Robert H. Siemann

Proceedings

Symposium in Memory
of Robert H. Siemann

July 7th, 2009

SLAC National Accelerator Laboratory

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Preface

The Symposium in Memory of Robert H. Siemann was held on July 7th, 2009 to honor the life and achievements of one of the pioneers of accelerator physics. Speakers and participants from around the world came together at SLAC to celebrate his contributions to science and to the scientific community.

Speakers from each of the chapters of Bob's life were asked to provide a broad retrospective of the man and his career. Students provided brief perspectives of specific aspects of his work, and of his role as a mentor. The Siemann family attended and offered personal reminiscences. A redwood tree and memorial plaque were dedicated on the SLAC main Green in his honor. Many who attended the event felt they had learned something they did not know about their longtime colleague.

In the pages that follow, the prepared remarks and slides of speakers have been collected. The richness of the contributions and breadth of topics covered are a reflection of the richness and breadth of Bob's life. The event was video recorded, and may be viewed at the SLAC library or online through the SLAC website.

Persis Drell's sponsorship and Tor Raubenheimer's assistance in arranging the event are both gratefully acknowledged. I am grateful for the help of the Symposium Committee in selecting the program and speakers: Marty Blume, Bob Byer, Alex Chao, Mark Hogan, Gerry Jackson, Chan Joshi, and Maury Tigner. I am especially grateful to the local organizers: Joan Valine and Naomi Nagahashi for flawless logistics, InfoMedia Solutions Team for conference Website application design and support, Nick Arias and Stephanie Santo for help with computer support and the Guestbook, Rob Reape and Communications for A/V support, Ken Soong and Edgar Peralta for photography, Joel England, Johnny Ng, Ian Blumenfeld, and Neil Kirby for setup and preparations, Dieter Walz and Jim Spencer for a fine choice of wines at the reception, and to Sharon West, Terry Anderson, and Robert Noble for help in preparing the Proceedings.

Eric Colby
Symposium Chair
August 2009

Robert Siemann Symposium Welcome

7/7/09 -- Persis Drell

Good morning. On behalf of Stanford University and SLAC National Accelerator Laboratory, I would like to welcome you all to the Siemann Symposium in memory of the life of Robert Siemann and his many contributions to accelerator physics and the scientific community. I extend a special welcome to Bob's family; his wife, Hannah, daughters Francesca, Gabriella and Liese and son Evan. You can look forward to speakers who will share reflections from many chapters of Bob's life offering their insights on his scientific and personal legacy. In the late afternoon there will be a dedication of a tree planted in Bob's memory here on the SLAC site, followed by a reception at the Stanford Guest House patio.

Throughout today's talks you will hear two strongly intertwined themes that frame Bob's legacy. You will hear Bob honored for his distinguished work in the area of ~~advanced~~ accelerator science and you will hear about his legacy of teaching and mentoring young scientists.

Bob Siemann completed his bachelor's degree in physics at Brown University in 1964, and his doctorate in experimental particle physics at Cornell University in 1969. His first association with SLAC was as a research associate from 1969 to 1972. After a year as a physicist at Brookhaven National Laboratory, he joined the faculty of Cornell, where he advanced from assistant professor to full professor. During that period his interests shifted from particle physics to accelerator physics and he made major contributions to the theory and modeling of the beam-beam interaction and beam instabilities, as well as to beam diagnostics. In 1991, already by now a recognized leader in the field of accelerator physics, Burt Richter persuaded Bob to come to SLAC as a Professor. With Bob's arrival, a new Stanford academic program in advanced accelerator physics dawned. A few years after joining Stanford, Bob established a department at SLAC specifically devoted to long-term accelerator research with an emphasis on educating graduate students.

Bob's professional accomplishments in the field of accelerator physics are widely acknowledged and documented as he was named APS and AAAS Fellow and he participated in or led numerous high level advisory committees. He was widely known as a champion of Accelerator Physics as a scientific discipline. He was instrumental in creating the widely respected peer-reviewed journal for accelerator physics, the ***Physical Review Special Topics – Accelerators and Beams***, and served as the first editor of that journal for nine years. This

publication attracted students and promoted accelerator physics as its own discipline

Bob's dedication as a mentor and advisor is best memorialized in the outstanding students he trained, many of whom you will hear from today. Bob enjoyed working with his students more than anything else, and despite Bob's rigorous and demanding style, his former students universally laud him.

Bob's students appreciated his high level of intellectual honesty and his basic desire to place good science ahead of convenience. Several students have noted that those qualities would lead him to reject 'popular' scientific analysis or conventional solutions, leading to greater depth in understanding of the fundamental accelerator science. Bob taught his students how to ask the right questions, and encouraged them both to create their own ideas and to carry them through to fruition.

Bob's students are a wonderful legacy. More than anyone he championed the importance of educating young accelerator scientists and thanks to Bob and his efforts Stanford University is one of the premier institutions in the country for educating accelerator physicists, probably rivaled only by Cornell, Bob's former home.

The future of the field of particle physics depends critically on innovations in accelerator science, and many other fields, fusion, nuclear physics and photon science, have come to rely on accelerators as their primary tools for discovery. Above all, the future of these fields depends on new generations of accelerator scientists, inspired by mentors like Bob, to make the essential discoveries that we need to advance the frontiers of knowledge.

It is incredibly fitting that in honor of Bob's scientific achievements and dedication to teaching, that today Stanford University ^{is announcing} ~~has announced~~ the establishment of two graduate fellowships in Bob Siemann's name. Bob's wife, Hannah Siemann, has very generously given the foundational gift to endow the fellowships to support outstanding physics PhD students who are conducting research at SLAC or in the fields of high energy, particle, or accelerator physics at Stanford.

Dean Richard Saller of Stanford's School of Humanities and Sciences has matched Mrs. Siemann's gift with funds from the William and Flora Hewlett Foundation. Dean Saller stated: "We are all grateful for Mrs. Siemann's generosity in creating these fellowships, which allows us to support talented physics students who – like Robert Siemann – will shape the future of their field"

Siemann's unquestioned commitment to science and the University coupled with his dedication and love of educating young physics students inspired the founding of these fellowships in Bob's memory. I would like to ask all of you, his colleagues and students, who valued him as a friend and as a mentor, who

enjoyed his company and who now miss him, to strongly consider contributing to this fellowship fund. I personally cannot think of a more fitting tribute to Bob. Bob understood the intimate connection between teaching at advanced levels and research. Accelerator science at SLAC and nationally bears the stamp of Bob's accomplishments and of his character. We are proud to have called him our colleague. With graduate fellowships endowed in his honor, young students will continue to benefit from his legacy.

I hope you enjoy today's Symposium, the science and the personal reflections that you will be hearing.

Thank you and once again...welcome!

Experimental Particle Physics:
Bob Siemann and the DECO Experiment

Roger Erickson
SLAC Linear Accelerator Center

Robert H. Siemann Symposium
July 7, 2009

Thirty five years ago last week, I arrived in Ithaca, hoping to find interesting summer employment before starting graduate school in the fall. I found John DeWire, who was the deputy director of the Laboratory of Nuclear Studies, sitting in his office in Newman Lab that July morning. He looked up, squinting, from his desk and said “Go talk to Bob Siemann. He’s the young guy just down the hall.”

I walked down the hall as directed and looked into the designated office. Here was a big guy hunched over his desk, writing intensely on a pad of paper. He gripped a pen tightly in his fist as if he were trying to engrave his notes into the desktop.

I introduced myself and said I wanted to work on a high energy physics experiment. Bob dropped the pen, smiled, and launched into a description of a new experiment. This experiment would measure the distributions of all the charged pions and kaons produced by deep-inelastic scattering of electrons off a proton target. The experiment would use a streamer chamber to photograph the tracks of the particles as they curved through a strong magnetic field. The streamer chamber was being built in Hamburg and would be arriving in a few weeks.

Bob explained that we at Cornell had to provide a set of detectors that could unambiguously distinguish which of several charged particles in each event was the electron and provide a trigger pulse to the streamer chamber whenever an electron was scattered at a large angle. To do this, we would need a scintillator hodoscope immediately behind the streamer chamber, followed by a detector that could distinguish electrons from pions.

“Will we use sodium iodide crystals?” I asked. “Too expensive”, he said, dismissing the idea. “Lead glass blocks?” “Nope. We can’t afford that either.” “So what else is there?” I asked. He grinned and said, “Look at this.”

In the room across the hall were two rectangular stainless steel boxes, each a little smaller than a kitchen refrigerator. There were fifteen holes on one face of each box to accommodate portholes for photomultiplier tubes. Bob explained that we would fill these boxes with a saturated solution of lead perchlorate in water. This is a crystal clear

liquid with nearly three times the density of plain water and an index of refraction of 1.6, about the same as flint glass. Electrons will shower in this stuff and produce flashes of Cerenkov light, which can be seen by the phototubes. This was starting to sound interesting.

Bob then said that he wanted the inside walls of the boxes painted with bright white titanium dioxide paint to ensure we would collect as much light as possible. So before lunch of my first day at Cornell, I was painting the interiors of these Cerenkov shower counter tanks.

A few weeks later, the tanks had been moved to the Wilson Synchrotron Lab with their bright white interiors and glass portholes and phototubes installed and were ready to be filled. Lead perchlorate is nasty stuff: it's highly toxic, and it's a strong oxidizing agent. The solution was delivered in small cylindrical barrels, each holding five gallons, and each barrel was covered with warning labels about its toxicity and reactivity. Twenty barrels of this solution were needed to fill each of the two tank detectors.

I looked at the first empty tank and the step ladder and the twenty little barrels lined up on the floor, and realized I had a problem. The filler tube on the top of each tank was nearly six feet above the floor, and each little barrel weighed about 120 pounds. There was no way I would be able to carry these barrels up the ladder.

Bob came to the rescue with a clear plastic hose and a gadget with spinning rollers that could squeeze the hose and force liquid to move through it. But most importantly, he gave me a quick lesson on how to operate the overhead bridge crane and how to use nylon lifting straps in creative ways. By the end of the day, the first tank was filled.

The first tests of the first Cerenkov tank with the real electron beam were disappointing. We couldn't reliably distinguish the signal of a single electron shower from the combined signal of two or more other particles, even if they were widely separated in the tank. When we removed the optical shroud cover from the back and peeked into one of the portholes, we immediately realized what had happened. Instead of the crystal clear liquid, all we could see were millions of tiny white flecks suspended in a milky white fog.

With fresh resolve, we cleaned the tanks, abandoned the paint idea, and installed shiny stainless steel baffles in the tanks, dividing each tank into fifteen optically isolated compartments, each viewed by one phototube. This arrangement gave us a much cleaner trigger signal, and it allowed us to remove the tubes closest to the beam from the trigger logic, which further reduced the rate of spurious triggers and allowed us to collect more of the interesting higher Q^2 events.

During the summer while all this was happening, we started assembling the scintillation counters, using salvaged materials Bob had found in the basement of Newman Lab. When the time came to attach the plastic light pipes and phototubes to the rectangular panels of plastic scintillator, Bob insisted the joints should be glued to ensure optimal

light transmission. So he led me down to the mysterious subbasement, where there were gallon cans of Hysol epoxy and a triple-beam balance, just like I had used in high school, except this one was all sticky with spilled epoxy.

Under Bob's watchful eye, I carefully weighed out the proper amounts of resin and hardener into a large paper cup. Then, following his instructions, I stirred it very briskly to thoroughly mix the ingredients. With some dismay, I saw that the resulting syrupy mix was nearly opaque. It was full of tiny air bubbles.

"Can we use this stuff like this?" I asked. "Won't the bubbles absorb the light?" He grimaced; "What do you think?" Then with a grin, he said, "Watch this." He set the cup down on an aluminum plate on the table and positioned a glass bell jar over it. The bell jar was connected by a thick flexible hose to a mechanical vacuum pump on the floor. He flicked on a toggle switch, and the pump starting chugging away. Within a few seconds I could see the epoxy start to boil vigorously in the cup. Then quite suddenly, it stopped boiling. Bob turned off the pump, opened the vent valve, and removed the bell jar. The epoxy in the cup was sparkling clear, without a trace of any bubbles.

Bob grinned broadly and said, "Isn't that the cat's meow?" That was the first of many times I heard him use that phrase to express his delight whenever something worked out particularly well.

Bob's familiarity with the epoxy corner in the basement made me think he must have been up to something else involving epoxy. Back in the lab upstairs, I noticed various small saddle-shaped coils of copper wire, some encased in blocks of epoxy. He had been trying to make corrector magnets to fit around the beam pipe of the synchrotron, hoping to improve the beam quality. This was an omen. His interests were already beginning to shift toward the accelerator.

By the time the streamer chamber and magnet arrived from Hamburg, we had an electron beam and a set of trigger counters ready in the L0 experimental hall. The experiment became known as DECO, a contraction of DESY and Cornell, and it produced a series of new results on quark fragmentation functions, charge retention by forward hadrons, and inclusive rho meson production.

By the end of 1977, the CESR storage ring project was getting started, and DECO had to be dismantled to make way for the CLEO detector. This marked the end of the era of fixed target experiments at the Wilson Synchrotron Lab. By all measures, DECO was a great success. Bob was promoted to Associate Professor, and eventually, I finished a Ph.D. thesis. It was a wonderful time, for which I will always be grateful.

**Bob's Evolution from Particle Physics to
Accelerator Physics**

Richard Talman

Cornell Laboratory of Elementary-Particle Physics

July 7, 2009, Bob Siemann Symposium

A POLARIZED PHOTON BEAM PRODUCED BY COHERENT PAIR PRODUCTION IN ORIENTED GRAPHITE*

R. L. EISELE, D. J. SHERDEN, R. H. SIEMANN[†], C. K. SINCLAIR

Stanford Linear Accelerator Center, Stanford University, Stanford, Calif. 94305, U.S.A.

D. J. QUINN, J. P. RUTHERFOORD, and M. A. SHUPE

Tufts University, Medford, Mass. 02155, U.S.A.

Received 18 April 1973

Attenuation by coherent pair production in highly oriented, compression annealed, pyrolytic graphite has been used to polarize a 16-GeV bremsstrahlung beam. The polarizer consists of 61 cm of graphite crystals, whose reciprocal lattice vectors are oriented at 10.5 mrad to the normal to the beam direction, and can be rotated by 90° about the beam line to rotate the

plane of polarization. A functionally identical assembly of length 30.5 cm was used as an analyzer to measure the polarization of the beam with the SLAC pair spectrometer. The beam produced intensities greater than 4×10^8 equivalent quanta per beam pulse and had a measured polarization of 0.255 ± 0.020 .

Study of charged-pseudoscalar-meson photoproduction from hydrogen and deuterium with 16-GeV linearly polarized photons

D. J. Quinn,^{*} J. P. Rutherford,[†] and M. A. Shupe[‡]

Tufts University, Medford, Massachusetts 02155

D. J. Sherden, R. H. Siemann,[§] and C. K. Sinclair

Stanford Linear Accelerator Center, Stanford University, Stanford, California 94305

(Received 23 April 1979)

The asymmetries in forward π - N , π - Δ , and K^+ - $(\Lambda + \Sigma)$ photoproduction have been measured with a 16-GeV linearly polarized beam. The experimental method and the procedures for extracting cross sections and asymmetries from the data are discussed in detail. Information on the energy and momentum-transfer dependence of cross sections for natural- and unnatural-parity exchange, interference between exchanges of opposite G parity, and vector-meson dominance is obtained and discussed.

Fig. 1. A measured coherent bremsstrahlung spectrum from a diamond radiator. See ref. 4 for an explanation of the curves.

1. Introduction

It is a measure of success to be recognized unambiguously by first name alone, especially because this honor is bestowed democratically. With a name like Barak or Kobe or Pief it is not so surprising that no ambiguity arises. For that matter, how many people with the name “Persis” do you know. But with a name like “Bob” it is harder to be recognizable by first name alone. When one is reminiscing about old times at Cornell mention of “Bob” gets the response, “Do you mean Bob Wilson or Bob Siemann”?. Certainly both Bobs are warmly remembered at Cornell.

Bob received his PhD from Cornell in 1969 measuring wide angle bremsstrahlung with Karl Berkelman. After a stint as Research Associate at SLAC, Bob returned to Cornell in 1973 as assistant (later full) Professor. During the SLAC period Bob was beginning to evolve from elementary particle physicist to accelerator physicist. Perhaps surprisingly (since I was at Cornell) this is the period during which I worked most closely with him, and it is the period to which this remembrance will be limited.

This was a time when particle physicists were still optimistic that sufficiently accurate and detailed measurement of meson scattering and production processes could unlock the secrets of the strong nuclear interaction. Vector mesons had recently been discovered and vector meson dominance was being investigated. More significantly, the quark model had just been introduced, and the organization of mesons and baryons into SU3 families had just been understood.

I will restrict myself to a solitary thread through Bob's research during this period—Polarization Dependence of Meson Photoproduction. Furthermore I will refer to just the two papers I showed initially. The first paper describes the development of a polarized photon beam at SLAC. It contains no elementary particle results. The second paper, written some years later with very nearly the same authors, summarizes important results obtained with the polarized beam.

Since I wish to concentrate on the instrumentation paper, let me dispense with the second paper right away. You will have to take my word for it that it was an excellent and significant paper reviewing results that could not have been obtained without the beam described in the first paper. Significant comparisons of the data with theoretical models were also given.

I will discuss only the beam line itself. Of course many scientists were involved in the series of advances to be described here. I assume you are more interested in the general flow of the field of research and in the people involved, than in the technical details. I will just drop the names of the leading characters (as it happens, four of them would later become lab directors) along with just enough description of their contributions to support a continuous, somewhat anecdotal, narrative. An analogy to American football may be apt. Moving the ball up the field was the work of many. Bob Siemann carried the ball over the goal line.

As well as honoring Bob I hope also to make physicists jealous of an era in which amazing and varied advances could be made on a time scale of months, not years.

2. Preliminary Answers to Some Natural Questions

Before starting this narrative, let me anticipate some questions that can serve as themes for my talk:

- Why did Bob leave Cornell?
- Why did SLAC recruit Bob?
- Why did Bob convert from particle physics to accelerator physics?
- What did Bob accomplish?

The first question is easy. Bob thought he could accomplish more at SLAC than at Cornell.

Why SLAC recruited him I can only conjecture, but the accomplishments I intend to describe in this talk surely were important or, I venture to guess, even dominant.

Concerning Bob's evolution in accelerator physics, in spite of my title, in preparing this talk, I have come to realize that, in fact, Bob never really changed in any important sense. Explaining this will be one of the themes of my talk.

Finally the question "What did Bob accomplish?" I will give my own answer to this even though it carries me way beyond the time interval I have been assigned. My answer is that he accomplished two and a half things. To protect myself from the charges of being both too stingy and too flippant, I have to explain my scoring system. I am using the word "accomplish" in the strictest possible sense. Rephrasing the question it becomes "What things would never have happened if Bob had not accomplished them?"

One of these things is obvious—the on-line Phys. Rev. journal. For this Bob gets one point on my accomplishment scale. I leave it to others to say more about this accomplishment. Next, harsh grader that I am, for leading the development of novel acceleration techniques, Bob gets half a point. I am *certain* Bob would have agreed with this grade, especially after I have explained my grading system. This will be another theme of my talk.

Finally, the actual subject of my talk is justifying the remaining one point that brings Bob's score up to two and a half.

3. Early Polarized Photon Measurements

GeV-scale photons are produced by bremsstrahlung from material targets. In 1956, Uberall showed that, by using an appropriately-oriented single crystal as the target, the radiation would have quite high polarization, especially close to a “resonance edge” in the energy spectrum. I showed this spectrum at the start of the talk. By 1968, workers at Frascati, especially Diambrini, had demonstrated the feasibility of this method.

By 1970, Richter and Schwitters (I could just as well have said Burt and Roy) and others (including Bob) had measured polarization dependence of π^+ photoproduction using a beam polarized by the Uberall process. Their method of measurement used a subtraction procedure relying on the edge in the energy spectrum mentioned earlier. Back then the data for this technique was referred to as an “excitation curve”. Nowadays it would be referred to as an “inclusive” measurement. This technique was adequate for the early measurements, but one feature was found to be disadvantageous. The edge near which the high polarization exists lies well below the upper end point of the energy spectrum. Photons above the edge gave background counts that tended to swamp the foreground signal.

By that time rho meson photoproduction from various elements had been measured at Cornell by Mistry, Silverman, me, and others. In 1970, Diambrini spent the year at Cornell, bringing with him the goniometer needed to produce polarized photons from an oriented diamond target. Soon we had measured the polarization dependence of rho photoproduction and found it to be maximal—the rho (vector) mesons “remember” the photon polarization and the plane defined by the pions into which the rho decays remembers the rho polarization.

Diambrini also called our attention to a paper by Cabibbo and others, which described an alternative method for polarizing a photon beam. (Cabibbo has the even greater honor of being identifiable by just his initial—he is the Cabibbo of CKM matrix fame.) For the Cabibbo process the photon beam passes through a thick, properly-oriented single crystal. Photons polarized in one transverse plane are preferentially absorbed by pair production. As with visible light passing through a polaroid film, the emerging radiation is polarized.

Relying on differential absorption, there is an important trade-off between intensity and degree of polarization. But the essential advantage is that the high energy end of the spectrum is polarized in this process.

During 1970 I was on sabbatic leave in the SLAC group of Dave Ritson (who was himself on leave somewhere else). Bjorn Wiik, Dave Gustavson, and I contemplated the possibility of one-upping the Richter group by using the Cabibbo process to overcome the problems imposed by the Uberall process. For inclusive measurements like this, the low degree of polarization disadvantage is completely out-weighed by the advantage of being able to polarize the high energy end of the spectrum.

But we were not expert in crystallography. We knew that Roy Schwitters had developed an elegant code for calculating the Uberall production process being used in the Richter beam line. From QED, we understood the close connection between bremsstrahlung and electron pair production, and assumed that Roy's code could be adapted for our purposes. We approached Roy about using his code (without, I now confess, explaining how Gustavson intended to modify it) and Roy graciously approved.

By that time we were confident enough to submit a proposal to use the rho photoproduction process, mentioned earlier, as an analyzer to confirm the applicability of the Cabibbo process and to measure the degree of polarization achievable. McDaniel, the Cornell lab director, had assigned us a few weeks of running time at the Cornell Synchrotron, some months in the future. Such proposals were far less formal in those days than they are today, but it was still a serious commitment. As yet we had no appropriate crystalline target, but we assumed that we could quickly acquire and use a long single crystal of silicon.

Soon, to our horror, with the help of the newly modified code, we realized that the process was not going to work with silicon. The Debye temperature is too low. For that matter, the material proposed by Cabibbo, namely copper, would have failed for the same reason.

There is nothing like the threat of hanging to concentrate a person's mind. After a few feverish days in the Stanford library I came to realize that carbon was the only practical material for the Cabibbo process. All we needed was a foot-long diamond. But we couldn't see any way of getting one.

Then I considered graphite and, to my amazement, found that carbon exists in crystalline graphite form. Nowadays this form is known as "graphene". Scarcely a week goes by without a seminar on its remarkable properties. At the time, based on its fabrication method, this form of carbon was referred to as "pyrolytic graphite".

A trip to meet Arthur Moore, of Union Carbide, in Cleveland, established that he had some graphene samples. They were shaped like inch-square counter-top tiles. They were a few millimeters thick, which was considerably thicker than the millimeter or so beam width. With care these tiles could be lined up to form the equivalent of a foot-long single crystal.

Unfortunately the tiles were too expensive for us to purchase. Fortunately, through Moore, Union Carbide graciously allowed us to borrow the material. (Later, when Bob Siemann built the serious polarized beam I will get to next, he purchased pyrolytic graphite from the same source—probably including some of the very same tiles.)

With the crisis averted (and hardly anyone even aware it had ever occurred) we completed planning for the test of the Cabibbo process at Cornell. As I already explained, the azimuthal plane defined by the two pions into which a rho meson decays can be used to infer the polarization of the photon which produced the rho meson. This process can therefore be used as a polarization analyzer.

The Cornell measurements confirmed the (appropriately-updated) Cabibbo theory. Though the measurements were not very accurate, they were sufficiently promising to justify proceeding to the rest of the story.

4. Development of the Polarized Beam

Everything up to this point could be called *research*. So far Bob has been an important actor, but not yet the star. To exploit this research for a usable accelerator facility required *development*.

Even more than being a futuristic thinker, Bob was an accomplished builder. While the rest of us went on to other things, he did the serious work needed for full implementation of the Cabibbo-polarized beam line. This work is described in the paper I am concentrating on.

If one insists that Bob evolved from particle physicist to accelerator physicist, then this paper may define when the transition occurred. If so then this would be Bob's first accelerator physics paper.

Many physics and engineering tasks needed to be performed: construction, alignment, cooling, reliable control system, safety assurance, installation of the entire apparatus in a magnetic field to sweep the produced electrons and positrons out of the beam, and so on.

Before the beam line could be used for practical experiments, it was still necessary to determine the optimal crystal orientation, and to accurately measure the resultant degree of polarization. As mentioned already the crystal quality was fairly poor, and the polarization quite low. So empirical measurement was required.

As well as describing the polarization process, the original Cabibbo paper also described how the polarization could be measured, using a second, approximately identical, crystal. For this it was also necessary to measure the attenuation in both polarizer and analyzer in a subsidiary experiment using an electron-positron pair spectrometer.

All this is described in the paper.

As I already mentioned, the results obtained in this beam line are reviewed in the later paper. At the risk of offending other contributors, I think it would be fair to call this the “Bob Siemann beam line”. The subsequent polarization measurements *could not have been made* without this beam line.

Preliminary results using this beam line were first reported in 1973 at the Bonn Symposium on Electron and Photon Interactions at High Energies, and I had the good fortune to be the rapporteur for that session. Corny thought it was, and wishing to explain the method, I asked the Bonn lecture room apparatus attendant whether he had the sort of oven-rack-like wire grid that one uses for lecture demonstration of microwave polarization. The process is the same—one polarization is absorbed, the other is not. The attendant proudly showed me, and allowed me to show, what he said was the very apparatus that Heinrich Hertz had used to demonstrate the polarization of electromagnetic waves generated by sparks a century earlier.

5. Bob's Skill and Style

The apparatus paper is a real *tour de force* of experimental physics. I have already outlined its content. This paper would be ideal for intensive study in a course on experimental physics. You will have to take my word for its excellence in technical content; but I will say a few words about its style.

I cannot be sure what fraction of the paper was in fact written by Bob. But, from my reading the whole paper “sounds like” Bob. To show what I mean let me read out a couple of paragraphs:

“The mounting of the carts was the only serious design flaw of the crystal assembly. Wear in the pivot sockets and at the points where the spring plungers exerted pressure caused the behavior of the pivots to deviate from that of the true pivots. Fortunately the problem was discovered before installation in the beam, and the pivot sockets were replaced with anodized sockets and the pressure points were reinforced with tool steel. Although not a permanent solution, it was an adequate temporary one. A captive pivot is contemplated as a permanent solution.”

Rather than suppressing this episode, which many scientists would be tempted to do, after describing the problem in succinct detail, Bob gives both a temporary and a permanent fix; all in five sentences.

Here is another example:

“During the experiment several of the non-critical functions of the controller failed due to radiation damage or water damage (which occurred as a result of the bursting on several occasions of radiation-damaged water hoses on the sweeping magnet). At several points during the experiment the symmetry measurement was repeated. These measurements showed that the polarization remained constant to within ± 0.008 , indicating no evidence of deterioration in the polarizing power of the crystals.”

I defy anyone to pack more information (even including a numerical error estimate) into a paragraph that would not be out of place a short story.

For Bob paragraphs like these amounted to telling the truth, the whole truth, and nothing but the truth.

6. Completing Bob's Accomplishment Score

To conclude let me complete my tallying up of Bob's accomplishment score.

Success of his polarized beam could only be claimed after it had completed a significant measurement that could not be made any other way. In this he was gloriously successful. So, in my scoring system, Bob gets a full point for this effort. With the point for the online journal this brings his score to 2.

Much of Bob's subsequent career has been devoted to the far more ambitious task of overcoming the MeV per meter barrier that prevents one from obtaining truly high energy electrons.

Many people would say that Bob lived long enough to have succeeded in this. But Bob would surely disagree. When he worked on apparatus his definition of success certainly included the requirement that the apparatus make possible physical measurements that would not otherwise be possible. But the advanced acceleration schemes he worked on have not yet met that test. I am certain, that Bob would claim, at most, that the advances have been promising. So I can only award him half a point for this accomplishment.

Finally, what about Bob's evolution from particle physics to accelerator physics? I hope I have convinced you that no such evolution ever needed to occur. From beginning to end Bob was an simply an experimental physicist *par excellence*. What he enjoyed most, and was best at, was building apparatus that would contribute to the advance of science.

Bob as a Mentor at Cornell (and later at Fermilab)

Gerald P. Jackson

President
Hbar Technologies, LLC
gjackson@hbartech.com

Cornell Timeline with Bob

- Autumn of 1981 – Bob was assigned as my faculty advisor upon starting my graduate studies at Cornell.
- First year studies in 1981/82 – Suffered through 510 Lab, the only required graduate course for physics (that was also Bob's pride and joy) and a literal right of passage.
- Summer of 1982 – I asked Bob if he would be my thesis advisor in accelerator physics. Worked for Bob that summer and started in earnest the following summer.
- April 1986 – I departed Cornell after finishing my thesis research, while waiting for Bob to change each page of my draft thesis into red-colored modern art.

Post-Cornell Timeline with Bob

- ~1988 – Bob comes to Fermilab to work on Tevatron Collider emittance growth, beam instrumentation, and to help expand and finish my thesis.
- June 21 to July 2, 1993 – Bob, Jacob Flanz of Mass. Gen. Hospital, and I teach the first USPAS laboratory class at MIT Bates. I serve on Bob's program committee.
- 1994 to 1995 – Bob is program chair of the 1995 PAC, and I am on his committee. Bob initiated electronic paper submission and proceedings publishing.
- 2000 to 2001 – Bob helps me (and PAC2001 and APS DPB) with gaining IEEE electronic publishing permission.
- 2002 to 2003 – Bob is conference chair of the 2003 PAC, and I am on his program committee.

Accelerator Physics Training at Cornell

- When it came to accelerator physics education, Bob emphasized a formula:
 - Translation of physics theory into simulations
 - Implementation of existing beam diagnostics as simulation outputs
 - If existing beam diagnostics were inadequate, innovate new detectors and/or processing hardware/software
 - Use of simulations as predictions for accelerator experiments
 - If simulations were too slow to run enough particles or turns or terms, improve the simulations through the development of better algorithms, computer hardware, or acquire access to better computers.
 - Publish scientifically significant papers : comparison of theory with experiments using simulations as the bridge

Computer simulation is a tool for studying beam stability which complements theoretical and experimental work, and in the case of future storage rings it can play the role of experimental observations.

Beam Instability Simulations

- My first summer working for Bob involved learning to run his single beam stability simulation program. He applied his program to CESR, PEP, and PETRA.
- Bob had a graduate student Bob Meller at this time. Because of their effect on operations, the CESR staff was diligently working on instability problems. In typical fashion, the entire laboratory was involved.

- **References**

- R.H. Siemann, “Computer Simulation Studies of Single Beam Stability”, IEEE Trans. Nucl. Sci., Vol. NS-30, No. 4, 2373 (1983).
- D. Rice, et. al., “Single Bunch Current Dependent Phenomena in CESR”, IEEE Trans. Nucl. Sci., Vol. NS-28, No. 3, 2446 (1981).
- J. Seeman, et. al., “A Single Beam Multibunch Instability at CESR”, IEEE Trans. Nucl. Sci., Vol. NS-28, No. 3, 2561 (1981)

Continued Instabilities

- This work at Cornell, and the expertise derived from it, greatly influenced the careers of those involved, including Bob's. It even influenced later work at Fermilab.

References

- L.E. Sakazaki, et. al., "Anomalous," Nonlinearly Current-Dependent Damping in CESR", IEEE Trans. Nucl. Sci., Vol. NS-32, No. 5, 2353 (1985).
- R. Siemann, "Theory, Simulation and Observation of Beams in Storage Rings", USPAS Lecture (1988).
- G. Jackson, "Measurement of the Resistive Wall Instability in the Fermilab Main Ring", Proc. U.S. Part. Acc. Conf., 1755 (1991).

Early Beam-Beam Studies

- When Bob and I started discussing the topic of my thesis research in 1983, John Seeman had already made the key experimental observations that set the direction of future research (see the next slide). Theoretical studies of electron-positron limitations had already started.
- These studies, along with Bob's instability simulation program, served as the starting point for my thesis.
- **References**
 - R.E. Meller and R.H. Siemann, "Coherent Normal Modes of Colliding Beams", IEEE Trans. Nucl. Sci., Vol. NS-28, No. 3, 2431 (1981).
 - J. Seeman, et. al., "Observation of the Beam-Beam Limit in CESR", IEEE Trans. Nucl. Sci., Vol. NS-30, No. 4, 2033 (1983).
 - S.G. Peggs, "Beam-Beam Synchrotron Resonances", IEEE Trans. Nucl. Sci., Vol. NS-30, No. 4, 2457 (1983).

Experimental Beam-Beam Evidence

Bob's Simulation Expertise

- Bob's instability simulation employed many innovative numerical techniques that were incorporated into my beam-beam simulations:
 - Digital filters to simulate beam detectors
 - Random number generators for a variety of probability distributions
 - Time to frequency domain transforms
 - Hermite polynomial expansions to simulate irregular bunch shapes
- He encouraged me to significantly improve the error function calculations drawn from Dick Talman's work.
- Understanding that correctly simulating beam size with random number excitations to simulate synchrotron radiation, we:
 - Used number theory to build faster uniform random number generators
 - Built a wire-wrapped PDP-11 memory-plane board that was a dedicated random number generator.

Beam Detector Improvements

- Bob believed that part of the job of an accelerator physicist was to design and build the instrumentation needed to understand the physics at work in an accelerator. He supported my need for faster (higher counting rate) luminosity monitors in the CLEO detector.

- References

- G.P. Jackson and S.W. Herb,
“Description of a High Rate Luminosity Monitor Installed at CESR”, IEEE Trans. Nucl. Sci., Vol. NS-32, No. 5, 1925 (1985).

Predictions and Measurements

- The most stable period of CESR operations, with the least amount of competition for accelerator access, was the shift from 4am to noon. Though it was virtually impossible to arrive at the lab before Bob or leave after him, he eventually let me handle these shifts solo.

Note that the simulation was performed 3 different ways, with symplectic mapping algorithms provided by David Rubin.

G.P. Jackson and R.H. Siemann, "A Computer Simulation Study of e-e- Storage Ring Performance as a Function of Sextupole Distribution", IEEE Trans. Nucl. Sci., Vol. NS-32, No. 5, 2541 (1985).

More Comparisons

Vertical size growth with
beam current.

Flip-Flop Effect

G.P. Jackson and R.H. Siemann,
“Comparison between the
Simulated and Measured
Luminosity Performance of
CESR”, Proc. U.S. Part. Acc.
Conf., 1011 (1987).

Better Beam Height Measurements

- In addition to the CLEO and CUSB high energy physics experiments at the south and north interaction regions, there was also the CHES synchrotron light facility operating in the CESR tunnel.
- Bob launched us into a measurement of vertical beam height using very hard x-rays measured by crystal collimators in CHES. Rotating these collimators, not only could the beam size be determined, but also beam divergence. This was an independent measurement of the CESR beta function and the vertical emittance.

- **References**

- G. Jackson, R. Siemann, and D. Mills, "Vertical Emittance Measurements of the CESR Electron Beam using Synchrotron Radiation", Proc. 12th International Conf. on High Energy Accelerators, Fermilab (1983), pg. 217.

Thinking in the Frequency Domain

- Probably the single most important technique I learned from Bob was the ability to think simultaneously in the time and frequency domain.
- At one point we were worried about synchrotron coupling via beam-beam interactions that were off-center longitudinally in the “hour-glass” vertical mini-beta.
- Filling CESR with a single electron and positron bunch of equal intensity, and plugging well surveyed beam position buttons on either side of the mini-beta into a spectrum analyzer, measurements of the frequency modulation yielded highly precise crossing-point positions with respect to the mini-beta quadrupoles.
- In the end we did not have to move the CESR RF cavities to correct the collision point.

Bob's Sabbatical at Fermilab

- Bob was very excited by all of the opportunities to learn new aspects of accelerator physics in hadron machines. We worked on instrumentation and machine studies diagnosing instabilities in the Main Ring and Tevatron.
- At the same time, Bob was involved with the SSC. While some of this time led to fulfilling diversions, such as the E778 experiment, much of that duty involved distasteful political incidents that caused Bob to recoil from similar positions in the future and to focus even more narrowly on students and accelerator physics.
- The “second-class citizenship” of Fermilab accelerator physicists also influenced the manner in which Bob later moved to Stanford/SLAC.

Schottky Receivers

- One of the novel differences between hadrons and electrons is the lack of oscillation damping. This allows one to measure Schottky betatron and synchrotron oscillation signals.

- References

- D. Martin, et. al., “A Schottky Receiver for Non-Perturbative Tune Monitoring in the Tevatron”, Proc. U.S. Part. Acc. Conf., 1483 (1989).
- D. Martin, et. al., “A Resonant Beam Detector for Tevatron Tune Monitoring”, Proc. U.S. Part. Acc. Conf., 1486 (1989).
- P.J. Chou, et. al., “A Transverse Tune Monitor for the Fermilab Main Ring”, Proc. U.S. Part. Acc. Conf., 2479 (1995).

Tevatron Transverse Emittance Growth

- The measured betatron signals were far too large on the Schottky detectors to be due to Schottky signals. They were in fact driven coherent oscillations.
- Bob's role in this work was central to improving Tevatron operations to today's 300x design luminosity.

- G. Jackson, et. al., "Luminosity Lifetime in the Tevatron", Proc. of the European Part. Acc. Conf., Rome, 556 (1988).
- G. Jackson and S. Mane, "Studies and Calculations of Transverse Emittance Growth in Proton Storage Rings", Nucl. Instr. and Methods in Phys. Research A276, 8 (1989).

Finding the Dominant Source of Tevatron Emittance Growth

Figure 1: Horizontal emittance as a function of time while the antiproton abort kicker is on and off.

Tevatron Longitudinal Damping

- Proton (top) and antiproton (bottom) synchrotron tune spectra during collisions are shown to the right.
- When only protons are in the Tevatron, there was an longitudinal instability.
- The loop noise in the damping system was found to drive emittance growth if left on during stores.

- References

- Q.A. Kerns, et. al., “Longitudinal Damping in the Tevatron Collider”, Proc. U.S. Part. Acc. Conf., 1882 (1989).

Main Ring Bunch Spreader

- Since the Main Ring had a longitudinal instability which was difficult to diagnose, a bunch spreader was installed to grow the longitudinal emittance of the beam.
- References
 - G. Jackson and T. Ieiri, “Stimulated Longitudinal Emittance Growth in the Main Ring”, Proc. U.S. Part. Acc. Conf., 863 (1989).

Real Time Bunch Length Monitor

- In order to tune the bunch spreader and to diagnose longitudinal instabilities, Bob suggested new processing electronics for existing longitudinal beam detectors based on the frequency domain.
- Until this point, a diode/resistor/capacitor circuit was used to measure the peak voltage, which the computer normalized with DC beam current, in order to synthesize a signal that was proportional to the bunch length.
- Bob suggested monitoring a RF harmonic of the beam signal, taking the logarithm of that signal with an Analog Devices chip, and then taking the square root with another Analog Devices chip. The entire chain had a 10kHz bandwidth.

Next Generation Bunch Length Monitor

- Taking Bob's advice, we took this method to the next step and subtracted the logarithm of the third RF harmonic of the beam signal from the logarithm of the beam signal at the RF frequency itself using heterodyne electronics to match the changing RF frequency up the Main Ring acceleration ramp.
- These two matched arms of the electronics provided a 10kHz bandwidth bunch length signal to the control system that did not need to be normalized or calibrated.
- **References**
 - T. Ieiri, R. Gonzalez, and G. Jackson, "A Main Ring Bunch Length Monitor by Detecting Two Frequency Components of the Beam", Proc. 7th Symposium on Acc. Sci. and Tech., Osaka, Japan (1989), pg. 367.

SSC Nonlinear Beam Dynamics

- By my memory, Alex Chao knows a lot more about the beginnings of E778 than I do. My version of the story is that there was a review down at the SSC which Bob was chairing, and the SSC staff were presenting calculations of beam lifetime using simulations and employing terms such as “smear” and “shmear”. At some point the question arose as to whether anyone really knew what they were doing: hence the formation of the E778 experiment at Fermilab.

NONLINEAR DYNAMICS EXPERIMENT IN THE TEVATRON

N.Merminga, D. Edwards, D. Finley, R. Gerig, N. Gelfand, M. Harrison, R. Johnson, M. Syphers
Fermi National Accelerator Laboratory, Batavia, IL 60510

R. Meller, R. Siemann, R. Talman
Newman Laboratory of Nuclear Studies, Cornell University, Ithaca, NY 14853 U.S.A.
P. Morton

Stanford Linear Accelerator Center, Stanford, CA 94305 U.S.A.

A. Chao, T. Chen, D. Johnson, S. Peggs, J. Peterson, C. Saltmarsh, L. Schachinger
SSC Central Design Group, Berkeley, CA 94720 U.S.A.

Sample E778 Result

- The experiment took place, though in a very unusual fashion ...

- **References**

- N. Merminga, et. al., “Nonlinear Dynamics Experiment in the Tevatron”, Proc. U.S. Part. Acc. Conf., 1429 (1989).
- A.W. Chao, et. al., “Experimental Investigation of Nonlinear Dynamics in the Fermilab Tevatron”, Phys. Rev. Lett. P.2752 (12/12/88).

Figure 2: Smear, tuneshift, measured and calculated dynamic aperture plotted as functions of the product of the sextupole strength, the particle's amplitude and the beta function at the observation point. Data points are measured values; curves are predictions.

Finishing My Thesis

- The work Bob and I did on Schottky detectors and emittance growth diagnosis led to dramatic improvements in our visualization and interpretation of our e⁺e⁻ beam-beam simulation results.
- During this entire period Bob steadily trashed my thesis drafts, and I diligently rewrote them. Finally, on the fourth iteration, the thesis (and his sabbatical) were done.

Bob's Return to Cornell

- Bob returned to Cornell to continue working with students:
 - CBN 88-1: “A Simulation Study of Radiation Treatment with a Quadrupole Beam-Beam Kick” by R. Siemann and S. Krishnagopal
 - CBN 88-2: “Linear Beam-Beam Force” by R. Siemann, S. Krishnagopal
 - CBN 88-8: “Synchrotron Radiation in Simulations” by S. Krishnagopal and R. Siemann
 - CBN 89-2: “Synchrotron Radiation in Simulations – II” by R. Siemann
 - CBN 89-3: “Simulation of Round Beams” by S. Krishnagopal and R. Siemann
 - CBN 89-4: “Simulations of Electron-Positron Storage Rings” by R. Siemann
 - CBN 89-6: “Cavity Dissipation Dependence on Beam Pipe Radius” by S. Krishnagopal and R. Siemann
 - CBN 90-2: “A Comparison of Flat Beams With Round” by S. Krishnagopal and R. Siemann
 - CBN 91-6: “Resonances in the Beam-Beam Interaction Due to a Finite Crossing Angle” by D. Sagan, R. Siemann and S. Krishnagopal (EPAC90)
 - CBN 91-18: “Nearly Equal beta* at CLEO” by P. Bagley , M. Billing, S. Krishnagopal, D. Rubin, R. Siemann and J. Welch (PAC91)

Concluding Remarks

- Bob started out as my Cornell student advisor, became my thesis advisor, transformed into a colleague, and over the years remained a role model and friend.
- When I heard that Bob had passed, I found myself profoundly effected.
- I grew up in a farming community, and my parents ran the local gas station and auto repair shop. I was the first person in my family to finish college. Though my parents and I were always close, they really did not understand my life as a physicist.
- I now realize that when I strived to make someone proud of my career, that person was Bob. I will always value his role in my life, and be thankful to the Siemann family for sharing him with me.

From SLAC Post-doc to SLAC Professor

Robert H. Siemann memorial Symposium

Burton Richter
7/8/2009

Post-doc Days

Bob Siemann's association with SLAC began 40 years ago when he accepted a 3-year post-doc job with my group and was with us from 1969 – 1972. The linac was running. My group, Group C, was doing experiments mostly with photon beams using the spectrometers in End Station A while continuing our efforts to get funding for the colliding beam program. Dick Taylor and the MIT group were doing inelastic electron scattering. Martin Perl was looking for long lived heavy leptons. Joe Ballam and David Leith were getting the bubble chamber program together.

It is hard to remember after all this time exactly why I offered Bob a job. He was clearly at least as smart as he was tall, and he was very tall indeed. His PhD thesis may have has something to do with it because it surely reminded me of my first big experiment as a post-doc a little more than a decade earlier. Bob's thesis was on wide-angle bremsstrahlung. My experiment had been on wide-angle pair production. We both were testing the limits of QED, and perhaps I suspected a kindred spirit.

The SPEAR storage ring construction project would begin in the fall of 1970, but when Bob arrived we were not yet sure of funding and physics with it would in any case be too far in the future for a post-doc. Bob had not yet shifted his interests to accelerator physics so did not work on SPEAR design or beam stability issues, but he could not avoid hearing all the talk about it going on in the group and maybe that piqued his interest in accelerators.

Instead of colliding beams, he became part of many experiments with the SLAC spectrometer facilities, mainly the 20 GeV spectrometer. He had an enormously productive three years at SLAC. There are 14 papers with his name on the author list; some with my group, some Richard Taylor's group, and some with the Tufts group. They included photoproduction experiments including some with polarized targets and beams (Including Roy Schwitters' PhD thesis experiment), electroproduction, deep inelastic scattering, and technology development.

I am particularly fond of one of these experiments; a search for photoproduction of free quarks. The proposal itself had only 2 names on it – Robert Siemann and Burton Richter. It was based on a magnetic trick – set a magnetic spectrometer for a momentum beyond the end point of the bremsstrahlung spectrum of the beam and no integral charged particle could get through. However, a particle with $1/3$ charge would get through if it had a momentum of only one third of the setting. To the surprise of some (Dick Taylor in particular), the program committee was intrigued and the experiment was approved for a month of running time. To no one's very great surprise we did not find any sign of free fractional charged particles - but we had a good time trying.

A Long Interlude

Bob left SLAC in 1972 and did not return for 18 years, 17 of which were at Cornell. There he did switch his focus from experiments with accelerators to the accelerators themselves. That story has been well told by Dick Talman earlier in this symposium.

I did keep track of Bob's developing career as I did with most of the Group-C graduate students and post-docs, and some of the others young ones that came through SLAC that I thought particularly interesting (Persis Drell was one of those). I wrote many letters for them over the years.

However, the only thing I did to advance Bob's career at Cornell was indirect. Boyce MacDaniel asked me to tell the NSF why they should fund the CESR machine when the much higher energy PEP would be available. I told the NSF that for studies at the lower energies, a machine optimized for those energies would be much more productive than one optimized for higher energy and then dialed down. CESR was built, the upsilon was discovered at FNAL, and CESR became a physics engine until superseded by the B-factories of KEK and SLAC.

Bob had switched his interests to accelerator physics, and his work contributed to the continuous improvement of the Cornell machine. His increasing prominence in accelerator physics meant that I would see him at conferences and found his work innovative as well as interesting. I particularly liked his papers on the beam – beam interaction.

Return to SLAC

Here at SLAC, we had finished the SLC, the SLAC Linear Collider, in 1987. It was the first of a new kind of machine. We had foreseen some of its birth problems, but had not clearly foreseen them all. We needed to increase the luminosity of the machine, and one of the arguments going on among the machine experts was about round beams versus flat beams. Bob had written papers about performance in colliding-beam storage rings with round beams versus flat beams which I found particularly interesting.

In colliding beams, the luminosity depends on the particle density in the collision region which is proportional to one over the beam-area; the smaller the beams the better. But, there is a countervailing problem. What is called the tune shift in storage rings or the disruption parameter in linear colliders depends on

the magnetic field at the surface of the beam. The higher the field the larger the problem and the field depends on the reciprocal of the perimeter of the beam. In a round beam as the area goes down increasing the potential luminosity, the perimeter goes down as well increasing the tune shift or disruption parameter and placing a limit on how small the beam can be. For any beam area, however, I can increase the perimeter by flattening the beam thereby reducing the tune shift.

In going over some old papers for this symposium I found a copy of Gerry Jackson's PhD thesis on work he had done with Bob on the Beam-Beam interaction. SLAC offered Gerry a job, which he turned down for a Wilson Fellowship at FNAL. Maybe I thought if we could not get the student, perhaps we could get the professor. It would have been best, of course, if we could have gotten both.

In any event, we went after Bob. I am not sure if it was the accelerator faculty pushing me or me pushing the faculty. Some informal soundings showed that he was interested in us as well. I can only speculate about why we were attractive at the stage of his career that Bob had reached in the late 1980s. I guess that the reason lay with the directions in which the NSF was willing to move. They were the financial supporters of Cornell, but did not seem interested in advanced accelerator work while Bob was very interested. The attraction of SLAC surely lay in the resources of a larger lab and the directions in which SLAC was moving.

Though it was easy to get Bob Siemann interested in coming back to SLAC, it was not so easy to close the deal and bring him here. The problem was SLAC's academic organization which was much different from the situation at Cornell. Among the DOE labs, SLAC was then and is still the most academically integrated with its contracting university. We have two faculties, high-energy physics and photon science, and the director of the laboratory functions as a kind

of dean as well as managing the budget and the scientific affairs of the lab. Some of you have had the experience know that dealing with faculty affairs is often more difficult than dealing with the DOE.

When I became director in 1984 the high-energy physics faculty organization was different from what it is now. When the laboratory was first put together by Pief Panofsky, his thinking was that the academic part of the laboratory would just cover high-energy physics. Experimenters and theorists would be regular Stanford University faculty while the accelerator physicists would be research-professors. There was a distinction in title at SLAC, though there was no distinction in practice. But in a university titles do count. Bob was a regular faculty member at Cornell and he made it perfectly clear that if he would not be that at SLAC, he simply wouldn't come. That left the dean, me, with a problem. I had to get SLAC's charter changed because without that charter change, it would not be possible to appoint him as a regular faculty member.

Every department in every university has an area of responsibility and Stanford is no different from the rest. The French Department, for example, can appoint professors to teach French language, French history, and French literature, but it can not appoint a professor to teach physics. The Physics department can appoint people in condensed matter, plasma physics high-energy physics, but not in French. As originally set up, SLAC could only appoint professors in experimental and theoretical physics, but not in accelerator physics. To get Bob we had to get Stanford to amend our academic charter to make accelerator physics a part of our academic mission.

That took a bit of work. A paper was written for the Stanford Provost, who had to be convinced, that laid out the academic side of accelerator physics. Fortunately, there were precedents already at Cornell, the University of Maryland, and U.C.L.A. where accelerator physics was part of the academic curriculum. That helped a lot. The Provost took our paper to his advisory board

and all came out well. SLAC's charter was amended, accelerator physics was part of SLAC's academic mission, and we now could offer Bob the job that he wanted and could bring him out here. Making the sale, if I can put it that way, was a permanent change. Ever since we brought Bob in, all of our appointments in accelerator physics have been regular academic appointments in both the high-energy physics and the photon science faculties.

Bob brought the family out during and set to work. You will hear about the things that he did in other talks and so I am not going to go into them. I will only say that his appointment that made considerable difference to the laboratory. When he came here, Bob started to work on the linear collider's problems and was a big help in curing some, thus getting its performance improved. Eventually it performed to the point where it could produce physics comparable in importance to that produced by the LEP facility at CERN, thanks both to improved luminosity and to polarized beams.

Bob continued to do some work on colliding beams with linear colliders. He and I produced a design for a 5 TeV center-of-mass machine with a reasonable beam-strahlung parameter. I can't find my copy of our notes and hope they turn up in Bob's papers. Who knows, we might still build it.

Bob's interests evolved toward advanced accelerator systems such as laser and plasma acceleration which became his principle focus. He started a program on wake-field acceleration that has worked very well. His contributions to the development of the kinds of new techniques of acceleration may very well make much higher energy machines more affordable, something badly needed for the future. He was the driver of the effort to set up a facility here to test out advanced accelerator ideas that is finally going to be realized too, unfortunately, not in time for him to use it.

Bringing Bob here was good for the laboratory as well as the accelerator science field because of the larger scope that he had for his advanced accelerator work. That larger scope came from the DOE's willingness to spend more money looking to the future than the NSF was willing to spend to develop the accelerator technologies to enable that future. It helped us recruit Bob and recruiting Bob was part of our good fortune.

Bob Siemann: SLAC Professor and SLC Physicist

Tor Raubenheimer
Robert Siemann Symposium
July 7, 2009

Topics

- * Professor
- * SLAC Linear Collider
 - Positron task force 1991 - 1993
 - Flat beams 1992 - 1993
 - DR studies and sawtooth 1992 - 1995
 - Beam loading and bunch compression
 - Vacuum chamber upgrade
 - Diagnostic pulse and DFS 1995 – 1996
- * ATF Damping ring 1992

Students at SLAC

- * Bob always had a large number of students and a larger number of advisees
- * Bob was a great professor
- * Students were first priority
- * He drove people very hard but he was fair and very supportive
- * Bob brought both a rigor and an enthusiasm for the academic side of Accelerator Physics to SLAC – he was a great experimentalist and a great teacher
- * Students while at SLAC
 - Chris Barnes
 - Ian Blumenfeld
 - Ben Cowan
 - Robert Holtzapple
 - Neil Kirby
 - Chris McGuinness
 - Caolionn O'Connell
 - Boris Podobedov
 - David Pritzkau
 - Bruce Rohrbough
 - Chris Sears
 - Walt Zacherl

1991 SLC Team

* Bob came to SLAC in early 1991, in part, to work on the SLC which was struggling with luminosity

* He believed that the SLC was a prototype for the next collider

SLAC July 7th, 2009

BAKER, Al	4849	424-7091	STEGE, Bob	3915	11-614
BURROWS, Kathy	3888	424-7715	TOGE, Nobu	2631	424-7339
CHAN, Kathy	3829		WALKER, Nick	3677	424-7335
CHAO, Yu-Chiu	2487	424-7235	WOODLEY, Mark	4081	424-7794
CHRISTENSEN, Jim	3971	424-7720	WOODS, Mike	3609	424-7828
CLENDENIN, Jim	2962	424-7133	YEREMIAN, Dian	4444	424-7340
CORBETT, Jeff	3645	424-7150	ZIEMANN, Volker	4775	424-7994
DECKER, Franz-Josef	3606	424-7318	ZOLOTOREV, Max	4670	424-7793
ECKLUND, Stan	3182	424-7229			
EMMA, Paul	2458	424-7160			
FEATHERS, Larry	2496	12-131			
FISCHER, Gerry	4198				
FRISCH, Josef	4005	424-7792			
GARDEN, Christina	2061	424-7835			
GRAY, Robin	3213	424-7138			
HSU, Ian	2073	424-7353			
HUTTON, Andrew	2976	424-7157			
JOBE, Keith	2084	424-7025			
KREJCIK, Patrick	2790	424-7183			
KULIKOV, Artem	2071	424-7197			
LIMBERG, Torsten	4512	424-7315			
MCCORMICK, Doug	2470	424-7368			
NIXON, Robbin	2938				
PENNACCHI, Roz	3665	424-7051			
PHINNEY, Nan	2957	424-7817			
RITSON, Dave	3476				
ROSS, Marc	3526	424-7237			
SCHULTZ, David	2459	424-7247			
SEEMAN, John	3566	424-7327			
SHEPPARD, John	3498	424-7320			
SMITH, Patrick	2413	424-7568			
SODJA, Joe	2390	424-7269			
SPENCE, Bill	3866	424-7799			

rin 5/7/91

SLC Task Force (1991 - 1998)

SLC STEERING COMMITTEE

Bob was a cornerstone of the Steering Committee from beginning to end

Nan Phinney - Chair
Bill Ash
Stan Ecklund
Tom Himel
Marc Ross
Ron Ruth
John Seeman
Bob Siemann
Nick Walker

ex officio

John Sheppard
Burt Richter

CHARGE TO THE COMMITTEE: PLANNING AND COORDINATION OF 1991 RUN

- 1) Develop a run plan (with the System Physicists) with reasonable goals and milestones
- 2) Review projects critical to meeting those goals and ensure that they have sufficient priority and resources
- 3) Review and approve machine physics experiments
- 4) Schedule the run on a weekly basis with goals, priority experiments and backup alternatives, SLD runs (Detailed scheduling of shifts to be done with the Program Deputy at a weekly scheduling meeting)
- 5) Review the progress of the run and make necessary mid-course corrections

SLC: The 1st Linear Collider

Built to study the Z_0
and demonstrate
linear collider
feasibility

Energy = 92 GeV

Luminosity = $3e30$

Had all the features
of a 2nd gen. LC
except both e+
and e- shared the
same linac

Much more than
a 10% prototype

SLC luminosity: Many Challenges

Lessons learned:

1992 - 1998 SLD Luminosity

Flexible design to
allow parameter
optimization

Extensive
diagnostics for
troubleshooting
and tuning

Reliable and stable
operation

Well designed
collimation to
limit backgrounds

→ Provides the basis for the next generation LC

SLC POSITRONS

The e^+ yield is in reasonable agreement with shower and beam transport Monte Carlo's through the West Turn-Around.

- Monte Carlo gives $2.2 e^+/e^-$ at this point
- Measured yield during last cycle was $2.0 e^+/e^-$

The problems are in the acceleration from 0.2 to 1.2 GeV, the transfer line from linac to damping ring, and the damping ring.

- Final Monte Carlo yield is $1.4 e^+/e^-$
- Measured yield was $0.6 e^+/e^-$

For the 1 GeV accelerator we are developing beam-based alignment procedures to position quadrupoles and increase the acceptance.

- Present quadrupole alignment is $\sim 200 \mu\text{m rms}$
- This needs to be reduced to $150 \mu\text{m}$ for adequate acceptance.
- Work is in progress. Some displaced quadrupoles have been identified and moved.

Positron Task Force

When Bob started at SLAC and worked on the positron system he was thrown into the deep end. He brought mathematical rigor to our data analysis procedures and guided us towards proper error analysis in the results.

Bob Siemann
for the
Positron Task Force

SLC Positrons

- * Much improvement in transport systems
- * Final scans of SDR aperture in 1992

ACCEPTANCE MEASURED WITH e^- BEAM

- TIGHTEST APERTURES ARE EXPECTED FROM DESIGN ENLARGED FOR 1992
 - FEW UNEXPECTED APERTURES DUE TO MECHANICAL AND/OR ALIGNMENT ERRORS COLLECTED
- ⇒ MEASURED APERTURES ARE LARGE ENOUGH FOR e^+ BEAM IF STEERING AND OPTICS ARE CORRECT

Flat Beams in the SLC

* Issues

- Damping rings
- Emittance preservation
- Spin control
- Diagnostics

* 1990 SLC White paper

- No luminosity improvement expected from flat beams
- Do not pursue!

SLAC-PUB-6118
May 1993

Bob strongly supported experimental study of flat beams

Flat Beams in the SLC*

C. Adolphsen, T. Barklow, D. Burke, F.-J. Decker, P. Emma, M. Hildreth, T. Himel, P. Krejcik,
T. Limberg, M. Minty, N. Phinney, P. Raimondi, T. Raubenheimer, M. Ross, J. Seeman, R. Siemann,
W. Spence, N. Walker, M. Woodley
*Stanford Linear Accelerator Center
Stanford, California 94309*

ABSTRACT

The Stanford Linear Collider was designed to operate with round beams [1]; horizontal and vertical emittance made equal in the damping rings. The main motivation was to facilitate the optical matching through beam lines with strong coupling elements like the solenoid spin rotator magnets and the SLC arcs.

electron and positron ring tunes is historical. For normal SLC operation, simply splitting the horizontal and vertical tunes is sufficient; producing beams with normalized rms emittances of $\gamma\epsilon_x=3\times10^{-5}$ m-rad and $\gamma\epsilon_y=0.3\times10^{-5}$ m-rad.

The vertical emittance could be reduced further with more sophisticated coupling and dispersion correction. However, in the electron damping ring, the beam is stored for only 2.4 damping times and thus the extracted vertical emittance is

Flat Beams in the SLC

Figure 1

Histories of SLC invariant vertical emittances for e^- (left) and e^+ (right) at 3×10^{10} at linac locations: entrance (1.2 GeV, top), 1 km (15 GeV, center), and exit at 3 km (47 GeV, bottom). The period is March 3 - June 2, 1993.

Fig 2. Flat beam-beam vertical deflection scan which measures the quadratic sum of the e^- and e^+ vertical beam sizes. The individual vertical beam sizes are $<0.9 \mu\text{m}$. The horizontal beam sizes (not shown) are typically $2.2\text{-}2.5 \mu\text{m}$.

SLC Damping Rings

- * The SLC damping rings were critical for the collider performance
 - Errors were amplified by linac and made tuning FFS difficult
- * Many challenges in the rings
 - Space was tight – hard to install new diagnostics
 - Magnets pushed to their limits
 - High single bunch charge and injection/extraction meant single bunch instabilities and beam loading issues turned into downstream jitter
- * Bob wanted to turn the rings into precision machines
- * The damping rings were ideal for Bob and his students because we could create every known instability plus some new ones

SLC Damping Ring RF Feedback & Transients

Bob was the driving force behind the RF studies which were useful for SLC and critical for PEP-II

SLAC

July 7th, 2009

Bob Siemann: SLAC Professor and SLC Physicist

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Simulation and Analysis of RF Feedback Systems on the SLC Damping Rings

M. Minty, T. Himel, P. Krejcik, R.H. Siemann, R. Tighe

Stanford Linear Accelerator Center, Stanford University, Stanford CA 94305 *

Abstract

The rf system of the SLC Damping Rings has evolved since tighter tolerances on beam stability are encountered as beam intensities are increased. There are now many feedback systems controlling the phase and amplitude of the rf, the phase of the beam, and the tune of the cavity. The bandwidths of the feedback loops range from several MHz to compensate for beam loading to a few Hz for the cavity tuners. To improve our understanding of the interaction of these loops and verify their expected behavior, we have simulated their behavior using computer models. A description of the models and the first results are discussed.

1. Introduction

During the 1992 SLC/SLD run, accelerator operation became sensitive to the microwave instability¹ at beam currents above 3×10^{10} particles per bunch. To avoid the onset of bunch lengthening at higher currents, an rf voltage ramp was implemented. However, the depth of the ramp was limited by a Robinson-like instability^{2,3}, which was corrected for using direct rf feedback^{3,4}. With these modifications, the parameter space has become more complex. Operating conditions must be adjusted and optimized such that the rf system operates stably under varying condi-

The rf system is modelled by calculating the dynamics of the beam cavity interaction. These equations along with measured loop characteristics and klystron saturation data are then incorporated into numerical integration programs such as Matrix-x⁵ and Simulink⁶. These algorithms use state space representations of the system to solve the differential equations. The system can be analyzed in both the time and frequency domains.

Figure 1. SLC damping ring rf system.

SLC longitudinal Phase Space

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Measurements of Longitudinal Dynamics in the SLC Damping Rings*

R.L. Holtzapfle, R.H. Siemann, and C. Simopoulos

Stanford Linear Accelerator Center, Stanford University, Stanford, CA 94309 USA

I. Abstract

Measurements of longitudinal beam properties in the Stanford Linear Collider (SLC) damping rings have been made using a Hamamatsu, model N3373-02, 500-femtosecond streak camera[1]. The dependence of bunch length on current and accelerating RF voltage was measured. The energy spread dependence of current was also measured. The turbulent instability threshold for the SLC damping ring is at the current of $I=1.5-2.0 \times 10^{10}$ particles per bunch.

II. Data Analysis and Systematic Errors

The measurements were performed on either the SLC electron or positron damping ring when the opportunities for beam time became available. We are not aware of any

and the incident light was filtered until the bunch length measurement was stable over a range of light intensities. Figure 1 exhibits the intensity effects on bunch length measurement and in this example the light is filtered by 50% to eliminate this effect.

Figure 1. The bunch length dependence on light intensity measured at the damping ring with a current of $I=3.6 \times 10^{10}$.

SLC Damping Ring Instabilities

* In the early 1990's, jitter at the IP was tracked back to a bursting longitudinal instability in the SLC damping rings

– Small changes in DR phase and energy caused large changes at the IP

* A number of fixes were developed including complicated rf tricks to 'pin' the instability onset

SLAC

July 7th, 2009

Bob Siemann:

SLC Damping Ring Upgrade

- * The bursting instability was identified in the early 1990's as the longitudinal microwave instability
 - Shielding of the bellows in 1989 increased the threshold by 2 as expected but was to 3×10^{10}
 - Measurements agreed fairly well with modeling which predicted a further increase in threshold by smoothing the ring chamber
 - Bob led the effort with Torsten Limberg
 - Karl Bane provided the theoretical calcs
 - In 1994 when the threshold was measured to be 2x lower!

When an issue arose, Bob was great at pulling the experts together, listening to what they had to say and then making a decision

Sawtooth Instability version 2.0

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High-Intensity Single Bunch Instability Behavior In The New SLC Damping Ring Vacuum Chamber*

K. Bane, J. Bowers, A. Chao, T. Chen, F. J. Decker, R. L. Holtzapple, P. Krejcik, T. Limberg, A. Lisin, B. McKee, M. G. Minty, C.-K. Ng, M. Pietryka, B. Podobodov, A. Rackelmann, C. Rago, T. Raubenheimer, M. C. Ross, R. H. Siemann, C. Simopoulos, W. Spence, J. Spencer, R. Stege, F. Tian,

J. Turner, J. Weinberg, D. Whittum, D. Wright, F. Zimmermann

Stanford Linear Accelerator Center, Stanford

New low-impedance vacuum chambers were installed in the SLC damping rings for the 1994 run after finding a single bunch instability with the old chamber. Although the threshold is lower with the new vacuum chamber, the instability is less severe, and we are now routinely operating at intensities of 4.5×10^{10} particles per bunch (ppb) compared to 3×10^{10} ppb in 1993. The vacuum chamber upgrade is described, and measurements of the bunch length, energy spread, and frequency and time domain signatures of the instability are presented.

I. VACUUM CHAMBER UPGRADE

Bob Siemann

Bob loved experimental physics and loved designing experiments to get at the physics.

Bob was sometimes gruff, sometimes measured and sometimes excited, but he was always a model of intellectual integrity.

He pushed us to think for ourselves and provided guidance by asking the right questions.

We will all miss him greatly!

SLC and the Sawtooth (from grad student's perspective)

Boris Podobedov
NSLS

Robert H. Siemann Symposium

July 7, 2009

Background

- I graduated from Moscow Inst. of Physics & Technology, MS in Physics, summer of 1993
- I enrolled in Stanford Ph.D. program in Appl. Phys., fall 1993
- I knew nothing about accelerators or acc. physics
- In October 1993 I met Bob Siemann ...

Professor

AP 324: Introduction to Accelerator Physics

Boris Podobedov

AP 324. Problem set 4.

13/15

1. Smooth approximation

$$\beta = \frac{R}{Q} = \frac{C}{2\pi Q} = \begin{cases} 0,67 \text{ m} & (H) \\ 1,74 \text{ m} & (V) \end{cases}$$

4/5

$$\eta = R/Q^2 = \begin{cases} 8,08 \cdot 10^{-2} \text{ m} & (H) \\ \cancel{0,84 \text{ m}} & (V) \end{cases}$$

$$\alpha = \eta/R = 1/Q^2 = \begin{cases} 1,45 \cdot 10^{-2} & (H) \\ \cancel{9,7 \cdot 10^{-2}} & (V) \end{cases}$$

There is no dispersion or momentum compaction in the vertical plane (only in the plane with bending)

Coherent beam-beam interaction with four colliding beams

B. Podobedov and R. H. Siemann

Stanford Linear Accelerator Center, Stanford University, Stanford, California 94309

(Received 4 November 1994; revised manuscript received 12 April 1995)

The coherent beam-beam interaction in the absence of Landau damping is studied with a computer simulation of four space-charge-compensated colliding beams. Results are presented for the modes, phase space structures, widths, and growth rates of coherent beam-beam resonances. These results are compared with solutions of the Vlasov equation, and with measurements made at the Dispositif de Collisions dans l'Igloo (DCI) storage ring in Orsay, France, which operated with space-charge-compensated colliding beams.

Saw-tooth Instability History

- 1992

Attempt to raise current above 3×10^{10} /bunch

Severe single bunch longitudinal instability

Transient, “Saw-tooth” behavior

Inability to operate the linac

- 1993

Solution - vacuum chamber replacement. Total inductance was reduced by a factor of 5.

Simulations predicted threshold of 5×10^{10} /bunch

- 1994-1998

The actual threshold went down $\sim 2 \times 10^{10}$ /bunch

Instability less severe. Saturates at lower level. It is no longer the main limiting factor for the SLC

P. Krejcik et. al., PAC-93

Instability after Vacuum Chamber Replacement

BPM Signals on a Spectrum Analyzer

Other Measurements

- Wire scanner - energy spread growth above the threshold
- Streak Camera - no signs of instability other than bunch lengthening
- No measurable effect on the linac

Mysteries remained:

- Effect on the beam
- Effect on SLC, if any
- Instability mechanism and cause

At Bob's suggestion this became a topic of my PhD

Detecting Instability Signals

Clearly a better diagnostics was needed

Detector Schematics

Detector

- provides single bunch "instability signal"
- the phase of this signal relates to the azimuthal orientation of the instability-induced phase space structure

RF Switch and Crystal Specifications

	WJ-MSE203	MITEQ-S138B	ACTP-1506N
Type	SPDT Reflective	SPST Absorptive	Square Law Detector
Bandwidth	2-18 GHz	2-18 GHz	8-18 GHz
Switching time	25 ns	20 ns	N/A
Isolation	~60 dB	80 dB	N/A

Accelerator Physics Through Building Your Own Diagnostics

New Diagnostics Works Out

Signal after the Detecting Circuit

Instability frequency
 $160 \text{ kHz} < \sim 2 f_s$

Envelope frequency
 $1 \text{ kHz} \sim \gamma_d$

We finally got the t-domain instability signal.

We could now combine it with the streak camera

Saw-Tooth Instability: Main Results

- Recover complete phase-space picture
- Unstable mode contains ~3% of the beam
- Detector signals correlated with SLC beam downstream

PhD Thesis Writing

Second, since the instability was in a continuous mode we implemented a better fitting routine for the oscilloscope traces. Instead of relying on just two zero-crossings during one instability period we used a nonlinear least squares algorithm to fit the whole 40 μ s trace with a sine-like curve with the amplitude, the frequency and the phase being the fit parameters.

With the two changes above included we processed a major portion of the data when the instability was in a quadrupole mode. Unexpectedly, we observed a strong correlation between the centroid positions of fitted profiles and their bunch lengths. The bunch length for the files in the center of the CCD was about 20% longer than for those closer to either edge of the CCD. This systematic effect of course hid the instability induced features in the beam distribution. We have traced this effect to a local sensitivity drop in the streak camera system (most likely the photocathode). To correct for this we had to boost the values for the CCD pixels from 191 to 256 by various amounts up to 14%. To find the ~~exact~~ correction coefficients for each pixel we used a procedure described in detail in reference [65]. As a source profile we used a sum of all the profiles from 96/97 run. In contrast to the data from the present experiment those profiles were much more uniformly distributed in their centroid positions due to larger trigger jitter. Adding this correction has eliminated the bunch length dependence on the centroid position. We have used this correction to get the results to be described next.

5.2.4 Results

Using the data processing with the changes described above we have separately processed the files that correspond to the sextupole mode at $V_{RF}=730$ kV and to the quadrupole mode at $V_{RF}=800$ kV. This choice is obvious from Figure 51. For the purposes of this section we will refer to these data as a sextupole and quadrupole batches. After making cuts in the profile area, beam current, and instability frequency and amplitude the total number of files remained in the sextupole and the quadrupole batches was 1164 and 1590 respectively. The average instability frequency was calculated to be 161.5

Section 4 - basically fine. Only a few small corrections and questions

Section 5

- ~~correction~~ correction procedure on page 125 must be expanded upon
- would like to see comparisons of coverage profiles & Haisinski distribution
- where do you plan to cover the correlated bunch phenomena that you mention in doing the section?

- Bob strongly believed that thesis writing is essential part of graduate education
- Writing required many iterations
- Huge effort from Bob

What Those Theses Meant for Bob

-----Original Message-----

From: Robert H. Siemann [<mailto:siemann@SLAC.Stanford.EDU>]

Sent: Monday, July 10, 2000 10:40 AM

To: Podobedov, Boris V

Subject: Re: PHD stuff etc

Boris,

Your thesis is on my shelf with the others. I was telling Angie the other day that I consider those theses to represent some of my most important accomplishments - the education of young people.

Glad to hear about the success of your work at NSLS. Please consider PRSTAB when thinking about publishing it.

Bob

Reminiscence of Bob Siemann and His Dream on 1 GeV/m

Ping J. Chou
Taiwan Light Source (NSRRC)

– Early Encounter in My Grad Student Period

LUM-2

Fermilab

Fermi National Accelerator Laboratory
P.O. Box 500 • Batavia, Illinois • 60510

EXP-155

Fermilab EXP-155

August 13, 1987

To: C. Ankenbrandt, Don Edwards, Helen Edwards, Dave Finley, Mike Harrison, Gerry Jackson, Rol Johnson, Quentin Kerns, Keith Meisner, Jack McCarthy, Bill Miller, Alvin Tollestrup

From: R. Siemann

RS

Attached is a draft of a note on emittance growth in the Tevatron. I hope it will be useful in solving the problem. Corrections and or comments are welcome. I can be reached as LNS61::SIEMANN

- 1992 in US Particle Accelerator School (Stanford Univ.)

A warm and inspiring teacher

- 1995 in Particle Accelerator Conference (Dallas, TX)

The chair of that big conference presenting a poster himself !

– Being Bob's Postdoc and Joining His Dream on 1 GeV/m

Accelerator **R**esearch **D**epartment **B**

Our mission (in Bob's words):

Invent, Design
Build and Commission

a 1 GeV/m x1 m Linac

with a technology scalable to a **5 TeV e+e- COLLIDER !**

First move:

Let's get the Lab 223 cleaned and organized

ARDB Lab 223

Heino Henke
Technical University, Berlin

Dennis Palmer

Bob

PJ

W-band Linac Structure – the first prototype

First 90 GHz Traveling-Wave Accelerating Structure
Made by Wire Electrodischarge Machining

Accelerator Research Department B at SLAC

Sketched by Gordon Bowden

Home-made W-band Network Analyzer

Sketched by Bob Siemann

Al Menegat

<1/3 the price of HP w-band network analyzer

Bob Working in His Tree House

RF Measurements of the First Prototype:

The Fun Time in Group Bob

David Whittum Al Menegat Marc Hill Boris Podobedov David Pritzkau Mike Seidel

Frank Zimmermann Ping J. Chou Bob Siemann

Graduation and Beyond

- I moved to BNL in Sept 1999
- We kept in touch discussing topics ranging from PRST-AB to his grad. students, and of course, Science
- My last meeting with Bob was on my SLAC visit in Feb 2007 ...

RF Pulsed Heating and Bob Siemann

David Pritzkau

Introduction

- I was a grad student of Bob's from 1994 to 2001
- I first met Bob in 1994 at an orientation at Stanford for new incoming grad students in physics
- I was immediately captured by Bob's enthusiasm and passion as he described all of the cool things in accelerator physics he was working on
- I joined Bob's group as part of the quarterly rotation that incoming students participate in, but I never rotated out
- Not only did I catch the accelerator physics bug, but after one quarter I could already perceive what a terrific mentor Bob was and I could not let go of a good thing

ARDB and 1 GeV/m

- For the first two years I was working on experiments on the SLC damping rings studying the effects of residual gas on electron orbits
- After I passed my qual, Bob informed me he was creating a new research group, ARDB, and that I would be the first “guinea pig” to complete a thesis under the new charter
- However, instead of working on techniques to try to achieve accelerating gradients of 1 GeV/m, we wanted to understand more about what may limit the gradient for RF linear normal-conducting accelerator structures
- First point of study would be RF pulsed heating in which Bob thought it may only be 1 chapter of a bigger thesis
 - That chapter turned out to be about 290 pages
 - After writing three chapters of my thesis, I eventually succumbed to Bob’s infamous red pen

Accelerating Gradient vs. RF Wavelength

Scaled from
NLC numbers

$$G_{pulse} \propto \omega^{1/8}$$

Pulsed Heating

This graph, based on calculations from Perry Wilson in 1996 and Bob Siemann in 1997, was our motivation to study the effect of RF pulsed heating on OFE copper

What Is RF Pulsed Heating?

- Joule heating from pulsed high power RF
- Cyclic stress induced from repetitive heating and cooling of the surface
- Induced stress may exceed yield strength of the metal
- May lead to fatigue of metal surface causing RF performance degradation
- Damage accumulates slowly over time and is not necessarily catastrophic or initially obvious like RF breakdown
- Pulsed heating is more of a “reliability” problem as it can shorten the lifespan of an accelerator structure
- For Oxygen-Free Electronic (OFE) Copper: Simple calculation of Threshold $\Delta T = 40^\circ\text{C}$

Thermal Fatigue From Pulsed Heating

If $\Delta T_{\max} > 2\Delta T_y$ then continuous permanent damage occurs ($\sim 40\text{C}$)

RF PULSED HEATING EXPERIMENT OVERVIEW

$\Delta T \sim 120$ K
for 56 x 10⁶ pulses

$\Delta T \sim 80$ K
for 86 x 10⁶ pulses

$\Delta T \sim 120$ K:
Numerous cracks

$\Delta T \sim 80$ K:
Some cracks
Numerous Slip Bands
Electrical Resistivity increased
25 to 50%

Endcaps From an Experiment Run

After 86 million pulses (or 33 days of run-time at 30 pulses per second)

THICKNESS OF
DAMAGE REGION 6 MM

$65\text{ K} < \Delta T < 82\text{ K}$

Estimation of threshold $\Delta T \sim 40\text{K}$ may be correct

Damage of Coupling Aperture

Aperture couples energy
from klystron into RF cavity

Aperture rounded in
direction of magnetic
field where the field is
largest

ΔT estimated at 250C

This prevented us from achieving higher power and higher ΔT on the endcaps
I later heard that the NLCTA used these results to redesign their input couplers
to reduce the magnetic field near the apertures and achieve higher power

Personal Reminiscences of Bob Siemann

- Despite Bob's incredibly busy schedule, his door was always open and he always made time for me over the years, whether I had an issue, ran into a roadblock, or just wanted to show him something cool
- Bob's attention to detail, his quick insight, and his multitude of ways of looking at a problem, both experimentally and theoretically, along with his infectious enthusiasm helped me to grow tremendously as a scientist and engineer
- As I evaluate my work over time, I find myself often thinking how Bob would perceive it, for he has become my yardstick for which I measure myself
- I do not believe I could have found a better mentor than Bob and I am grateful for the time he has shared with me as his student

Leadership at the Spallation Neutron Source

**Stuart Henderson, Director
Research Accelerator Division**

July 7, 2009

Bob Siemann as Advisor

Spallation Neutron Source History

- 1984 – National Academy of Sciences “*Major Facilities for Materials Research and Related Disciplines*” (Seitz-Eastman Panel) delivered a prioritized list of recommendations for major new research facilities:

1. A 6-GeV Synchrotron Radiation Facility (✓ APS)
2. An Advanced Steady-State Neutron Facility (✗ ANS)
3. A 1-2 GeV Synchrotron Radiation Facility (✓ ALS)
4. A High-Intensity Pulsed Neutron Facility (✓ SNS)

- 1986 – DOE Memo: *Secretarial Site Selection Decisions on Specific Energy Research Projects*

- RHIC – BNL
- 1-2 GeV SR Facility – LBNL
- 6-GeV SR Facility – ANL
- Steady-State Research Reactor – ORNL

- 1993 – Basic Energy Sciences Advisory Committee (Kohn Panel) reaffirmed need for both Reactor and Accelerator-based Neutron

Sources

3 Managed by ORNL-Dahe for the U.S. Department of Energy

Bob Siemann Symposium, July 7 2009

Spallation Neutron Source History

- 1995 - Advanced Neutron Source cancelled
- 1995 – Congress appropriates \$8M for conceptual design of accelerator-based neutron source
- 1996 – BESAC (Russell Panel) strongly recommended that a 1-MW pulsed spallation neutron source, with upgrade capability into the multi-MW range, be constructed.
- 1996 - Five DOE Laboratory Collaboration Established
- August 1996 – CD-1 Approval
- May 1997 - Conceptual Design Report issued
- December 1997 CD-2 Approval
- October 1998, First Accelerator Systems Advisory Committee Meeting

Spallation Neutron Source Project: Challenges

- Multi-laboratory collaboration (LBNL, BNL, LANL, ANL, ORNL, and later JLAB) with ORNL having Project Management responsibility
- Several partner labs already had advanced designs
- Built at a laboratory on a green-field site without a large accelerator infrastructure or culture
- Construction model called for partner lab design and ORNL installation, commissioning and operation
- Baseline was not optimized
- Large project: \$1.4B, 7 year construction
- Technical goals were well beyond the state-of-the-art:
 - 1-2 MW beam power baseline was a factor of ~10 beyond existing short pulse neutron sources
 - Beamloss criteria a factor of ~10 better than achieved

David Moncton, Former Director of APS and SNS

"I have great respect for Bob, his integrity, his work ethic, and his strong sense of commitment to physics beyond his own field. I knew him since the early 80's when he began to get involved in the accelerator issues associated with 3rd generation synchrotron sources. He led the first "strawman" design of APS. Subsequently he was strongly involved in the accelerator advisory committee to APS. He had such a talent for seeing both the forest and the trees! And when he focused on a potential problem area, he was always right about the importance of that particular issue, and he never missed an important issue. Finally he had a gift for expressing his concern in such a way that motivated people to focus on the issue and solve it. He created a sense of urgency without creating panic. If you are building an accelerator, and if you solve the problems and address the issues he raises, then the machine will work as planned. At least that was my experience. So he was a great comfort to have on your side. Having had this experience with APS it was a no-brainer to invite him to play the same role in SNS, which he did with remarkable effectiveness for three meetings, I believe, while I was SNS director. Please express my sincere admiration and condolences. "

Thom Mason, Oak Ridge National Laboratory Director, and Former Director of SNS

"Bob was with the project all the way through. To some extent his role was to help us navigate the turmoil through start-up and management changes without losing sight of the tactical basis of what we were doing. I think he went beyond that in the sense that he was very helpful in providing advice of a more informal nature on people, roles, and responsibilities and realities. In addition I think he was able to function in a way that had sufficient standing with the accelerator folks across the multi-lab partnership, so that once we had ASAC buy-in, any dissent on a decision was largely behind us – people bought in."

SNS: A Dynamic Environment (1997-2006)

- ORNL Contractors: 2
 - LBNL SNS Team Leaders: 2
- ORNL Directors: 3
 - LANL SNS Team Leaders: 5
- SNS Project Directors: 3
 - BNL SNS Team Leaders: 2
- SNS Accelerator Division Directors: 2
 - JLAB SNS Team Leaders: 1
- SNS Conventional Facilities Directors: 2
 - ANL SNS Team Leaders: 1
- SNS Experimental Facilities Directors: 3
 - ASAC Committee Chairman: 1

SNS Accelerator Systems Advisory Committee Members 1998-2006

- Bob Siemann, Chair (SLAC)
- Matt Allen (SLAC)
- Daniel Boussard (CERN)
- Alex Chao (SLAC)
- David Finley (Fermilab)
- Bob Jameson (LANL)
- Bill McDowell (Argonne)
- Gerry McMichael (Argonne)
- Graham Rees (Rutherford Appleton Lab)
- Paul Schmor (TRIUMF)

- Jean-Louis LaClare (CEA/Saclay)
- Dieter Proch (DESY)
- Mike Harrison (BNL)

And later.....

- Bob Kustom (Argonne)
- Helen Edwards (Fermilab)
- Frank Zimmerman (CERN)
- Yoshishige Yamazaki (J-PARC)

Key Themes and Technical Issues

- The need for a conservative and flexible design
- The need to focus on reliability and operability in the design stage
- The need to focus on beamloss as the single over-riding technical issue
- The need to build a strong team at ORNL to take ownership of the machine

The Major Issues: ASAC Report 1, October 1998

"... The SNS Project is in an unusual state outside much of our experience in that it is a construction project with significant money appropriated and yet there remain issues at the conceptual design level that need to be resolved. Three particular areas that should be given attention by the SNS collaboration and management are reliability, beam loss, and the implications of the transition from a multi-laboratory construction project to an operating accelerator at Oak Ridge National Laboratory..."

Need for Strong ORNL Leadership: ASAC Report 1, October 1998

"... The SNS is being designed and constructed by a five laboratory collaboration. Oak Ridge has a central role in this collaboration. It has the responsibility of integrating the accelerators delivered by the other laboratories. It will participate in the commissioning, and then it must operate and improve the SNS. This has implications now.

The design and construction will require trade-offs between the accelerators that are the responsibilities of the other laboratories. Some of these trade-offs will be complex and will have to be made with only partial information. This will require strong, experienced leadership at Oak Ridge to deal with the other laboratories and, in addition, to recruit and lead the people who will commission, operate and improve the SNS.

Documentation must be at an unprecedented level for an accelerator project because ORNL will not have the equipment designers and builders in residence to diagnose and repair equipment. Documentation must be exceptional, and, in addition, there could be significant value in having ORNL people involved in the construction and design at various collaborating laboratories..."

Flexibility: ASAC Report 1, October 1998

"... There are no reliable beam physics models which can credibly predict losses at this small level, and it is unlikely that such models will be developed before the design is finalized. Therefore, the linac and accumulator ring designs must be conservative by today's understanding, and they must be flexible..."

Flexibility: ASAC Report 3, October 2000

“ ... There is one situation that needs immediate attention and is highlighted here. The Drift Tube Linac (DTL) design has permanent magnet quadrupoles, and this feature is close to being frozen... This planned use of permanent magnet quadrupoles is neither conservative nor does it provide tuneability and flexibility. Space charge is most important at lower energies, and the present plan is to provide no focusing adjustment to 90 MeV. This concern was raised at the last ASAC meeting, and at this meeting it was stated in response to questions that fixed quadrupole gradients are preferable so people couldn't readjust gradients. But this could be necessary and is exactly our concern - study of phase and quadrupole laws in the high-energy end of the linac are to continue, but results of this work could not be applied later to a DTL with permanent magnet quadrupoles.”

Reliability and User Expectations: ASAC Report 6, February 2002

“A high level of reliability is a priority for the neutron scattering community. The ultimate goal for SNS operation is 5000 hours per year at full beam power of 1.4 MW with 95% reliability. In past reports we have stated our opinion i) that reliability did not appear to be an important consideration in the design, and ii) that it was important to develop realistic goals for the initial reliability and for the rate of improvement over the first years of operation.”

Operability: ASAC Report 6, February 2002

“The emphasis on Project Completion is appropriate, but care is needed, as decisions made now will affect the project over its full lifetime. Options that are inexpensive now but costly to retrofit should be implemented if possible. Site engineering designs should concentrate on long term issues associated with ease of access and ease of maintenance of active accelerator components, including vacuum leak testing and replacement of faulty items. Exposure of staff to radiation during maintenance in the tunnels can be greatly reduced by the standardization”

Beamloss: ASAC Report 3, October 2000

“The SNS has stringent beam loss requirements. The linac design must minimize beam halo generation, potential beam loss along the linac itself, and deterioration in output beam quality to the HEBT and ring. The underlying physics depends on space charge and its nonlinear interactions via the system resonances. The linac dynamics has never been presented to us in a framework of space-charge physics, e.g., in terms of transverse and longitudinal tunes at low and high currents, tune trajectories along the linac, tune spreads, relation to structure resonances and potential unstable modes, and the effects of errors in this framework.... There is brute force running of simulation codes and simplistic evaluation of results by comparing phase-space scatter plots or RMS quantities, without relation to the underlying physics. The best strategy to continue making progress with incomplete understanding of the physics is to 1) make conservative decisions and 2) provide tuneability and flexibility so there will be the ability to respond to either improved understanding from future physics studies or experience gained during commissioning.”

Evolution of SNS Design

ORNL 97-5017A/vlb

Fig. 8.0-1. Schematic of fully upgraded facility.

Evolution of SNS Design

Adoption of Superconducting Technology: ASAC Report 2, December 1999

"...Superconducting RF has already been accepted for intense, light-ion cw applications. Its technical application to pulsed light-ion applications, such as the SNS linac, awaited confidence that the RF fields and phases could be controlled precisely with Lorentz force detuning and microphonics present. This control has been developed and demonstrated at the TESLA Test Facility, and the TESLA design seems adequate to fulfill the SNS needs... The SNS design study clearly indicates that superconducting RF is economically and schedule competitive. With superconducting technology also applicable to pulsed high-intensity accelerators, it is clear that superconductivity is the technology of choice for many future linacs....In summary, we give a strong endorsement for the use of superconducting technology."

ASAC Dinners....

Personal Reflections

SNS Ramps-Up

In memory of Bob

Robert H. Siemann

Bob Siemann Memorial Symposium

July 7, 2009

Bob Siemann and the Plasma Wakefield Accelerator Collaboration at SLAC

Tom Katsouleas

Professor and Dean, Duke University, Pratt School of Engineering

Plasma Accelerators – History (BB)

- 1979 Tajima & Dawson Paper
- 1981 Tigner Panel rec'd investment in adv. acc.
- 1985 Malibu, GV/m *unloaded* laser 'beat' wakes, world-wide effort begins – Richter poses "GeV Challenge" for advanced concepts
- 1988 ANL maps beam wakes (5 MV/m)
- 1992 1st e- at UCLA
- 1994 'Jet age' begins --100 MeV in laser-driven gas jet at RAL (but scaling from mm to cm or m with lasers not obvious)
- 1997 Bob enters the field at Snowmass...

Plasma Accelerators – History (AB)

- 1997 Snowmass – Bob approached with idea of GeV energy gain in a 1m SLAC-driven wake; identifies Ralph Assmann to work with us on proposal to PAC
- 1998 PAC Approval
- 1999 E-157 begins – Dom Perignon on ice in Bob's office - focusing, matching, deceleration
- 2001 E162 250 MeV acceleration, refraction, e+
- 2004 E164 -- GeV acceleration!
- 2004 'Dawn of Compact Accelerators' (monoenergetic beams at LBNL, LOA, RAL)
- 2007 E167 Energy Doubling at SLAC (**champagne opened**)

Leonardo deVinci, Study of Wakes:1509

Nonlinear Wakefield Accelerators

(Blowout Regime)

Rosenzweig et al. 1990

Pukhov and Meyer-ter-vehn 2002 (Bubble)

- Space charge or radiation pressure of driver displaces plasma electrons
- **Plasma ion channel** exerts restoring force => space charge oscillations
- Linear focusing force on beams ($F/r=2\pi ne^2/m$)
- Optical guiding of lasers

The E-157 Plasma Wakefield Experiment

Collaboration:

T. Katsouleas, S. Lee, P. Muggli USC

P. Catravas, S. Chattopadhyay, E. Esarey, P. Wolfbeyn LBNL

R. Assmann, P. Chen, F. J. Decker, M. Hogan, R. Iverson,
S. Rokni, R. H. Siemann, D. Walz, D. Whittum, SLAC

B. Blue, C. Clayton, R. Hemker, C. Joshi, K. Marsh, W. Mori, S. Wang, UCLA

Schematic of E-157 Plasma Wakefield Experiment

Week 1: Spot Size Data at Cerenkov detector Show Betatron Oscillations in Agreement with Simple Blowout Theory => Focusing Strength 6000 T/m!

The E-162/E-164 Collaboration:

C. Barnes, I. Bluenfield, F.-J. Decker, P. Emma, M. J. Hogan, R. Iverson, R. Ischebeck, N. Kirby, P. Krejcik, C. O'Connell, P. Raimondi, R.H. Siemann, D. Walz

Stanford Linear Accelerator Center

B. Blue, C. E. Clayton, C. Huang, C. Joshi, D. Johnson, K. A. Marsh, W. B. Mori, W. Lu, M. Zhou

University of California, Los Angeles

T. Katsouleas, S. Deng, S. Lee, P. Muggli, E. Oz
University of Southern California

P. Muggli

E-157: Experimental Layout

E-162 Apparatus & Runs

1. Better magnetic optics upstream $\Rightarrow$ smaller spots at plasma entrance
2. Quadrupoles downstream + FFTB dump magnet $\Rightarrow$ imaging spectrometer

Runs 2&3, Summer 2001
 e^+ acceleration, e^- acceleration

Plasma Accelerator Research: Bob's Perspective of Experimental Work

From: Chan Joshi, UCLA Personal archives

Plasma Accelerator Research: Bob's Perspective of Computer Simulation Work

Refraction of an Electron Beam: Interplay between Simulation & Experiment

**Experiment
(Cherenkov
images)**

**3-D OSIRIS
PIC Simulation**

- 1st 1-to-1 modeling of meter-scale experiment in 3-D!

P. Muggli et al., Nature 411, 2001

□ E-157 & E-162 have observed a wide range of phenomena with both electron and positron drive beams:

e⁻ & e⁺ Focusing

Phys. Rev. Lett.
2002, 2003

Electron Beam Refraction at the Gas-Plasma Boundary

Nature 2002

X-ray Generation

Phys. Rev. Lett.
(cover) 2002

Wakefield acceleration

To Science 2003

Time resolved acceleration of positrons

E-162

- Loss ≈ 50 MeV
- Gain ≈ 75 MeV

B. Blue et al., Phys. Rev. Lett. 2003

R. Bingham, Nature, News and Views 2003

GOAL: An Energy Doubler Based on Plasma Afterburners

0-50GeV in 3 km
50-100GeV in 30 m!

S. Lee et al., Phys. Rev. STAB, 2001

Plasma Wakefield Scaling Law

Enter Paul Emma and Pat Krejcik...

energy profile

phase space

temporal profile

Particle tracking in 2D...

E164X breaks GeV barrier

$L \approx 10 \text{ cm}$, $n_e \approx 2.55 \times 10^{17} \text{ cm}^{-3}$, $N_b \approx 1.8 \times 10^{10}$

Energy gain exceeds $\approx 3 \text{ GeV}$ in 10 cm
M. Hogan, et al. (PRL, July 2005)

UCLA

Data is very reproducible!

...

Data is very reproducible!

E-167 Experimental Setup

10-100 GeV

E-167: Energy Doubling with a Plasma Wakefield Accelerator in the FFTB

Linac running all out to deliver compressed 42GeV Electron Bunches to the plasma
Record Energy Gain
Highest Energy Electrons Ever Produced @ SLAC
Significant Advance in Demonstrating Potential of Plasma Accelerators

Some electrons double their energy in 84cm!

[Nature vol 445.p741 \(2007\)](#)

Work supported by DOE

Plasma Acceleration has put Physics at the Forefront of Science

Acceleration, Radiation Sources, Refraction, Medical Applications

From good Physics to a good Collider is a Grand Challenge worth pursuing

NAE Grand Challenges for the 21st C

1. Solar energy economical	8. Secure cyber-space
2. Fusion energy	9. Prevent nuclear terror
3. Carbon sequestration	10. Urban infrastructure
4. Manage N cycle	11. Reverse engineer the brain
5. Clean water	12. Virtual reality (think Holodeck)
6. Engineered medicines	13. Personalized learning
7. Health informatics	14. Tools of scientific discovery

A Concept for a Plasma Wakefield Accelerator Based Linear Collider

- TeV CM Energy
- 10's MW Beam Power for Luminosity
- Positron Acceleration
- Conventional technology for particle generation & focusing

First Self-consistent PWFA-LC

Luminosity	$3.5 \times 10^{34} \text{ cm}^{-2}\text{s}^{-1}$
Luminosity in 1% of energy	$1.3 \times 10^{34} \text{ cm}^{-2}\text{s}^{-1}$
Main beam: bunch population, bunches per train, rate	1×10^{10} , 125, 100 Hz
Total power of two main beams	20 MW
Main beam emittances, $\gamma\epsilon_x$, $\gamma\epsilon_y$	2, 0.05 mm-mrad
Main beam sizes at Interaction Point, x, y, z	140 nm, 3.2 nm, 10 μm
Plasma accelerating gradient, plasma cell length, and density	25 GV/m, 1 m, $1 \times 10^{17} \text{ cm}^{-3}$
Power transfer efficiency drive beam=>plasma =>main beam	35%
Drive beam: energy, peak current and active pulse length	25 GeV, 2.3 A, 10 μs
Average power of the drive beam	58 MW
Efficiency: Wall plug=>RF=>drive beam	50% $\times$ 90% = 45%
Overall efficiency and wall plug power for acceleration	15.7%, 127 MW
Site power estimate (with 40MW for other subsystems)	170 MW

FACET/BELLA Joint Review July 21 - 23, 2008

Critical Issues

- Positron acceleration
- Modeling
- Beam loading - create/phase 2nd bunch
- Transverse beam dynamics
 - Hosing
 - Lenses
 - Pointing jitter sub-nm
 - Ion motion (Rosenzweig, 2005)
 - Synchrotron radiation
- Plasma source development
 - Beam-ionized sources, μs - ns refresh?

FACET will address most issues of a single stage

FACET: Facility for Advanced Accelerator Experimental Tests

- Use the SLAC injector complex and 2/3 of the SLAC linac to deliver electrons and positrons
 - Compressed 25 GeV beams → ~20 kA peak current
 - Small spots necessary for plasma acceleration studies
- Two separate installations
 - Final bunch compression and focusing system in Sector 20
 - Expanded Sector 10 bunch compressor for positrons

US and Worldwide Experimental Effort on Plasma Accel

Courtesy Rasmus Ischebeck

**Plasma accelerator research and Bob's legacy still
burning brightly**

Present Collaborators

*	B. Allen	USC	*	N. Li	SLAC
*	W. An	UCLA	*	W. Lu	UCLA
*	K. Bane	SLAC	*	D.B. MacFarlane	SLAC
*	L. Bentson	SLAC	*	K.A. Marsh	UCLA
*	I. Blumenfeld	SLAC	*	W.B. Mori	UCLA
*	C.E. Clayton	UCLA	*	P. Muggli	USC
*	S. DeBarger	SLAC	*	Y. Nosochkov	SLAC
*	F.-J. Decker	SLAC	*	S. Pei	SLAC
*	R. Erickson	SLAC	*	T.O. Raubenheimer	SLAC
*	R. Gholizadeh	USC	*	J.T. Seeman	SLAC
*	M.J. Hogan	SLAC	*	A. Seryi	SLAC
*	C. Huang	UCLA	*	R.H. Siemann	SLAC
*	R.H. Iverson	SLAC	*	P. Tenenbaum	SLAC
*	C. Joshi	UCLA	*	J. Vollaie	SLAC
*	T. Katsouleas	Duke University	*	D. Walz	SLAC
*	N. Kirby	SLAC	*	X. Wang	USC
			*	W. Wittmer	SLAC

Plasma Accelerators

Chris Barnes

not defined

R_{90}	σ_z	Asym	ξ
1.4 mm	$25.8 \mu\text{m}$	0.54	0.19 THz
1.7 mm	$19.6 \mu\text{m}$	0.71	0.21 THz
2.0 mm	$17.3 \mu\text{m}$	0.74	0.27 THz
3.2 mm	$13.6 \mu\text{m}$	0.72	0.19 THz

TABLE I: Results for the main fit parameters to the four autocorrelator data runs.

you can naturally
be!

yes
already
twice

does it lie between?
or not?

8 my underwear shrinks, the bunch length decreases!

DOUBT

IN THE BATTLE BETWEEN YOU AND THE WORLD,
BET ON THE WORLD.

www.despair.com

Laser Accelerators: A Path toward Coherent Attosecond X-rays

Robert L. Byer,
Tomas Plettner, Eric Colby, Ben Cowan, Chris Sears, Jim Spencer

and

Robert H. Siemann

Department of Applied Physics
SLAC National Accelerator Laboratory
Stanford University

ABSTRACT

In 1947 W. W. Hansen and colleagues accelerated electrons with microwaves generated from a Klystron. That work led to the 3km linear accelerator at the Stanford Linear Accelerator Center completed in 1968.

In November 2005 we successfully accelerated electrons with a visible laser light.

Today we are conducting experiments at SLAC to develop photonic bandgap dielectric based accelerator structures to efficiently couple laser radiation to electrons. The dielectric structures allow laser accelerators to operate at accelerating gradients of 1GeV/meter.

We have explored the possibility of laser accelerator driven coherent X-rays using a free electron laser. The approach looks promising because of the replacement of the traditional magnet based undulator with a laser driven dielectric based undulator.

Robert H. Siemann Symposium
July 7, 2009

Contents

Historic Background

Laser Electron Accelerator Project - LEAP

The TeV-Energy Physics Frontier

Future Opportunities

Coherent X-ray lasers

The Attosecond Physics Frontier

"Don't undertake a project unless it is manifestly important and nearly impossible."
Edwin Land - 1982

Professor Robert Siemann and Chris Sears June 15, 2008 - Stanford Graduation Ceremonies

E-168 Byer
Group

W. W. Hansen

1947
Mark I
6 MeV

The First Linear Accelerator at Stanford - 1947

1st Klystron (Varian, 1930s')

1st Linac 1946

The superconducting linac
In HEPL, 1960

The 2-mile collider (SLAC)

LEAP, 1997-2004

Demonstration of the FEL, 1977

First Operation of a Free-Electron Laser*

D. A. G. Deacon, J. L. R. Elias, J. M. J. Madey, G. J. Ramian, H. A. Schwettman, and T. I. Smil
High Energy Physics Laboratory, Stanford University, Stanford, California 94305
(Received 17 February 1977)

A free-electron laser oscillator has been operated above threshold at a wavelength of
3.4 μm .

The Klystron tube

The "Microwave" Lab (Now HEPL and Ginzton Labs) played a crucial role on the development of particle accelerators and the corresponding RF technology

Fig. 10 Marvin Chodorow comparing the CV-150 to the Mark III klystron

Marvin Chodorow & Klystron

W. W. Hansen - back right

Ed Ginzton

The development of the linear accelerator at Stanford University

E-168 Byer Group

“we have accelerated electrons”
Hansen's report to the Office of Naval Research

1947: **The Mark I**, 1m, 6 MeV

$$U = \int \vec{E} \cdot d\vec{r}$$

The Mark III

A high-energy physics research tool

1953: 400 MeV

1955: 600 MeV

1960: 1 GeV

Meson Physics carried out by W. K.H. Panofsky

High resolution electron scattering from nuclei by R.Hofstadter

SLAC: The two-mile accelerator

“Project M”

1955 first brainstorming and informal discussions

SLAC CHRONOLOGY

April 1957	Proposal for two-mile accelerator submitted by Stanford University to Federal Government
September 1961	Project authorized by U. S. Congress
April 1962	Contract signed by U. S. Atomic Energy Commission and Stanford University
July 1962	Ground breaking; construction begins
July 1964	Start of accelerator installation
October 1, 1965	First "Users Conference," attended by 150 people from laboratories all over the world, to be made acquainted with SLAC.
December 1965	Installation of accelerator complete
February 12, 1966	Program Advisory Committee met, and approved and scheduled the first experiments to be performed with the two-mile beam
May 21, 1966	First beam transmitted over entire two-mile length of the accelerator
June 2, 1966	18.4 GeV of beam energy achieved
June 22, 1966	Second "Users Conference" held at SLAC
July 13, 1966	Positrons accelerated
October 17, 1966	First interlaced multiple beams of different energies and intensities accelerated
November 1966	Experiments begin with the beam in the end stations
January 10, 1967	20.16 GeV of beam energy achieved

- \$100M proposal
- numerous studies and reports
- > 10 years of effort

First beam at SLAC, 1966

Was SLAC worth building?

- 1968: First evidence of Quarks
- 1974: Discovery of the ψ particle
- 1976: Discovery of the charm quark and the τ lepton
- 1997: The BaBar experiment
- 2009: LINAC coherent X-ray source

Other developments

- SSRL user facility
- Computer science, software
- KIPAC Particle Astrophysics

Existing SLAC - 50 GeV

Proposed ILC Accelerator 1 TeV

The goal of the Laser Electron Accelerator Program - LEAP - is to invent a new approach that will allow TeV physics on the SLAC site.

To achieve the goal we need an acceleration gradient of 1 GeV per meter.

The Livingston curve

1. near-exponential growth in the beam energy up until about 1990
2. LHC and future NLC/ILC lie below the exponential growth curve
3. Exponential curve important for new physics

RF based accelerator technology is nearing its practical high-energy limit

For future high energy collider facilities beyond the LHC and ILC it becomes increasingly appealing to invest in new accelerator technologies

Future Goal

Maximum gradient $\sim >1\text{GeV/m}$

Klystron Microwave generators (1930s)

Varian brothers

M Chodrow

E.L. Ginzton

RF linear accelerator;
based on
Klystron technology
(1930s)

Diode pumped solid state lasers

**efficient
pump
diodes**

πLIGHT

60 W/bar, 50%
electr. efficiency

**high peak –
power lasers**

**ultrafast laser
technology**

< 10 fs

MenloSystems GmbH

**Features of
interest to us**

- very compact, tabletop systems
 - optical phase control
 - pump diodes → 50% efficiency
 - solid state gain medium → 80%
- Overall wallplug efficiency > 30%
- possibility for ultra-short pulse operation (100 fsec or shorter)

Laser-driven particle acceleration

- Idea came about soon after the invention of high-peak power lasers (earliest articles go back to 1971)
- different laser particle acceleration concepts
 - ponderomotive
 - **linear electric field**
 - inverse cherenkov
 - inverse FEL
 - active gain medium
 - laser driven plasma wakefield
 - ...
- experimental demonstrations are fairly "recent"
- still a controversial topic

NETWORKING IN THE IMMUNE SYSTEM • NANOTECH BATTERIES

SCIENTIFIC AMERICAN

How to Protect
New Orleans
from Future Storms

FEBRUARY 2006
WWW.SCIAM.COM

Big Physics Gets Small

Tabletop Accelerators
Make Particles Surf on
Plasma Waves

How to Stop
Nuclear Terrorists

Guess Who
Owens Your Genes?

CSI: Washington (George, that is)

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TABLETOP ACCELERATORS producing electron beams in the 100- to 200-million-electron-volt (MeV) range are just one type of machine made possible by plasma acceleration.

PLASMA ACCELERATORS

A new method of particle acceleration in which the particles "surf" on a wave of plasma promises to unleash a wealth of applications

By Chandrashekhar Joshi

www.sciam.com

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SCIENTIFIC AMERICAN 41

BOOSTING A CONVENTIONAL ACCELERATOR

A major experiment using the Stanford Linear Collider (SLC) could demonstrate the feasibility of so-called plasma wakefield afterburners to boost the energy of an otherwise conventional accelerator. The afterburners (inset), consisting of two 10-meter-long units installed at the end of the 50-GeV electron and positron beams of the three-kilometer-long SLC, would double the beams' energies to 100 GeV. Plasma lenses would help to focus the doubled beams to collide at a small point. For technical reasons, the electron afterburner would be filled with plasma, but the positron afterburner would have a hollow axial channel. This proposed experiment has not yet been funded.

"An accelerator is just a transformer" – Pief Panofsky

"All accelerators operate at the damage limit" – Pief

"To be efficient, the accelerator must operate in reverse"

– Ron Ruth, SLAC

" It is not possible to accelerate electrons in a vacuum"

Lawson – Woodward theorem

"An accelerator requires structured matter – a waveguide –
to efficiently couple the field to the electrons" Bob Siemann

1974 –sabbatical leave, Lund **1994** – SLAC summer school **2004** – Successful 1st Exp

Infrared and Optical Masers

A. L. SCHAWLOW AND C. H. TOWNES*
Bell Telephone Laboratories, Murray Hill, New Jersey
(Received August 26, 1958)

The extension of maser techniques to the infrared and optical region is considered. It is shown that by using a resonant cavity of centimeter dimensions, having many resonant modes, maser oscillation at these wavelengths can be achieved by pumping with reasonable amounts of incoherent light. For wavelengths much shorter than those of the ultraviolet region, maser-type amplification appears to be quite impractical. Although use of a multimode cavity is suggested, a single mode may be selected by making only the essential part of the resonant cavity a suitably small angular aperture. Then extremely monochromatic and coherent light is produced. The design principles are illustrated by reference to a system using potassium vapor.

high power solid state lasers

The Hoya Production II laser glass melting campaign was completed, yielding over 1700 amplifier slab blanks

Target chamber with steel frameworks for catwalks being installed.

60 W/bar
50% now **78%**
electrical
efficiency

high power fiber lasers

NUFERN

ALABAMA LASER

modelocked laser technology

FIG. 1. Frequency chain that allows the comparison of the precisely known frequency of a methane stabilized He-Ne laser at 88.4 THz (3.39 μm) with the cesium D_1 transition at 335 THz (895 nm).

Fig. 2. Interferences between two separate NOFAs for various center wavelengths.

Improve efficiency of bar from 50%-80% damage threshold of dielectric materials

Stuart, et. al., Phys Rev. Lett. Vol 74, No 12 p. 2248 (1995)

10 GV/m fields for 100 fsec laser pulses

Contents

Historic Background

Laser Electron Accelerator Project - LEAP

HEPL Experiments from 1997 - Nov 2004
Future E163 Experiments at SLAC

The TeV-Energy Physics Frontier

Future Opportunities

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"Don't undertake a project unless it is manifestly important and nearly impossible." **Edwin Land - 1982**

Laser Electron Accelerator Project - LEAP

E-163

Laser driven particle acceleration

collaborators

ARDB, SLAC

Bob Siemann^{*}, Bob Noble[†], Eric Colby[†], Jim Spencer[†], Rasmus Ischebeck[†], Melissa Lincoln[†], Ben Cowan[†], Chris Sears[†], D. Walz[†], D.T. Palmer[†], Neil Na[†], C.D Barnes[†], M Javanmarad[†], X.E. Lin[†]

Stanford University

Bob Byer^{*}, T.I. Smith^{*}, Y.C. Huang^{*}, T. Plettner[†], P. Lu[†], J.A. Wisdom[†]

ARDA, SLAC

Zhiu Zhang[†], Sami Tantawi[†]

Technion Israeli Institute of Technology

Levi Schächter^{*}

UCLA

J. Rosenzweig^{*}
and

DOE

David Sutter

[†] grad students

^{*} faculty

Participants in the LEAP Experiment Laser Electron Accelerator Program

Bob Siemann²

Chris Sears²

Ben Cowan²

Jim Spencer²

Tomas Plettner¹

Bob Byer¹

Eric Colby²

New students

- Chris McGuinness²
- Melissa Lincoln²
- Patrick Lu¹

Atomic Physics collaboration

- Mark Kasevich³
- Peter Hommelhoff³
- Catherine Kealhofer³

- 1 E.L. Ginzton Laboratories, Stanford University
- 2 Stanford Linear Accelerator Center (SLAC)
- 3 Department of Physics, Stanford University

1

Energy gain through longitudinal electric field

- gradient = longitudinal electric field
- linear e-beam trajectory
→ no synchrotron radiation
→ **energy scalable**

$$\Delta U = \int E_z \cdot dz$$

2

Dielectric based structure with vacuum channel

Gradient $\rightarrow 1 \text{ GeV/m}$

$E_s \rightarrow 10^{10} \text{ V/m}$

3

Inherent attosec electron pulse

2 μm laser $\rightarrow 6 \text{ fsec period}$
 $\rightarrow 1 \text{ deg of phase} = 20 \text{ attosec}$

The interaction length is limited by phase drift to less than 400 microns

(a)

The properly phased crossed laser beams have zero transverse field and only a longitudinal field component E_z .

(b)

An early concept for resetting the phase every 334 microns to keep the electrons and the applied laser field phased.

The SCA Accelerator provided a source of 30MeV electrons for LEAP

The crossed-beam laser accelerator
Cell and magnet for electron beam
energy measurements.

(b)

The view of the 30 MeV super-conducting
linear accelerator in the underground
tunnel on campus in the HEPL lab.

The LEAP experiment (Laser Electron Accelerator Project)

We have accelerated electrons with visible light!

The simplified single stage Accelerator cell that uses gold coated Kapton tape to terminate the Electric field.

The LEAP experimental apparatus that Includes the LEAP single stage accelerator cell and the inverse FEL.

The key was to operate the cell above damage threshold to generate energy modulation in excess of the noise level.

Energy Modulation vs Laser Peak Electric Field

(This is a modest laser with ~200 micro Joules in 4psec)

We accelerated electrons with visible light

PRL **95**, 134801 (2005)

PHYSICAL REVIEW LETTERS

week ending
23 SEPTEMBER 2005

Visible-Laser Acceleration of Relativistic Electrons in a Semi-Infinite Vacuum

T. Plettner and R. L. Byer

Stanford University, Stanford, California 94305, USA

Colby, B. Cowan, C. M. S. Sears, J. E. Spencer, and R. H. Siemann
SLAC, Menlo Park, California 94025, USA
(Received 19 April 2005; published 22 September 2005)

- confirmation of the Lawson-Woodward Theorem

- observation of the linear dependence of energy gain with laser electric field

- observation of the expected polarization dependence

$$\int_{-\infty}^{+\infty} E_z dz = 0$$

$$\Delta U \propto |E_{laser}|$$

$$|E_z| \propto |E_{laser}| \cos \rho$$

Tomas Plettner - Experimental Success

E-168
Byer
Group

High-Harmonic Inverse-Free-Electron-Laser Interaction at 800 nm

Christopher M. S. Sears, Eric R. Colby, Benjamin M. Cowan, Robert H. Siemann, and James E. Spencer
Stanford Linear Accelerator Center, Menlo Park, California 94025, USA

Robert L. Byer and Tomas Plettner
Stanford University, Stanford, California 94305, USA
(Received 4 March 2005; published 2 November 2005)

Cross-correlation in time

FIG. 2. Example data run with 1500 laser on events. The solid curve is the least squares fit to all data points and gives the mean interaction of 18 keV. The dashed curve is the maximum estimate and gives the peak interaction of 25 keV. The width of cross correlation is 2.2 ps rms.

Observation of harmonic interaction

FIG. 4. IFEL gap scan data, with 164 runs total. Comparison to simulation (solid line) shows very good agreement to the shape and spacing of resonance peaks. The harmonic numbers are given next to each peak. Simulation has been rescaled vertically by 0.67 to better visualize overlap.

* graduate student C.M. Sears

Chris Sears - LEAP Notebook #7

E-168
Byer
Group

Jim Spencer - LEAP success!

E-168
Byer
Group

LEAP Control Room - HEPL Nov 2004

E-168
Byer
Group

Ben Cowan Tomas Plettner Chris Sears Jim Spencer

LEAP Team - Feb 2006

E-168
Byer
Group

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Planar waveguide structures

Z. Zhang et al. Phys. Rev. ST AB 8, 071302 (2005)

Hollow core PBG fibers

X.E. Lin, Phys. Rev. ST Accel. Beams 4, 051301 (2001)

SLAC National Accelerator Laboratory

Periodic phase modulation structures

T. Plettner et al, Phys. Rev. ST Accel. Beams 4, 051301 (2006)

3-D photonic bandgap structures

B. M. Cowan, Phys. Rev. ST Accel. Beams, 6, 101301 (2003).

"Laser Accelerators"

July 7, 2009

Energy efficiency of laser accelerators, single and multiple bunch operation

PHYSICAL REVIEW SPECIAL TOPICS - ACCELERATORS AND BEAMS, VOLUME 7, 061303 (2004)

PHYSICAL REVIEW SPECIAL TOPICS - ACCELERATORS AND BEAMS 8, 031301 (2005)

Energy efficiency of laser driven, structure based accelerators

R. H. Siemann

Energy efficiency of an intracavity coupled, laser-driven linear accelerator pumped by an external laser

Y. C. Neil Na and R. H. Siemann

Stanford Linear Accelerator Center, Stanford University, Stanford, California 94309, USA

R. L. Byer

Edward L. Ginzton Laboratory, Stanford University, Stanford, California 94305, USA

(Received 26 January 2005; published 11 March 2005)

Coupling Efficiency vs bunch charge

Beam loading calculations vs N

The E163 experiment at SLAC

The new E163 experiment hall

The NLCTA

The E163 experiment at SLAC

Accomplished milestones so far

- construction of the experiment hall
- installation of the E163 control room
- commissioning of the laser system
- installation and commissioning of the RF gun

Expected 1st experiment in autumn 2006

NLCTA Accelerator - Injector

E163 Experimental area

The next step: staged accelerators at E163

We have achieved net acceleration of electrons with attosecond phase control

Expected bunching

Expected energy gain

Experiment features

- IFEL modulates energy spread
- electron drift creates optical bunches
- second accelerator → net acceleration

Chris Sears Thesis Defense

E-168
Byer
Group

Future Experiments

Goal: test multiple stage acceleration

E-163

Cascading of microstructure accelerators

Professor Robert Siemann and Chris Sears June 15, 2008 - Stanford Graduation Ceremonies

E-168 Byer
Group

Robert H. Siemann – encounters and essays

Robert H. Siemann—encounters and essays - Mozilla Firefox

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APS » Journals » Physical Review Special Topics - Accelerators and Beams » Robert H. Siemann—encounters and essays

Robert H. Siemann—encounters and essays (December 1, 2008)

We deeply regret the passing of our friend and colleague, Robert H. Siemann, on September 16, 2008. He was the founding Editor of Physical Review Special Topics - Accelerators and Beams. We are now publishing a dedicated section of essays in memory of Bob Siemann. The essays are from different angles of Bob's remarkable career and life. We hope these anecdotes and educational remarks are a benefit to our readers.

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Tor O. Raubenheimer (Published 16 December 2008)
- **Essay: Bob Siemann and PRST-AB**
Martin Blume (Published 17 December 2008)
- **Essay: Bob Siemann and the Spallation Neutron Source: A remembrance**
Stuart D. Henderson (Published 18 December 2008)
- **Essay: Robert H. Siemann—a great scientist, teacher, mentor, and friend: From LEP and SLC to advanced accelerators**
Ralph W. Akmann (Published 22 December 2008)

Laser Electron Acceleration Group

Chris	Bob	Bob	Eric	Chris
McGuinness	Siemann	Byer	Colby	Sears

Rasmus	Chris	Ben	Tomas	Jim
Ischebeck	Barnes	Cowan	Plettner	Spencer

Bob Noble
Dieter Walz

past collaborators

Y.C. Huang
T.I. Smith
H. Wiedemann

Low-energy electron laser acceleration group

Bob Byer
Patrick Lu,

Anthony	Catherine	Peter
Serpy	Kealhofer	Hommelhoff

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2. Low bunch charge problem

- Take advantage of high laser repetition rate
- Multiple accelerator array architecture

Laser pulse structure that leads to high electron bunch repetition rate

	SLC	NLC	SCA-FEL	TESLA	laser-accelerator
f_{RF} (GHz)	2.856	11.424	1.3	1.3	3×10^4
f_m (Hz)	120	120	10	4	10^4
N_b	1	95	10^4	4886	10
Δt_b (nsec)	-	2.8	84.7	176	3×10^{-6}
f_b (Hz)	1.2×10^2	1.1×10^4	1×10^5	1.6×10^4	3×10^6
N_e	3.5×10^{10}	8×10^9	3.1×10^7	1.4×10^{10}	10^4
I_e (sec ⁻¹)	4×10^{12}	9×10^{13}	3×10^{12}	2×10^{19}	3×10^{10}

Requires 10kW/meter or 10MW/km and ~40% efficiency Laser Source!

(~ 10 microjoules in 100fsec per micropulse)

Dramatic increase of

- electric field cycle frequency $\sim 10^{14}$ Hz
- macro pulse repetition rate ~ 1 GHz

target gradient G 1 GeV/m

laser pulse duration T_{laser} 1 psec

electron bunch duration T_e 20 attosec

total beam current I_b

10 μ A

total beam power at 1 TeV P_b

10 MW

TeV scale laser accelerator parameters

accelerator field wavelength	λ	2 μm
laser pulse repetition rate	f	1 GHz
bunches per laser pulse “macro-pulse”	n	10 } $10^{10}/\text{sec}$
electrons / bunch	N	~ 6000 (1 fC)
accelerator beam diameter	σ	0.1 μm
beam diameter at IP focus	σ	0.1 $\text{\AA}$
transverse geometric emittance	ε	10^{-11} m-rad
β at IP	β_0	10 μm
approximate luminosity at IP	$L \approx \frac{nfN^2}{4\pi\sigma_x\sigma_y}$	$\sim 10^{34}/\text{cm}^2\text{-sec}$

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RF-accelerator driven SASE FEL at SLAC - 2009

TTF: Tesla Test Facility; fsec EUV SASE FEL facility
XFEL: Proposed future coherent X-ray source in Europe...

LCLS properties

- 1 km-size facility
- microwave accelerator
- $\lambda_{RF} \sim 10$ cm
- 4-14 GeV e-beam
- 120 m undulator
- 23 cm period
- 15-1.5 A radiation
- 0.8-8 keV photons
- 10^{14} photons/sec
- ~ 77 fsec
- **SUCCESS – April 09**
- **1mJ per pulse**
- **10 Hz**
- **8 keV X-ray photons**

"Laser Accelerators"

Question: can we generate coherent X-rays
With table top laser accelerator?

Reference: First suggestion that Laser accelerator could drive an X-ray FEL.

Y. C. Huang and R. L. Byer, "Ultra-Compact, High-Gain, High-Power Free-Electron Lasers Pumped by Future Laser-Driven Accelerators," in Free Electron Lasers 1996, G. Dattoli and A. Renieri, eds. (Elsevier Science B.V., 1997), pp. II-37-II-38.

attosec light sources

Take advantage of ultra-low emittance laser-accelerator e-beam and new magnetic materials

Table Top attosec x-ray source with medical and chemistry applications

1° of optical phase at 2 μm $\rightarrow$ 20 attosec

Preliminary model studies

- 1st initial feasibility study with the 1D FEL model
- Attosec bunching of 1fC helps enhance the gain
- “low” 1 MHz rep. rate $\rightarrow$ low avg. power
- Further more refined studies under way
- It deserves a closer look

The Key Components of the SASE-FEL architecture

SASE - Self Amplified Spontaneous Emission

source of free particles

laser-driven
high rep. rate
very compact

accelerator section

dielectric structure
based laser-driven
particle accelerators

undulator

dielectric structure,
laser driven

Concept: Summer 2007

Field envelope growth

$$\frac{\partial}{\partial t} \tilde{E}(z', t) = -\chi_2 \left(\sum_{j \in \Delta} e^{-i\psi_j} / v_\Delta \right)$$

Smallest possible beam size

electrons per unit volume

$\phi_b < 500 \text{ nm}$

develop MEMs based Dielectric laser-driven accelerator structures

Objective:

1. Dielectric optical MEMs structures
2. High acceleration gradients ($\sim 1 \text{ GeV/m}$)
3. Mono-energetic, maintenance of low emittance

Proposed parameters for laser driven SASE-FEL (Theoretical Study of FEL operation)

Attosecond X-ray FEL Source - detail

New Idea: Laser-Driven Dielectric Undulator for FEL

accelerator structure

deflection structure

$$\langle \vec{E}_{\perp} + (\vec{v} \times \vec{B})_{\perp} \rangle = 0$$

$$\langle \vec{E}_{\parallel} \rangle \sim \frac{1}{2} E_{\text{laser}} \rightarrow \sim 4 \text{ GeV/m}$$

$$\langle \vec{E}_{\perp} + (\vec{v} \times \vec{B})_{\perp} \rangle \neq 0$$

$$\langle \vec{F}_{\perp} / q \rangle \sim \frac{1}{5} E_{\text{laser}} \rightarrow \sim 2 \text{ GeV/m}$$

key idea

Extended phase-synchronicity between the EM field and the particle
Use modelocked laser to generate periodic magnetic field

T. Plettner, "Phase-synchronicity conditions from pulse-front tilted laser beams on one-dimensional periodic structures and proposed laser-driven deflection", submitted to Phys. Rev. ST AB

The calculated phase-synchronous force components

A dielectric structure laser driven undulator

- **same loss factor as the laser accelerator: $\sim 100 \text{ GV/m/pC}$**
- **similar MEMS structure geometry $\rightarrow$ fabrication compatibility**

Calculated FEL Performance - 0.1 Angstrom X-rays (Pulse duration of X-rays - 5 attoseconds)

$$\begin{aligned} U_b &= 2 \text{ GeV} \\ \varepsilon_N &= 10^{-9} \text{ m-rad} \\ Q_b &= 20 \text{ fC} \\ \Delta\gamma/\gamma &= 0.1\% \\ \sigma_r &= 200 \text{ nm} \\ \beta^* &= 4 \text{ cm} \end{aligned}$$

$$\begin{aligned} L_c &\sim 21\lambda_r \\ \sigma_b &\sim 136\lambda_r \end{aligned} \quad \rightarrow \quad \sigma_b / L_c \sim 6$$

G. Dattoli, L. Giannessi, P.L. Ottaviani, C. Ronsivalle, J. Appl. Phys. 95, 3206 (2004)

Schematic of the tabletop radiation source

There is a path forward based on a modelocked laser driven dielectric structure

Future: Experiments to be conducted at SLAC

E-163

Test of laser-deflection Structure

Prove the concept of a phase-synchronous deflection force

Look for undulator radiation

$$N_{ph} = \pi\alpha \frac{K^2}{(1+K^2/2)^2} \left(J_1 \left(\frac{K^2}{4(1+K^2/2)^2} \right) - J_0 \left(\frac{K^2}{4(1+K^2/2)^2} \right) \right)$$

Prove the concept

$$\text{measure} \left\{ \begin{array}{l} K \propto \lambda_u \\ \Delta\omega/\omega = 1/N_u \\ \Delta\theta = \sqrt{2\lambda_r/\lambda_u N_u} \end{array} \right.$$

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1

Staged acceleration

- precise control of optical phase
- control of focusing and steering of the electron beam

2

Implementation of real accelerator microstructures

- fabrication
- coupling of the laser
- electron beam transmission
- survival of the radiation environment
- heat removal

3

Laser technology

- wavelength 2 μm
- optical phase control
- wallplug efficiency
- lifetime

Selected publications

1. Y.C. Huang, D. Zheng, W.M. Tulloch, R.L. Byer, "Proposed structure for a crossed-laser beam GeV per meter gradient vacuum electron linear accelerator", *Applied Physics Letters*, 68, no. 6, p 753-755 (1996)
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3. X. Eddie Lin, "Photonic band gap fiber accelerator", *Phys. Rev. ST Accel. Beams* 4, 051301 (2001)
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10. C.M.S. Sears, E. Colby, B. Cowan, J. E. Spencer, R.H. Siemann, T. Plettner, R.L. Byer, "High Harmonic Inverse Free Electron Laser Interaction at 800 nm", *Phys. Rev. Lett.* 95, 194801 (2005)
11. T. Plettner, R.L. Byer, E. Colby, B. Cowan, C.M.S. Sears, J. E. Spencer, R. H. Siemann, "Proof-of-principle experiment for laser-driven acceleration of relativistic electrons in a semi-infinite vacuum", *Phys. Rev. ST Accel. Beams* 8, 121301 (2005)

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- R. Route, V. Drew
- B. Noble, D. Walz (ARDB, SLAC)
- Y.C. Huang, C. Barnes, S. Waldman, J. Wisdom
- S. Sinha, R. Gaume, S. Wong
- J. Rosenzweig

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BACK UP SLIDES

historical trend of high energy physics experiments

TeV e^+e^- collision experiments

- Top Quark Physics
- Higgs Boson Searches and Properties
- Supersymmetry
- Anomalous Gauge Boson Couplings
- Strong WW Scattering
- New Gauge Bosons and Exotic Particles
- e^+e^- , $e^+\gamma$, and $\gamma\gamma$ interactions
- Precision Tests of QCD

The NLC ZDR Design Group and the
NLC Physics Working Groups
Snowmass '96 workshop

The tape boundary

Improve on

- Operation tolerances
- Poor reliability
- Ease of operation

1. Damage threshold

- ignore it!
- devise a "disposable" unit
- materials retain their optical properties for a few picoseconds after a destructive laser pulse

2. Cell geometry

- simplify to one semi-infinite boundary
- make boundary thin enough to run e-beam through it
- make boundary movable to present a new surface for each laser shot

3. Crossed laser beams

- two laser beams too difficult? → eliminate one of them
- no more optical phase uncertainty problems
- negligible transverse deflection forces

Conceptual drawing
of the improved setup

The tape boundary

a) Setup for the reflected spot measurements

reflected spot camera images

- i) low power, below damage threshold
- ii) 1st shot, above damage threshold

b) Reflected pulse intensity versus laser pulse duration

Radiation damage studies

IEEE TRANSACTIONS ON NUCLEAR SCIENCE, DECEMBER 2002

Gamma Radiation Studies on Optical Materials

Eric Colby, *Member*, Gary Lum, *Member*, Tomas Plettner and James Spencer, *Member, IEEE*

Fig. 4. Transmission spectra through 500 μm wafers of Quartz (upper group of curves) and Silicon (lower group) as a function of integrated dose (Si).

Fig. 6. Comparison of spectra from 0.20-10 μm for different forms of Si in Si equivalent dose. Spectra were matched at 3.2 μm .

Fig. 5. Transmission spectra through a thin wafer and a thick (6.35 mm) GaAs wafer as a function of integrated dose (Si). The water was scaled downwards by 0.23 for better comparison.

Fig. 1. Transmissivity spectra through 1.1 cm thick plate glass after $\text{Cd}^{10}\gamma$ -irradiation. Spectra are stacked according to their order in the insert.

Laser damage threshold studies

material	air	vacuum
CaF ₂	2.212	2.260
borofloat	2.064	2.054
fused silica	2.027	1.795
sapphire	1.574	1.152
MgF ₂	1.455	1.455
800 nm HR	0.769	0.768
LiNbO ₃	0.504	0.194

OPTICAL PHASE LOCKING OF MODELOCKED LASERS FOR PARTICLE ACCELERATORS*

T. Plettner, S. Sinha, J. Wisdom, Stanford University, Stanford, CA 94305
E. Colby, SLAC, Menlo Park, CA, 94025

(A) Laser dispersion control

(B) Comb offset error signal generation

(C) Error signal, square wave modulation

(D) Error signal, sawtooth wave modulation

Wavelength (nm)

In Memory of Bob Siemann

Ben Cowan

Last September, when I heard that Bob had passed away, I was naturally quite saddened – and I was also surprised. Of course, I knew that Bob had been fighting health problems for several years, but through it all he always exuded a great optimism. I remember eating lunch in the courtyard with some other students when Bob returned to SLAC after being away for treatment for a few months, and he came right out to join us. He was excited to return to work, to catch up with us, and with all the advanced accelerator work that was going on – and to give me a gentle push to get more of my thesis written. He really knew that each day would bring new ideas, new discoveries, and new possibilities, and he wanted to be part of every minute of it.

The great thing about this optimism and enthusiasm that Bob had in such great supply was that it was so contagious. Being around him, and working with him, I couldn't help but be confident that success wasn't far off, and that the next modification to the experimental setup, or the simulation process, no matter how small, would yield a breakthrough.

In the laser acceleration project, optimism was really something of a necessity. As you just heard from Bob Byer, it was a very long road from the conception of the experiment, through the instrumentation of the setup, to the results. The experimental work was quite intense. The accelerator we used was superconducting, and quite expensive and time-consuming to cool down. So once it was ready, it was important to squeeze every last ounce of data out of the time we had with it. That meant that there would be months – sometimes years – of preparation work building up to an experimental run of just a few weeks. You really had to believe that the next run would be the one that would yield the great result. And during the run, Bob would be there to inspire confidence in the next shift, and even the next data set.

I'd like to show you a little bit of the environment of this experiment. Of course, we'd run around the clock, in shifts. The action was centered in our control room; here's what it looked like. If it seems to be about as big as a closet in this picture, that's because it was. And this was taken in 2000, and in subsequent years it only got more crowded. Bob would usually be there from 8 in the morning until about 8 at night, every day of the run. It would be amusing to see someone of his size, sitting in this tiny room, hunched over his tiny laptop, methodically analyzing every data set. His size was sometimes a point of contention between us, since he wouldn't think twice about installing an important piece of equipment on the very highest

shelves, and then I'd arrive for the night shift and not be able to reach it. But then, I'd install things under tables or tucked away in corners that were hard for him to reach.

Of course Bob wasn't just around for the experimental runs; he'd be actively involved in the preparation as well, often taking the lead on key diagnostics. Here's the first of two streak cameras he set up for the experiment. His depth of electronics knowledge was also invaluable to deciphering some of the systems we worked with. Here's some instructions he wrote down on reviving one of the feedback loops. We all quickly learned that the effort required to read Bob's handwriting was well worth it. And not only would he be there to provide encouragement and advice in difficult times, he'd also be there to celebrate our successes.

As the LEAP experiment at HEPL was reaching its conclusion, an effort was ramping up to develop a new class of optical accelerators, based on photonic crystals and other microstructured geometries. What was amazing to me, working on this topic as my thesis project, was that Bob didn't have any formal training or extensive background in photonics, and yet he'd always be able to provide insight and advice that kept the work moving forward.

This is something that has been remarked upon several times today: Bob's accessibility to his students. The optimism that Bob engendered didn't come just from kind words; it came from always being available to get advice from him or just bounce ideas off him. We knew that we could always count on Bob in a tough situation. And he made us, his students, a priority, making sure we had the resources we needed. He would often utter the words that a student, especially one working on an experiment, wants to hear (second only, of course, to "your thesis looks good, you can go ahead and defend"): He'd usually say, when we needed to get some equipment, "just write a purchase order."

Bob made students a priority even over some of the more prestigious activities that come to scientists of Bob's accomplishments. I remember discussing something with him in his office when he got a phone call related to some administrative responsibility, and he almost gleefully told the caller that he'd have to call back, since he had a student in his office. In fact, Bob had a sanctuary where he could concentrate fully on science. That was building 223, our electronics prep area. And you can tell how important that was to him by the occasional emails he'd send out reminding us gently (most of the time) to keep it clean and well stocked. (The label maker was one of his favorite pieces of equipment. Bob had one bin labeled the "clearly something bin" for miscellaneous mystery items.)

Speaking of Bob's approachability in the face of high-level responsibilities, I remember when he was chair of the PAC 03 conference. His consolation prize, I guess you could say, for the responsibility of organizing the conference was the presidential suite at the Portland Hilton. So of course, he invited the whole group, including the students, up for a party one evening. The status he had earned just became another opportunity to have a good time, like so many celebrations at the Alpine after a new result or successful thesis defense. In fact we just joked about the setting, especially the presence of a mysterious bright red button in the bathroom of the suite; Bob speculated that it meant "eject."

As another example of Bob's humility, I remember trying to find a shelf I could use to mount some equipment on a rack in that LEAP control room you saw. I couldn't find one anywhere, and the stockroom didn't have any, but a day or two later he managed to find one. When questioned about where he got it, he freely admitted to finding it while dumpster diving in the research yard. Another student recalls Bob setting up BPMs for E163, the successor to the LEAP facility. He always stayed in direct touch – literally – with the experiments.

Bob's approachability, his down-to-earth character, made him mean much more to me than a mentor and scientist; he was also a friend. I could never have made it through grad school without him. And though I'm no longer at SLAC, I think of Bob often – especially when I'm walking around my neighborhood, since I happen to live just a few hundred feet from his daughter in Longmont, Colorado. I will always be grateful to him. Thank you, Bob.

In memory of Bob Siemann

Ben Cowan

Recipe for fixing the 11.8/82.7 MHz box in FEL1 that is the heart of HEPH

- 1) Required tool $5/16"$ wrench in drawer on left backside of LAMP desk (where connectors are located). Use grips on tool (wrench FEL3)
- 2) Look at monitor panel - cupilling monitor on back of box. It is probably on the negative rail (-12V). This is not good
- 3) Remove the box that is the heart of HEPH from the rack & remove its cover. I never knew I will see a short gold cable with one end soldered to circuit board and the other end on SMA that connects to a filter
- 4) Using the surgical tool kit, disconnect the cable carefully watching the monitor. Heat shorts positive reconnected it. If it looks it will settle at $\sim 3-5V$.
- 5) Close the box if successful, sterilized return tool

This is not a joke!

Kubler turned to zero cross 5.474

18:30 Kubler appears to be fixed - cool motion. No smoke, fine driving downstream

to after 1 mm from edge (inter beam)

next attempt at Iik run 308 - ref, problem with
operation of DAA

Run 309 - just ITR run - no obvious sign of insects

0316 = 2nd IIR run

0311 - 3rd ITP under time range 300 - 420 p.p.m.
~~approach becoming unstable... data away and be resampled~~

6312 - 365-405 negative at 392 - !! $\Delta_{100}^{100} \text{H}^{100}$

0313 - repeat of 0312 for all of Argentina at 392 Revvinto

Signal appears!
We have seen IIR
signal

0314 - time seen 390-400
Wood plot 19,5'

0315 Polymerization plate
rotated 90° from
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Bob Siemann and the Community Organizer

Mel Month
USPAS Founding Director
Longtime DPB Secretary-Treasurer

Bob Siemann was a good man. It's not that everything he did was right and proper, or not even that he had a moral edge. No! Like most, sometimes his judgment failed him. Sometimes he did something that hurt someone. So, come to think of it, maybe it's better to see and describe him as a principled man. Before acting, he would instinctively balance the scale of right and wrong, and more often than not, what he did moved our science forward. It's not that the end justified any means he used, but to him it was important, not paramount mind you, but important to leave his chosen field better than he found it. And in that he succeeded.

I met Bob sometime in the eighties. It was probably in 1985 at the Vancouver PAC Conference. There, I ran all over the place with a clipboard trying to accumulate signatures for a petition to form an APS Topical Group (TG) on beams and accelerators. What fun it was cornering people in the effort to persuade them that beams had scholarly merit and so should be represented as a distinct field of study in its own right. In doing this organizing work, as memory recalls, I had a lot of contact with Bob and came to see a mature philosophical side to his nature. I was convinced that work on beams was worthy of scholarly merit, that's why I was there, but he would not easily concede and tended to play devil's advocate. Our talk then was no more than bantering. We both knew he would add his name to the petition. But, as we would discover in a couple of years, this back-and-forth had only been a prelude to discussions about the USPAS method, discussions that would lead to his deep involvement in the accelerator school.

Now, for the record, allow me to take a brief detour and tell you a little story of how the APS Division of Physics of Beams (DPB) got started. As spirited as my Vancouver discussions were with Bob, they were equally so with others the likes of Richter, Panofsky, Lederman, Wilson and Wallenmeyer. And when the dust settled, I found I had over 250 signatures, ten times more than needed according to the APS rules for TG formation. More important than the numbers, the list boasted many of the big boys. Yet, in spite of the size and quality of the petition, permission to form a beams topical group was not a done deal. Most of you probably don't know the inside story of how the petition fared in its travels to find a place for beams in the APS, so let me tell it to you. The APS Executive Committee (EC) met sometime later in 1985 with the matter of the beams topical group on the agenda. There, Havens, the EC Executive Secretary, called for a decision on the petition of the beams community. Almost immediately, Sid Drell, the APS President, objected, saying that beams were an integral part of high-energy and should remain represented within the Division of Particles and Fields (DPF). A topical group on beams would divide the two allies, he said, and, in the end, would be detrimental to both. For a bit after the president spoke, the room was silent. But, just when Havens was about to table the motion, Bob Wilson, the Past President, stepped in. He countered Drell with an eloquent defense of the accelerator field as separate and distinct and claimed that a Beams Topical Group would open the door to the participation of other areas of physics requiring beams and thereby become an even greater asset to high-energy. Well, to make a long story short, Drell backed off, the motion for a beams topical group was passed, membership flourished and, within a year or so, the magic

1000 mark was reached and the topical group was upgraded to a bona-fide APS division. I'm not sure why Wilson spoke up like that at the last minute, but maybe that hour-long discussion I had with him about a month before the EC meeting played a part.

During the eighties, USPAS was changing and advancing at a fast clip. In 1987, in conjunction with the University of Chicago, the university-style courses were initiated. In 1988, the second such school was held at Cornell. Bob Siemann, then on the Cornell physics faculty, agreed to give a course on "Beam Simulation". He was a tireless teacher and really gave his students a run for their money. During those two hot Ithaca weeks in August of eighty-eight, though he was intensely occupied with the fine-tuning of his lectures, Bob and I found the time to talk and became close friends. There was one issue that clung to us and, in the coming years, we would argue endlessly about the relative merits of a USPAS course as compared to a standard university course. The formal contact hours are much the same, but in USPAS they are concentrated in a 2-week period, while in the university they are spread out over many more weeks. He argued that the time was crucial. For deep and difficult concepts, he would say, the mind needs the time to absorb new ideas so that it can work its miracle of learning. I, on the other hand, argued that immersion provided the mind with the raw material, stimulating it to build a base of knowledge that in time would make for learning of an equal or even greater depth. I further argued that concentration, isolation and nighttime study with teacher contact, all characteristic of USPAS courses, is more conducive to learning than the dilute lecture schedule, competing courses and interfering life activities that prevail in the university environment.

During the years that followed, Bob and I took every opportunity we got to see each other and I came to know his varied talents. Early on, I realized that he was a perfect candidate for an administrative role in the School. After a few conversations about this, he was finally persuaded to take on the job of Program Committee Chairman on a quasi-permanent basis. Understand that Bob was a man with gravitas. He could access the top and the men there would listen to him. Many a time, he would intercede in a disagreement among USPAS Board members as to whether USPAS should or should not do more. In such instances, he and I were a formidable growth-promoting duo. On program matters, he would inevitably come down on the side of curriculum expansion. This turned out to be a key factor in permitting USPAS, in 1992, to move from annual to semi-annual programs. Not only was he a welcome administrative ally, he was also a hands-on type of guy. For the Harvard school in the summer of 1993, I asked him to introduce an innovative and difficult program – a course teaching experimental methods. He grabbed the challenge. Recruiting Gerry Jackson, they dived right in and the result was our Microwave Laboratory course, still a USPAS hallmark.

Our talks on the scholarly value of beams continued into the nineties and, as the decade progressed, I kept pushing for accelerator articles to be published in Physical Review, using my long-term Secretary-Treasurer position in the DPB as base. At the time, beam physics was essentially limited to the two journals, Particle Accelerators and Nuclear Instruments and Methods. But something kept gnawing at me. Since we were part of the APS, why shouldn't beam physics have a place in Physical Review, the society's flagship journal? My efforts had a small effect and articles on beams did begin to appear here and there, but they were spread thin in the various sectors of the journal. We just did not have a place that we could call our own. To accomplish that took Bob's ingenuity and know-how. He discovered the path that gave Beams

and Accelerators a true publication home in Physical Review. The story of how Bob, with Marty Blume's visionary thinking and collaboration, created the electronic journal, Physical Review Special Topics – Accelerators and Beams, is truly a fascinating one. But that happened long after I was out of the picture and I'll leave that story to Marty.

Since I heard of his impending passing, many a time thoughts of him would appear spontaneously in my consciousness. I recall our breakfast dialogues at hotels during conferences and meetings all over. And how could I forget the private strategy sessions we engaged in! There were instances that we found ourselves in a moral dilemma. I remember, in the late nineties when I was on the verge of retiring, TD Lee asked me to organize a USPAS School in Beijing. To me, it seemed a good thing to do, for USPAS and for US-China relations. But when I asked Bob to teach one of his patented experimental courses in microwaves, he surprised me. Not only did he refuse but he voiced concern about teaching the communist Chinese advanced technology. I disagreed with him and went on with the program. However, I must say that as time passed, I began to wonder whether perhaps his moral instinct might have been superior to mine.

Yes, Bob was a great friend and the loss of this strong-willed yet thoughtful man will be felt not only by those he was close to but also by the accelerator community as a whole. What a great force he was. He lifted up accelerator education at Cornell, Stanford and at USPAS. Also, recognizing the scholarly value of accelerators and beams, he promoted its publication and was the key figure in giving the field an independent journal within the prestigious Physical Review. There is no doubt that both professionally and personally he will be sorely missed.

Bob Siemann

**Brookhaven and the American Physical
Society**

Marty Blume

**APS Editor-in-Chief Emeritus
BNL Emeritus**

DESIGN AND PERFORMANCE OF THE MULTIPARTICLE ARGO SPECTROMETER SYSTEM*

E. W. ANDERSON[†], G. B. COLLINS⁺, G. P. FISHER[§], E. LAZARUS, K. M. MOY^{**},
A. RAMANAUSKAS^{††}, P. SCHÜBELIN, R. SIEMANN⁺⁺, A. M. THORNDIKE,
F. TURKOT^{§§} and L. VON LINDERN^{***}

Brookhaven National Laboratory, Upton, New York 11973, U.S.A.

T. S. CLIFFORD^{†††}, J. R. FICENEC, D. R. GILBERT⁺⁺⁺, W. N. SCHREINER^{†††},
B. C. STRINGFELLOW^{§§§} and W. P. TROWER

*Virginia Polytechnic Institute and State University, Blacksburg, Virginia 24060, U.S.A.*****

A. R. ERWIN^{††††}, L. J. GUTAY^{++††}, E. HARVEY^{††††}, N. C. HIEN^{*****}, A. LAASANEN^{††††},
G. P. LARSON^{††††}, M. O'NEILL^{§§§§}, W. SELOVE^{§§§§}, K. STANFIELD^{++††} and R. B. WILLMANN^{††††}

Received 24 September 1974

The Multiparticle Argo Spectrometer System, MASS, consists of a magnetic vertex spectrometer, two external trigger spectrometers, an on-line monitoring system and an off-line data analysis chain. The performance of MASS has been evaluated from physics data taken in a study of 28.5 GeV/c pp collisions covering a large range of transverse momentum.

Bob and the BNL Light Source 1983-1987

I must disagree with your characterization of the relationship between machine builders and users as a "service- to- customer commitment". Design and construction of an advanced state-of-the-art device is more than that, and we do not regard users as customers, but rather as fellow scientists working on different aspects of the problems. In any event, We hope to have the x-ray ring on line as soon as possible.

With best wishes,

Sincerely,

A handwritten signature in dark ink, appearing to read 'M. Blume', written in a cursive style.

M. Blume
Associate Director

MB/jns

Bob and the APS

- Division of Physics of Beams
- Physical Review Letters and Reviews of Modern Physics
- Physical Review Special Topics - Accelerators and Beams

Physical Review Special Topics— Accelerators and Beams

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Essay: Accelerators, Beams and *Physical Review Special Topics - Accelerators and Beams* (May 22, 2008)

Robert H. Siemann¹, Stanford Linear Accelerator Center, Stanford University, Stanford, California 94305, USA

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Accelerator science and technology have evolved as accelerators became larger and important to a broad range of science. *Physical Review Special Topics - Accelerators and Beams* was established to serve the accelerator community as a timely, widely circulated, international journal covering the full breadth of accelerators and beams. The history of the journal and the innovations associated with it are reviewed.

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I. ACCELERATOR SCIENCE AND TECHNOLOGY

Accelerators and the beams they produce are critical for a broad range of sciences. In addition, the study of accelerators and beams is, by itself, an interdisciplinary science with interplay between considerations ranging from basic physics to engineering. It is a discipline that evolved over time as the small accelerators that were commonplace at universities were replaced by the national and international laboratories needed for exploration of the energy frontier of particle physics. Another equally important trend has been the increasing breadth of science enabled by accelerators. These factors have resulted in profound changes in the practitioners of accelerator physics and technology. They evolved from being predominantly university based and engaged with accelerators as part of research in particle physics to being concentrated at laboratories and having accelerators as their central discipline.

There are interconnected consequences of this evolution including a tenuous connection with universities, a service component of accelerator science, and informal ways of disseminating results. Universities are the heart of intellectual and scholarly endeavors with their vibrant atmosphere, breadth of pursuits, and the stimulation that comes from students bringing energy, excitement, and creativity to their studies. The service component of accelerator science was an inevitable development because funding is almost always tied to the science being served rather than to the instruments producing the science. Finally, the standards of universities for promotion and recognition, which include publication of peer reviewed articles, are not as important for a service profession, and accelerator work was often published informally.

These consequences are natural and there would be little downside if accelerators were not the limiting factor in so many sciences. Operating accelerators are always striving for better performance: higher beam current, higher luminosity, greater beam brightness, etc., and optimizing performance and controlling costs are of major importance in designing new accelerators. Often these will require understanding basic accelerator physics. The electron-cloud effect is an example with critical importance for B-factories, spallation neutron sources, and the Large Hadron Collider, among others. The frontiers of science are often shaped by accelerator physics and technology. As examples, superconducting rf has opened up a world in nuclear physics and neutron production; the short x-ray pulses from free electron lasers will revolutionize atomic and molecular physics, and accelerator science will determine the future of its progenitor, particle physics, where inventions are needed to overcome size and cost limitations.

Robert H. Siemann

The accelerator field must have breadth, depth, and creativity to meet the challenges of the future. This can only be accomplished by attracting excellent scientists and engineers and educating the young people so crucial for the long term. While members of the accelerator community are making essential contributions to a range of sciences, their peers are other accelerator scientists and their professional interests are related to accelerators and beams. Therefore, it is essential to value the advancement of knowledge of accelerators and beams as well as contributions to other sciences.

II. THE DIVISION OF PHYSICS OF BEAMS

The Division of Physics of Beams (DPB) of the American Physical Society (APS) was an important step in doing this. The division was established in 1985 with the objective of "the advancement and diffusion of knowledge regarding the nature and behavior of beams and the instruments for their production and use. ...In addition, the Division of Physics of Beams: (i) promotes research and development in the science of beams; (ii) encourages scholarly publication; (iii) promotes education in beam science and technology; and (iv) enhances the professional standing of its members" [1].

Encouraging scholarly publication is one of the DPB objectives. Peer review, which authors and readers benefit from substantially, is an integral part of scholarly publication. A knowledgeable colleague carefully reads a manuscript and gives a frank opinion whether it presents correct, new and important results. In addition, the referee often gives helpful criticism that can improve content and presentation. Acceptance and publication of a manuscript is an indication that the work has been judged to be making a meritorious contribution. For this reason peer reviewed publications are considered a measure of an individual's scholarly accomplishments. This measure is particularly important for universities, and it can play a crucial role in hiring and promotion.

The DPB initiated a study of peer reviewed publications in accelerator and beams in 1997. A conclusion of the study was that while many

» Meetings and Events

journals published such articles there was no single, widely circulated journal with this as the focus. As a result the literature was fragmented and not effective as a means of communication within the accelerator community. The division recommended establishing a scholarly, peer reviewed journal devoted to the science and technology of accelerators and beams that would (1) cover the full breadth of accelerators and beams, (2) be timely, (3) be inexpensive to promote wide circulation, and (4) be international with an international editorial board and pool of referees. This recommendation led to the establishment of *Physical Review Special Topics - Accelerators and Beams* (PRST-AB) [2].

III. THE EARLY DAYS

The recommendation reached the American Physical Society at a most opportune time. Marty Blume was the APS Editor-in-Chief. He had previously served as Chair of the National Synchrotron Light Source Department at Brookhaven, and this experience gave him appreciation for accelerator science itself and for the contributions accelerators were making to a wide range of science. In addition, he understood the intimate connection between accelerator science and accelerator technology, and he was willing to champion an editorial policy of publishing both science and technology. This was critical for covering the full breadth of accelerators and beams.

The other factor that made the timing opportune was that the APS recognized that scholarly publication was moving rapidly from hardcopy to electronic format. A small journal like PRST-AB would be an ideal initial effort where lessons could be learned before embarking on electronic versions of the much larger, established *Physical Review* journals.

Electronic publication satisfied the objectives of timely publication and the potential of wide circulation. However, it had the problem of covering costs, which include, among other things, editorial expenses, composition and production costs, and the costs associated with buying and maintaining servers. Charges to authors or readers would be a significant drawback when establishing a new journal, and it was decided to seek sponsorship from laboratories with large accelerator programs instead. Eight U.S. laboratories agreed to be the initial sponsors, and the APS covered the shortfall in the beginning as a service to the accelerator community and in recognition that PRST-AB was a testing ground for electronic publication. As a result, PRST-AB was available to authors and readers at no cost and without a subscription.

Sponsorship has grown steadily, and PRST-AB now has twenty sponsors: (i) Argonne National Laboratory, (ii) Brookhaven National Laboratory, (iii) The Cockcroft Institute, (iv) Cornell University Laboratory for Elementary-Particle Physics, (v) Deutsches Elektronen-Synchrotron (DESY), (vi) 11th European Particle Accelerator Conference (EPAC '08), (vii) European Organization for Nuclear Research (CERN), (viii) Fermi National Accelerator Laboratory, (ix) Gesellschaft für Schwerionenforschung mbH (GSI), (x) INFN-Laboratori Nazionali di Frascati, (xi) INFN-Laboratori Nazionali di Legnaro, (xii) INFN-Laboratori Nazionali del Sud, Catania, (xiii) Lawrence Berkeley National Laboratory, (xiv) Los Alamos National Laboratory, (xv) National Superconducting Cyclotron Laboratory at Michigan State University, (xvi) Oak Ridge National Laboratory, (xvii) Princeton Plasma Physics Laboratory, (xviii) Stanford Linear Accelerator Center, (xix) Thomas Jefferson National Accelerator Facility, and (xx) TRIUMF—Canada's National Laboratory for Particle and Nuclear Physics. Because of their generous support, PRST-AB continues to be provided without charge to authors or readers.

FIG. 1. PRST-AB submissions and publications.

Publication of PRST-AB was approved by the APS Council in November 1997, and I was appointed the Editor shortly afterward. A critical meeting was held at the APS Editorial Office in early 1998. It was decided to move forward on an aggressive schedule with the target of being prepared to accept the first submissions in March. This was accomplished. The first PRST-AB submission [3] was received on March 2 and published on May 12. PRST-AB has grown steadily since that first submission (Fig.). In 2007 193 papers were submitted, and 129 papers were published.

IV. INITIATIVES AND INNOVATIONS

PRST-AB is a small, electronic journal in a specialized field with a publisher who is interested in and encouraging of innovation. Sponsorship was one of the most profound and significant ones. As a consequence of sponsorship PRST-AB is freely available, making it one of the first open access journals and a harbinger of the developments at the forefront of scientific publication today.

It was a goal from the beginning to make PRST-AB a truly international journal. Initially this was done informally through the selection of members of the Editorial Board and by using referees from all over the world. These practices have continued to this day. A more formal arrangement was made in 2002 with the European Physical Society Accelerator Group (EPS-AG) [4] when they joined with the APS-DPB to form the Affiliated Professional Groups of PRST-AB. Together they share the responsibility for the health and vitality of the journal by providing advice and encouraging scholarly publication in accelerator science and technology. The EPS-AG has been instrumental in securing sponsorship from laboratories in Europe, and as part of this agreement either the Editor or an Associate Editor is from Europe.

There were a number of innovations related to the electronic only format. These included the following.

All electronic editorial process.—All communications with authors, referees, and editors were electronic and provided the first APS experience with an all electronic editorial process. PRST-AB is now integrated with the “paperless” office used for all the APS journals.

Special editions, conference editions, and special collections.—The all-electronic nature allows editions of peer reviewed articles that are

associated with conferences or that are about a closely related subject. These articles are published in a regular monthly issue as well as being added to the "special" Table of Contents that is updated when the article is published.

"Virtual Journal" of accelerator related articles published by the APS.—Links to beam physics articles in *Physical Review Letters* and *Physical Review E* are available on the PRST-AB Table of Contents so readers can find links to all of the accelerator related articles published by the APS in one location.

Single column PDF format.—The standard, double-column APS format is not easy to read on a computer screen, and PRST-AB articles are published in single- and double-column formats.

Movies.—Some things can best be visualized with a movie rather than a static figure, and movies can be included in articles. This has not been used by many authors to date, but looking at *Optics Express* [5] gives one an appreciation for the potential of this manner of presentation.

Readers requested other features that have been implemented.

Review articles.—Reviews of topics of particular interest to accelerator scientists and engineers are published.

Monthly Email notification.—The Table of Contents of each monthly issue is Emailed to interested parties. More recently RSS feeds have been made available for all the *Physical Review* journals.

V. PRST-AB, ACCELERATORS AND PUBLISHING

PRST-AB is a timely, widely circulated, freely available, international journal devoted to accelerator science and technology. It has become the premier scholarly, peer reviewed journal in this field. It is part of what is needed to advance the knowledge of accelerators and to attract excellent scientists and engineers. By doing this it will have a significant impact both within this field and in the broader range of sciences that depend on accelerators.

In addition, PRST-AB had the good fortune of opportune timing combined with a publisher interested in serving the physics community and exploring new methods of publication. As a result PRST-AB was an early, successful experiment in electronic, open access scientific publication.

PRST-AB would not have been possible without Marty Blume. Debbie Brodbar is the PRST-AB Senior Assistant Editor. She began her crucial role of providing assistance with and insight into PRST-AB editorial matters from the very beginning. Chris Wesselborg was the Project Manager who oversaw the implementation in early 1998 that took the journal from an idea to realization. I wish to thank Judy Franz, the APS Executive Officer, and Tom McIlrath, the APS Treasurer and *Physical Review* Publisher, for their support of PRST-AB and the accelerator community. PRST-AB started as an initiative of the Division of Physics of Beams. Martin Reiser was the DPB Chair who commissioned the study of accelerator publication and was a strong backer of establishing PRST-AB. Mel Month was the DPB Secretary/Treasurer for many years. I learned much from him about accelerator science and scholarship. Many people at the APS Editorial Office in Ridge, NY, made important contributions. In particular, I want to acknowledge Stanley Brown, Bob Kelly, Bill Rushka, Mark Doyle, Arthur Smith, Margaret Malloy, Joanna Popadiuk, and Evelyn Flores. Angie Seymour and Stephanie Santo provided invaluable editorial support for me at SLAC. Mike Poole, Francesco Ruggiero, Brant Johnson, and Chris Wesselborg were Associate Editors during my tenure, and I wish to thank them for their contributions and hard work. Thanks also to the members of the Editorial Board. They were an excellent sounding board and always willing to help with difficult papers. Frank Zimmermann is the present Editor, and Georg Hoffstaetter and Brant Johnson are the Associate Editors. They took over PRST-AB about a year ago, and I am delighted to see it thrive under their leadership. I hope they find the satisfaction that I did.

¹Robert Siemann is a Professor of Particle Physics and Astrophysics at the Stanford Linear Accelerator Center. He earned a Sc.B. degree in physics from Brown University in 1964 and a Ph.D. in physics from Cornell University in 1969. He returned to Cornell in 1973 as a faculty member in the Physics Department after a postdoctoral appointment at SLAC and a staff position at Brookhaven National Laboratory. He rose through the faculty ranks at Cornell, and his professional interest shifted from experimental particle physics to accelerator physics. He joined the SLAC faculty in 1991 and is also a Professor of Applied Physics, by Courtesy, and the head of the Advanced Accelerator Research Department. His present research is in the areas of plasma and laser-driven acceleration. He was the Editor of *Physical Review Special Topics - Accelerators and Beams* from its founding in 1998 through 2007.

1. Abstracted from the DPB homepage <http://units.aps.org/units/dpb/index.cfm>.
2. <http://prst-ab.aps.org/>
3. F. Clapier *et al.*, *Phys. Rev. ST Accel. Beams* 1, 013501 (1998).
4. The EPS-AG homepage is <http://epac.web.cern.ch/EPAC/EPS-AG/Welcome.html>.
5. *Optics Express* published by The Optical Society of America and available at <http://www.opticsexpress.org>.

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Viewpoint

Join the open-access revolution

Ken Peach argues that particle physicists can lead the way in a paradigm shift in scientific publishing to give everyone free access to research results.

There is a quiet revolution under way in academic publishing that will change how we publish and access scientific knowledge. "Open access", made possible by new electronic tools, will give enormous benefits to all readers by providing free access to research results.

The scientific articles published in journals under the traditional publishing paradigm are paid for through subscriptions by libraries and individuals, creating barriers for those unable to pay. The ever-increasing cost of the traditional publishing methods means that many libraries in Europe and the US - even the CERN Library, which is supposed to serve international researchers at a centre of excellence - are unable to offer complete coverage of their core subjects.

Ken Peach

In 2003 the Berlin Declaration on open access to knowledge in the sciences and the humanities was launched at a meeting organized by the Max Planck Society. Six months later, the first practical actions towards implementing the recommendations of the declaration on an international level were formulated at a meeting held at CERN in May 2004. So far the declaration has been signed by 61 organizations throughout the world, which are now taking concrete measures for its implementation.

An obvious prerequisite for open access is that institutions implement a policy requiring their researchers to deposit a copy of all their published works in an open-access repository. The Council for the Central Laboratory of the Research Councils' library committee in the UK sponsored such a project, ePubs, with the aim of achieving an archive of the scientific output of CCLRC in the form of journal articles, conference papers, technical reports, e-prints, theses and books, containing the full text where possible ([CERN Courier](#) May 2005 p44).

The feasibility study, carried out from January to March 2003, demonstrated the business need for this service within the organization. The data, going back to the mid-1960s, can be retrieved using the search interface or the many browse indices, which include year, author and journal title. In addition the ePubs system is today indexed by Google and Google Scholar. The scientific content of the system has further led Thomson ISI (the provider of information resources including Web of Knowledge and Science Citation Index) to classify ePubs as a high-quality resource.

The next step is to encourage the researchers - while of course fully respecting their academic freedom - to publish their research articles in

Author:

Ken Peach

open-access journals where a suitable journal exists. In recent years new journals applying alternative publishing models have appeared in the arena. The problem so far is that none of these journals have a long-term business model. They are sponsored either by a research organization or by other titles in the publisher's portfolio, or enjoy sponsorship that will not last forever.

Scientific publishing has a price and will continue to have a price, currently mainly covered by academic libraries through subscriptions. Moving to an open-access publishing model should dramatically reduce the global cost for the whole of the academic community. The publication costs should be considered a part of the research cost and the research administrators should budget for these when the research budgets are allocated. However, a change must not take place without safeguarding the peer-review system, which is the guarantor of scientific quality and integrity.

Outside biology and medicine, few journals that support open access are given the same academic credits as the traditional journals. This situation is further reinforced if there is a direct coupling between research funding and the "impact factors" of journals where results are published. However, by taking the risk and publishing important work in new journals that implement the open-access paradigm, the impact factor will automatically be enhanced.

The example of the *Journal of High Energy Physics* (JHEP) is striking. This relatively new journal was launched by the International School for Advanced Studies (SISSA) in Trieste in 1997. Today some studies give it an impact factor close to that of Physical Review Letters in publishing papers on high-energy physics. JHEP was launched ahead of its time and was forced, because of the lack of financial support, to become a subscription journal. However, with the support of the main physics laboratories, it would be possible in the present climate for this successful journal to enter the open-access arena once again.

If a change is wanted, it is up to us. Particle physics cannot change the world alone, but a clear position among our authors and our members of editorial boards will have a strong synergy with our colleagues pulling in the same direction in other fields.

• For more about the Berlin Declaration see
www.zim.mpg.de/openaccess-berlin/berlindeclaration.html.

Author:

Ken Peach, director of particle physics, CCLRC Rutherford Appleton Laboratory.

Article 31 of 31.

Open access in accelerator physics

It pleases me that the CERN Courier, itself an open-access journal, has put the debate of scientific publishing on the agenda. Ken Peach's "Viewpoint" (CERN Courier June 2005 p50) is an excellent contribution to the discussion and should be carefully read by everyone in the publication chain.

In the field of accelerator physics a major fraction of our literature is nowadays made available through open-access publications. Our main conference series, i.e. PAC, EPAC, and APAC, are all published as open-access through JACoW (<http://accelconf.web.cern.ch/AccelConf/>). Our scientific articles are to a large extent submitted to Physical Review Special Topics – Accelerators and Beams (PRST-AB), a peer-reviewed, all-electronic journal published by the American Physical Society. The journal is available to everyone without subscription or pay-per-view fees and even without author charges, all thanks to the support of sponsors.

PRST-AB has quickly become the primary means of communicating new results in accelerator physics, but so far, unfortunately, no European institutions are among its sponsors. I hope that this situation will soon change to ensure the continuity of open-access publishing in accelerator physics.

Frank Zimmermann, CERN.

A question of sponsorship

Ken Peach's article on open access was met, on my part, with a combination of a grain of salt and wry amusement. The American Physical Society started an all-electronic open-access journal called Physical Review Special Topics – Accelerators and Beams that has been successfully published since 1998. The journal has been endorsed by the Division of the Physics of Beams of the APS and the Accelerators Group of the European Physical Society. It has been supported by sponsorship contributions from large accelerator laboratories in the US and Canada, with no charges either to authors or to readers. This only partially covers its costs, and the shortfall has to be made up by the APS. This is sustainable in the short term, but becomes an increasing burden as more articles than the current 10 per month are published.

Periodically I hear of endorsement by one or another part of CERN for open-access journals, but in spite of occasional requests CERN

has declined to become a sponsor of PRST-AB. To date 52 articles have been published with one or more CERN authors, and more have been based on results from CERN accelerators. Isn't it time that CERN – one of, if not the, largest accelerator laboratories in the world – came forward?

Martin Blume, editor-in-chief, APS.

Sponsoring open access: more than just wry amusement

(November 2005)

I am delighted to witness the high interest in open access that exists and has been demonstrated by the series of letters on the topic recently published in these pages. In addition, a well-attended debate on "The changing publishing model" held at CERN in September shows that physicists are concerned with this issue.

Following the endorsement of the new CERN publishing policy in March, the library has taken certain steps to support open access where it can. It is therefore my pleasure to announce that CERN will be, as of 2006, the first European financial sponsor of Physical Review Special Topics – Accelerators and Beams. I hope publishers and editors of journals in particle physics will also consider a similar model: in the current climate a publishing model based on sponsorship shows significant potential, at least for the mega-sciences.

For the future, I would like to propose that we do not mix peer-review and long-term archiving into the debate. Both are of obvious importance, but neither is relevant to the difference between open-access publishing and the traditional model. The open-access model can of course incorporate peer-review, and with regards to archiving, even traditional subscription journals will most likely be electronic-only in a few years. The real debate is how to finance publishing activities and at the same time ensure equal access for everyone to results from publicly funded research.

A change to full open-access publishing cannot be made by one of the actors in the publishing chain alone. CERN is therefore hosting, in December, a tripartite meeting, which will include funding agencies, research organizations and publishers who already implement some form of open-access publishing. At the workshop we will hope to use our years of open-access experience to work

out a common strategy for the transition to the new publishing paradigm.

Corrado Pettenati, CERN Library.

Remarks from the APS Chair
Swapan Chattopadhyay

Well, this has been a wonderful day of reminiscing....and celebrating Bob and his multi-faceted contributions – as an outstanding scientist, teacher, mentor, severe critic of mediocrity at all times and most notably, an untiring servant of the professional society that we call the APS (American Physical Society). On behalf of the entire community of scientists that hails APS as its professional home, in particular on behalf of the scientists and engineers who call APS's Division of Physics of Beams as their home, it is my distinct privilege, honor, and humility to say 'thank you' to the entire Siemann family for sharing this extra-ordinary family member with us in all its glory and grief...!

I have shared a meal or two with Hannah while on invitation from Bob at Cornell or SLAC as symposium/colloquium speaker. I have known about Hannah's love of horses. I have heard about his children, though they do not know me. I have known Bob's frustration while spending time away from Cornell at Fermilab in transition to SLAC; I have known the brilliance of his students and post-docs who he shared with me over time; I have known his impatience as a collaborator in plasma-acceleration experiments at SLAC.

But it is his total dedication to the society APS and all it entails—DPB, Particle Accelerator Conferences, the APS journal in our field PRST-AB, ... many of which he has founded and created himself—that I must uphold, celebrate and bring to our collective acknowledgement from my platform. And it is with this sense of gratitude that I will close my remarks today—reinforcing Persis Drell's announcement this morning—that APS, and its Division of Physics of Beams, is motivated more than ever to contribute to Siemann-fellowship and I pledge to work with our Executive Committee to make it happen.

To "Bob"
Swapan Chattopadhyay
Chair, APS-DPB

Robert H. Siemann Symposium Closing

E. R. Colby, 7/7/09

Thank you, Swapan, for sharing remarks from the APS.

I have the privilege of closing the Symposium today by offering a few final comments and words of thanks to those who helped make this event possible.

I met Bob at an annual review of Fermilab in 1996. I was an impressionable grad student presenting a poster; he was the extremely tall committee member who questioned me at length. He asked pointed questions about the simulations and the hardware I had developed. He warmed toward the end of the exchange, leaving me feeling relieved that I had not damaged the program. What I'd not appreciated was that I had been interviewed.

This combination of technical insight and, to use one of Bob's favorite words: 'craftiness', were two of his essential attributes. These traits, together with the conviction that accelerator science is an intellectual pursuit worthy of a university that has given rise to a tremendous legacy.

Bob's contributions to accelerator science span more than 200 publications, covering such diverse topics as wakefield theory, pulsed heating, acceleration of electrons with light, and energy doubling of the SLAC linac, among others.

Bob's technical judgment was prized by the community. Through service on more than 100 national and international committees, Bob advised governments, lab directors, corporations, accelerator schools, machine designers, journal editors, and others. He served on panels that established the long-term directions of High Energy Physics, Light Sources, Fusion and Nuclear Research. He gave advice on such diverse machines as the Tevatron, SSC, Advanced Photon Source, Laser Interferometer Gravitational-wave Observatory, and Spallation Neutron Source.

Trained with one of the last generations to span both the accelerator and experimental sides of high energy physics, Bob willingly rolled up his sleeves and engaged in theory, computer simulation, experiment, and wire wrapping. [explain wire wrapping]. He was as excited about discovering physical connections between the subtleties of complex analysis and spectral features of wakefields as he was seeing a new BPM circuit work flawlessly. Bob often relaxed on Saturday mornings in the electronics lab developing new circuitry for his experiments.

Bob appreciated that progress in accelerator design required more than technical prowess. Advancements in the performance of accelerators—of the scale and magnitude depicted in the Livingston plot—require deep understand of electromagnetism, material science, and an insight that penetrates faster than simulations can provide. This portion of the craft—the science of accelerators—demands the mathematical rigor and clear-headed application of the scientific method to uncover subtleties that are not evident from a

“simple application of Maxwell’s equations”. This portion of the craft is clearly worthy of the University.

Bob’s conviction in this idea contributed to the expansion of Stanford’s academic charter to admit accelerator science, and accelerator scientists as full professors. It led to the founding of a department—with the character of a university research group—dedicated to accelerator science. Through this department—as earlier at Cornell—Bob trained many young students in accelerator science. He was widely respected as a teacher, giving his time willingly when asked, and even when the frowning face of the little statue on his desk was facing the door. (This was Bob’s “do not disturb” sign).

Bob developed courses in accelerator physics and in experimental techniques, teaching at the USPAS, NATO ASI, and US-CERN joint accelerator school in addition to Cornell and Stanford. In several cases, the courses were the first of their kind—for example the USPAS Microwave Laboratory developed with Gerry Jackson—and are still a valued part of the curriculum today.

Bob contributed to the founding of a new unit of the American Physical Society to provide a new forum and voice for beam physicists, and served as an early chair of the division.

He contributed to the establishment of a new segment of the prestigious Physical Review journal dedicated specifically to accelerator science, where he served for a decade as Editor-in-Chief. Where there once was diffused documentation of accelerator science through the conference proceedings and journals, there now is a central archival journal for recording this body of work. The journal is innovative both in content and in form: it’s both free and all-electronic.

I hope you will join me in a round of applause in recognition of all that Bob has done for our field.

Bob trained more than 30 young scientists and touched the lives of hundreds more. These individuals today contribute, as Bob did, to a wealth of human endeavors ranging from computer chips, to medicine, to solar power, to—of course—accelerator physics.

I am grateful for opportunity to have known and worked with him.

[thanks to organizers]

Thank You

Joan Valine
Naomi Nagahashi

Stephanie Santo “Nixx” Arias

Jimmy Pham, Terry Anderson, Chip Dalby, Stephanie West
Rod Reape & the Communications Group

Members of AARD

Thank You

Symposium Speakers

Persis Drell, Roger Erickson, Dick Talman, Gerry Jackson, Burt Richter,
Tor Raubenheimer, Boris Podobedov, Ping-Jung Chou, David Pritzkau,
Stuart Henderson, Tom Katsouleas, Chris Barnes, Bob Byer, Ben Cowan,
Dave Sutter, (Mel Month), Marty Blume, and Swapan Chattopadhyay

Symposium Committee

Marty Blume, Bob Byer, Alex Chao, Mark Hogan,
Gerry Jackson, Chan Joshi, Maury Tigner

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Special Thanks

Persis Drell

Hannah Siemann

Robert Herman Siemann
1942-2008

Tree Dedication

E. R. Colby, 7/7/09, 5pm

Good afternoon. We're gathered here to dedicate this tree in Bob's memory.

This is a Coastal Redwood tree, a native of the northern coasts of California, where it draws moisture from the fog and the streams. It routinely grows to heights exceeding 300 feet, making it the tallest living thing on earth. Its wood is highly prized for its extraordinary resistance to termites and rot. It is also an endangered species, having been logged aggressively in the early 20th century to supply railroad ties and structural members for the railroads of the Pacific Northwest.

It is appropriate, then that this adaptable, hardy, and extraordinarily tall tree is planted in memory of a man who shared these qualities. Bob nurtured the professional growth of many of us who are here today, and cast a long shadow over his scientific field. In a few short years this redwood, like Bob, will both provide shelter and cast its long shadow at SLAC.

A dedicatory plaque has been placed in Bob's memory; mounted to a stone donated by Hannah Siemann from their estate, providing a "piece of home" upon which to display it.

[Hannah Siemann uncovers the plaque]

The inscription reads:

"In Memory of

Robert H. Siemann

Pioneering accelerator physicist,
respected leader and mentor

1942-2008."

This tribute was the idea of Joan Valine, who ably brought the idea to reality. Thank you Joan, for making this fitting memorial to Bob happen.

[Group photo]

[Reception]