser ranging experiment had its origins in the late 1950s in Professor R. H. Dicke's gravitational research group at Princeton University. This Princeton group was particularly concerned with ways to look for possible slow changes in the gravitational constant G by precision tracking of a very dense artificial satellite in a high-altitude orbit. The use of both optical retroreflectors on the satellite and pulsed searchlight illumination from the ground to measure angular motion with respect to the stars was one of the methods considered in detail. When pulsed ruby lasers were developed, it became clear that laser range measurements to satellites containing retroreflectors would provide much more accurate tracking information.

At that time, I was a graduate student working with Professor Dicke, dropping corner cubes to measure g and participating in and benefiting from the weekly evening meetings of the Princeton gravity group. In 1962 I realized the feasibility of placing retroreflectors on the moon and ranging to them. Accordingly, I prepared a written analysis that discussed, given the possibility of a semi-soft landing by a ranger mission, the value of placing retroreflectors on the Moon that would then permit the Earth-Moon distance to be successfully measured using point-to-point laser ranging with available pulsed-laser powers. I also built a model (Fig. 7) of this proposed lunar package that involved encapsulating a corner-cube in a spherical package (a carved-up Woolworth's 25 ¢ rubber ball) with its center of gravity situated such that if it were thrown out of a spacecraft it would roll on the lunar surface and position itself with the reflector pointing up. On being hit by a laser pulse from the earth, the retroreflector would simply send it back to the transmitting station on the earth. Precisely measuring the approximately 2.5 second round-trip travel time would then yield the Earth-Moon distance.

Discussions on the idea of a lunar laser ranging experiment continued for some years amongst an ever-expanding group of interested scientists. In 1965, a proposal was submitted to NASA to place a number of retroreflector packages on the moon. Three factors led to NASA's decision to carry a lunar ranging retroreflector package on the Apollo 11 flight (and subsequently on the Apollo 14 and 15 flights) : the importance of the scientific objectives that could be achieved; the reliability inherent in the completely passive nature of the retroreflector array (on Apollo 11, an array of 100 1.5" diameter retroreflectors); and the very short time required for deployment by the astronauts on the lunar surface. Though modest in its beginnings, lunar laser ranging has proven a real scientific success; it has been described as one of the most cost-effective and scientifically productive projects arising from the space program. Today this still scientifically productive experiment remains as the only ongoing experiment from the Apollo era.

At this point, I will revisit an interesting period in physics—the fifth force era—that played a major part at several Moriond meetings (1988, 1989, and 1990). [2,3,4] This period falls in the time frame between the last two points on the plot of instrumental accuracy versus time (Fig. 2) for absolute gravity. I will discuss why certain things were done, and, most importantly, how the details learned from "one picking" can often be applied to some other experiment. This era began when the suggestion was made that there may be an heretofore unnoticed short range and possibly composition-dependent component to Newtonian gravity. This suggestion appeared in an article by Fishbach et al. [5] The suggestion that gravity might have a short-range component (Fig. 8) was taken up actively by the scientific community with experiments that took many forms. Although, in the end, Newton seems to have been vindicated, (Fig. 9), the achieving of this vindication produced an interesting interlude in experimental gravitational physics in which considerable creativity was brought to bear in designing experiments to try and detect this suggested "fifth force." Much of this creativity could be, and was, transferred to other experiments; it also continues to be applied in gravitational physics today. Since the suggested size of the possible violation of pure Newtonian gravity was roughly at the 1% level, a lot of experiments were rather quickly undertaken—some carefully and others not so carefully—but the one thing that they had in common was that they all were published.

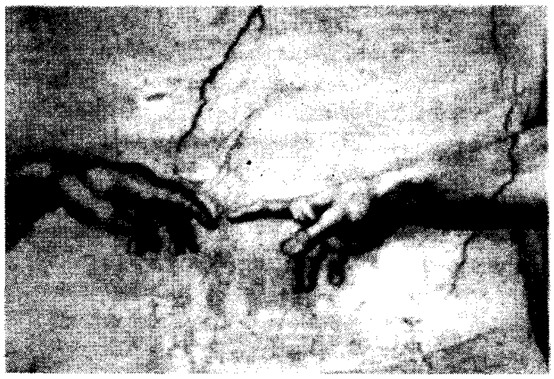

Figure 8: Example of "short range" interaction.


Figure 9: DeRùjula's $5^{th}$ force summary slide (Moriond 1990).

My small group entered this fray by noting that the fifth force suggestion invited experiments involving dropping things—something we were good at. Since an equivalence experiment (which asks whether different things fall at the same rate) responding to local mass was called for (rather than using the distant mass of the Sun as had been done in the most recent equivalence tests), we simply modified one of our JILAg-series little-g apparatuses to use two dropping chambers (Figs. 10 and 11), rather than one dropping chamber and one super-spring chamber. At the same time, we modified the two dropped objects in such a way that the normal composition of one was augmented with a considerable amount of copper while the composition of the other augmented with a considerable quantity of (depleted) uranium. We then performed [6] a modern-day version of the experiments Galileo reputedly performed, namely dropping two different material objects off of the leaning tower in Pisa and finding that they fell at the same rate. As a matter of experimental technique, we released the two dropped objects at slightly different times with the result that the signal that measured any difference in rate of free fall appeared as a (nearly) constant frequency rather than simply as a DC offset in their arrival times.

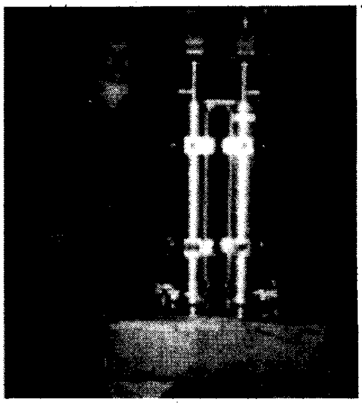

Figure 10: Galilean apparatus.

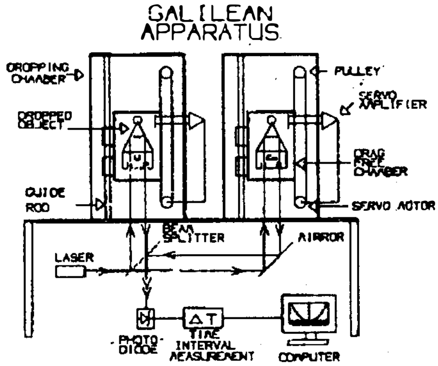


Figure 11: Schematic of Galilean apparatus.

The conclusion of this experimental was that with a precision of 5 parts in $10^{10}$ the two different masses fell at exactly the same rate. This null result permitted us to put new limits on the strength and range of the proposed fifth force. While limiting the size of the hypothesized fifth force, this experiment also taught us something; for while the limit we were able to set was very good, it was not quite as good as we had expected it would be. Thus the question we asked ourselves was: where did the unforeseen precision-limiting noise come from? The answer was tilt. What should have been obvious, but somehow up to this point had been missed in the design of a side-by-side dropping chamber and super-spring apparatus, was that this geometry is **first-order** sensitive to tilt! Any tilt of the base in this type of "horizontal instrument" results in a differential arm length change that is directly proportional to the angle of tilt. Our Galilean experiment—in hindsight—should have been performed with one dropping chamber above the other; for in this case tilt would have shown up only as the square of the angle.

The important thing is that we *learned* from this experiment. This type of learning—in this case the effect of tilt—is a critical aspect of this kind of apple picking in which one tries a slightly different tree at a level of picking that is designed to be the best one can possibly do. The result of all this was that when we "transferred" our little-g technology to a private company, this learning was included. As a result, today's commercially available absolute gravity instrument, the FG-5 (far-right point in Fig. 2), utilizes the one-above-the-other design for the dropping chamber and super spring (Fig. 12).

Another suggested aspect of fifth-force gravity would be a possible failure of the inverse-square-law character of gravity at distances in the range of centimeters to hundreds of meters. In fact, in 1998 an experimental result was reported [7] (based on measurements made on a tower located in North Carolina) that indicated a failure of $1/r^2$ at about the 1% level. Since there was a 300 meter tall tower (Figs. 13 and 14) within a 20 minute drive of Boulder in Erie, (which was built for NOAA meterological studies), we decided to try this experiment ourselves. We "borrowed" this tower for the cost of $1000 and a signed release to the effect that if we "fell off," we would not hold NOAA responsible. A series of measurement runs—at night to avoid the solar input tilting of the tower, and choosing "windless" nights to minimize tower swaying—resulted in a very clean confirmation of Newton's inverse square law in contradiction to the previously reported result.

In this experiment, we measured gravity at eight different heights on the tower using LaCoste and Romberg (relative) gravimeters. The observed values were adjusted for the tides,

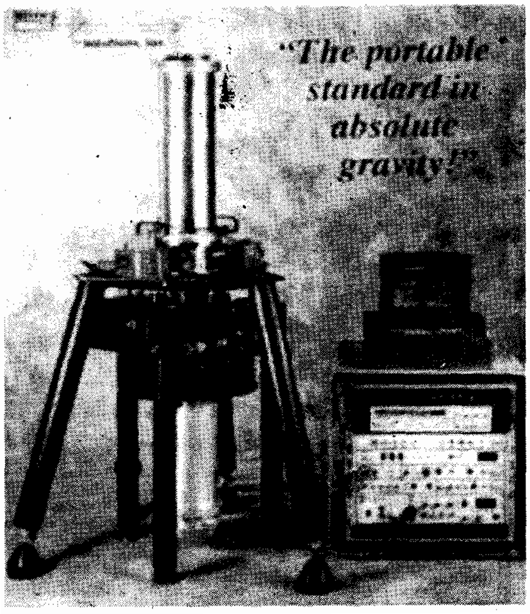


Figure 12 : FG-5.

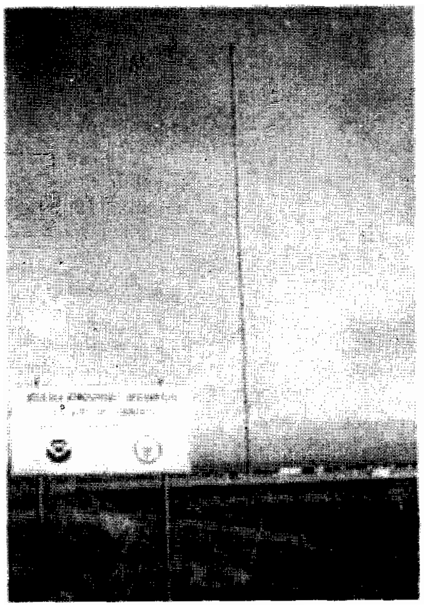

Figure 13 : Erie tower
(300 m high - as is the Eiffel tower).

Figure 14 : Base of Erie tower.

instrumental drift, and gravimeter-screw errors. The gravimeters were also tested for possible systematic effects due to tower motion. The results were then compared with values predicted from surface gravity by upward-continuing surface results to various heights of the tower using Newton's inverse-square law. The differences exhibited no systematic trends and their rms value was only $10 \times 10^{-8}$ msec$^{-2}$, well within the estimated errors of the experiment. These results served to place new constraints on the possible strength and range of any non-Newtonian force, while at the same time vitiating the previously reported positive result. And none of us fell off the tower. Originally our *Physical Review Letter* [8] was submitted with the title: "Test of the Inverse Square Law of Gravitation Using the 300 m Tower at Erie, Colorado: Newton Vindicated on the Plains of Colorado." However, to get it accepted (and to confirm the sobriety with which physics is to be viewed) we had to delete the portion following the colon.

During this same several-year fifth-force time frame, an intriguing paper appeared in *Physical Review Letters* by two Japanese physicists announcing that a right-spinning top changed its weight as it spun down (at the sub-percent level), whereas a left-spinning top did not.[9] [Although not referenced in the paper, 11 years earlier one of the two authors had predicted such an effect,[10] perhaps explaining how the idea came about to do this experiment.] Since we had a good balance in our lab (capable of weighing up to a kilogram with a precision of 1 in $10^9$), our reaction to this paper was "why not," someone has to check it. Eight weeks later our *Physical Review Letters* paper entitled "Gyroscope-Weighing Experiment with a Null Result" appeared.[11] We found no anomalous weight changes of the magnitude previously reported that depended on either rotor speed or rotational sense about the vertical axis. This remarkably quick turn-around from start to finish, from idea to completed experiment, reflected the facts that the experiment was relatively easy to do and that this was not the first "precision measurement" experiment that we had carried out. The rapid time-to-publication, I suspect, reflected the desire of *Physical Review Letters* to not be "scooped" by *Nature* which was rushing to publish a similar result by a European colleague who, after he heard what we had done, decided he could do it too. The joy of this particular "picking" was that the popular press enthusiastically picked up and reported on this understandable-to-all result (Fig. 15).


Figure 15: Collage of popular press clippings (from *Christian Science Monitor, New York Times, Wall Street Journal,* and *Houston Chronicle*).

I will now move on to a more recently completed experiment: a free-fall determination of the Newtonian constant of gravitation. Why did we do this, given that determinations of G, initially seen as a way to measure the mean density of the Earth, go back 300 years to the time of Newton? The reason is that the accuracy with which this fundamental constant was believed to be known is surprisingly low—only 1.5 parts in $10^3$. A few years ago, it was believed to be known to a few parts in $10^4$, but then a careful determination resulted in a number for G which disagreed with the accepted (CODATA) value by more than 40 standard errors. In recognition of this, metrologists, while not changing the numerical value, increased the error assigned to this fundamental constant to include this new result at something like a three-sigma possibility. At JILA, to try to shed light on this problem, we devised a novel approach to measuring G. Our approach, purposely devised to be "different" in hopes of avoiding possible systematic errors that may have resulted in a common bias in a number of the more traditional measurements, involved the use of an external mass to modify the local value of g (Fig. 16). For the external mass we used a well-characterized 500 kg ring-shaped tungsten mass which we positioned so as to alternately increase and then decrease the rate of fall of a test mass dropped in vacuum. Since the required gravity apparatus already existed, all we needed to build was an elevator to move in a smooth and reproduceable fashion the 500 kg mass which surrounded the dropping chamber (Fig. 17) from just above to just below the FG5's dropping region.

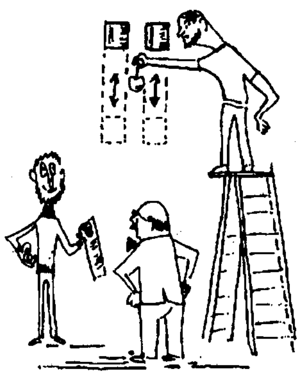

Figure 16: Modifying g to measure G.

Figure 17: G apparatus.

By measuring the acceleration difference for these two positions (Fig. 18), we were able to obtain a new value for G that has an accuracy of 1.4 parts in $10^3$ and that numerically lies 1.5 of the measurement's standard errors above the CODATA value—an agreement that should help rule out the possibility that previous measurements suffered from some common, value-shifting systematic bias. [12]

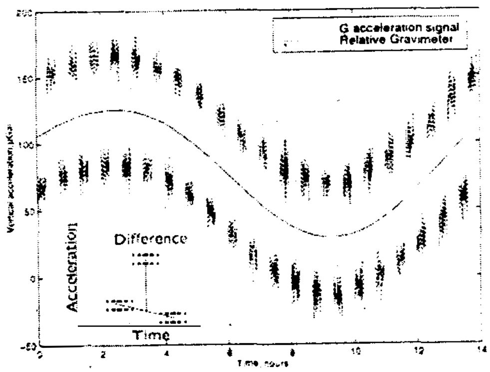


Figure 18: G data (tidal variations are also seen).

Finally, I would like to finish where I began, by returning specifically to the measurement of g. At this Moriond meeting, we have heard about ways of measuring g by simply dropping atoms and measuring their position using "atom interferometry." Is this the wave of the future? Maybe, but in the meantime (and I believe there will be an important meantime) let me describe a new "old fashioned" way that we are presently working on. What the reader should recognize is, no matter how you do it, somewhere near the parts in $10^9$ level, you will run into the sea of geophysical and man-made noise sources depicted in Fig. 2. So I would suggest the challenge, therefore, is not to have exquisite sensitivity to changes in gravity (as is already presently achieved using cryogenic gravimeters) but to have the simplest, most user-friendly, and portable instrument that will achieve an accuracy of a few parts in $10^9$. To this end, we are presently working on a simple cam-driven dropping mechanism that will create a drop of 2 cm three times every second. Furthermore, the instrument is "inertially compensated" through the use of a second cam which drives a balancing mass that keeps the center of mass of the total instrument fixed (while all sorts of things are going on such as dropping, catching, and returning the dropped object to the start position). As a result, there will be no (appreciable) phase-related-to-the-drop recoil effects that can oftentimes systematically bias a result. The beauty of this new, but quite old-fashioned, idea is its simplicity as well as its small size (Fig. 19). In addition, because it will be an absolute instrument, it avoids the problems of relative instruments that, because they drift and have tares, require repeat measurements over a line in order to just measure gravity differences.

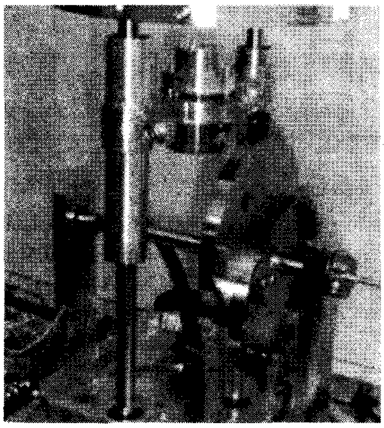

Figure 19: Cam-based g "engine."

Over the past 50 years, the instrumental workhorse of gravitational geophysics has been the LaCoste and Romberg (relative) gravimeter, mentioned earlier and used in the Erie tower experiment. This instrument grew out of a clever answer by a student (Lucien LaCoste) to a sophmore mechanics homework problem (set by Professor Romberg at the University of Texas in Austin). The problem asked for the period of a mass on a horizontal boom supported by a diagonally positioned spring. What LaCoste noticed was that in the special case of a "zero-length" spring (a spring whose length is directly proportional to the applied force—no force, no length), the period could be made infinite, which means that an infinitesimal change in the gravity acting on the mass would yield a large displacement. The LaCoste and Romberg commercial gravimeter (Fig. 20), which eventually grew out of this student-faculty interaction, measures small position changes of a mass supported in a nearly neutral condition by a zero-length spring. It can sense changes in g to parts in $10^8$ and, with a lot of care, to parts in $10^9$. In the future, I believe that both (improved) mechanical gravimeters and atom-type gravimeters will provide the enhanced absolute gravity-measurement capability that will serve to continue implementing geophysical progress.

In conclusion, I have tried to give a sampling of what precision measurement physics is all about, at least in the gravitational domain. I have tried to show that, as Daumier has pointed out in his "Monsieur Babinet Prevenu par sa Portière de la Visite de la Comète" (Fig. 21), it is sometimes useful and important in science to look in another direction (or try another tree).

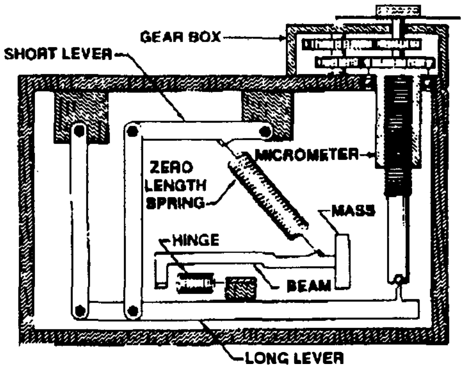


Figure 20: Schematic of LasCoste and Romburg relative gravimeter.

The character of this field is such that it lets us undertake a considerable variety of experiments that involve precision measurement (not measurements). The experiments, however, are usually quite demanding of both time and techniques. Einstein once said "I have little patience with scientists who take a board of wood, look for its thinnest part, and drill a great number of holes where drilling is easy." The field of precision measurement physics is a "thick board." It is also fun. There is a great deal of satisfaction to be had in carrying out experiments that involve measuring, with extreme care and high precision, quantities that are either zero, very small, or nearly constant. It is an area of physics where "much more is known than is actually true" ( J.R. Pierce). Time in the lab *is* required because often what one needs to know is either not in the library or, if it is, it may be incomplete or simply wrong. Finally, I hope I have convinced you that precision measurement is an important, as well as a facinating, area of physics.

Figure 21: Daumier lithograph.

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