History of Physics NEWSLETTER

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Early History of Gravitational Wave Astronomy: The Weber Bar Antenna Development

by Darrell J. Gretz
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Preface

"An experiment every day and a calculation every week."
Joe Weber

The incentive to write a memoir about gravitational wave astronomy is substantial when one has spent twenty-five years on the frontiers of science under the guidance of a noted experimentalist such as Professor Joseph Weber of the University of Maryland Physics Department. The rewards are self-evident in the form of many varied experiences both in terms of personal involvement with a superlative person and the many aspects of physics one becomes involved in as peripheral yet essential to the mainstream of gravitational wave studies. Gravitational Wave Astronomy grew out of Einstein's General Theory of Relativity, but this memoir will not treat the theory. Rather, it is about the man and the development of the hardware over a several year period without regard for the controversy that consumed the program.

Over the years I have been repeatedly advised by others to keep a diary and someday write a book about Weber and the discovery of gravitational waves. To me it seemed to be a violation of confidence placed in me by Professor Weber to think of writing down the path he took, exposing the details of his work in the laboratory. On the other hand, this man will take his place in the history of physics and mankind as one of the most outstanding scientists of the twentieth century. As he told me many times: "I am by a very wide margin the best physicist in the world." Surely the world deserves an intimate look at the developments that brought about a new window in astronomy. I was privileged to know him and privileged to work on his experiment.

The material for this paper was collected over the years from both published and unpublished sources and personal recollections from my contact with Joe Weber and the graduate students who contributed to the program with their PhD thesis projects. Still, it would not have been produced were it not for Dr. Howard Brandt who continually reminded me that I kept a wealth of information in my desk drawer that would

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Weber, programmer Greg Wilmot, engineer Darrell Gretz

In This Issue

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Remembering John Rigden

By Bill Evenson

John Rigden was a great friend to many in the history of physics community. I first met him in the summer of 1972. We hit it off immediately, talking for hours about physics education in the breaks and evenings of the meeting where we met. He had that ability to reach out to people who might have been strangers but quickly became confidants.

When he was in the Chair line for the FHP (1993-97), John called and asked me to help organize some history of physics sessions for the March and April APS meetings in 1996 – the centennial of the discovery of radioactivity – and in 1997 – the bicentennial of Joseph Henry's birth. While I had been a member of FHP for many years, this call from my friend and colleague brought me into direct involvement and service with the Forum. To an unusual extent, John had the kind of personality that would draw one in, and his creativity and willingness to do the work necessary to bring his ideas to fruition made it a satisfying part of our friendship to jump in and go to work with him.

In addition to his earlier service as FHP Chair, John again joined the Executive Committee from 2004 to 2007. He played a leading role in establishing and implementing the APS Historic Sites Initiative, to commemorate important sites in the history of physics around the country.

While he was Director of Physics Programs for AIP (1997-1998), John worked closely with the Center for History of Physics and the Niels Bohr Library at AIP. In his work as editor of the *American Journal of Physics* (1975-85), he encouraged high quality and thoughtful reports on history of physics, especially relating to physics education. I recently sorted some files from that period and came across some of his letters to authors relating to papers I had refereed. They were clear, encouraging where possible, and strong in their expression of standards of quality.

He and Roger Stuewer founded the journal *Physics in Perspective* in 1999 and worked as co-Editors-in-Chief there until 2013.

John also contributed much to history of physics through his books – which all give evidence of his prodigious and vigorous work practices. His book on *Rabi: Scientist and Citizen* (1987) was the first of several major writing projects in history of physics. This was followed by *Most of the Good Stuff: Memories of Richard Feynman* (1993), co-edited with Laurie Brown. His book *Hydrogen: The Essential Element* (2002), was a creative look at the development of physics in the 20th century through a focus on physicists' understanding of hydrogen. Then, at the centennial of Einstein's "miraculous" year, he published *Einstein 1905: The Standard of Greatness* (2005). Along the way he was also Editor-in-Chief of the *Macmillan Encyclopedia of Physics* (1996) and the *Macmillan Encyclopedia of Elementary Particle Physics: Building Blocks of Matter* (2003), volumes that were obviously of much broader scope than history of physics but which contained considerable history.

I remember John's great sense of humor. I don't think I ever had a conversation with John that did not include some laughter. I can hear his chuckle in my head as I write this and visualize his ready smile. The last time I talked with him on the telephone, about a month before he died, he was upbeat and still ready to laugh, to enjoy life. I am one of many with memories of inspiring times and pleasurable experiences with John Rigden. I am grateful for his leadership, his friendship, and his lasting contributions to FHP and history of physics.

Bill Evenson
Corvallis, Oregon

John Rigden

History of Physics NEWSLETTER

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The Brutality of Physics

By John S. Rigden

I always knew I was at the top until the brutality of physics informed me otherwise. I knew then that my top position was a thing of the past.

For a near genius like me, physics can be brutal. The operative word is near, *near* genius, not genius, only near genius. For an ordinary genius, physics can be even more brutal. The operative word is ordinary, *ordinary* genius.

Why is physics brutal? Physics is hierarchical and most physicists know where in the hierarchy they stand. As a young physicist moves into the profession, as physicists begin to settle into the hierarchy above and below him or her, and when brutal physics makes its appearance, fantasies begin to come face to face with reality. Physics is brutal because it is so revealing, so efficiently and accurately revealing.

I was fortunate to have had several conversations with Hans Bethe. In one of these conversations, we talked about the immediate years after World War II when he and Feynman were colleagues in the physics department at Cornell University. They were both working on quantum electrodynamics (QED) and Bethe and I were talking about this. At one point the great Hans Bethe said, "Feynman was just down the hall and I knew he was miles ahead of me." Bethe was an active physicist from about 1930 until his death in 2005. Throughout this 75-year time span he knew his abilities exceeded those of most other physicists. Bethe also knew early in his career that Feynman's abilities exceeded his. He knew Feynman could do things he could never do. Physics is revealing… and brutal.

The second-grade classroom was quiet. Mrs. Blake had asked each of us to tell our classmates what we planned to do when we grew up. Some students thought and thought and finally said some predictable answer like "be a nurseryman," or "be a teacher," or, sometimes, "I don't know." The nurseryman answer was obvious because we lived in northeastern Ohio which was, in a big way, nursery country. When my turn came I said, "I am going to get a BS degree and go to medical school." Billy, a boy from a nursery family, sat across the aisle from me and, with his hand over his mouth, he said quietly, while laughing softly, "BS? That means Bull Shit." "No," I said as I looked directly at Billy, "a BS degree means bachelor of science degree." Mrs. Blake gave me an almost imperceptible affirmative nod. I think she was pleased.

There were eight grades in the Madison Avenue School – grades one through eight. It was a rural school with two grades in every classroom. The school was in the heart of nursery country where shrubs of various kinds, a large variety of evergreens, and trees were propagated; some of the kids, like Billy, came from nursery families. Back in those olden days there was no time to waste making log cabins out of clay, so there was no kindergarten. Mrs. Blake taught grades one and two.

I was smart and self-confident. Only one time during my public schooling was my confidence challenged. Miss Carrig taught grades three and four and in one of those grades long division was introduced. I got sick and missed the very week of school when long division was introduced. When I returned students were sitting, heads down, pencils in motion. I didn't know it, but they were struggling to learn this new math. I sat there looking at long division problems and was devastated because I did not know how to do them. Finally Miss Carrig came to my desk, stood beside me, told me how to get started and continued through to an answer. I got it almost immediately and said to her, "Oh I get it. It's just multiplication backwards." She smiled at me. By the end of my first day back in school, I was beating all the other kids in long division. I thought I was an intellectual barracuda among intellectual gold fish. I had yet to encounter an intellectual shark.

In grade seven students were given an IQ test. The teacher told my mother that I tested just "a hair below genius" and then the teacher, Miss Goodwin, went on to say, "Be thankful he is not above the genius level." Miss Goodwin then said that geniuses were weird and that geniuses lived troubled and unhappy lives. I have often wondered why she believed that.

I always led my class in getting assignments done, finishing a test, reading a book, and I always got the highest grades. When I graduated from eighth grade I was clearly the prize student and I received Madison Avenue School's highest honor, the Good Citizenship Medal.

Physics is a quantitative subject: measurements are made that produce numbers and physical theories are called on to determine whether the experimentally-measured numbers are embraced by the theories. Good theories shrink-wrap the empirically-determined numbers so tightly that there is no wiggle room for those numbers which means that experiment and theory are in perfect agreement. Take for example the orbit of Mercury. The orbit of Mercury around the Sun is an ellipse and the point at which Mercury is closest to the Sun is called the perihelion. That point itself orbits the Sun, but very slowly; specifically, as seen from Earth, the perihelion of Mercury's orbit is measured to move through an angle of 1.5556 degrees per century. Newton's theory of gravitation predicted 1.5436 degrees per century so it was short by 0.012 degrees per century. This discrepancy of a little over one hundredth of a degree per century – per century! - provided too much wiggle room. In 1915, Einstein's General Theory of Relativity reduced the wiggle room to zero: Einstein's theory predicted that the perihelion should advance by 0.012 seconds of arc per century more than that predicted by Newton's theory. With Einstein's General Theory, experimental measurement and theoretical prediction were in agreement. It was a monumental intellectual achievement. Among all the great physicists of the 20th century, Einstein stands alone at the top of the hierarchy and all other physicists know it. For Einstein, physics was not brutal.

When I got to high school, things changed slightly for me. The high school was in town and there were a number of elementary schools in the township that were feeder schools for the high school. So I went from 16 classmates to over 180. I was still at or near the top of

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2017 History of Physics Essay Contest

by Alan Chodos

Ryan Chaban, a graduate student at William & Mary, has emerged as the winner of the first annual FHP history-of-physics essay contest. The contest was established last year by vote of the FHP Executive Committee. As stipulated in the announcement, its goal is "to promote interest in the history of physics among those not, or not yet, professionally engaged in the subject. Entries can address the work of individual physicists, teams of physicists, physics discoveries, or other appropriate topics."

The announcement, printed in APS News and also emailed to FHP members, advertised a stipend of $1000 for the winner, with $500 going to possible runners-up. In addition, the winning essay is published as a Back Page in APS News. The FHP Executive Committee serves as the panel of judges.

The contest is aimed primarily at undergraduate or graduate students with an interest in the history of physics, but is open to anyone who had not (yet) received a PhD in either history or physics.

Contest organizers were pleasantly surprised that a total of 13 entries were received by the deadline of September 1. In addition to the expected submissions from US students, there was an international component, including two from the UK and one from the Philippines.

Because of the large number of entries, judging took place in two phases, the first of which produced a list of 3 finalists. The Committee then chose a winner and one runner up from among the finalists.

Chaban's essay is titled "Doublet Dudes: Shaping the Future of Fusion". MIT senior Shaun Datta is the runner up, for "Quantum Mechanics as a Stimulus for American Theoretical Physics". Both essays are available on the FHP website at www.aps.org/units/fhp/essay/index.cfm . In addition, as advertised, an edited version of the winning essay appeared in December as the APS News Back Page (see http://www.aps.org/publications/apsnews/201712/backpage.cfm).

Ryan Chaban

For information regarding the second FHP essay contest see the box below.

2018 History of Physics Essay Contest

The Forum for History of Physics (FHP) of the American Physical Society is proud to announce the **2018 History of Physics Essay Contest.**

The contest is designed to promote interest in the history of physics among those not, or not yet, professionally engaged in the subject. Entries can address the work of individual physicists, teams of physicists, physics discoveries, or other appropriate topics. Entries can range from about 1500-2000 words, and while scholarly should be accessible to a general scientific audience.

The contest is intended for undergraduate and graduate students, but open to anyone without a PhD in either physics or history. Entries with multiple authors will not be accepted. Entries will be judged on originality, clarity, and potential to contribute to the field. Previously published work, or excerpts thereof, will not be accepted. The winning essay will be published as a Back Page in *APS News*, and its author will receive a cash award of $1000, plus support for travel to an APS annual meeting to deliver a talk based on the essay. The judges may also designate one or more runners-up, with a cash award of $500 each.

Entries will be judged by members of the FHP Executive Committee and are due by September 1, 2018. They should be submitted to fhp@aps.org, with "Essay Contest" in the subject line. Entrants should supply their names, institutional affiliations (if any), mail and email addresses, and phone numbers. Winners will be announced by December 1, 2018.

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be a "Magnum Opus" on the development of gravitational wave astronomy. In my mind the 'early history' was the period from 1960 to about 1975, when other groups got involved and the controversy really began about how science is processed by the human mind.

Joseph Weber followed an unusual path to reach the highest rank. He was born in New Jersey of immigrant parents from Lithuania who couldn't speak English. Eventually he received an appointment to the US Naval Academy, from which he graduated in 1940. He was always proud to say: "I received an appointment by competitive exam." He often would tell the story of how his name was not really 'Weber' but as his father waited in line to leave the country without having his name on the migration documents, he heard the name called for 'WEBER, WEBER,' and no one answered the call he spoke up, "I'm Weber! I'm Weber!" and made it onto the ship. Following graduation Joe Weber, the son, performed wartime service as a Deck Officer of the aircraft carrier Lexington, survived its sinking by the Japanese, and went on to become Commanding Officer of the submarine chaser SC 690. Weber resigned from the Navy in 1948 and began an academic career at the University of Maryland, Electrical Engineer Department. While teaching he also attended Catholic University and received a PhD in Physics in 1951, moving on to join the faculty of the Physics Department at the University of Maryland. In these early years he worked on theoretical aspects of the maser and laser phenomena and published the first paper discussing the possible hardware. During this period another physicist named Charles Townes theorized about similar equipment and, based on Weber's paper, Townes went on to build an actual laser instrument for which he was later given the Nobel Prize in Physics. Weber always felt that he had been cheated out of the prize and from that time forward he always kept a carefully documented notebook. In personal conversations he told me: "After the laser problem I decided to go into a field that was so difficult that no one would compete with me."

Opening the door to a new era he wrote a paper titled, "Physics, Geometry, and the New Technology for Relativity." In it he provides the simplest, elegant, and beautifully complete discussion as to how he was led to the discovery of gravitational waves. Here's an extract:

> "I was led to the idea of a gravitational wave antenna in the following way. The important field quantities in gravitation theory are the quite abstract curvature elements of space-time. Earlier we saw that the radii of curvature are independent of the coordinates or equivalently the frame of reference. A spacecraft in orbit has no gravitational effects within. But to be more precise there is no gravity at its center of mass. The edges of the spacecraft do have a residual field. If this could be measured it would enable the curvature to be computed. The gradients of the gravity field contribute to the curvature. In this way I discovered that any elastic body can be used to measure the space-time curvature. To be sure, if it is in free fall there is no gravitational effects at its center of mass. But we have noted that the gradient of the field results in small forces at the edges and these distort the body. In different language we may say that an elastic body will be deformed if it is placed in a curved space. If the space curvature is due to a wave, it will change with time in a periodic fashion. This will tend to excite oscillations. Observation of these oscillations permits measurements of the curvature of space-time."

Introduction

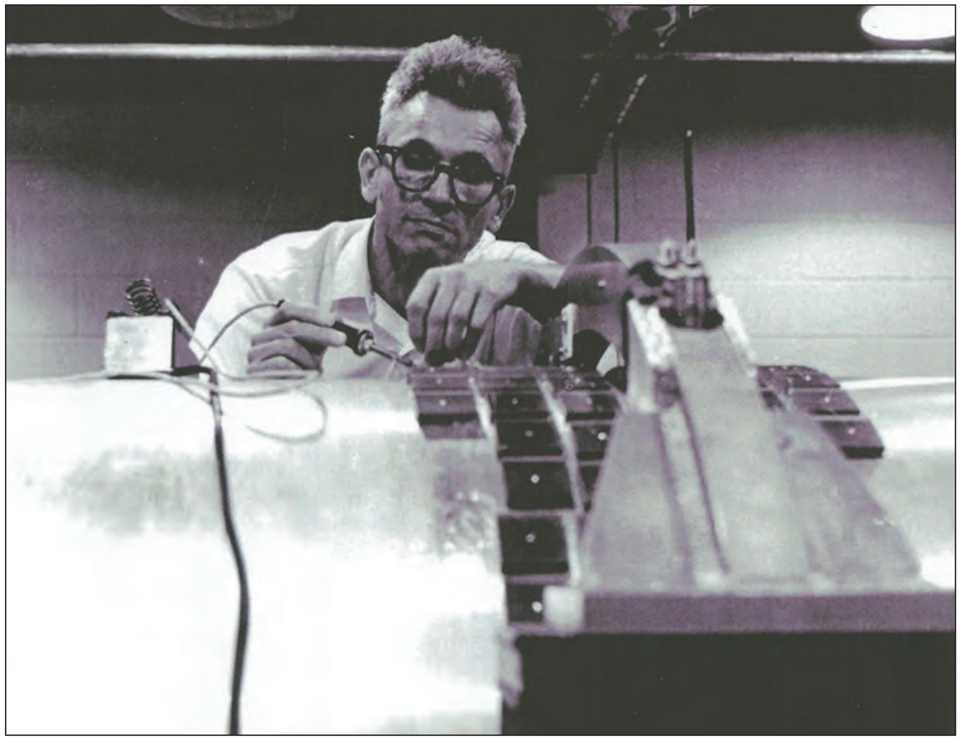
When "It can't be done" means
"It sometimes takes effort"

Gravitational waves, a central issue in the general theory of relativity, manifest themselves as propagating fluctuations in the curvature of space. Once emitted, a gravitational wave propagates virtually unimpeded forever.[1] The only modification in the wave front as it propagates are red-shifts and decreases in amplitude due to spreading of the wave front. These waves carry energy at the speed of light with a well-defined energy flux when one averages over several wavelengths. When an object is placed in this force field it experiences time varying stresses due to the wave's relative gravitational force field. This can produce mechanical strains in an elastic solid such as a large solid aluminum cylinder, a quadrupole force that acts at right angles to the vertical and horizontal plane. Imagine the vertical dimension becoming smaller while the horizontal becomes larger. On the next half cycle the distortion is reversed and the vertical becomes larger while the horizontal becomes smaller.

A lot of theoretical work had been done over a period of several decades on the radiation problem, but experimental work was not possible until 1959. By using Einstein's field equations Weber was able to show that the Riemann Tensor would induce strains in an elastic solid. This initial discovery was followed by proposals for the detection of gravitational waves from interstellar sources. The thought was that the most abundant and powerful sources of gravitational waves would be stars collapsing and becoming supernovae. This determined the size of the laboratory antenna. Most stars rotate slowly until their nuclear fuel is consumed and gravitational collapse begins. As the star collapses it begins to spin faster and faster, becoming deformed and radiating away much of its rotational energy as an intense burst of gravitational waves. The energy is estimated to be on the order of 10^{50} ergs/sec, centered near a frequency of about one thousand cycles per second.[2]

From these considerations, a receiving antenna was chosen to be a solid cylinder of aluminum suspended at its center by a wire around the circumference and attached to an arch in a vacuum chamber with the arch being mechanically decoupled from its surroundings by an attenuation filter in the form of alternate layers of iron and rubber. The cylinder dimensions were five feet long and two feet in diameter, with a weight about three thousand pounds. The length provided mechanical tuning to a frequency of 1660 cycles per second. This frequency was selected based on mathematical analyses of supernova collapse, predicting the range of frequencies swept through.[3]

The detector responds to the energy deposited by the wave by 'exciting' the longitudinal mode of the bar at 1660 cycles per second and 'storing' the energy in the 'Q' of the bar. The 'Q' is a measurement of the ability of the bar to store energy and was normally thirty to forty seconds duration. This means that the bar actually 'rings' for that length of time. The concept is similar to a tuning fork. Tap a tuning fork on a hard surface and it will ring for an extended period of time. This same concept works with a large aluminum cylinder suspended in a vacuum chamber and isolated from the environment.



Joe Weber after installing new gold plated quartz piezoelectric strain gauges

To detect the strains induced in the bar antenna one can bond piezoelectric crystals to the cylinder near its center. As the bar 'breaths' from the ringing induced by the passage of a gravitational wave the strain causes the piezoelectric crystals to expand and contract and produce a voltage which is then amplified through low noise amplifiers, passed through a narrow band filter, and eventually displayed on a pen and ink recorder. The early detectors had sensitivity on the order of 10^{-15} cm. The ultimate limit in sensing this motion is the thermal noise of the cylinder, which is the incentive to move to lower temperatures to reduce thermal noise. Better crystals and operating at four Kelvin gave a sensitivity of 10^{-17} cm. Compare this to the LIGO Laser detectors that ultimately detected the elusive waves and had a sensitivity of 10^{-21} cm.

With the early Weber bars the process of detecting the possible gravitational waves was a statistical exercise that involved comparing the pen and ink recordings and measuring the height of the pulses. Only those pulses that exceeded a level five times the average noise level were considered as possible candidates. Weber was not a statistician, but developed his own algorithm to analyze the pulses.

To improve the probability of detection a second antenna was used. From 1967 through 1969 we had set up a small 8" diameter bar that was also five feet long and tuned to a frequency near the large bar antenna, located in the basement of the Molecular Physics Building at the University of Maryland. The advantage of the Molecular Physics Building was the large isolated cement piers in the basement, which were isolated from the surrounding soil, and the fact that the building was separated from the Gravity Building by a distance of one mile. The electronic signal from the detector in Molecular Physics was transmitted by a telephone line to the Gravity Building on the golf course. Using a electronic device similar to a small computer developed by electronic design engineer John Giganti, we were able to observe simultaneous increases in the noise power of the cylinder within two tenths of a second of each other. This became the first 'coincidence detector'.

Weber felt considerable pressure in these early days of the experiment, since the probability of success was small. On one occasion he talked to me about the possibility of failure. We were in his car driving from the gravity building to the other detector in the basement of the Molecular Physics Building on the main campus of the university. With some hesitation he mentioned the coincidence experiment and the fact that results may be negative. He went on to point out, matter-of-fact-like, that there are other things we can do. As if to verify a skeptic's view of the experiment I was called to the office of Dr. Thurston Griggs, a physics department administrator, and given a copy of the departmental picture showing Weber and myself working over a not-so-successful thousand-cycle detector. Griggs said he thought I might like to have this picture as a souvenir. At the time it was difficult to determine if that was good or bad news. Later that day I took the picture to Weber and asked him to autograph it for me. Looking somewhat nervous he simply wrote 'Joe Weber' and handed the picture back to me without a word. In those days none of us expected that the number of coincidences would become so large as to indicate a release of energy so enormous that all other forms of energy are dwarfed in comparison.

Gravimeter Experiments

A second type of detector developed used the earth's normal modes and those of the moon with the possibility of observations in the range from about one cycle per hour upwards in frequency with a cross section of 10^4 m^2 for the earth and roughly 50 m^2 for the moon. The device first used was a LaCoste-Romberg survey meter that was modified by installing a capacitive sensing and feedback device. The gravimeter was incorporated into a null seeking servo system whose error signal is proportional to the change in g from its average value. It was essentially a device to measure the surface acceleration of the earth and thereby the force of gravity. A more common use for such a meter has been in the area of oil prospecting where the density of material below the meter effects the *g* measurements and can be used as an indicator for oil, since the density of material below the meter is affected.

In actual operation when the surface of the earth undergoes acceleration the position of a reference mass in the instrument will tend to change and unbalance the radio frequency bridge and provide an output voltage. This output, plus amplification and phase detection, can be utilized to provide a D.C. voltage with polarity depending on the sense of displacement. The D.C. voltage is then fed back to the capacitor plates, providing restoring forces until the mass is returned to a position of equilibrium. The restoration voltage is a measure of the change in surface *g* brought about by the comparison of gravitational and electrostatic forces.

Weber had a gravimeter operating for some years early on using the earth as a sensor but it was never fruitful in the search for gravitational waves. Using the earth as a large elastic body with its quadrupole mode at one cycle every 54 minutes is exciting, yet this mode has the highest noise level[4]. This noise is partly due to cultural activity, the irregular character of the winds, and partly associated with irregularities in the tides. The end result is that earth strains are several orders larger than the effects detected by delicate instruments. If the tides were absolutely regular their effect could be removed from the record. Another solution might be the use of two gravimeters in different places with cross correlation of their outputs. The idea was that random effects of the tide will not be completely correlated at different points on earth.

Darrell Gretz and Joe Weber checking the 8" diameter bar

Finally it was decided that a better celestial body for use as a detector might be the moon. With NASA's support, Weber was able to take on Giganti's assistance, as well as that of Jerry Larson, a Professor of Electrical Engineer at the University of Maryland. Their improvements in gravimeters made possible sensing changes in surface gravity of one part in 100 billion. After several years of development the gravimeter was finally sent on its way to the moon on December 6, 1972.

Graduate student Russell Tobias' PhD thesis project was to analyze the lunar gravimeter output for excitations indicative of gravitational waves passing through the body. The experiment was a failure due to an error of a machinist who was given the responsibility of producing the reference mass within the gravimeter. The machinist failed to take into consideration that the gravity on the moon is one sixth the value on earth, so the mass weight must be adjusted accordingly – and it wasn't. It was a million dollar failure early in the search for gravitational waves.

Calibration of the Bar Antenna

"There exists a passion for comprehension, just as there exists a passion for music."
Albert Einstein

Testing the sensitivity of the detector was a task assigned to graduate student Joel Sinsky for his PhD thesis project. Sinsky developed a gravity signal source consisting of a small aluminum cylinder driven acoustically at the natural frequency of the detector. If a signal could be generated in the detector bar by the known output of the acoustically driven source bar positioned a foot or so away in a vacuum chamber, then it would confirm the belief that the normal output signal from the detector bar is due to the induced strain. The same experiment showed that there is no gravitational radiation from outer space contributing a continuous signal above the thermal noise level and did in fact establish the noise temperature of the cylinder as close to room temperature. The detection of the source signal was by means of close coupling induction.

When the Sinsky experiment was completed in 1967 the small bar was moved to the Molecular Physics Building. Joel had worked on the project for seven years and was at the limit of time allowed for a graduate thesis project. I remember one day asking him, "Joel, why do you continue to struggle with this experiment?" His reply stayed with me for years for its simplicity and profound insight. He said, "Darrell, perseverance is a powerful tool." It gave me a better appreciation of a line in Kipling's poem 'If— ': "And so hold on when there is nothing in you/Except the Will which says to them 'Hold on.'"

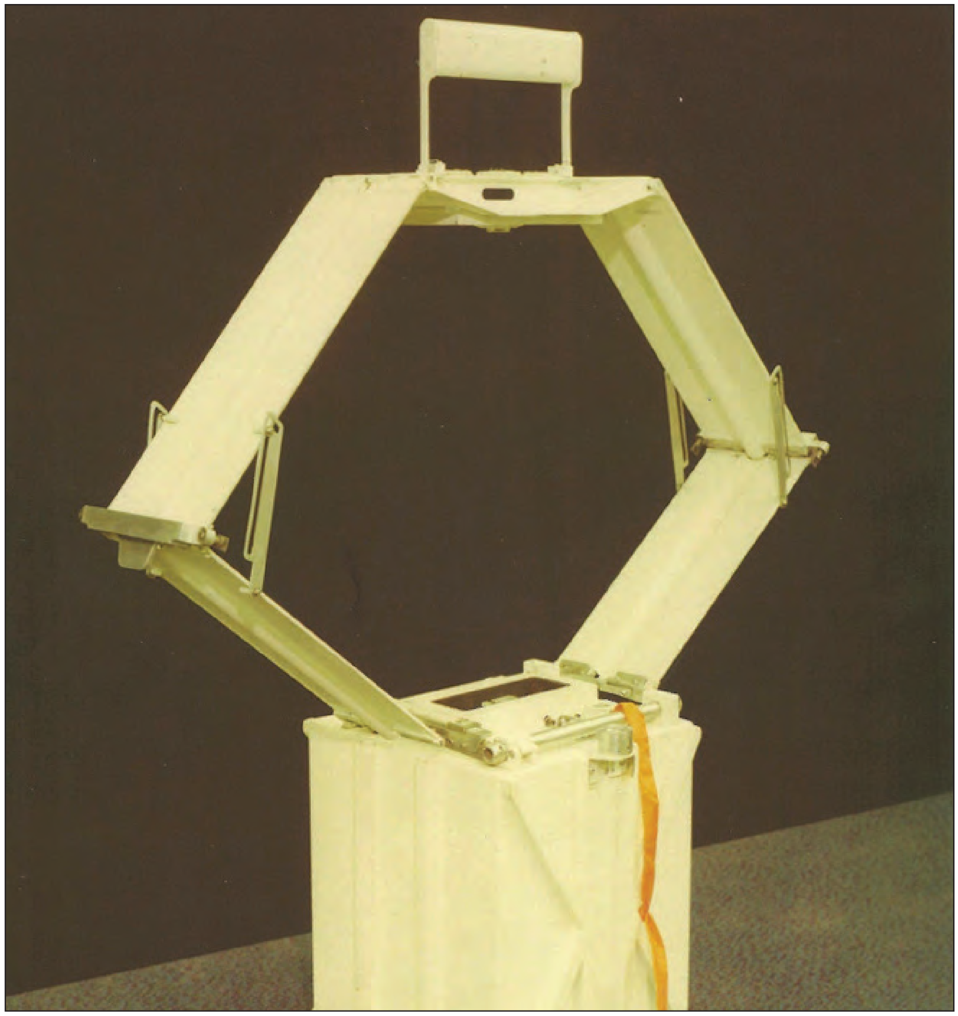
The 'gravity wave generator' that

Sinsky developed was an aluminum cylinder eight inches in diameter and approximately five feet long suspended by a wire in a milled groove around its center and hung in a vacuum chamber. At 70 degrees Fahrenheit its natural resonant frequency was 1659.45 cycles per second. Driving the generator was accomplished by bonding barium titanate piezoelectric crystals to the surface and exciting the lowest longitudinal mode of oscillation by applying a two thousand volt peak to peak signal to the crystals. This method produced high dynamic strains to be coupled by Newtonian gravitation interaction.

The detector cylinder was suspended in a separate vacuum chamber and was two feet in diameter and five feet long, with a suspension similar to the generator and a natural resonant frequency of 1657.52 cycles per second. By controlling the generator temperature it was possible to maintain the two cylinders within .01 cycles of each other.

Throughout the period from 1961-1967 while Sinsky was performing these experiments a number of unusually strong peak events were seen on the chart recorder connected to the output of the electronics to constantly monitor the noise output from the 24″ diameter detector bar. These events were characterized by a very rapid rise in the power output from the detector to several times the mean power level before the event and then a return to the previous level with a time constant that was characteristic of the relaxation time of the detector. It was clear that, whatever the source of these events, it was dumping energy into the detector in a time period very short compared with the relaxation time. Of course it gave the group hope that it was a meaningful event.

At the time we had a number of instruments to monitor the external sources of excitation. This included an electromagnetic disturbance monitor to observe disturbances entering the building on the power lines. A gravimeter measured changes in the earth's vertical acceleration while seismometers and a tilt meter detected seismic energy across the spectrum. All these instruments were in addition to a room temperature monitor and controls that maintained the room to within a tenth of a degree. The chart recorder output gave no indication that the bar was being excited during these events. This also worked in the opposite direction. When the monitor instruments were excited, even strongly, there was no related change in output of the gravity wave detector.



Lunar gravimeter engineering model

Weber often joked during lectures concerning the experiments that periodically the Atomic Energy Commission would set off an H-bomb just so we could test the isolation of the detectors. On those occasions it was common to see the seismometers and tile meter go wild while the gravity wave detector would be completely unaffected by the bomb's seismic wave. In fact, we were even able to time the passage of the seismic event around the world by watching for a smaller seismic event hours later and of smaller magnitude as the wave circled the earth.

The events that Sinsky saw were so far above the thermal fluctuations of the detector background noise and with a rate of occurrence of one every few weeks that they were out of line with theoretical predictions. They had occurred without simultaneous events on the monitor instruments. The most sensitive of the monitoring instruments was the gravimeter that could detect a vertical acceleration of a part in 10^{10}.

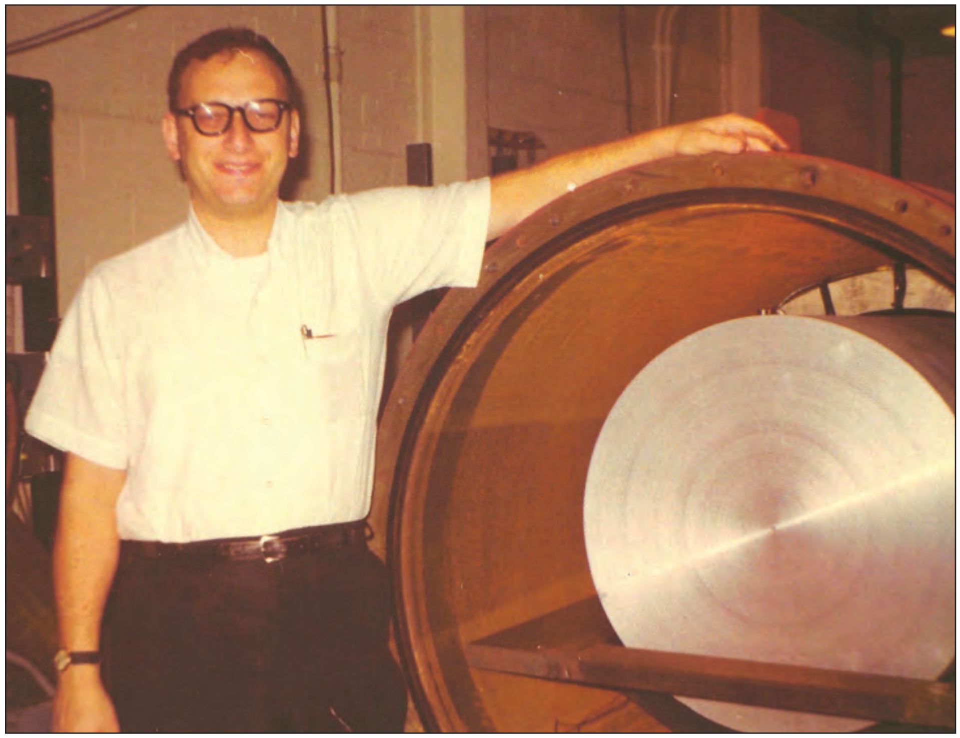
The following table lists the events by date and Greenwich time[10].

0924	21 Sept. 1965
2342	5 Aug. 1966
1015	7 Aug. 1966
1645	22 Nov. 1966
0130	1 Dec. 1966
0720	17 Dec. 1966
0140	20 Dec. 1966
1730	10 Jan. 1967
2309	22 Jan. 1967
1320	17 Feb. 1967

Events like these that gave hope that they must be real phenomena worth searching for, and that gravitational waves must be the source.

On one occasion I remember receiving a phone call from an individual at the National Bureau of Standards who had heard about the search for gravitational waves, and had apparently come across a reference to the 'calibration' of the detector. His question to me was: "Can you tell me in about a minute how Sinsky calibrated the gravity wave detector?" I laughed as I pointed out to him that Joel had worked on the 'calibration' for six years and I certainly couldn't summarize it in one minute.

The task was so difficult that it took Joel and myself (who had been hired as a technician to help Joel finish his thesis project) a full year to properly build an enclosure and isolate the generator. Eventually we achieved excellent electrical and acoustic isolation, and the experiment was completed. Joel went on to work in one of the government acoustics lab in the Washington DC area, from which he later retired.



Joel Sinsky with the 26" twin detector

Diurnal Noise Effects

"Many people look but not all people see"

Graduate student Reg Clemens, an excellent computer programmer who came to us from the California Institute of Technology, was assigned the task of searching the data output file to identify slow time varying signals of extraterrestrial origin. The data collection process began in October 1967 and continued into February of 1968. The ultimate result was indicative of white noise, and served to confirm the high degree of isolation of the detector from its environment. It was estimated that the selected run of data would allow signals close to the thermal noise level to be observed but as it turned out the results were negative indicating that there was no fluctuation 'riding' on the data of a time varying nature.

First Results and Extending the Baseline

'Ask and you shall receive, seek and you shall find.'

With the completion of Sinsky's work in the spring of 1967 the bar and vacuum system became available for conversion in a short period of time to an additional gravity wave detector. This bar was not ideal for a second detector since it weighed only three hundred pounds compared to the three thousand pounds of the large detector, but our incentive was the speed and relative ease with which it could be put into operation as a second detector near the frequency of the other bar. This become the twin bar experiment between the Gravity Building and the Molecular Physics Building.

As time progressed it was possible to obtain coincident events during periods of tranquility. The initial data was analyzed by viewing separate chart readouts with an accuracy of one minute. It immediately became apparent that events were being seen, and it was decided to run a phone line between the two buildings and do a real time coincidence experiment.

Weber normally spent several weeks during the summer at the Aspen Institute for Humanistic Studies with his close friend Charlie Mullen, the Dean of the Notre Dame Physics Department. I was left alone to tend to the experiment and maintain the operation, as the only person Weber trusted to touch his equipment. He wrote me the following letter in the summer of 1972.

> Dear Darrell,
>
> This concerns the new black box. We have two detectors, one in Molecular Physics and on in the Gravity Building. Every now and then both give signals. These are 1660 cycles damped wave trains like this: the leading edge rises quickly in less than 100 milliseconds and the decay lasts 30 seconds for the gravity building instrument and 17 seconds for the molecular physics instrument. These numbers may not be correct but if we obtain that oscilloscope camera they can be measured.
>
> We need a device which provides an output pulse roughly one second long for an Esterline Angus recorder if and only if two signals arrive with a leading edge within a quarter second of each other. The Molecular Physics signal will be transmitted over a telephone line- probably a balanced twisted wire pair. Audio transformers to go from balanced to unbalanced outputs together with some amplification will be needed at both ends.
>
> Yours truly,
>
> (J. Weber)

This was to become the format for our working together. After Weber's first wife Anita died of a heart attack in July 1971 he married Virginia Trimble, an astronomy professor at University of California, Irvine, and spent half of the year in California and the summer at Aspen or the Princeton Institute where he concentrated on evaluating the experiments. My job was to operate the experiment and oversee the data collection and general developments in the lab.

Non-Gravitational Excitation Considerations

Based on the coincidence experiments in the Molecular Physics Building and the Gravity Lab, Weber was convinced that spatially separated gravitational wave detectors were being excited by a common source, though others searched for possible sources in the environment. A cautious man, Weber set up an elaborate system of instruments to monitor various sources such as seismic activity, electromagnetic pulses, cosmic rays, temperature changes, line voltage fluctuations, and general building activity.

The seismic array consisted of a vertical axis seismometer tuned to the cylinder frequency of the detector, a three axis seismometer covering frequencies near 100 cycles per second, and a two axes tilt meter. He often boasted when giving lectures that the seismic array could detect a bird landing on the roof of the building. I was never convinced of that, but one experiment we routinely did when visitors came to the building was to take a pin and drop it on the floor near one of the seismometers and watch the indicator needle go full scale. The usual response was an amazed "Wow!"

Another seismic sensor that always brought out a shocked expression was the two-axis tilt meter. The meter was a steel bob suspended from a very thin meter-long wire. The bob hung between plates that formed a capacitive sensing system and allowed for sensing changes in the tilt on the order of two seconds of arc. Alignment of the instrument was by compass heading so that floor tilt either in the east west or the north south could be detected. Demonstrations of this instruments capability were very exciting when the Atomic Energy Commission ran H-Bomb tests. As I mentioned earlier, it was possible on this instrument to see the shock wave travel around the world and return again. Another demonstration we used for visitors was to have them walk near the tilt meter and let them observe personally that body weight was sufficient to cause a tilt in the floor.

Great care was also taken to isolate the bar antenna from the floor vibrations. Early in the program David Zipoy and graduate student Richard Imlay spent considerable time in 1960 working on isolation problems. The end result of their research was a contribution to the program in the form of vibration isolation stacks that the detector arch support rested on inside the vacuum chamber. These same stacks made up of alternate layers of steel and isomode rubber were used to isolate the vacuum chamber feet from the floor of the building. Outside the chamber we could only use three alternate layers because air coupling to the chamber walls would overcome the effect of having more stages, but inside the chamber we had as many as twelve stages of alternate layers of iron and rubber. Isomode rubber was a sheet of rubber about 3/8" thick with ribs on the top side that ran north south while the bottom of the rubber had ribs that ran east west. Each stage gave an attenuation factor of five and so using ten stages in the chamber gave an overall attenuation of 5E10. They made a very respectable filter.

Electromagnetic signals can enter the system in many ways: power supply cables, currents induced in the vacuum chamber, transmission lines from vacuum chamber to the electronics, or even through the electronics directly. To counter these we first had two completely independent power supplies, one for the pre-amp and one for the post-amp and each had twenty four volts of automobile batteries across the output to act as a regulator. Shielded cables were used from batteries to power the amplifiers. The electronic amplifiers themselves were enclosed in acoustic boxes. In addition the amplifiers within the boxes were enclosed in a large heavy aluminum electromagnetic shield.

Weber was taken aback at the June 1972 Relativity and Cosmology Conference at Halifax, Nova Scotia when J.A. Tyson of the Bell Telephone Laboratories reported on a two year study he made of electromagnetic effects between Argonne National Laboratory and Bell Labs. His conclusion was that the coincidences observed on the gravitational wave detectors could not be the result of electromagnetic excitation. Unknown to us, he had installed an antenna and a radio receiver on the roof of the physics building at Argonne and was monitoring the experiment for interference.

Detector interactions with cosmic rays was investigated by N.S. Wall, G.B. Yodh, and D. Esrow. They searched for coincidences between the gravitational detectors and Cerenkov radiation counters placed under each of the vacuum chamber. In a later experiment they used meter square plastic scintillators but no significant correlations were observed.[4]

Even human activity was monitored in the building through a logbook placed on a desk by the door where everyone entering had to enter their name and the time of entry and exit. Weber even had visitors sign the log as proof that gravitational wave detectors did exist. One of the humorous entries was made on March 16, 1972 by Virginia Trimble: "This is absolutely the silliest way anybody ever spent a wedding day."

The actual date of Anita Weber's death was July 7, 1971 about 2 o'clock in the afternoon. Our secretary came to me and said that someone must answer the phone and talk to a policeman from Harrison, New Jersey who says Mrs. Weber has died and they must find Dr. Weber. It happened that Weber was at a Physics conference in Copenhagen, Denmark but the secretary was panic stricken. I picked up the phone and police officer George Harris told me that Mrs. Weber had died at 1:40 PM of an apparent heart attack. He said, "The body will be taken to Harrison Funeral Home on Halstead Avenue in Harrison, New Jersey" and gave me the phone number of the funeral home. My first comment was: "Are you sure its Mrs. Weber and not her mother Mrs. Oppenheimer whom she was visiting because her mother was sick?" His reply was definite: "Yes sir, the dead person is Mrs. Anita Weber." I went to the Chairman's office to report the news and have the Chairman contact Weber at the Sheraton, Copenhagen where he was staying during the conference. I stood there by the Chairman as he dialed the number. When Weber answered the phone the Chairman simply said: "Joe, your wife has apparently ceased breathing." Then he added, apparently in response to Joe's comments, "Yes, she is dead." It was a shockingly sad day for him and his four sons.

Scalar Tensor Disc Experiment

"A strong motivation for searching for the radiation lay in the fact that no one ever searched for it."

The objective of the disc antenna experiment was to test the scalar-tensor theory proposed by Robert Dicke at Princeton University. Scalar radiation interacts with matter by causing radial oscillations with respect to the direction of propagation that is different from quadrupole radiation of gravitational interaction. Using an aluminum disc seven feet in diameter and six inches thick at the University of Maryland in coincident operation with the standard cylinder antenna at the Argonne National Laboratory would test the theory. A coincidence was defined as a simultaneous increase in the noise power of both detectors within a fraction of a second of each other, and both detectors operated at frequency of 1660 cycles per second.

The disc antenna was put into operation in 1970. It was Weber's usual policy to not tell me the details of what was coming out of the data since my contact was on a day-to-day operational bases. I can tell an interesting story about the suspension of the disc. I had been doing experiments trying different types of epoxy to bond strain gauges to the bar antennas for increased coupling. Frank Desrosier from the machine shop and I both suggested we should use this same epoxy to bond 'eyelets' to the flat sides of the disc at its center for support, since Weber simply refused to have any holes drilled and tapped into the face of the disc to attach eyelets. We finally got the disc suspended at a twenty-three degree angle to compensate for the tilt of the earth's axis and left for the night. Weber stayed to check the electronics. We closed the door and would start the pumps in the morning.

When I arrived the next morning the disc was lying on the vacuum chamber floor because the epoxy bonds snapped during the night. I went to Weber's office and told him there had been an accident and explained what happened. He turned red with rage and said: "There are no accidents. There are only lapses in human consciousness, and you failed to take all possibilities into consideration." I never forgot those words and I learned the lesson that epoxy in tension has no strength.

We collected data for some weeks, and Weber admitted to me that it didn't really make sense to him. Finally he said to me, "Darrell, I just decided to roll up my sleeves and spend day and night working on the disc records and now they make sense." It was a matter of interpretation of the forces and the geometry of the disc-earth relationship to the galactic plane. With the cylinder antennas a histogram based on sidereal time indicated a peak in activity when the galactic center was on the meridian. This happened twice a day due to the rotation of the earth on it axis. With the disc it was expected that there would be an absence of events if there were only tensor radiation, since the radiation would impinge on the plane face and the forces in the disc would cancel out. If the radiation had a large scalar component the results expected were the familiar double peak in the sidereal anisotropy.

What was actually being seen was a peak in activity during the sixth sidereal hour and a decrease during the eighteenth sidereal hour. How could this be explained as some combination of scalar-tensor radiation? The explanation was obvious after giving some thought to the geometry. The earth rotates with its axis tilted twenty- three degrees and the detectors are positioned in the east west direction at forty degrees north latitude. To have a perpendicular line from the disc to the galactic center requires that the disc be tilted north about twenty degrees. During the eighteenth hour of sidereal time the geometry is ideal to search for scalar components, but during the sixth sidereal hour the disc is on the opposite side of the earth and effectively has an additional tilt of twenty three degrees resulting in the disc edge 'seeing' the galactic center and being excited by tensor component of radiation, since now two of the quadrupole forces would be in the plane of the disc while the two other forces are outside the disc plane. The end result of these forces is that instead of the force cancelling out they now can excite a response in the disc. This one experiment and the results lead me to believe that Weber was honest and straightforward with his analysis and in fact *must* be seeing real events, since he did not understand the data at first and had to think it through again to make sense. How then could he have 'faked' the data? I believe he was seeing real events.

The Move to Low Temperatures

"There's a way to do it better – find it." Thomas Edison

At room temperature where the bar antennas operate the thermal noise of the bar is essentially based on the thermal bath of the environment. To improve sensitivity and open a new avenue to better data it is important to lower the temperature and a natural point to reach is four Kelvin, the natural boiling point of liquid helium. Our first attempt at low temperature experiments began in 1970 when it was realized the room temperature experiments had been successful and the need for improved sensitivity became apparent. Temporary assistance came from John Purcell and Mike Morgan at Argonne National Laboratory High Energy Physics Group. Purcell supplied facilities and manpower in addition to his low temperature experience while Morgan did the cryostat design to enclose the aluminum cylinder and provide for cool down from room temperature to -452 degrees F with operation at liquid helium temperature.

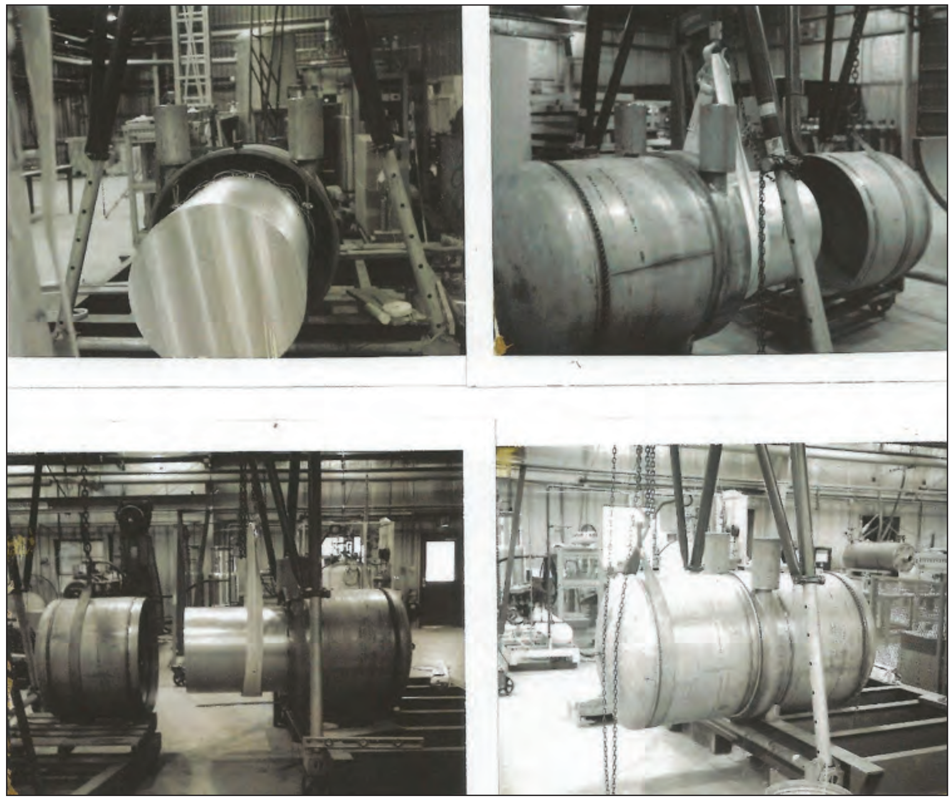
Weber selected the number of piezoelectric crystals to be used and Purcell decided on the bonding method. We used twenty five 2" x 2" x 375" lead zirconate titanate crystals bonded to the aluminum with a sandwich formed from two layers of Dupont adiprene rubber separated by a 10 mil piece of indium. At four Kelvin the rubber becomes rigid while the indium allows for strain relief to prevent crystals from cracking due to the large differential of contraction between aluminum and crystal.

That first cryostat was an abomination. After the experience Weber told me that I should become the cryogenic engineer for the group, and so I did become a self-taught cryogenic engineer. The Argonne detector had the usual 60" x 26" bar suspended on a stainless steel cable in a groove around the center of the bar. The ends passed through holes in the center of the isolation stacks and were pinned at the top of the stack. Instead of rubber we used wool felt, which remains soft and pliable at low temperatures. This may be the only new idea that carried forward in all our cryostat designs. The final assembly was actually welded together and the inner tank was covered with a layer of lead sheet as a superconducting electromagnetic shield. The second lesson learned in this process was to use indium wire as a sealing material, which also remains soft and pliable down to absolute zero.

Cooling the vacuum chamber with the internal bar was accomplished with two coaxial radiation shields. An internal copper shield with plumbing to provide for flow of liquid helium and an out copper shield to provide for liquid nitrogen flow to provide and intermediate temperature range of 77K form the 300K room temperature and then the 4.2K shield protected by the 77K liquid nitrogen shield. Insulation between the two concentric temperature shells was provided with twenty layers of aluminized mylar on each radiation shield. My own education as a cryogenic engineer led me to the conclusion that the shields could actually be run as two concentric helium gas boil-off shields. In our later designs I used the inner vessel as a 250 liter liquid helium dewar with the boil off gas flowing through a copper radiation shield having 60 layers of super insulation and then into a second concentric copper radiation shield with an additional 60 layers of insulation and then returned to the liquefier. This design allowed the inner shield to operate at 20 Kelvin and the outer shield to operate at 100 Kelvin and provided for a liquid helium holding time of about eight days with boil-off of about one liter per hour.

The original Argonne cryostat required a cooling operation which lasted several months and consisted of lowering the temperature three or four times, with the limiting factors usually hardware problems in one form or another. I joked that I had been down to four Kelvin with a gravity wave detector more times than any man in the world.

After solving all the hardware problems we eventually did reach 4.2K and operated for a period of several minutes. The noise level was far beyond imagination and we began to turn off equipment to study various sources.



The first attempt to assemble a 4 Kelvin Gravitational Wave Detector at the Argonne National Laboratory with a 26" x 60" aluminum bar and lead covered and welded inner chamber

First the vacuum pumps were turned off and then the liquid nitrogen shield and finally the lights and other electrical equipment. At last we had performance consistent with four-Kelvin expectations but were left with no means to maintain the temperature since connecting radiation shields resulted in such a large noise level. We were left with no choice but to terminate the experiment and redesign the cryostat with improved noise performance our number one objective.

A meeting was arranged with a cryogenics consultant and John Purcell, Weber, myself and people from our mechanical development group. Analysis of the cryostat by P.C. Van der Arend of Cryogenic Consultants left no doubt that the design was very poor indeed. He suggested that our plumbing should never have been designed with pipes and multiple path lengths since such an approach allows for differential pressures and flow with resulting poor noise isolation. A better approach is to have a large dewar surrounding the inner vacuum chamber, which allows for the accumulation of liquid helium and subsequent boil off to reach a relatively quiet operation compared with fluid flow.

A final design was settled on and the Mechanical Design Group at the University of Maryland, consisting of Frank Desorsier and Jim McClure, prepared an entire cryostat on paper. A final meeting was arranged to obtain Vander Arend's blessing on the design and production of hardware began.

Cool-down calculations indicated that about two thousand liters of liquid helium would be needed, and the cost was about two dollars per liter. We had to find a better, cheaper method. Salvation arrive through Cryogenic Technology Inc., a division of Arthur D. Little, from whom we bought two complete CTI 1400 helium liquifier/refrigerators. Now we could have one at Argonne and one at Maryland. I really become the Cryogenic Engineer and traveled to Argonne regularly to build the systems. I had the help of our machine shop from Karl Harzer and Gordon Hughes who were invaluable. Karl was a very mild mannered giant of a man who stood over six feet and weighed in at probably 275 pounds. He had been a rigger in the Merchant Marines during World War II and could move a mountain as delicately as a dozen of eggs. They worked 'with' me rather than 'for' me.

Eventually the Argonne facility became too expensive to maintain with weekly trips for three people to fly out on Monday morning and return home on Friday afternoon for a week-end off. During the week we worked a full twelve-hour day from seven in the morning to seven at night. Usually after work we went into Lemont, since it was only several miles from Argonne to Nick's Tavern. They had a cheeseburger that was at least five inches across, probably six. I had one every night, and Karl usually had their steak sandwich which was about a pound of steak on a roll.

We removed the equipment from Argonne and retuned to Maryland, where we took control of the former Cyclotron Facility that was four floors below the basement. It certainly provided a quit environment for a gravity wave detector. With a cryostat in the Physics basement and another complete installation in the Gravity Building my job became a twenty-four hour a day task. I had now built two large cryostats with helium liquefier-refrigerators feeding them cold gas and ultimately liquid helium. Continuous operation required my presence at all hours of the day and night even to the point of setting up a cot in my office for a sleep over now and then. Helium liquefiers are very demanding machinery and require regular maintenance and attention.

After a year of 24-7 operation we had a very long stream of data for the computer programmer to analyze. At this time our programmer was Greg Wilmot who was a very quiet, reserved and withdrawn guy. Finally he went to Weber and told him that the noise temperature of the bars was not consistent with four degree noise and in fact there was too much noise to get a good signal-to-noise ratio. In fact, the numbers where reversed the noise level was so bad. It was a real disappointment after all those long hours of baby-sitting the systems. Finally it was decided that we had to go even lower in temperature and work was started on a cryostat to reach milliKelvin temperatures using a He^3-He^4 dilution refrigerator.

These were the days when we were beginning to have financial problems, because other groups had joined the search for gravity waves and were reporting negative results. But our long-term objective was to eventually lower the temperature to millidegrees, but I certainly had my doubts considering the problems in approaching four Kelvin. At one point Weber gave me a paper on dilution refrigeration using He^3-He^4 and asked that I read it over and make recommendations to incorporate it in our system. After reading the paper it became apparent that we knew how to talk low temperature and cryogenics, but it would take several years at best to reach millidegrees. We needed a completely different cryostat approach to have a dilution refrigerator insert down through the middle of all the radiation shields and connecting to the internal dewar to cool the bar.

Even lower temperatures could be reached using adiabatic demagnetization of a paramagnetic salt, but I seriously doubted that we could have worked out the details in this life. Our work was coming to an end because money was drying up.

Others in the Field

"The real need is for more observations." Joe Weber

Weber's 1969 paper, 'Evidence for the Discovery of Gravitational Radiation,' was the catalyst that brought the world to his door. Not all of the visitors were friendly and those who were soon realized that something was not right with the data. During an 81 day period there were more than 17 significant two-detector coincidences, 5 three-detector coincidences and 3 four-detector coincidences.[3] Because of the prolific number of published events it was quite certain that not all coincidences could be accidental. A double two-detector coincidence on Feb. 20, 1969 had a probability of occurring accidentally every 7×10^7years. These stunning numbers gave the incentive for others to join the search. Among the first to come running was Hans Billing of the Max Planck Institute in Munich and William Fairbank of Stanford University. The German group was interested in duplicating the experiment exactly even to the extent of using the same transistors. Since I was the keeper of all the schematics for the electronics, it was my job to provide copies from the huge three-inch thick notebook I kept in my office. Of course they were more than pleased that we were so cooperative with them.

The research group from Stanford decided to completely ignore Weber's approach. At first I think Weber felt it was intended as a put-down but later as their program unfolded it became apparent that William Fairbank was supremely capable in low temperature physics. With his colleague at L.S.U., William Hamilton, they set out to develop two 24" diameter by 10' bars operating at three milliKelvin.

Their approach was to use aluminum cylinders two feet in diameter, ten feet long and weighing five tons. A plasma sprayed niobium-titanium coating was applied with sufficient thickness to 'float' the cylinder on a magnetic field passing through the superconducting coating. We thought it was a brilliant idea and an excellent way to isolate the cylinder from the vibrations of the earth. The front-end electronics used an inductor coupled to a toroidal super conducting quantum interference device which goes by the acronym SQUID. As the reader knows by now, going from room temperature to two Kelvin requires multiple concentric copper radiation shells with their own individual cooling mechanism. The Stanford group also added an extra temperature by vacuum pumping on the 4.2K helium liquid and thereby lowering the temperature to 2K. To this shield was attached a superconducting shield which completely isolated the gravity wave antenna from electromagnetic disturbances. An inner shield surrounding the antenna was cooled to 50 millidegrees by a He^3-He^4 dilution refrigerator and then further cooling using adiabatic demagnetization of a paramagnetic salt.

It was an incredibly sophisticated proposal and left me completely overwhelmed. In discussing it with Weber I said to him that we might as well just prop up our feet and wait for the results since our efforts would appear trivial by comparison. He replied: "Well, Darrell, I'd be the last one to sell the other guy short because I know how it feels having had it done to me so many times, but I've heard these types of proposals before and I'm still waiting for results."

Another group to enter the discussion was Tony Tyson of the Bell Laboratory and David Douglas at the University of Rochester who built detectors of similar size and frequency range as the Stanford group but operated at room temperature. Tyson used a small disc with its bending mode tuned to the detector frequency and a single crystal mounted in the center that could, he thought, improve performance and resolution time. We called it a 'mushroom' since that was the way it looked but our experiments with such a system indicated that it was about a factor of two worse than the present room temperature detectors. Each time Weber asked Tyson for more details of the design he never got a response. Finally Weber visited Tyson's lab to set up a cooperative experiment and discovered that Tyson was using the same crystal pick-off system that we had.

In the end the visit finally did work to our advantage when Dr. John Paul Richard picked up on the idea and developed a three-mode mechanical amplifier. His approach was to rely on the large cylinder as the 'driving' mechanism, and the three-mode transducer would have the appearance of the mushroom but actually be far more sophisticated with a smaller mass coupled to an even smaller thin plate where he had a coil and SQUID electronic pick up. Imagine a plant sitting on a table and you tap the edge of the table and observe the small leaves of the plant move. The table 'drives' the plant pot and the pot 'drives' the stem and finally the step passes the vibration to the leaf. We used the device on our 'new and improved' four Kelvin system.

The disadvantage of the Tyson-Weber meeting was that Weber agreed not to publish results of the cooperative experiment without Tyson's approval. It turned out that within a month of starting the experiment we were seeing a standard deviation about four-sigma. We were actually seeing simultaneous excitations of both detectors just as we had observed in our lab using our own detectors. The question remained as to what was the true origin of the events, since most scientists were skeptical.

Finally, Tyson, Maclennan, and Lanzerotti of the Bell Lab published a paper titled "Correlations of Reported Gravitational Radiation Events with Terrestrial Phenomena," with data for a period during 1969. Their statistical cross correlation of 262 gravity wave events supplied by Weber resulted in a 2.7 standard deviation level correlation with the magnetosphere ring current intensity, while further investigation found a broad area of correlation with geomagnetic activity. The physical interpretation of the correlation is that the geomagnetic effects on the gravity wave signals produce some of the actual event observed. The paper certainly didn't help our credibility with the scientific community.

The University of Maryland experiments are indicative of a galactic mass loss rate of approximately 10^4 solar masses per year and is a loss rate 100 times larger than the expected upper limit allowed by current astronomical data, suggesting that some of the gravity wave events may be of geophysical origin rather than astrophysical since the fundamental source remains as yet unknown. Weber's sidereal anisotropy histogram indicates sources in all directions throughout the galaxy although a large increase in the number of events when the galactic center is on the meridian of the detectors. Weber suggested to me that the large amount of energy from the gravity wave events might interact with the earth's magnetic field and produce some unsuspected coupling.

It should be pointed out also that a controversy erupted over the computer analysis. Douglas asked for a copy of the program from Weber and it was provided by our programmer at the time, Brian Reed. Douglas reviewed the information and found a computer error that actually injected pulses in the data. Of course this went viral and was published across the land that Weber had faked the data and the events were all due to a 'glitch' in the computer program. Reed fixed the 'glitch' but I was suspicious since he was an excellent programmer and it was hard to imagine that he made such a mistake, since I felt it would take conscious effort. I asked him about it and he just gave me a sly grin and said: "Well, he wanted to see pulses didn't he?" That event destroyed confidence in our effort on into the future. One paper I saw published had the title, "Are Weber's Pulses Illegal?" but it actually referred to the physics rather than computer errors.

A Russian group led by Vladimir Braginski at the Moscow State University built two detectors similar to Weber's but used capacitive pick off for the signal. His search for a better room temperature detector resulted in poorer performance. Braginski confided to Weber that it would be impossible for him to publish successful results because then his funding would be cut off. As long as he continued to work on improvements the funding would continue. An interesting sidelight to the Russian visit was the agent with them. A sinister looking man followed Braginski everywhere he went and took pictures. When questioned about the man Braginski just commented: "Oh, he's my minder." But one day the 'minder' stepped into my office and just as fast as stepping in he pulled out a camera and took a picture of a schematic diagram I had on the blackboard of a helium liquefier.

Another group from the University of Bristol in the United Kingdom tried cutting a cylinder transverse to its length and placing the piezoelectric transducer sandwich style in the middle. We, too, tried this idea and realized it was just crazy.

Perhaps the greatest hope of a group verifying the results was with the European Space Research Center in Italy headed by Maischberger, Bertotti and Fiocco, who developed a Weber type detector. Their engineer, Mr. Orhammer, who seemed quite friendly and competent, reviewed all the necessary schematics. Their approach was to first build a Weber type detector and study its performance and then move on to try improvements.

Skepticism has always been a part of the program from the beginning. Typical of the comments received were those of John Wheeler in April 1971, when he wrote in our log book at the Gravity Building: "Waiting to see coincidences as a function of time 'untouched by human hands'- best wishes on getting to that 'first stage' by the fourth of July." It was the hope of everyone that we could see the events via computer and bypass the chart recorders and human interaction. The problems with humans reading the charts is that it is a common failing of the human mind to 'find' what you are looking for even if it may not be there. As the expression goes, "Seek and ye shall find."

I was always drawn to the details of the sidereal histogram as evidence that Weber could not have 'faked' the data. The coincident events were collected in solar time-what you see on the clock either by human hands or later by computer. These times have to be converted to 'star time' which we call sidereal time since the solar clock is based on the earth's rotation around the sun, but sidereal time is based on the rotation of the movement of the earth with respect to the galactic plane and monitored over a period of a year. When the events were plotted in solar time the histogram was flat with no particular peak standing out more than any other. But when the coincident event were converted from solar time to sidereal time and plotted on the histogram it was found to have two peaks in the diagram. One peak when the detectors where on the meridian of the galactic center and another peak twelve hours later when the detectors were on the backside of the earth. I don't believe that a man of Weber's character would have 'plotted' to deceive the scientific community. This was a man of Naval Academy principals and his principals would not have allowed him to lie.

Weber was impatient with those who collected data for a few weeks and made statements that nothing significant was observed. He told me: "We'll just have to take the world by the ears and scrub and dry them, then show them how to do it." On one occasion his frustration over the skeptics was vented by saying: "Let's just forget the rest of the world and do the experiments we feel should be done." He continued. "Even if the experiment is wrong in some respect the design of the antenna is sound and is a major contribution to science." Often he would tell me: "Darrell, we will rewrite the physics textbooks before this program ends."

The scientific community, the mail, and the phone were constantly demanding time of Weber. It reminded me of a quote from Ronald Clarke's book *Einstein: The Life and Times*: "Since the flood of newspaper articles I have been so swamped with questions, invitations, and challenges that I dream I am burning in hell and that the postman is the devil eternally roaring at me and throwing new bundles of letters at my head because I have not yet answered the old ones."

Weber never cursed or used foul language except for once. We were standing in the lab and he was talking about the frustration of attending scientific meetings and contending with the constant and relentless criticism. He said, "Darrell, we will take an ax to the sons-of-bitches and chop them down until they see the experiment our way." That was the only time I heard such language in twenty-five years of working with him. In fact, one time in a fit of frustration I blurted out: "Ah, shit!" and his quick replay was: "Well, I suppose you think that will help? I left that kind of language behind when I left the Navy."

Implications for Astronomy

"And on the seventh day he rested."

The gravitational wave coincidence data that Weber published was dismissed as just wrong. First the computer error caused him major problems, then the questions about his statistical methods regarding threshold crossings and accidentals rates and the sidereal data. But the one fact that can't be denied is that he made a major contribution to the development of astronomy by opening a new window to the universe. Without his pioneering effort we can't predict where the science would be.

His data, were it correct, indicated that the equivalent of 1000 solar masses per year were being radiated away. This baffled the theoreticians since the upper limit they might consider would allow less than twenty percent of Weber's findings based on the expansion of the galaxy.[6] In addition, the galaxy would have radiated away its entire mass in less than one percent of its age.[7] Weber suggests[8] that the energy problem might be even more spectacular since the detectors may actually be responding to only ten percent of the signals because they cannot identify signals that are much weaker than the thermal noise. These assumptions lead to a possibility of as many as 100,000 solar masses per year being converted entirely to gravitational waves.[8] As Weber has mentioned, "This is an incredible transformation, so large that it casts doubt on the experiment." The reader can now better understand the controversy that Weber started and why the scientific community simple disregarded his findings. Perhaps it was just a case of "you will find what you are looking for" because the human mind is wired that way.

Our ability to detect gravitational waves is severely limited with current apparatus. The twenty-year effort of the LIGO resulted in one 'chirp' but it has started a new race to the finish line. If we have a few of these detectors operational we can expect to actually determine the direction of arrival and pinpoint sources in the heavens. In years to come we may also have LISA in operation and the search for source and understanding will continue.

The Final Days

A supernova was reported on Feb. 23, 1987, and became known in the record books at Supernova 1987A. This event was expected to be the answer to twenty years of work. The mass annihilation of the event would certainly spread gravitational waves and the detectors should respond. Unfortunately we had a bad snowstorm that day of almost blizzard proportions. In fact, we had a power failure due to the weather. Weber called me to go to the gravity building and look at the chart recorder to see what of the event had been recorded. It was hard to determine what was real chart data and what might have been the recorded supernova, since the chart had stopped almost at the time of the supernova. I reported all this to Weber by phone and went back to the physics building.

A couple weeks later Weber returned from California, as was his custom periodically to check on things. I was walking down the hall when Vol Moody stopped me to ask what I had seen on the chart recorder the day of the supernova. I told him we had a power failure but we also had several pulses of significant amplitude just before the failure and so it was difficult to tell what the real situation was. As I walked into my office Weber was sitting at my desk and I could tell he was enraged. Finally he said with a raised voice: "IT'S NOT YOUR PLACE TO PUBLISH DATA ALL OVER THE PHYSICS BUILDING. THAT IS MY JOB." He went on: "How could you say there were several pulses? How could you be so stupid to think that? Not even God would do such a thing. If there was a gravity wave event it would be a single pulse and nothing more." After several other very stinging remarks about my incompetence he said: "The chart record has no relation to reality and you apparently are unable to admit you made a mistake." I felt a rush of adrenalin and my heart beating rapidly.

Weeks later when I had to see a doctor about chest pain he questioned my life style and I repeated the story to him and the adrenalin sensation. Then he told me that a large pulse of adrenalin can cause heart damage and I was experiencing skipping of my heart because of that surge. Eventually other groups analyzed the event and Weber came back to my office weeks later and sat down to tell me very casually that the records of other institutions indicated a series of pulses for a period of a minute during the supernova. He never did apologize and the supernova became one of the additional events that soiled his record because he insisted we had seen the event when others believed the coincidences he was looking for actually occurred at another lab. This was the beginning of a personality change in Weber. He became very hostile and mean, lashing out at almost every comment made to him.

Finally, on November 30, 1990, the Chairman of the Physics Department called me in to his office and told me that due to a lack of funding I would be terminated at the end of December. During these last days Weber made it obvious that he resented the salary I was being paid compared to his. As an employee I received eighty thousand dollars a year, while as a retired professor he was limited to a twelve thousand dollar salary plus his retirement because of state retirement regulations. He lectured me about how I should be working long hours to justify my salary. The final attack was when he said to me: "The reason this program is failing is because of your inability to do the mathematical analysis necessary to help me out. If you were a PhD you would be able to give me the help I need."

In an attempt to prevent me from taking my retirement, which I was eligible for with twenty-five years of service, Weber talked to the Physics Department Facility Engineer Harry Kriemelmeyer to see if Harry could find a position for me. After a few days Weber came back to me and said: "Harry finds nothing impressive in your resumé. He says there is no way he could use you. Probably because of your age you are unemployable." It seemed to me to be an exercise in how to humiliate Darrell. Finally I talked to Nick Chant the Department Vice Chairman and told him that no matter what happened I wanted my termination to stay in effect and not be rescinded as Weber had tried to do. I walked away saying to myself: "Free at last, free at last, thank God almighty I am free at last." From the University I went on to accept a position at the Goddard Space Flight Center as a Facility Engineer in charge of a 40′ x 27′ cryogenic vacuum chamber. Three years later I joined the cryogenics group and used my talents to build cryostats for testing space flight hardware. After fifteen years at Goddard I left having formed my own company manufacturing indium wire cryogenic vacuum seals and eventually being bought out by Indium Corporation of America.

Weber continued through the 90's with no funding and, working alone, he maintained the detectors. On several occasions he called me at Goddard and asked if I would meet him at the Gravity Building. It was usually because he had forgotten some aspect of the maintenance or adjustments to the electronics. Each time we met he would review the entire program and work that we had done over the many years perhaps still trying to convince the world that all the data was correct. Once he called when my supervisor at Goddard was in my office and I told her: "I've got to go to the University because Weber needs me." She replied, "Do you have to go each time he calls?" I told her I felt that I owed him the courtesy since he had raised my salary over the years to the point that I was able to retire at the age of fifty-two. Once he even told me his reasoning was that the program would not last forever.

On one occasion, perhaps in January 2000, he called from Holy Cross hospital in Silver Springs and asked if I could come to pick him up and take him home. He had been at the Gravity Building to do the usual checks and when leaving the building he slipped on a wintry ice mixture in the parking lot and fell and broke his ankle. Then he crawled on his stomach dragging the injured leg about one hundred and fifty yards up the dirt and snow covered access road to reach the main maintenance road where a police officer happened by on his routine checks and found him. Of course I agreed to go.

When I entered the hospital room Weber was sitting on the edge of the bed in his still filthy dirty cloths he was wearing the day of the accident. No one was available to bring him a fresh change of clean cloths. He looked so old, frail, gray and tired lifting his cast to show me the injury. I will never forget the look in his eyes. He looked so sad that I thought for sure he was going to cry. In a soft voice breaking with emotion he said to me: "Darrell, you're my only friend."

Joe Weber died from cancer in a Pittsburgh hospital on September 30, 2000.

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March Session Reports:

The Author in Dialogue: Stone on Einstein and the Quantum

By Paul Cadden-Zimansky

A Douglas Stone's *Einstein and The Quantum: The Quest of the Valiant Swabian* was the featured book for this year's Author in Dialogue session. Stone began the session by introducing his book as an attempt to combat the misconception drawn from Einstein's opposition to aspects of quantum mechanics in his later years that he sought to impede the quantum revolution. In fact, Stone argues, not only did Einstein embrace the idea of reconstructing the rules of physics using quantum hypotheses, but he was in the vanguard of this revolution. Stone's talk focused mainly on the first decade of Einstein's work, highlighting not only his well-known use of the quantum to offer a heuristic explanation of the photoelectric effect in 1905, but his deployment of it to tackle the problem of the specific heats of solids in 1906-7. Stone contrasted Einstein's conviction in this era that the rules of electromagnetism needed to be rewritten to account for localized quanta that could exist in vacuum, with Planck's more equivocal views in this period on the necessity of a quantum hypothesis.

Massimiliano Badino, a philosopher of science at the University of Verona, argued that it was not only Einstein's role in the development of quantum theory that was widely misunderstood, but Planck's. Rather than being a "reluctant revolutionary" introducing the quantum hypothesis as "an act of desperation," Badino presented a case that the hypothesis had its roots in 19th century physical frameworks and was the byproduct of Planck pushing these frameworks to their limits. Badino reviewed the series of five papers Planck produced on black body radiation in the 1890s, and the shift in their approach to the problem using entropy after an objection by Boltzmann proved fatal to derivations in the first three of them. These papers began the construction of theoretical machinery that incorporated thermodynamics, kinetic theory, electromagnetism, and, finally, Boltzmann's approaches to statistical mechanics and

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The Brutality of Physics

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my class academically, but there was one boy, Dick, and one girl, Molly, who challenged me. We competed. In geometry class, for example, Mr. Shields always gave out the next day's assignment 10 or 15 minutes before the end of class. I always got the homework done in the minutes before class ended and I beat Dick consistently (Molly wasn't in the class); however, Dick beat me in Latin class and Molly beat me in the English class. Molly, Dick, and I competed for top standing. Nothing challenged my self-confidence in high school.

College was a repeat of high school. At my parents urging, I went to a small religious college in the Boston area. There were a few really good students at the college, but I had little competition. (One of my chemistry professors used my homework and my exams as his answer sheets to grade other students' work.) I could still harbor the belief that I was at the top of my class.

Things changed for me in graduate school at Johns Hopkins University.

The scene was an Electromagnetism class. Bob always sat in the front row. I sat a couple of rows behind him. This physics course was a challenge for me partly because the professor developed the subject completely through mathematics. Every class period the professor filled the blackboards three or four times with vector equations and uttered a word or two between every equation. It went like this: equations were separated by words such as, "and now we have", and "next we see". In conversations with Bob, I discovered that the equations spoke to him in ways that they did not speak to me. But most of all it was the homework assignments and the exams that forced me to recalibrate myself with respect to Bob. Just like I had done in the high school geometry class, Bob got the assigned work done very quickly; by contrast, it often took me a couple of hours to complete my homework. Bob almost always got a better grade on exams than I did. I couldn't believe it. I came to recognize that, as far as Bob was concerned, he was above me in the hierarchy. I had to adjust.

But there was worse to come.

My thesis advisor was a good man, but I was not intimidated by his intellectual powers. My dissertation was pretty ordinary. My objective was to finish my dissertation quickly, get my PhD, and get famous later.

After graduate school I had a postdoctoral position at Harvard. There I met people who were distinctly superior to me and I recognized that some of them could eat my lunch intellectually. I remember when I asked my boss a question and, whatever the question I asked, he always preceded in the same way. He would start with something very basic, something I thought I knew well. And then in just a couple of intellectual steps, steps that I followed easily, he would arrive at the doorway to my question. This troubled me because I knew and understood everything he did. "Why did I ask the question?" I would ask myself; "I knew everything he did." After witnessing this a couple of times I came to the conclusion that my boss, a famous scientist, really had a deeper understanding of the basics from which he started and because of this understanding he could proceed directly to the answer of my question.

As a post-doctoral fellow at Harvard, I stayed pretty close to the group I was part of; therefore, I did not get to know other Harvard faculty members very well. Getting to know Harvard physicists came later. Nonetheless, I was constantly recalibrating myself as I compared myself to other physicists I came to know in the department. I struggled with the results of this comparison; I placated myself by saying, "You have always been a hard worker and through hard work you can compete with them." Sadly, this line of thinking was also to change.

If a person chooses to become a physicist, he or she becomes part of a community populated with some – not all – very smart people. Just as Hans Bethe, then in his 30s, came to recognize that Feynman could do things he couldn't do, he recognized earlier in his career that Paul Dirac, Werner Heisenberg, and Enrico Fermi could also do things that were beyond his inherent abilities. Here was Bethe who had been the best in almost every group he had been a part of, but he was brought to the recognition about the time he finished his doctoral dissertation that he was not as good as Dirac, Heisenberg, or Fermi. In a somewhat similar fashion, Gino Segrè wrote in his book *Ordinary Geniuses: Max Delbrück, George Gamow and the Origins of Genomics and Big Bang Cosmology* (p.xvii) "Max and Geo are not like the three men who helped steer the quantum mechanics revolution." Even for very good physicists, physics can be brutally revealing.

Bethe did his dissertation under Arnold Sommerfeld at Munich. Sommerfeld was an outstanding physicist who trained some of the best physicists of the 20th century including Werner Heisenberg, Wolfgang Pauli, Peter Debye, Paul Ewald, Hans Bethe and others. Sommerfeld was never, ever simply "Arnold" to his students, he wasn't even Professor Sommerfeld: he was *Herr Geheimrat Herr Professor Sommerfeld*. Sommerfeld was good and he knew it: he "knew his standing in the profession and his status in the institutional framework"[1]; but he also knew that some of his students had abilities that surpassed his own Herr Professor abilities.

Physics is brutal because, even if you are famous, you can be pushed down by your own students.

After my postdoctoral fellowship at Harvard, I was talked into going back to my undergraduate college to expand a minor physics program into a full major program. I vowed I would stay no longer than five years and then move to a better institution. That meant that I would have to maintain my marketability and be wanted by other physics departments. I was confident that I could do this; I continued to harbor the belief that through hard work I could still compete even though I knew there were physicists who were smarter than me.

My time as a professor at my undergraduate college was not wasted. I got several grants for research, I started a research program, I got papers published, I had great students working into the nights, I got grants to bring equipment into the teaching laboratories. My teaching inspired new interests: interests in teaching itself and in the history of physics. The first course I taught was a

1 Silvan S. Schweber, *Nuclear Force: The Making of the Physicist Hans Bethe*, Harvard University Press 2012, p. 119

physics course designed to meet part of the science requirement for non-science students. I took an historical approach to physics thinking that non-science students might connect better with physics through history and understand the physics better.

At the small college, I was once again a big fish riding a wave of success, but the pond was small and there were no physicists or other standout scientists to measure myself against.

As my five-year time limit approached, I was invited to return to Harvard to participate in Harvard Project Physics (HPP), a project that was just getting underway. The objective of HPP was to design a high school physics course that would attract a large percentage of high school students. To accomplish this objective, the leaders of the project decided to take an historical and more humanistic approach in order to show not only how some of the great physics came to be, but also to showcase the physicists who brought the great physics into being. We would not only describe the path the great physicists took to arrive at their seminal accomplishments, but we would also pull aside the cadaver-like masks that often hide the faces of physicists in textbooks and reveal some of their smiles and frowns.

The textbook being developed consisted of six units; I ended up writing a good part of Unit One: "Concepts of Motion." In this unit, I had the privilege of writing about the works of Galileo and of Newton, the context of their work, about the men themselves, and about their great physics.

This was a tremendous learning experience: improving my writing and expanding my knowledge of the history of physics. I would hand in a draft of a chapter, thinking my draft could have come off the pen of Herman Melville; however, the pages of my draft copy would come back to me covered with questions, comments, and clarifications - all written with colored felt-tip pens: red, green, blue. At first I was devastated, but then I realized that my colleagues were teaching me a great lesson: what is clear to a writer may not be clear to a reader. And writing about Galileo and Newton in that most interesting 17th century, was, for me, addictive.

Since my long past days at Madison Avenue School I had been attracted to good writing. I would, occasionally, show a sentence or paragraph to one of my classmates and say "Read this." My classmate would read it and then look at me and say, "Well?" I would respond, "It's written so nicely. Isn't it beautiful?" He or she would look at me with a puzzled look, grunt, and say, "I guess so." So here I was at Harvard, 18 years after Madison Avenue School, writing sentences, paragraphs, and pages about physics and its history and trying to make the words go together beautifully. I left Harvard University and Harvard Project Physics a different person.

Physicists glorify their Nobel Laureates and like many physics graduate students, I fantasized about winning the big prize. For those physicists who work in a small department, have no colleagues who are altering the frontier of physics, and remain somewhat aloof from the larger physics community, those fantasies may never die. There are stories about physicists (some of whom I have known), some pretty well known and some *not* so well known, who sit by the phone every October waiting for the call from Stockholm and it is also well known that particular physicists became embittered and die embittered because the Swedish Academy passed them by. By contrast, for those physicists who become active in the profession, go to professional meetings, converse with colleagues far and wide, Nobel dreams can die quickly. Self-calibration can place a physicist in the hierarchy and that hierarchical position can clearly reveal to a physicist that a Nobel Prize is not likely to come their way. Physics is always revealing. Physics can be brutal.

Physicists are generally respectful to those above them in the hierarchy, but they can be inconsiderate and even rude to those below them. Bethe mellowed as he grew older, but as a young man, he acknowledged that he was arrogant.

In 1934, Victor "Viki" Weisskopf, who became a prominent physicist, was working as Wolfgang Pauli's assistant. Pauli asked Weisskopf to do a particular calculation. Weisskopf happened to encounter Bethe and asked him how long he thought it would take to do the calculation. The 28-year-old Bethe responded: "Me it would take three days. You it will take three weeks."[2] Weisskopf was below Bethe in the hierarchy.

2 Personal communication between author and Hans Bethe.

When Weisskopf was in Copenhagen, he was on the upside. In a 1965 interview Weisskopf said:

> I was in right away due to my old friends who were already there….It is very difficult to get into Copenhagen: I have seen cruel things happen if you come and cannot get through the "Guard." Bohr was surrounded by five or six of his disciples, who were a very arrogant crowd. If you were not accepted by them you would have a very difficult time with him.[3]

Weisskopf was below some of Bohr's "disciples" in the hierarchy, but he had friends to help him.

As a young physicist Bethe saw himself above Göttingen's Max Born (who later won the Nobel Prize). Bethe wrote a paper that superseded an earlier paper by Born. Born wrote him a letter of appreciation for his work. In response, Bethe wrote a letter in which he chided Born for missing an obvious connection in his earlier paper. Later Born described Bethe's letter as the kind of letter "an angry teacher would write to a feeble student, not that of a young scholar to a much older one."[4] Born was below Bethe in the hierarchy.

I have always been a hard worker and for some years after my formal education ended, I tacitly assumed my work ethic together with my abilities would allow me to compete at a high level in the world of physics. However, as I got to know more and more of my colleagues, both near and far, I came to realize that some of my physicist-friends could do in "three days" what would take me "three weeks." But I shrugged that off because I was willing to work the three weeks. What I could not shrug off was the recognition that I knew physicists who could do in two hours what 100 of me could never ever do. As was written in the business section of the *New York Times* (June 22, 2014, p. 5): "You could not replace Einstein with 5, 10, or 100 physicists and get the same results. Einstein was Einstein."

As my ongoing recalibration of

3 Interview of Victor F. Weisskopf by Thomas S. Kuhn and John L. Heilbron on July 10, 1965, Niels Bohr Library & Archives, American Institute of Physics.

4 Max Born, *My Life: Recollections of a Nobel Laureate*, Charles Scribner's and Sons, 1975, p. 234

myself continued, I also came to what for me was a disturbing recognition: while I knew I could do physics research that would lead to publishable results, I wondered: what difference would it make? I knew physicists whose research and papers published in physics journals modified the frontier of the discipline. I also knew physicists whose research brought them nowhere near the frontier of physics and their papers were of interest to very few. Getting papers into physics journals is very different from altering the subject of physics itself: getting physics manuscripts published is easy; altering the frontier of physics is hard.

Could my research put a little scratch on the frontier? I came to believe that my chances were slim. If slim, why publish? Why add another paper to the already overloaded literature of physics? A comment of Pauli's kept going through my thoughts: when asked to read a manuscript of another physicist, Pauli made the damning remark, "It's not even wrong." Physics is brutal.

Physics is brutal because the evidence that drives physics is so unambiguous, so uncompromising. Research that produces a ground-breaking result can be unambiguously linked to the particular physicist who did the research and that physicist will get the accolades.

John S. Rigden on deck, 2014

Every physicist knows those physicists whose research results will command the attention of the entire physics community; likewise, every physicist knows physicists whose published papers are "not even wrong."

The physics community does not dish out its admiration casually: those who deserve admiration get it; those who do not deserve admiration don't get it. Physics is brutal.

March Session Reports:
History of Soviet Physics

By Paul Cadden-Zimansky

The session on the history of Soviet physics at the 2018 March Meeting brought together four diverse views on a topic that spans almost a century. The first talk, by University of British Columbia historian of physics and session organizer Alexei Kojevnikov, was aimed at squarely at the predominantly condensed matter audience of the Meeting. Taking its title "More is Different" from the famous 1972 essay by Philip Anderson, Kojevnikov sought to trace back the early embrace of solid state physics in the Soviet era and some of the non-reductionist views that Anderson advocated in the U.S. only many decades later. Highlighting the role of crystallographer Abram Joffe, who was proficient at piecing together funding for scientific work at the beginning of the Soviet era from various state agencies, Kojevnikov explained how Joffe's invention of the concept of a physicist-engineer not wedded to pursuing "pure science" helped provide a framework where individuals in the 1920s and 30s such as Vladimir Fock could pursue the "fundamental importance of approximation methods" and Yakov Frenkel could pioneer the understanding of quasiparticles. One connection of note between Soviet ideology and science was that quasiparticles were originally termed "collectivized particles."

Moving into the post-W.W. II era, Samoil Bilenky brought his personal remembrances of working many decades at the Dubna Joint Institute for Nuclear Research. Started in 1948 with the construction of the world's largest proton accelerator, its existence was only revealed to foreigners in 1954 with visits from such noted physicists as Wolfgang Panofsky and Owen Chamberlain. Bilenky described a geographically isolated, but thriving environment for research that enjoyed robust government support, provided access

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the surpising experimental deviations from the earlier Wien radiation theory, all necessary ingredients that guided Planck to the quantum hypothesis.

University of Minnesota historian of science Michel Janssen took up the relevance of a quantum contribution by Einstein emphasized in the later part of Stone's book: Einstein's suggestion to Max Born that one could make a consistent theory relying on Schroedinger's wavefunctions if they were interpreted probabilistically. Janssen noted that Born's formulation of this interpretation in the 1920s, which still bears his name, was only posited in the limited case of the coefficients used in a superposition of waves being regarded as probability amplitudes. Much more general formulations of quantum mechanics as a tool for predicting the outcomes of experiments probabilistically were to be found, not in the interpretation of wave mechanics, Janssen argued, but in the coincident construction by Heisenberg, Born, and Pascual Jordan, of matrix mechanics.

Daniela Monaldi of York University used the final presentation to look at another late contribution of Einstein's to quantum theory, his fostering of Satyendra Nath Bose's method of deriving Planck's radiation formula into an early understanding of quantum statistics. Monaldi pointed out that the present-day conception of quantum indistinguishability contains within it several distinct notions, including statistical interdependence, loss of individuality, and symmetry under exchange. While Einstein's work on Bose's method was a critical step, Monaldi argued that it was not until after World War II, with the establishment of particle physics as a distinct subfield, that the contemporary synthesis of indistinguishability occurred.

Presenters at the session on Einstein and the Quantum. From left to right.: Michel Janssen, A. Douglas Stone, Massimiliano Badino, Daniela Monaldi

March Session Reports:

Pais Prize Section: Peter Galison

Peter Galison, the 2018 winner of the Pais Prize, devoted the first part of his talk, "Filming and Writing Physics," to recapitulating the trajectory of his career. He described how his work engaged the back-and-forth between high abstraction and concrete circumstance, exploring specific developments of instruments, experiments, images, and calculations. He outlined how he did so not only through writing, but also through filming – and he showed several examples during the course of his talk. His aim, as he described it, was to attempt to capture "how physics sits in the world"

Galison was followed by David Gross of the University of California, Santa Barbara, co-winner of the 2004 Nobel Prize in physics. Gross devoted his talk to "Einstein's Quest for a Unified Theory" which, he said, had inspired Gross's own desire to become a theoretical physicist. "Physicists are an ambitious lot," Gross said, "but Einstein was the most ambitious of all." Gross paid most of his attention to Einstein's fascination with Kaluza-Klein theory to carry out these ambitions. Einstein went wrong, Gross said, by ignoring developments in nuclear and particle physics. "But his intuition that gravity must be unified with other forces of nature has to be right," Gross concluded. "It is a task to be pursued with care and courage.".

Cathryn Carson, University of California, Berkeley spoke of Galison's students as "spokes radiating out from Peter." She described herself as one who, like him, sits at the intersection of physics and history, though she is also involved with data sciences as well as history. She thoughtfully described the boundary issues that arise between such disciplines, and in particular the "peace

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to the latest journals from around the world, and hosted weekly visits from Landau group theorists. While Dubna is now most well known for its production and analysis of transuranic elements, Bilenky emphasized its role, starting with the arrival of Bruno Pentacorvo in the 1950s, in establishing the theory of neutrino oscillations, subsequently confirmed experimentally and the subject of the 2015 Nobel prize.

Historian Asif Siddiqi of Fordham University took up another branch of large-scale science in the post-W.W. II era: the Soviet space program. Siddiqi emphasized the fundamental paradox of the program – its successes provided some of the best advertising for the preeminence of the Soviet system, but the desire for secrecy often prevented its scientists from claiming the priority that would allow recognition of their excellence. As an example, Sergei Korolov, a lead designer of the Sputnik satellites, was unknown in the West until his death. Four case studies examining this paradox were presented, including Sergei Nikolaevich Vernov's low-orbit radiation measurements that predated Van Allen's discovery of the Earth's radiation belt, and how the centralization of the space program in the mid-1960s led to a stifling of research and fewer successes until it recovered under the leadership of R. Z. Sagdeev in the 1980s.

The session ended with a presentation by Gerson Sher, who worked for many years at the National Science Foundation coordinating scientific exchange between the U.S. and U.S.S.R. Sher spoke of both his own experiences and those of scientists he's interviewed for a forthcoming book, *The Great Experiment: A Critical History of Scientific Cooperation Between the United States and the Former Soviet Union*, emphasizing the positive scientific collaborations, often impeded by bureaucracy or misunderstanding on both sides. In one anecdote Sher recalled how a small grant from his office to Vladimir Braginsky provided critical for the success of LIGO and the recent observation of gravitational waves.

Top left: Alexei Kojevnikov, University of British Columbia; Top right: Samoil Bilenky, Dubna, JINR; Bottom left: Asif Siddiqi, Fordham University; Bottom right: Gerson S. Sher, National Science Foundation

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Upper left: Peter Galison, of Harvard University, 2018 Pais Prize Awardee; Upper right: David Gross, University of California, Santa Barbara; Bottom left: Cathryn Carson, University of California, Berkeley; Bottom right: Theodore M. Porter, UCLA

treaty" that the German philosopher Wilhelm Windelband had proposed in which the natural sciences – whose model was physics – devote themselves to nomothetic explanations, or ones providing a generalized understanding, while social sciences provide idiographic descriptions of unique cases. It was an intellectual division of labor, she said, that came with an intellectual price, for it polarized the disciplines against each other, and she provided some examples.

Ted Porter, of the UCLA Department of History at UCL, spoke about "(Physics) Statistics and the Ideals of Human Reason." His talk considered episodes from the history of physics involving statistical tools and methods – the benefits and dangers. Statistics, Porter said, was "state-istics" from the beginning, "an engaged, often bureaucratic form of social science." Data indeed drives science, Porter said. "But people (with their data and their instruments) drive science." Understanding science as a human pursuit, he concluded, is a better way to include the public into science than to stress its law-like behavior.

Filmmaker Jon Else spoke about "Science Films in America." He described his field as the megaphone for the work of scholars like Galison and other historians of science. In many respects it's a deal with the devil, Else said. One has to search for vehicles in which to deliver the "precious cargo" of historians of science to citizens, but one can obscure the science and the history if one is not careful. "I don't make the rules by which I have to play in primetime television," he said, "but it's fun trying to navigate those rules." Science films are seldom purely about science, with "Powers of 10" one of the few. Most are films about technology. A few are films about the politics and policy of science, a famous example being "An Inconvenient Truth." At the bottom in terms of viewership are films about the process of science, with "Particle Fever" being an example. He concluded by stressing the need for historians to help keep the "feet to the fire" of the filmmakers.

March Session Reports:

Staged Reading of the Play Silent Sky

by Brian Schwartz
Brooklyn College and the Graduate Center of CUNY

From left: Jennifer Parsons (Williamina), Leslie Stevens (Annie), Jennifer Cannon (Henrietta) and Eric Wentz (Peter) in International City Theatre's Silent Sky. Photo by Tracey Roman

The Forum on the History of Physics sponsored a staged reading of the play ***Silent Sky*** at the March meeting of the APS in Los Angeles. The play is based on the true story of 19th-century astronomer Henrietta Swan Leavitt as she experiences a woman's place in society during a time of immense scientific discoveries, when women's ideas were dismissed until men claimed credit for them. When Henrietta Leavitt began work at the Harvard Observatory in the early 1900s, she was not even allowed to touch a telescope or express an original idea. The overqualified Henrietta ends up identifying more than 2,400 variable stars. She is credited with the discovery of the relation between the luminosity and the period of Cepheid variable stars. After Leavitt's death, Edwin Hubble used the luminosity–period relation for Cepheids, together with spectral shifts to determine that the universe is expanding.

The staged reading was performed by actors associated with the International City Theatre (ICT), Long Beach CA, http://ictlongbeach.org/. The March meeting audience was fortunate in that ICT had done a full production of the play ***Silent Sky*** in the summer of 2017. Thus the staged reading was even more theatrical in that many of the lines and emotions of the play had been committed to memory. The playwright of ***Silent Sky*** is Lauren Gunderson and originally from Atlanta. She received her BA in English/Creative Writing at Emory University, and her MFA in Dramatic Writing at NYU Tisch. At Emory she started her playwriting career as part of a science project in a class taught by physicist Sidney Perkowitz. Lauren is credited by *American Theatre Magazine* with being the most produced playwright in America in 2017.

Book Review:

Isaac Newton and Natural Philosophy

by Robert P. Crease
Stony Brook University

Richard Westfall's lengthy N*ever at Rest: A Biography of Isaac Newton* was published about four decades ago but is still authoritative, while James Gleick's succinct Isaac Newton is only about 15 years old. Rob Iliffe's excellent *Priest of Nature: The Religious Worlds of Isaac Newton* appeared just last year. What is there to be gained from another biographical treatment of Isaac Newton? This book is for those who are interested not so much in the biographical but the intellectual details. What specific puzzles did Newton face, how did he address them, and how did they shape his overall scientific oeuvre? Guicciardini presents in a short volume what until now had been treated mainly by specialists in the history of mathematics or physics.

In the introduction Guicciardini retraces the story of Newton's evolving relation in the light of things like the alchemical manuscripts, his fascination with Egyptian and Chaldean myths, and the dimensions of the Temple of Jerusalem, highlighting the fascinating role of John Maynard Keynes. It is tempting for Newton's modern fans to turn our eyes away from these interests of his. But Guicciardini says, in effect, "No, wait! You miss how it all happened!" His approach, he tells us, is to show how Newton wasn't spinning ideas out of his head but confronting problems that nagged his contemporaries. This approach makes Newton

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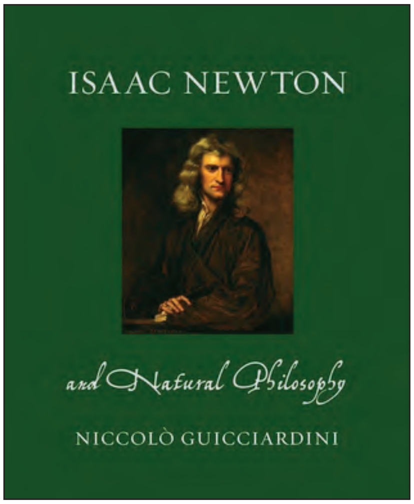

Isaac Newton and Natural Philosophy, Niccolò Guicciardini, 268 pp. Reaktion Books, London, 2018. ISBN 978-1-78023-906-4

harder to understand. "[W]e should not think that the 'traditional' Newton, the mathematician and physicist, is less remote to us than Newton the alchemist and theologian" (21). But the reward is a better grounded picture of Newton, and history of science.

Guicciardini reveals, for instance, the extent to which Newton's interest in mathematics was driven by practical concerns. "Too often," he writes, "we look at Newton as a natural philosopher whose thought flew high above the needs of mankind, and tend to underestimate how seriously he took the practical needs of the world of the so-called 'mathematical practitioners' such as gaugers and surveyors" (47). Such practitioners posed the grand challenges for ambitious young scholars of the day, and Newton tackled them by introducing novel mathematical conceptions of the infinite and the infinitesimal. Guicciardini is good at conveying the complex story of the origins of calculus; for each of its founders it was something a little different. "[W]e should conceive the discovery of calculus as a long process, at least spanning the period beginning with Pierre de Fermat and Kepler and culminating with the work of Euler" (49). Understanding this allows one to understand better Newton's anxieties when it came to publishing his discoveries.

Guicciardini's view-from-the-ground also helps to develop a greater appreciation of Newton's optical and telescopic work, and what made these so controversial in Newton's lifetime. It also helps to understand what was at stake in the contemporary controversies over Newton's mechanics, and why it appeared so implausible to many followers of Descartes. The more remote, in Guicciardini's hands, that Newton may appear to us as a mathematician and physicist, the better we are able to appreciate Newton's reasoning, and hence expertise as a scientist. Guicciardini's approach is especially effective when it comes to Newton's alchemical writings. He warns against the '*Da Vinci Code* effect," a Janus-face image of Newton's thinking, in which there is the scientist on the one hand and the practitioner of esoteric knowledge on the other. Newton was simply trying to make the best sense he could of things using all the intellectual tools at his disposal.

I also found refreshing remarks such as that "the Principia is written terribly" (150). Fortunately, Guicciardini is there to read and condense it for us, and put it in context. It was enlightening, too, to be told that Newton's "involvement in philosophical issues was defensive in nature," and did not stem from a spontaneous interest in them but arose from "his need to defend his mathematical and experimental edifice from criticisms," above all from Leibniz (179). "We get the impression that philosophy was for Newton a necessity rather than a vocation, a defensive strategy rather than a chosen line of research" (180). Guicciardini's book is a model for how to approach the work of an innovator like Newton not only historically but also philosophically.

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